WHEN DO RESEARCHERS COLLABORATE?
TOWARD A MODEL OF COLLABORATION PROPENSITY
IN SCIENCE AND ENGINEERING RESEARCH

by

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to the memory of

Kenny Birnholtz

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ABSTRACT

Geographically distributed and multidisciplinary collaborations have proven invaluable in answering a range of important scientific questions, such as understanding and controlling disease threats like SARS and AIDS or exploring the nature of matter in particle physics. Despite this, however, collaboration can often be problematic. There are institutional obstacles, collaboration tools may be poorly designed, and group coordination is difficult. To better design technologies to support research activities, we need an improved understanding of why scientists collaborate and how their collaborations work. To achieve this improved understanding, this study compares two theoretical approaches to collaboration propensity—that is, the extent to which collaboration is perceived as useful by individual researchers.

On one hand, cultural comparisons of disciplines suggest that collaboration propensity will be higher in disciplinary cultures that have a more collectivist orientation, as indicated by low levels of competition for individual recognition and few concerns about secrecy related to commercialization and intellectual property. In contrast, an approach based on social and organizational psychology suggests that collaboration propensity will vary as a function of resource concentration, fieldwide focus on a well-defined set of problems, and the need for and availability of help when difficult problems are encountered in day-to-day work. To explore this question, a mail survey of 900 academic researchers in three fields was conducted, along with 100 interviews with practicing researchers at 17 sites in the field.

Results support a focus on work attributes in interpreting collaboration propensity. That is, cultural factors such as competition for individual recognition and concerns about intellectual property were not perceived as significant impediments to collaboration. Instead, characteristics like resource concentration and the need for coordination were more important in determining collaboration propensity. Implications of these findings include a call for more careful examination of the day-to-day work of scientists and engineers, and a suggestion that concerns about scientific competition impeding collaboration may be unwarranted.

CHAPTER 1

INTRODUCTION

Collaboration has become a significant focus of attention for researchers in many domains. In industry, science, and government there is a growing need to achieve outcomes that are only possible through the efficient and effective collaborative effort of multiple individuals, groups and organizations. Scientific research is a particularly active arena for collaboration driven both by the rise of "Big Science" (Galison & Hevly, 1992; D. J. d. S. Price, 1986) and by the emergence of collaboration technologies that facilitate work in geographically distributed groups (Finholt & Olson, 1997; Hesse, Sproull, Keisler, & Walsh, 1993; Newell & Sproull, 1982). Moreover, recent research and reports on this topic suggest that we are currently at a crucial junction point in the development and adoption of computer and networked-based collaboration technologies (Atkins et al., 2003). They go on to note, however, that we need a better understanding of how scientific collaboration works and what makes it desirable in order to develop and implement effective cyberinfrastructure (Cummings & Kiesler, 2003; Nentwich, 2003).

In this chapter, I introduce a study of the "collaboration propensity" of individual researchers in three fields. I will outline the importance and difficulties of effective collaboration, define the notion of collaboration propensity, outline the two theoretical approaches that this study will compare, and then describe the research context. In Chapter 2, I review relevant literature and outline a set of hypotheses to be tested and discussed. Chapter 3 discusses the methods used for the study, and Chapter 4 presents quantitative and qualitative results. Chapter 5 provides a summarizing discussion, along with implications for theory, practice and design.

Collaboration is Important

We have seen recently in particular that geographically distributed and multidisciplinary collaborations can be crucial in answering scientific questions of significant interest to society at large. At a general level, there are several reasons for this.

In the first place, some researchers study phenomena and systems that are too complex and interrelated for a single investigator to effectively understand in isolation. Ecological researchers, for example, have historically worked alone in the study of specific slices, or "patches," of natural ecologies. Ecologies, however, are complex systems with phenomena of critical interest occurring at multiple levels of analysis and at multiple scales, from single-celled organisms up to regional environmental quality issues. Studying slices of these systems in isolation has proven inadequate in understanding deeply connected phenomena, and there is an acknowledged need for collaboration to integrate data from multiple perspectives and scales to generate a more complete understanding of these systems (*National Ecological Observatory Network*, 2004).

Another reason for collaboration is to bring multiple perspectives to bear on difficult problems. Such an approach has been particularly valuable in recent approaches to public health threats, such as AIDS and SARS. This latter example is particularly illustrative in that during the initial outbreak of this disease, global teams of experts were rapidly convened in emergent collaborations that eventually proved successful. This is similar to global teams of AIDS researchers who, with a somewhat longer time horizon, bring together researchers from fields such as immunology, epidemiology and others in answering critical questions (G.M. Olson, Teasley, Bietz, & Cogburn, 2002).

Finally, some fields of research require experimental or observational "big science" apparatus that are sufficiently complex, massive and/or expensive as to be beyond the reach of individual laboratories or investigators. In order to do any research at all in these fields, it is frequently the case that researchers must affiliate themselves with a collaboration that has access to one of these instruments. Astronomy, for example, has a rich history of shared access to large telescopes (McCray, 2000). As I shall illustrate below, high energy physics is perhaps the canonical example of this type of collaboration, with thousands of researchers collaborating on the Large Hadron Collider (LHC) experiments at the European Organization for Nuclear Research (CERN) in Geneva, Switzerland.

Important to Funding Agencies as Well

The value of collaboration in answering important scientific questions is further evident in efforts by funding agencies to encourage collaboration via large initiatives, requiring collaborative proposals, and programs that foster collaboration between technologists and domain scientists. The U.S. National Science Foundation, for example, has put in place programs such as the Knowledge and Distributed Intelligence Initiative (*Knowledge and Distributed Intelligence Initiative*, 2004) that lists collaborative, multidisciplinary proposals as a baseline requirement for funding. At the National Institutes of Health (NIH), multi-team projects have been funded to tackle large-scale efforts, such as the Human Brain Project in the neuroscience community. There have also been many deliberate efforts to understand the complexities of collaborative science and take steps to encourage this type of research, evident in particular in the NIH director's "roadmap document" for research teams of the future(*Research Teams of the Future*, 2004). Moreover, such efforts are not confined to the United States. The E-Science Program in the United Kingdom has funded a wide range of collaborations between computer scientists and domain scientists (*About the UK E-Science Programme*, 2004). And Nentwich (2003) describes a range of "cyberscience" projects in continental Europe and elsewhere.

But…It is also Difficult

Even as collaboration becomes increasingly useful in solving problems of interest, it must be pointed out that collaborating successfully can be nontrivial on several dimensions. Specifically, there are institutional and social obstacles in the system of scientific research, the information and communication technologies developed to support collaboration are not always effective, and communication and coordination can be difficult for groups, particularly when geographically distributed.

In the first place, the reward system in science is fundamentally structured around individual reputation, which is augmented via high impact publications and widespread recognition (Whitley, 2000). The value of contributing to collaborative projects or community-wide initiatives is not always clear, and there may not be any rewards at all. This is of particular importance for junior researchers seeking promotion and tenure in fiercely competitive domains. Indeed, recent efforts by the National Institutes of Health to promote collaborative approaches to science sparked editorials in the high profile journals *Science* and *Nature* that listed numerous institutional disincentives for team science and questioned why any researcher would want to engage in collaborative work (Kennedy, 2003; "Who'd want to work in a team?," 2003).

In addition, information and communication technologies (ICT) developed to support collaboration are not always effective. One well known example of this in the scientific domain is the Worm Community System that was developed in the biological community (Schatz, 1991-2). In this case, the system designers paid inadequate attention to the research culture and practices of the target users (Star & Ruhleder, 1994). Finholt

(2003) provides other examples of difficulties experienced in the development of collaboratories, and Olson and Olson (2001) provide a more general overview of the difficulties involved in developing ICT to support collaboration more generally.

Finally, recent data from a study of multidisciplinary collaborations funded by the National Science Foundation suggest that the communication and coordination difficulties faced by geographically distributed collaborations had a negative impact on their productivity and effectiveness (Cummings & Kiesler, 2003). One interpretation of these results in light of the other difficulties associated with collaboration mentioned here is that there has been too much focus on technologies to support collaboration, and not enough focus on collaboration itself. Specifically, we have an insufficient and incomplete understanding of what makes individual researchers likely to collaborate. Indeed, even the most advanced collaboration tools are unlikely to be used by researchers who have no interest in or need for collaboration. By enhancing our understanding of the factors that motivate and sustain collaboration, we will be better equipped to assess the readiness and needs of individual researchers for collaboration tools.

What Do We Know About Collaboration

A look at the literature on scientific collaboration (reviewed extensively in Chapter 2) reveals that our understanding of the detailed nature of collaboration is quite limited. There is a great deal of work demonstrating the existence of collaboration by examining the frequency of co-authorships in different disciplines and demonstrating that this frequency is rising (D. J. d. S. Price, 1986). Evidence also suggests that collaboration frequently occurs within small groups of researchers well known to each other, which have been referred to as “invisible colleges”(Crane, 1972). There have also been studies, reviewed in Chapter 2, of factors that predict collaboration success or efficiency. In terms of understanding *why* and *when* researchers collaborate, however, the literature is essentially limited to a high-level discussion suggesting that collaboration provides access to vital resources (W. Hagstrom, 1965) and a basic enumeration of reasons why collaborations form that lacks systematic or detailed exploration of these motives (Beaver, 2001). None of this work, however, involves or facilitates systematic assessment of when collaboration is likely to occur. Such an understanding, however, is vital if we are to successfully design and implement useful and usable collaboration tools. Thus, there is a need for future research on what makes individual researchers likely to collaborate; or, in other words, for a better theoretical understanding of what I will call "collaboration propensity."

A Controlled Vocabulary

Before I proceed with describing collaboration propensity and the study conducted here, I wish to define a controlled vocabulary to be used throughout this document. As I will discuss several concepts that are both similar and different in conceptually important ways, it is my hope that this controlled vocabulary will allow for a more precise discussion and eliminate some potential for confusion. As in any case where specific connotations of more general terms are used, the reader may wonder whether or not my subjects understood these terms in the precise sense that I explicate below. I argue, based on several reviews of the survey instrument and pilot interviews with researchers in the field (all described in Chapter 3), that their understanding of these terms was not always precisely as I intended but that it was sufficiently clear to render valid the claims that I make in the remainder of this document. I will define the following terms:

Collaboration

A collaboration is defined here as a group of people who are involved in the collection and/or analysis of a single data set. Involvement can range from being a principal investigator on the study to playing a consulting role in statistical analysis. A "single data set" can be any size, but must hold together such that the collaborators would consider it to be a coherent whole.

Research Method

For the purposes of this study, a "research method" constitutes a general class of similar approaches to research questions. Research methods are, in turn, comprised of procedures, skills and techniques (described below). For example, I would consider "laboratory experiments" to be a data gathering method in the social sciences, but laboratory experiments vary by field and investigator in the specific procedures and techniques that are employed. In this way, we can speak of "experimental economists," for example, as a cluster of researchers who agree generally that experiments are a useful method for conducting economics research even though they may not agree on precisely how these experiments should be conducted.

Procedures, Skills and Techniques

Though one could easily spend time distinguishing between these three terms, this is not necessary for the purposes of this study. What is important here is that procedures, skills and techniques represent the way that individual researchers carry out research methods (as defined above) in answering questions of interest. This is an important distinction in that there are frequent cases where there is broad agreement at a high level on what methods are appropriate for conducting a class of research, but that individuals will either have different approaches to this method or where individuals bring expertise to bear on different components of this method. In the case of high energy physics, for example, most researchers agree that accelerators and detectors are the method of choice for isolating traces of previously unseen particles. Different researchers are involved, however, with somewhat different detectors (such as the two major experiments at CERN, described below) and it takes many forms of expertise and hundreds or thousands of researchers to actually build a detector.

Research Question

I use a conventional definition of "research question" here, but define this term in order to draw distinctions between this and a "problem" as defined below. A research question is a specific question about a topic of interest to a field or group of scientists that can be answered empirically through the collection and analysis of data.

Problem

For the purposes of this study, a "problem" is a specific difficulty or obstacle encountered in carrying out one's day-to-day work. The important thing to realize here is that a problem occurs at the same level of analysis as procedures, skills or techniques. To summarize several of the terms described here in the way that I intend for them to be used, a "research method" is used in addressing a "research question." Implementing the research method requires specific "procedures" and "skills." In carrying out these procedures, an individual researcher may encounter "problems" with which he or she requires assistance. By contrast, it would not be appropriate or possible to solve a "problem" by using a "research method," in the way that these terms are used here because a "research method" provides only a general class of procedures and a "problem" is quite specific and possibly contextually based. Similarly, it would not be possible to address a “research question” solely with “procedures” because the procedures need to be guided and shaped by a methodological approach.

Collaboration Propensity: Two approaches

Given the lack of existing theory that explicitly addresses collaboration propensity, one important goal of the present study is the identification of a theoretical frame that seems promising for this purpose. I have identified two relevant streams of literature from prior studies of science (though they do not, admittedly, address collaboration in an explicit way) that seem potentially useful. I will draw factors from both of these theoretical approaches here and this study will compare their power and value in predicting collaboration propensity.

The first theoretical approach draws on social and cultural studies of science, and in particular the work of Knorr-Cetina (1999) and Collins (1998). These studies, in different contexts, draw an important distinction between disciplinary cultures that are collectively focused and those that are more individually focused. In disciplinary cultures that are collectively focused, groups bear responsibility for making discoveries and, in turn, groups reap the credit that accrues as a result of these discoveries. In more individually focused disciplinary cultures, on the other hand, individuals are generally viewed as responsible for discoveries, and credit and recognition are given at the individual level. Knorr-Cetina, for example, contrasts the collectivist culture of large high energy physics experiments at CERN with the more individually focused cultures of molecular biology laboratories. In applying such a theoretical framework to collaboration propensity, we should expect to see higher collaboration propensity in disciplinary cultures that are more collectively focused. In other words, people should be more willing to work together if the disciplinary culture in which they do their work (and were socialized initially) is one that focuses on collective achievement rather than individual.

On the other hand, collaboration propensity might also be approached by using organization and coordination theories to consider attributes of the work being performed and the people performing it. In his characterization of the production of scientific knowledge in different fields, Fuchs (1992) incorporates concepts from these literatures in suggesting that the degree to which researchers depend on each other for access to scarce resources and the extent to which researchers are focused on a narrow and well-defined set of research questions can impact the way work is done and the means of production in different areas. In applying this general theoretical approach to collaboration, we would expect to see higher collaboration propensity where researchers are more dependent on each other and the level of focus on specific research questions in the field is perceived to be higher. In other words, people should be more willing to work together if they are dependent on others in order to get their work done and if their colleagues are likely to be working on fundamentally similar problems using similar methods.

There is, admittedly, some possibility for overlap between these two theoretical perspectives that must be addressed here. It might be argued, for example, that it is attributes of the work performed in high energy physics, specifically the need for very large, expensive and sophisticated apparatus that causes the culture of that field to be collective in its orientation. In other words, were it not for the necessity of large collaborations, physics would have a culture just as focused on individuals as molecular biology (to use the fields studied by Knorr-Cetina for illustration). This is potentially problematic for the study at hand in that it suggests some overlap between the theoretical perspectives being contrasted here. I argue, however, that this potential overlap is precisely that – potential. It merits careful consideration, but is not necessarily the case as one can imagine cases where the opposite is true. In the end, the data presented later suggest that there is little relationship between the factors used here to measure culture and the factors used to measure attributes of the work being performed.

The remainder of this document uses a combination of quantitative and qualitative data to explore and compare these two theoretical approaches to collaboration propensity. I will demonstrate that, in predicting collaboration propensity, the individual or collective orientation of a disciplinary culture, as operationalized here, turns out to be far less powerful than an approach focused on attributes of scientists' day to day work and its performance. Specifically, researchers' concerns about scientific competition in achieving widespread recognition for their individual accomplishments do not appear to be related to their collaboration propensity, and nor does their perception of formalized means for attributing credit to groups. On the other hand, a dependence on others for access to scarce or concentrated resources and the perception of widespread agreement on what constitutes quality research prove to be quite powerful in making this prediction. Moreover, the qualitative data suggest a strong willingness on the part of researchers to overcome cultural and institutional obstacles to collaboration when it is useful in getting work done and solving problems of interest.

Research Context and Rationale

I have chosen to conduct this research in three fields: neuroscience, earthquake engineering, and high energy physics. These were chosen deliberately because they differ in ways that are important to the theoretical approaches being taken here. In this section I will describe these fields and they ways in which they differ.

High-Energy Physics (HEP)

HEP is a field with a rich history of experimental discovery that cannot be chronicled here, but the story is well-told in other sources (Close, Marten, & Sutton, 2002; Galison, 1997). Experimental investigations rely heavily on high-energy accelerators, which seek to recreate the conditions at the start of the universe by accelerating electrons and protons to extremely high energies. By recreating these conditions, physicists are able to generate particles that do not occur naturally on earth under current, more stable, conditions. In order to track the existence and behavior of these particles, detectors are used. Detectors, such as the Toroidal LHC Apparatus (ATLAS) detector currently under construction in the LHC at CERN, sense and record the energy "trails" left by particles. These recordings are then analyzed to isolate specific particles, track their behavior, and compare them to theoretical predictions.

The apparatus involved in HEP research dwarf all other scientific instruments. The LHC, for example, is comprised by circular underground tunnels 27 kilometers in circumference (see Figure 2) and sits 280 feet underground. The ATLAS detector that will sit within the LHC tunnels will be 20 meters in diameter and weigh 7000 tons when it is complete (Close et al., 2002). The human scale of HEP research is correspondingly large. The ATLAS experiment, for example, involves over 1,800 physicists at 140 institutes in 34 countries around the world. Interestingly, and as I will describe in detail in Chapter 4, HEP has a tradition of listing all members of large collaborations as authors on all papers published by any member of the collaboration.

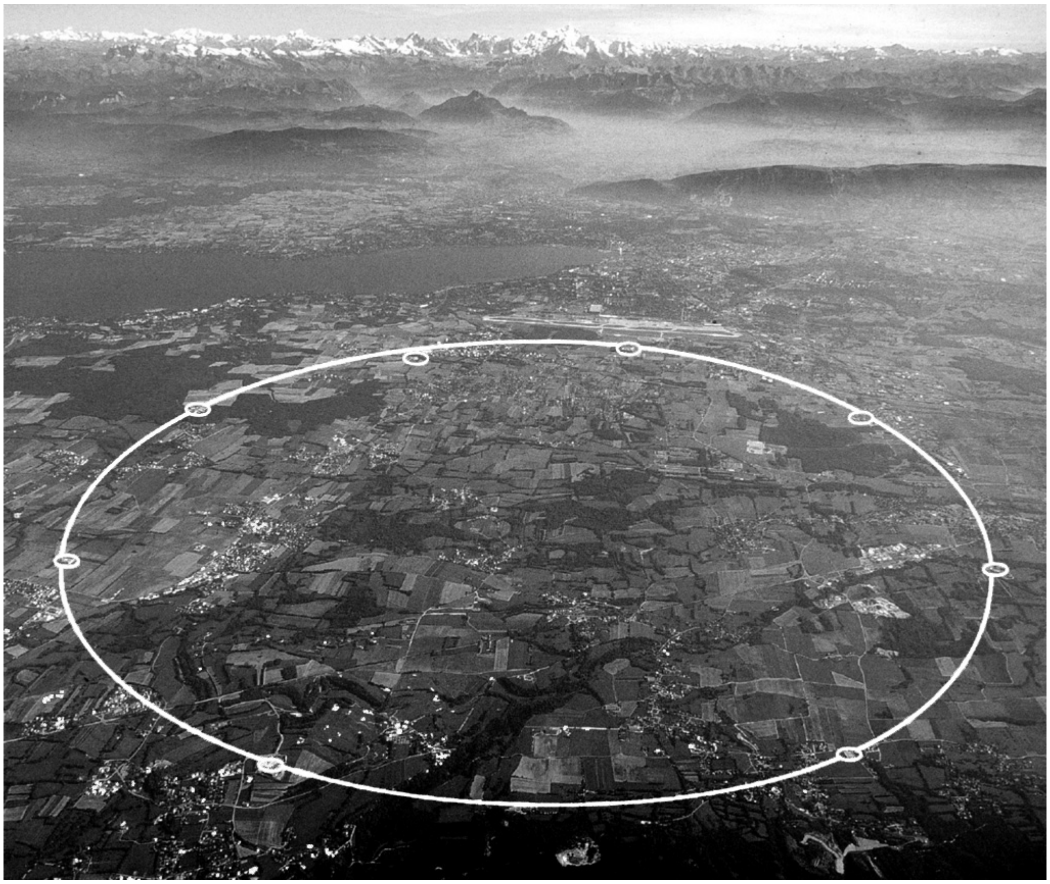

Figure 1 Aerial photograph depicting the location of the Large Hadron Collider at CERN with the Alps and Lake Geneva in the background (© CERN)

Earthquake Engineering (EE)

Earthquake engineering is a field dedicated to the mitigation of earthquake risks by:

> improving understanding of the impact of earthquakes on the physical, social, economic, political and cultural environment, and by advocating comprehensive and realistic measures for reducing the harmful effects of earthquakes" (EERI, 2003).

Experimental research in this area typically takes place in laboratory settings using specialized equipment such as shaking platforms (see Figure 1) and steel reaction frames for large scale structures, and centrifuges that proportionally increase the force of gravity for accurate modeling of soils and foundations at smaller scales (Zimmie, 1995).

Investigations typically involve exerting a simulated seismic force on a soil or structural specimen through the use of hydraulic actuators. The specimen is generally instrumented with a large number (several hundred in some cases) of sensors, from which numerical data that capture specimen performance can be acquired and analyzed. As I will illustrate later, earthquake engineering is focused largely on individuals, but there is some evidence to suggest that prestige and reputation also accrue at the level of the institution. Thus, in many respects, EE is situated between neuroscience and high energy physics.

Figure 2 Reduced scale bridge deck positioned on 3 biaxial shaking tables (Image courtesy of the University of Nevada)

Neuroscience

Neuroscience researchers, broadly speaking, seek to understand the workings of the brain with the goal of improved treatment and prevention of mental illness. This is achieved primarily through laboratory work and the statistical analysis of brain images, though there is also some interest in computational simulations of brain activity. Laboratory work frequently involves the analysis of brain tissue from mice, primates and humans using gene microarrays and other techniques. It is also common to use fluorescent protein tagging techniques and transgenic animals to isolate the expression of particular genes related to specific traits and illnesses. Though there is some evidence of large-scale efforts to integrate research activities in the field, such as the Human Brain Project and other data sharing efforts sponsored by the U.S. National Institutes of Health (Insel, Volkow, Li, Battey, & Landis, 2003; Koslow, 2000), most of the day-to-day work in neuroscience occurs in traditional bench-science laboratory space dedicated to individual researchers and their graduate students and postdocs. Where collaboration occurs, it is typically to share access to or bring multiple forms of expertise to bear on the analysis and production of different aspects of rare or expensive data sets. Moreover, neuroscience is generally held to be an example of a wider set of biomedical fields of research that are highly competitive and highly focused on individual researchers.

Figure 3 Researchers compare MRI images of a human brain (Image from: http://www.uhmc.sunysb.edu/neurology/original/training-program/Mri.jpg)

Comparing these fields

In these descriptions I have demonstrated that these fields differ along three dimensions that are important for the theoretical models being compared here. In the first place, they differ in terms of their collective vs. individual orientation. Neuroscience is highly focused on individuals, whereas massive HEP collaborations attribute credit and achieve outcomes only through collective effort. EE appears to be somewhere in between these extremes. Second, they differ in terms of their scale. Neuroscience operates at the smallest scale, with traditional bench science dominating the workload of most researchers, EE is somewhat larger with research taking place in large laboratories at individual institutions, and HEP is on a truly massive scale with research taking place only at a select few laboratories in the world. Finally, the fields differ in terms of their level of integration. Here, EE is arguably the least integrated in that research practices vary somewhat between labs and by equipment type, neuroscience is not well-integrated but there is some evidence of attempts to standardize and coordinate efforts widely, and HEP is clearly the most tightly integrated, with a focus on the narrow range of questions being investigated in the LHC experiments.

Summary

In this chapter I have set the stage for a study of collaboration propensity—that is, the factors that motivate individual researchers to collaborate—in three fields of research: neuroscience, earthquake engineering and high energy physics. I demonstrated with several examples that collaboration is critically important in answering questions that are of interest to society, but also difficult due to institutional obstacles and troubles with communication and coordination. To better develop information and communication technologies to support research collaborations, there is a need for a better understanding of collaboration propensity. There are two theoretical approaches that might be taken, though, to further our understanding of this concept. One of these relies on social and cultural approaches that have been applied to the study of science. Where research cultures are collectively focused, we might expect to see more collaboration – and could take steps to change the focus of research areas where more collaboration is desirable. On the other hand, we could also look to organization and coordination theories and their focus on attributes of work and its performance. With this approach it is the requirements of everyday work that motivate collaboration. I will argue and demonstrate in subsequent chapters that this latter approach is far more powerful than the former in the settings studied here and as these attributes were measured here.

CHAPTER 2

LITERATURE REVIEW

Existing studies of collaboration illustrate why it is important, how frequently it occurs, and some factors that influence the effectiveness or productivity of particular collaborations. There have been few systematic studies, however, of factors that motivate collaborative work. Multiple theoretical approaches might be considered in attempting to predict when collaboration will be useful. Two of these will be considered here. On the one hand, a cultural approach that distinguishes cultures that are individually oriented from those that are more collectively oriented would suggest that collaboration will be more likely in fields or laboratories with a more collective focus. On the other hand, a focus on work-related attributes that is rooted in contingency and coordination theories would suggest that individuals will choose to collaborate when it is useful and necessary in getting their work done. This chapter reviews the relevant literature and poses hypotheses that provide a critical comparison of these two theoretical approaches to predicting collaboration propensity.

General Studies of Scientific Collaboration

One of the historically common means for studying scientific collaboration is through analysis of bibliometric data. A wide range of studies have tracked the incidence of co-authored articles and used this as a proxy for collaboration, demonstrating both the existence of and increasing frequency of collaboration in science generally (reviewed in Cronin, 2001). Price (1963; 1986), for example, shows evidence of a dramatic increase in co-authorship during the twentieth century. In addition, researchers have used similar methods, along with network analysis more recently, to demonstrate the rise in collaboration across international borders (Luukkonen, 1992), the variation in frequencies of collaboration in different fields (Laband & Tollison, 2000; Newman, 2001), and the effects of computer-mediated communication media on the frequency and structure of collaborations (Walsh & Maloney, 2002). In addition, studies in this general class have found and analyzed patterns in these co-authorships, leading to findings that, for example, confirm Crane's (1972) notion of the invisible college of geographically distributed researchers well known to each other (D. J. d. S. Price, 1986), and the presence of intellectual centers in the global scientific arena (Luukkonen, 1992). In some cases, such centers are rendered explicit through the establishment of distributed multi-disciplinary research centers that foster collaboration (Hara, Solomon, Kim, & Sonnenwald, 2003).

From the notion of the invisible college of collaborating researchers, there emerges the sense that the conduct of science is, at least in part, a social endeavor. The next category of research on collaboration in science focuses on the social behavior that underlies collaboration. Hagstrom's (1965) analysis of the scientific community suggests that there is a gift exchange system at work in which information is exchanged for recognition in the form of acknowledgements, co-authorships, and so forth. Hagstrom also explores the reasons why scientists collaborate and suggests that the need for access to instrumentation and expertise necessary to do one's research plays a significant role in this process. Several researchers have chronicled the massive recent growth of experimental apparatus in high energy physics (and the corresponding reduction in the quantity of these devices that exist), for example, and found that collaboration is essential in this field in order to obtain access to any data at all (Galison, 1997; Knorr Cetina, 1999; Traweek, 1988).

In addition, Hagstrom suggests that there is a positive relationship between spatial propinquity and propensity to collaborate. This is echoed in work by Allen (1977) and in Kraut, Egido and Galegher's (1990) finding that communication frequency among scientists drops off sharply beyond 30 meters. One open question regarding collaboratories is the degree to which they can change the nature of this relationship between propinquity and propensity to collaborate. Evidence from a limited set of studies suggest that there is a positive relationship between email usage and remote collaboration (Cohen, 1996; Walsh & Bayma, 1996; Walsh, Kucker, & Maloney, 2000; Walsh & Maloney, 2002), but this question remains to be definitively answered. Hara, et al. (2003) also demonstrate that under certain conditions of complementarity, communication and collaboration technologies can help catalyze collaborations. At the same time, though, Cummings and Kiesler (in press) find that geographically distributed collaborations suffered more communication and coordination difficulties than collocated collaborations.

Finally, detailed descriptions of exceptionally effective or well-known collaborations have been published, such as Watson and Crick (Watson, 1968), but these do not likely represent the modal experience of scientists in collaborations.

What Makes Collaborations Effective?

Though the literature on scientific collaborations is somewhat sparse, one strong theme is the desire to understand what makes collaborations effective and/or productive. This, of course, raises the question of what constitutes an effective or productive collaboration. In existing studies, this has typically been measured using the number of papers or reports produced by the collaboration, often in combination with self-reported perceptions of effectiveness by the collaborators themselves. Pelz and Andrews (1976), for example, carried out an early study of scientists in both commercial and university settings, in which they measured a wide variety of individual traits and examined their correlation with productivity. In operationalizing "productivity" (see Appendix A of their book), they used a combination self-reported quantities of scientific outputs (e.g. patents, papers, technical reports) and solicited peer evaluation of work quality.

In comparison with the total set of benefits that collaboration might produce, however, any of these conceptions of effectiveness are rather thin. Other factors that might be considered include the quality and volume of actual new and useful knowledge produced (which does not necessarily correlate with the number of publications), the training of students who go on to produce exemplary work, and new jobs or career opportunities created or catalyzed by the collaboration. It is largely because measuring collaboration effectiveness in the short term is so uncertain that the present study remains essentially neutral on this dimension. Findings from the existing literature, however, can generally be divided into two categories: 1) those that focus on traits of the individual collaborators, and 2) those that focus on the traits of the collaboration or workgroup.

Individual traits

As was noted above, a small number of studies have found a positive relationship between the use of communications media, such as email, and collaboration productivity (e.g. Walsh & Maloney, 2002). In another study of collaboration in the cognitive science community, Schunn et al. (2002) examined the predictive capacity of several individual traits on the self-reported success of collaborations. They found that similarity of initial ideas among collaborators was a significant predictor for local collaborations, but not for those involving distant participants. They also found that using email as a primary means of communication, similarity of work styles and perceived equal status of collaborators were not significant factors for local or remote collaborations. It is, of course, important to bear in mind that these data are from only a single study within a single discipline, and that other disciplines may be different.

Group factors

A small number of studies have looked at the structural and organizational characteristics that predict successful collaborations. One such factor is the degree to which there are agreed-upon and formalized standards for authorship and sharing in the field. Knorr-Cetina (1999) suggests, for example, that one reason for the success of high energy physics collaborations is that credit for achievement is given to collaboration groups—not individuals. As a result of this, she argues that junior members of the physics field, such as advanced graduate students and post-docs, were substantially less focused on accumulating recognition via individual publications, presentations and the like. Shrum et al. (2001) looked at the relationship between presence of a formal agreement for data sharing and perceived collaboration success and found that more successful collaborations were associated with not having formal agreements to share all data collected by large, "public" instruments. The same study examined trust in collaboration groups and suggests that "institutional trust," that is, the trust between teams in a large collaboration or between institutions involved in a distributed workgroup is more important in predicting collaboration success than is interpersonal trust among the collaborators themselves.

When Does Collaboration Happen?

Though there have been a small number of studies on what makes collaboration effective, there have been even fewer studies of what actually leads people to believe that collaboration is useful or desirable—and when it is likely to happen. These are reviewed in (Katz & Martin, 1997), but this work is generally not systematic in nature. Thus, there is little work that rigorously considers the question of what drives researchers' propensity to collaborate. In thinking about this theoretically, there are two approaches that might be considered. On the one hand, a disciplinary culture argument that is rooted in the individual versus collective orientation of different cultures would predict that collaboration propensity will be higher in a culture that is more collectively than individually oriented, and where there is more focus on collective enterprise and achievement. On the other hand, it could also be argued from a perspective that focuses on the day to day work done by scientists that culture means little in the face of a need to get a particular type of work done. This approach would hold that it is the attributes of work and the nature of people's interactions and experience at the individual level that are more powerful in predicting collaboration. In this section, I will elaborate and operationalize a definition of collaboration propensity, and then detail a set of constructs and hypotheses for a comparison of these perspectives.

Collaboration Propensity

Collaboration propensity is a measure of how likely an individual researcher is to collaborate at a particular point in time and with regard to current research interests (i.e. this is not operationalized here as a persistent individual attribute). From work by Hagstrom (1965) and Beaver (2001) we know that one important dimension of collaboration propensity is whether or not researchers believe collaboration will provide them with access to expertise, experimental apparatus, data sets and other scarce resources that are useful or necessary in answering research questions of interest. At the same time, it has been suggested that collaboration in some fields can mask individual achievement and make it difficult to receive recognition for one's efforts via employment offers, promotion and tenure, and important prizes (Kennedy, 2003; Knorr Cetina, 1999;

"Who'd want to work in a team?," 2003). Thus, the second aspect of collaboration propensity is the extent to which researchers perceive collaboration as a component of a viable career path, which they think will lead them to success. Collaboration propensity will be measured here using a combination of eight Likert scale items (see Appendix E) and will be the dependent variable upon which the factors listed below are regressed in Chapter 4.

Individual vs. Collective Cultural Orientation

"Culture" is a broadly defined construct that includes a great many attributes shared by communities of individuals and passed on from generation to generation. Hofstede (1980), for example, refers to culture as a sort of "mental program" that shapes behavior and is inherited – not learned. The culture of science and research activity more generally is arguably a critical component of social theories of science (e.g., H. Collins, 1985; Hofstede, 1980; Kuhn, 1970; Latour, 1987; Latour & Woolgar, 1979). In this study, though I acknowledge the potential importance of other cultural attributes not directly measured here, I will focus most closely on the extent to which the research cultures of the disciplines being studied here are oriented toward individual vs. collective achievement and recognition. Such ideas have been discussed in the past by Collins (1998) and Knorr-Cetina (1999).

Specifically, Collins distinguishes between open and closed "evidential cultures" in laboratory groups across national borders. In this scenario, an open culture implies a willingness to share and discuss work in progress, and also to involve others in the discussion of what distinguishes informative phenomena in data from noise that must be deleted. A closed culture places these responsibilities in the hands of individual researchers. In addition, Knorr-Cetina distinguishes the "epistemic culture" of molecular biology from that of high energy physics by noting that the former is highly focused on individual achievement, as contrasted with the collective focus of the latter. Though these authors do not deal with collaboration propensity directly, we might reasonably expect that, controlling for other factors, collaboration propensity will be higher where there is a perception of greater collective focus. To measure the degree of perceived collective focus in the fields being studied here, I will use the constructs of scientific competition, commercial and industrial proximity, and the extent to which procedures for attributing credit to collective entities have been formalized.

Scientific competition

Fields that focus strongly on individuals are frequently described as highly competitive at the individual level in that researchers in these fields must compete intensely for reputation and recognition, achieved by being the first to produce and publish novel answers to important and difficult research questions. The importance of such recognition is evident in, for example, Zuckerman's (1977) use of the Nobel Prize to distinguish elite from non-elite scientists. This concern about augmenting one's own reputation has been shown repeatedly to impact people's willingness to share data (reviewed in Zimmerman, 2003) and adopt database systems for sharing resources even with their known collaborators (Birnholtz & Bietz, 2003). Additionally, broad surveys of scientists (W. O. Hagstrom, 1974; Walsh & Hong, 2003) suggest an unwillingness to discuss research currently underway due to fear of being anticipated, or "scooped" by colleagues. Data from the more recent of these studies suggests an increase in such tendencies, particularly in the biological sciences.

In this study, scientific competition will refer to the extent to which scientists are concerned about discussing their results in progress with colleagues, concerns about having their research results anticipated or "scooped" by other researchers and the importance of winning prizes and widespread recognition. This variable will be measured using two items from Walsh and Hong's (2003) survey and one that I developed. Given the individualistic nature of this sort of competition, it is expected that:

H1: There will be a negative relationship between collaboration propensity and the perceived level of scientific competition.

I also expect that, based largely on Knorr-Cetina's characterization of the significant differences between the epistemic cultures of high energy physics and molecular biology, that scientific competition will differ across fields of research. Where physics exhibits a collectivist culture that we might expect to be less competitive, neuroscience is an exemplar of the broader class of biomedical sciences that have been described elsewhere as fiercely competitive (e.g. Davies, 2001).

H1A: Scientific competition will be higher in neuroscience than in the other two fields.

H1B: Scientific competition will be lower in HEP than in the other two fields.

Moreover, I believe that the nature of work in different fields will mean that scientific competition functions differently in predicting propensity to collaborate. First, the collectivist nature of HEP research collaborations described earlier will mean that competition should function differently in HEP than in the other fields being studied with regard to collaboration propensity. Specifically, because physics competition has been described to occur primarily at the level of the collaboration and not the individual, I hypothesize that:

H1C: There will be an interaction effect between field of research and scientific competition in predicting propensity to collaborate.

Industrial proximity

In areas such as high-stakes pharmaceutical research, proximity to industry can impact the degree to which individual researchers must be concerned about proprietary intellectual property. It has been demonstrated that close ties to industry can affect people's willingness to adopt information technologies (Walsh & Bayma, 1996) and even to promptly publish important research results (Blumenthal, Campbell, Anderson, Causino, & Louis, 1997) due to pressures associated with intellectual property and profits from commercialization. This can arguably also have an important impact on the individualistic nature of a disciplinary culture, with specific regard to researchers' ability and/or willingness to share information and discuss important problems with their colleagues. Where nondisclosure agreements must be signed by specific researchers involved in projects with corporate partners, for example, a culture more focused on individuals than on groups seems likely to emerge. To be sure, there may be small group collaborations in these environments or collaborations with partners in industry who possess critical resources, but on the whole concerns about secrecy seem likely to create a culture of isolation and individuation.

It is admittedly the case that this assessment presumes no formal or norms-enforced means for guaranteeing trust between individuals involved in sharing or collaborating. Moreover, the volume of collaboration, sharing and publishing that does take place in many fields provide some evidence to suggest that such means do exist in some cases. Nonetheless, such norms have not been shown to be universal and prior studies do show a link between concerns about intellectual property and willingness to share or publish results and resources. Thus, the impact of such concerns on collaboration propensity remains an open and valid question.

In this study, commercial/industrial proximity is defined as the extent to which researchers receive research sponsorship from commercial or industrial organizations, and the extent to which there is an interest by researchers or others in commercializing or otherwise profiting financially from research discoveries. This will be measured using Likert scale items developed in light of the findings outlined above.

It stands to reason that collaboration propensity will be impacted by concerns about proprietary information, intellectual property and the commercialization of research discoveries. I therefore hypothesize that:

H2: There will be a negative relationship between the perceived proximity to industry and collaboration propensity.

Further, there is evidence to suggest that commercial and industrial proximity will vary in the fields studied here. HEP research, for example, is basic research with few immediate commercial applications. Neuroscience, on the other hand, is closely tied to the highly profitable pharmaceutical industry. EE likely lies somewhere in between these two, as researchers I have spoken with describe carrying out a blend of commercially sponsored and basic research investigations. Therefore:

H2A: HEP will have lower industrial proximity than EE.

H2B: EE will have lower industrial proximity than neuroscience.

H2C: Neuroscience will have higher industrial proximity than EE.

There is also evidence to suggest that, based on these differences, this construct will operate differently in these fields with regard to prediction of intent to collaborate. Specifically, it will be a more important factor in neuroscience and earthquake engineering than in HEP. Thus:

H2D: There will be an interaction effect between field of research and industrial proximity in predicting collaboration propensity.

Ease of collective credit attribution

A third factor that provides some indication of a field's orientation toward collective or individual entities is the way in which credit is attributed for collaborative discoveries and, more specifically, the extent to which it is easy to know whom to include as co-authors on a collaborative project, and the extent to which it is clear at the start of a project how contributors will receive credit for their efforts. Fields vary substantially in the extent to which standards have been developed, and in the ways that co-authorship is interpreted in assessing achievements. The American Psychological Association, for example, devotes a portion of the ethics section of its *Publication Manual* to the proper means for attribution of authorship credit (APA, 1994).

There are also a few examples in the literature of attribution behavior influencing work practice. In one study, Laband and Tollison (2000) point out differences in the ways biologists and economists attribute credit for collaborative work and suggest this as a possible explanation for the more rapid growth of co-authored works in biology. In a more extreme example, each HEP collaboration has a standardized author list that goes on every paper published by any member of the collaboration and include's the names of all members in alphabetical order. This practice has been referred to as "hyperauthorship" (Cronin, 2001) and Knorr-Cetina (1999) suggests that it accounts for some of the cultural differences she observed between HEP and molecular biology.

Specifically, she says that credit in physics is attributed to experiments and collaborations rather than individual authors. This focus on collaborations is pervasive in the culture of the field and many conferences do not even provide opportunities for updates on the work of individuals. She argues that this leads to an environment in which individuals are always contributing to a larger entity (the Collaboration, with deliberate capitalization) from which they will receive shared credit. This, in turn, "creates a form of emotional involvement that, in addition to authorship provisions and representational formats, strengthens and sustains the communal life form" (p. 169).

In this study, the extent to which collective attribution practices are standardized will be defined as the extent to which a field has developed and widely accepted means for attributing credit and dividing responsibility among individuals working collaboratively. I will measure this using a set of Likert scale items that measure the ease and clarity of dividing and assigning responsibility in collaborative endeavors. From the examples above, it can be seen that both the presence and degree of development/adherence to collective credit attribution standards, and the means by which credit is assigned seem likely to have an impact on scientists' propensity to engage in collaborative work.

H3: There will be a positive relationship between the ease of collective credit attribution and collaboration propensity.

Moreover, the literature and my own experience suggest that the ease of collective credit attribution will vary across fields. Specifically, it should be highest in HEP where standard practice is including all collaborators on all papers, and lowest in EE, which my experience suggests has few collaborations larger than a faculty member and a doctoral student. In neuroscience, on the other hand, it is likely that there will be some uncertainty, but that there will be some adherence to the practice described by Knorr-Cetina, whereby the laboratory leader is always the last author on a paper. Thus:

H3A: Collective credit attribution will be easiest (highest) in HEP.

H3B: Collective credit attribution will be hardest (lowest) in EE.

Moreover, there is evidence to suggest that collective credit attribution will operate differently in predicting collaboration propensity. In physics, researchers have little choice but to collaborate, so the collective credit attribution standards should not affect collaboration propensity as strongly as in EE or neuroscience, where collaboration is optional. Therefore:

H3C: There will be an interaction effect between field of research and the ease of collective credit attribution in predicting collaboration propensity.

Work-Related Attributes

Another theoretical approach to predicting collaboration propensity focuses on the day-to-day work of researchers and stems from contingency and coordination theories. Surprisingly, there has been little research on the practice of science that draws on these literatures, but the models of scientific organization and production presented by Fuchs (1992) and Whitley (2000) do this to some extent and provide a useful starting point for the present discussion.

These authors draw largely on work by contingency theorists of organizations (Perrow, 1972; Thompson, 1967; Woodward, 1980) in identifying the dimensions of mutual dependence and task uncertainty. I will define these concepts a bit more narrowly and specifically below, and refer to them as "resource concentration" and "focus" on a narrow and specific set of research questions, respectively. To these concepts I will add widespread agreement on what constitutes quality research, and the need for and availability of help in conducting day-to-day work. All of these are potential indicators of the usefulness of collaboration and probability of finding a suitable collaboration partner. Where collaboration is more useful, this approach would generally hold, we should expect to see an increase in collaboration propensity. The remainder of this section enumerates these dimensions and outlines a second set of hypotheses.

Focus

Fundamentally, focus is a measure of homogeneity with regard to research questions and methods (as defined in Chapter 1) within in a field of research. Where we see a variety of methodological approaches to several loosely related research problems, we would say that focus is low. This stands in contrast to fields where the methods for addressing a particular type of research question are widely agreed upon. Furthermore, heterogeneous fields may also be characterized by disagreement about what the important research questions are that need to be answered. HEP, for example, would not be considered heterogeneous in this way and is characterized by relatively strong agreement that investigation of the Higgs mechanism is a natural next step for the current experiments. Thus, we can say that HEP is a field that is highly focused. Researchers generally agree on the important questions and the methods that should be used to answer them. Focus will be measured using a series of Likert scale items developed for this study.

Focus is likely to impact collaboration propensity in that people in highly focused fields are more likely to be able to work together effectively and more likely to find likeminded collaboration partners. Therefore:

H4: There will be a positive relationship between focus and collaboration propensity.

Additionally, it is clear from previous studies and my own experience that focus will differ across the fields being studied here. HEP, with its extremely large collaborations and widespread agreement as described above, is likely to be more highly focused than EE and neuroscience. Focus in neuroscience is less clear at the outset, but it stands to reason that the focus on individual investigators and laboratories will render focus lower in neuroscience than in EE. Therefore:

H4A: HEP will exhibit greater focus than the other two fields.

H4B: Neuroscience will exhibit lower focus than EE.

There is also evidence to suggest that focus may operate differently in the fields being studied here in predicting collaboration propensity. HEP, for example, is highly focused in part due to necessity. Resources simply are not available to conduct all of the experiments that people want, so decisions must be made about what issues the field will focus on. This is in start contrast to neuroscience and EE, where research is more decentralized and focus is the result of individual alignments with particular problems and approaches. Therefore:

H4C: There will be an interaction effect between field of research and focus in predicting collaboration propensity.

Resource concentration

Resource concentration is the degree to which conducting research in a field requires large and sophisticated apparatus and/or amounts of money sufficiently large to be difficult for a single investigator to secure. Where resource concentration is high, interdependencies of the sort discussed by Thompson (1967) are created in that researchers become dependent on access to these scarce experimental and financial resources. Once again, we can turn to HEP as an extreme example where very large experiments dominate the research landscape and alignment with one of these experiments is virtually essential for individual researchers. Contrast this with, say, biology. Most major research universities have biology laboratory facilities on campus and it would likely be possible for an investigator to do work in such a lab with no collaborators at all.

Resource concentration will be measured here using a series of Likert scale items that ask about dependence on scarce financial or experimental resources. It stands to reason that scientists in fields with high perceived resource concentration are likely to exhibit a greater propensity to collaborate than are scientists in fields characterized by low resource concentration. Therefore:

> *H5: There will be a positive relationship between the perceived level of resource concentration and collaboration propensity.*

In addition, there is evidence to suggest that the perceived level of resource concentration will differ across the fields being studied. The nature of HEP research suggests that it will have the highest perceived resource concentration, where EE and neuroscience will be lower. EE may be somewhat higher than neuroscience, due to the large scale equipment used in some laboratories. Thus:

> *H5A: Resource concentration will be higher in HEP than in EE and neuroscience.*
>
> *H5B: Resource concentration will be higher in EE than in neuroscience.*

Further, there is reason to believe that resource concentration will function differently in different fields of research in terms of predicting propensity to collaborate. Specifically, physicists and earthquake engineers tend to depend on each other for access to large scale experimental apparatus, whereas neuroscientists tend to depend on each other for expertise and smaller-scale resources. Thus:

> *H5C: There will be an interaction effect between field of research and resource concentration in predicting collaboration propensity.*

Agreement on quality

Just as a high degree of focus in a field seems likely to affect the probability of finding like-minded collaborators and the need for collaboration in conducting one's work, widespread agreement on what constitutes high quality research seems likely to affect researchers' ability to find collaborators with whom they can work successfully. Others have demonstrated that agreement on quality differs between fields, but no studies have explored this concept as it relates to collaboration. Hargens (1975), for example, used the rejection rates of journals and the mean lengths of doctoral dissertations in different fields to illustrate such differences. His argument was, essentially, that there is more agreement about what constitutes good research in fields, such as chemistry, with low rejection rates and short dissertations. Rejection rates are lower because researchers have a better sense of whether or not the paper will be accepted before they submit it, and dissertations are shorter because less space must be used in reviewing competing streams of relevant literature.

Agreement on quality will be defined here as the extent to which a field is characterized by agreement on how research work is to be assessed, and the extent to which there are perceived hierarchies of institutions and journals in the field that are widely agreed upon.

I hypothesize here that:

H6: There will be a positive relationship between agreement on quality and collaboration propensity.

Moreover, evidence from Hargens' study suggests that there are likely to be differences in agreement on quality across fields, but that these differences may be quite small in the fields being examined here. Biology and physics, for example, are shown to be quite close together, as contrasted with fields in the humanities. Thus, I do not expect differences on this dimension to be large. Nonetheless, HEP is sufficiently different in practice (even from the rest of physics, which was the actual source of Hargens' data) that it stands to reason that it will differ here as well. Thus:

H6A: HEP will exhibit higher agreement on quality than the other two fields.

It further seems reasonable for the same reasons that agreement on quality will operate differently in different fields:

H6B: There will be an interaction effect between field of research and agreement on quality when predicting collaboration propensity.

Need for and availability of help

Another factor likely to be important in understanding collaboration propensity from a social and organizational psychological perspective is the amount of help-seeking interaction that typically takes place in day-to-day work, and the availability of this help.

Coordination theory (e.g. Malone & Crowston, 1994) suggests that such interactions result from interdependencies in complex task situations that must be coordinated via the exchange of information. Van de Ven et al. (1976), for example, characterize various modes of coordination and suggest the importance of such factors as the extent to which work procedures (as defined in Chapter 1) can be characterized as routine and the nature of interdependencies in work procedures. Specifically, this study illustrated an increased incidence of "team" coordination (as opposed to, for example, sequential or reciprocal) where procedures are not routine and there is uncertainty about how to proceed. For our purposes, the lesson here is a theoretical paradox of sorts. On the one hand, we might expect that researchers whose work procedures are non-routine to engage in more help-seeking interactions and collaborations. On the other hand, the nature of scientific research (as opposed to tasks in the commercial organizations studied by the authors mentioned above, for example) is such that researchers using non-routine procedures may be the developers of these procedures and there may be few, if any, people to whom they can turn for help. At the same time, it may also be the case that access to colleagues to whom one can turn for assistance is constrained by distance or design. In other words, the scale or nature of the work in some fields may render access to colleagues easier than in others. In neuroscience laboratories, for example, researchers tend to work labs dedicated to individuals or very small workgroups. This is quite different from earthquake engineering, for example, where most experiments take place in large, shared warehouse-like laboratories.

In discussing the nature of help-seeking interactions, we therefore need a sense of both the need for help and its availability. Where help is frequently needed and readily available, we should expect there to be more opportunities for formal collaborations. Thus, I hypothesize that:

> *H7: There will be a positive relationship between the need for and availability and collaboration propensity.*

Again, there is evidence in the literature and my personal experience to suggest that the need for and availability of help will vary across fields. HEP, for example, involves tasks that are not well-understood and involve complex interdependencies. It therefore involves tremendous amounts of interaction between large numbers of colleagues. Neuroscience and EE research, on the other hand, tends to be based in individual labs and consist largely of small laboratory groups with faculty members and graduate students or postdocs who develop their own experimental designs and protocols. Thus:

> *H7A: The need for and availability of help will be higher in HEP than in the other two fields.*

For the same reasons, I also expect that this factor will operate differently in the three fields being studied here:

> *H7B: There will be an interaction effect between field of research and the need for and availability of help when predicting collaboration propensity.*

Collaboration tool experience

Evidence from a limited number of studies (e.g. Cohen, 1996) suggests that usage of computer-mediated communication (CMC) tools may correlate with increased scientific productivity. One reason for this increase is likely that CMC makes it easier to communicate with more people and juggle more simultaneous projects with remote collaborators. Thus, the frequent usage of network-based communication and collaboration tools, in an important sense, represents a decision about how to conduct one's work and achieve research goals. The precise nature of the relationship between CMC usage and collaboration behavior, however, is not well understood. We might reasonably hypothesize, though, that:

> *H8:There will be a positive relationship between the frequency of network-based collaboration tool usage and collaboration propensity.*

Moreover, the size and extreme distribution of collaborations in HEP suggests that the frequency of network-based tool usage will, on the whole, be higher in HEP than in the other two fields. Thus:

> *H8A: Network-based collaboration tools will be used more frequently in HEP than in the other two fields.*

Collaboration tool experience will be measured here by asking respondents about the frequency with which they used Internet-based collaboration and communication tools on a specific collaborative project.

Covariates and Control Variables

Beyond the factors mentioned above, other studies of collaboration and theories about organizational behavior more generally suggest that there are other factors that might influence collaboration propensity. These factors will be included in this study as covariates and control variables, and they are described here:

Field tenure

It stands to reason that the amount of time an individual has spent doing research in a given field will impact their collaboration propensity, particularly in fields without a history of collaborative research activity. This factor should therefore be controlled for in the models to be tested here. Field tenure will be measured by asking respondents the year in which they received their highest academic degree. This will be subtracted from the current year and the difference will be used in the statistical analyses.

Traditional collaboration tool usage

To better assess communication behavior and understand the usage of different means for communicating, respondents will also be asked about the frequency with which they used more traditional collaboration tools, such as face-to-face meetings and the telephone. This variable will be included in analyses for control purposes

Individual collaboration experience

It is also important to also consider variations in individual experience with regard to collaboration. This can be considered along two dimensions: frequency and quality. On the quality dimension, people who have engaged in collaboration in the past may be more likely to do so in the future, because they are aware of the additional capabilities that it provides them with. This may be differently true for those who have collaborated only locally than for those who have collaborated with remote colleagues, so these will be measured and included separately. Collaboration experience on the frequency dimension will be measured by asking respondents if they have published a paper in the last five years with local co-authors, and if they have published a paper in the last five years with any remote co-authors.

With regard to the quality of collaboration experience, several items will ask respondents about characteristics of a recent collaboration they have been involved with.

Two of the items will ask respondents to rate their experience with this collaboration in terms of the quality of the work produced by the group and the ability of the group to work together effectively.

Summary

This chapter has reviewed the literature on scientific collaboration, suggesting that most existing studies focus on: 1) demonstrating the existence of collaboration, and suggesting that it is an increasing trend, 2) exploring the differences in collaborations across fields, and 3) exploring what makes collaborations effective. There have been few studies that examine what motivates researchers to collaborate. I have outlined here a preliminary model for predicting collaboration propensity. This model seeks not only to identify factors useful in predicting collaboration propensity, but also to test two theoretical approaches to making such predictions. On the one hand, a cultural perspective would suggest that a disciplinary culture that is collectively oriented would predict collaboration, and that developing such a culture would be an important precursor to the development of collaboration tools. On the other hand, an approach focused on work attributes would hold that attributes of individual work and experience will be more significant predictors. The next chapter will outline the details of operationalizing the investigation of these factors.

CHAPTER 3

METHOD

This chapter describes in detail the methods used in carrying out this study. This included the development and validation of a questionnaire instrument for assessing the relevant constructs, the piloting and administration of this questionnaire, and a set of interviews to gather qualitative data. This chapter is divided into five sections: 1) process overview, 2) qualitative data gathering, 3) preliminary instrument and measure development, 4) pilot studies, and 5) main survey study.

Process Overview

Because this study makes use of multiple methods employed over three years, I provide this overview section to explain the overall sequence of events and the methods used. This information is listed in Table 1.

Table 1 Summary of data gathering activities

Time Period	Event
May, 2001 – October, 2003	Interviews in Earthquake Engineering community
November-December, 2003	Preliminary survey instrument development
January, 2004	Pilot Survey I
February, 2004	Pilot Survey II, Pilot interviews
March, 2004	Survey instrument refining
April, 2004	Main Survey
June – August, 2004	HEP interviews at CERN
October – November, 2004	Neuroscience interviews

Qualitative Data Gathering

To validate and explore quantitative findings more deeply, I conducted a series of interviews with researchers in the fields being studied. Protocols, techniques and timing varied somewhat, so these are described separately below.

High Energy Physics

I spent nine weeks, from June 8 – August 10, in residence on site at CERN in Geneva, Switzerland conducting interviews and observations in the HEP community. Two prior week-long visits to CERN, in January, 2003 and May, 2004 had already provided me with basic familiarity with the site and the research activities underway.

Interviews

I conducted semi-structured interviews with 32 individuals affiliated in various capacities with the two LHC experiments at CERN, ATLAS and CMS (Compact Muon Solenoid). Five of these subjects had responded to the survey described below. Others were selected using snowball sampling techniques. Deliberate efforts were made to select individuals at varying levels of the experiment hierarchy, from first year graduate students up to members of the experiment management and leadership teams A uniform protocol was used to conduct interviews (see Appendix), but the order and selection of items were periodically changed to accommodate the flow of conversation and the respondent's experience and expertise. Interviews lasted 30-60 minutes and were digitally recorded for later transcription. I personally transcribed the interviews, to maximize retention of details and to minimize misinterpretation of jargon by a third party transcriber. Whenever possible, interviews were conducted in a private room with the door closed. More frequently, however, interviews were conducted in one of the open café areas at CERN, which is common practice for private discussions between individuals due to severe space constraints. There is no evidence to suggest that being in a public place caused participants to censor their responses.

Observations

Observations consisted first of touring various laboratory and construction facilities at CERN. Several visits were made to the ATLAS detector construction facilities, including the "pit" 280 feet below ground in which parts of the detector are currently being assembled. I also visited the muon drift tube testing site, the transition radiation tracker assembly and testing sites, and the test beam site for the ATLAS experiment. In all cases, tours were conducted by physicists engaged in work in the facility being toured. Where practical, tours were recorded for later analysis

Second, I became a fully-credentialed member of the University of Michigan ATLAS team and was assigned to work for the summer in open-plan office space in Building 40, which is the primary office site for the two large experiments. I had a CERN identification card and was listed in the CERN web directory. When asked why I was at CERN, I indicated that I was engaged in a study of geographically distributed research collaborations. The extremely large number of people involved in the experiment and the high turnover rate of people traveling between CERN and their home institutes, however, meant that my status and presence were questioned very infrequently. By spending extensive time in my assigned office, in the Building 40 café area and the CERN cafeteria, I was able to conduct unobtrusive participant observations of the day-to-day functioning of the collaboration. Field notes were typed on a daily basis for one hour during the first two weeks of the visit.

Third, I sat in on several ATLAS collaboration meetings during my visit. I tried, in particular, to select some meetings that had a high fraction of local participants, and others that involved a large fraction of remote participants. This enabled me to directly observe the use of collaboration tools, and the interactions between collaborators.

Earthquake Engineering

From May, 2001 until May, 2004 I was involved in the George E. Brown, Jr. Network for Earthquake Engineering Simulation (NEES), a National Science Foundation collaboratory built for the EE community. The NEES project is an $82 million project that provided advanced test equipment to fifteen US university laboratories in addition to high performance network and computing facilities to link these sites and facilitate data sharing, teleparticipation and simultaneous multi-site experimentation. This effort afforded the opportunity for collection of qualitative data as detailed below during our research team's visits to 16 US universities involved in EE research, of which I personally visited 14.

Interviews

A total of 94 subjects were interviewed at 14 universities. I was personally involved with approximately 50 of these at 13 sites. Subjects included faculty, technicians and graduate students directly involved in experimental EE research in both field and laboratory settings, and representing structural, geotechnical and tsunami research. With a few exceptions, all interviews were one-on-one and conducted in conference rooms or offices with the door closed. All interviews were tape-recorded and were typically conducted by two members of our research team: one person primarily asked questions, while the other was primarily responsible for note taking. The note-taker typed full interview notes for each interview after the interview was over, consulting the audiotape when details were unclear.

All Interviews were exploratory in nature and open-ended at first, but the protocol iteratively became more structured as we learned about the field and began to see emergent themes in the data (see appendix for the final interview protocol). Nearly all subjects were asked to describe the nature of and processes involved in their research, laboratory procedures, and their feelings and concerns about the NEES project. When subjects made reference to specific artifacts, such as laboratory equipment, or documents, such as laboratory manuals and procedures, we asked to see these materials and took digital photographs or made copies whenever possible.

Observation

At two of the sites visited, it was possible to conduct brief ethnographic field observations of tests in progress, following established methods (Lofland & Lofland, 1995). These observations were immensely helpful in furthering our understanding of work processes in the labs we studied. These observations consisted of being in the laboratory setting for the duration of the setup and execution of the experiment, which ranged from a few hours in one case to three full days in another. Field notes (Emerson, Fretz, & Shaw, 1995) were taken during meals, breaks and at the end of each day. Wherever possible during the setup, the observer(s) assisted with routine laboratory tasks, and asked the researchers to explain what they were doing. During the execution of the experiment, however, the observer did not ask questions. We did, however, ask researchers for clarification of key moments after the experiment was complete. In two of the cases, we videotaped both the researchers' activity and the data displayed on the computer display they were using to track experiment progress.

At sites where in-depth field observations were not possible, testing facilities were toured with lab personnel. Field notes, photographs and videos were taken.

Neuroscience

In October and November of 2004, I conducted 20-30 minute interviews with twelve neuroscientists affiliated with universities in the United States, or (in one case) who had been affiliated with a US university until three months prior to the interview. Seven of these were conducted by telephone from my office at the University of Michigan. The remaining five interviews were conducted on site in the researchers' office or workspace. Interviews conducted on-site included a short tour of the subject's laboratory space. A standard protocol was used for all of these interviews, which can be found in Appendix C. All interviews were digitally recorded and I transcribed them myself. All subjects had participated in the main survey component of this study, and had indicated their willingness to be interviewed. Subjects were involved in a variety of projects ranging from efforts to standardize numerical modeling of the brain to a large privately funded effort to better understand certain mental illnesses.

Preliminary Instrument Development

In creating the survey instrument used in this study, I followed the recommendations of DeVellis (2003) for scale development. In this section, I outline the steps in this process. Detailed descriptions of the actual items used in the final instrument can be found later in this chapter.

Generating an Item Pool and Determining Measurement Format

I gathered and developed an initial pool of 75 scale items to measure the relevant constructs. The number of items per construct ranged from four to eight. Wherever possible and appropriate, items from prior studies were re-used to help ensure reliability and validity. In addition, this would allow my results to be compared to the prior studies. In cases where scale items were not available from prior studies, new items were developed by consulting relevant literature for a clear understanding of factors likely at play in these constructs. Five point Likert scales were used for all items in this study, with the exception of those that asked for specific numerical answers (e.g. “When did you receive your highest degree?”). These presented respondents with the following choices: “Strongly Agree,” “Agree,” “Neutral,” “Disagree,” and “Strongly Disagree.”

Expert Review of the Initial Item Pool

Using a software framework that had been used for prior survey studies, I built a web survey instrument that included the full initial item pool in random order. This initial pool and the survey implementation were reviewed for clarity, relevance and validity. Reviewers included two persons who have conducted survey research at the University of Michigan’s Institute for Social Research, and four doctoral students in the School of Information. Two of these students are not native speakers of English, as is likely the case for a significant fraction of the population under study. Several clarity, interpretation and presentation problems were identified and attempts were made to fix these items.

Evaluate the Items

Data from the two pilot surveys described below were combined for a quantitative evaluation of the items. Scale reliability was assessed using the Cronbach’s alpha

(Cronbach, 1951) measure of internal consistency. Nunally (1978) states that in the early stages of research, alpha values between .5 and .6 are acceptable, while values of .7 and higher are more desirable for most exploratory social science research. All but three of the twelve scales[i] in the initial item pool met the .50 minimum threshold, and five had values of .70 or greater. As suggested by DeVellis (2003), factor analyses were also performed on the constructs to see if they loaded onto a single factor as expected. In several cases, subconstructs were identified and removed to increase the reliability of the scales.

Optimize Scale Length

With the simultaneous goals of minimizing instrument length and maximizing reliability, the size of the item pool was reduced to 39. When reducing the item set comprising a particular scale, Cronbach's alpha was computed for the reduced set using the pilot data to ensure adequate reliability. The number of items per construct in the reduced item pool ranged between three and six.

Pilot Studies

First Pilot Study

The initial pilot study was intended to be a small-scale simulation of the final survey deployment, in order to both evaluate the questionnaire item set and flag other potential problems with the administration.

A random sample of 101 scientists and engineers engaged in research was drawn from three sources: 37 from the Earthquake Engineering Research Institute membership directory, 39 from the experiment directory of the D0 (pronounced "D Zero") high energy physics experiment at Fermilab, and 25 from the membership directory of the American Thyroid Association. For additional feedback on the survey items, faculty and doctoral students in the School of Information at the University of Michigan were also invited informally by email to complete the survey and provide comments. In addition, twenty-five earthquake engineers known to be “friendly” to my research activities were similarly informally invited to complete the questionnaire.

A letter was sent by US mail to each of the 101 individuals in the formal sample. This letter introduced the project, invited them to log in to a secure web site with a provided username and password to complete the survey. In keeping with Dillman’s (1978) recommendations, a crisp one dollar bill was included with each letter and brightly colored reminder postcards were sent one week later to the entire sample.

Response from the formal sample was discouraging. Ten earthquake engineers, eight physicists, and three thyroid researchers completed the survey for a response rate of 21% from this group. In addition, 8 of the “friendly” earthquake engineers and 27 of the School of Information faculty and graduate students completed the survey, for an overall total of 56 respondents.

Second Pilot Study

In light of the disappointing response rate in the first pilot study and comments suggesting that the length of the instrument may have affected people’s willingness to respond, a small second pilot study was conducted as follows.

An email invitation to participate in this pilot study was sent to 16 high energy physicists at the University of Michigan. A second email encouraging them to participate was sent by a prominent physicist in the department. To get maximal feedback, this pilot study was conducted via interview. Each respondent participated in a 30 minute interview, during which he or she completed the questionnaire and answered a series of short questions about it. Respondents were instructed to indicate any confusing or difficult items, and were asked how this confusion might be resolved.

12 of the 16 physicists agreed to participate in these interviews. Several confusing aspects of the instrument were identified, but response was, on the whole, quite positive and confirmed qualitatively that the scale items were measuring what I thought they were.

Main Survey Study

Results and feedback from the two pilot studies resulted in several changes to the sampling frame and implementation strategies, as described below.

Participation

Based on available information and informal conversations with researchers in the fields under study here, three sources were used for sampling: 1) the membership directory of the Earthquake Engineering Research Institute, 2) the experiment directories for CDF, D0, ATLAS and CMS, which are the four preeminent high energy physics experiments in the world, and 3) the set of neuroscience researchers working on the Human Brain Project, a large NIH-funded research initiative, and a small, privately funded brain research consortium. In all three cases, persons affiliated with institutions outside the United States were excluded prior to sample selection. 300 persons were then randomly selected from each group.

Based on advice from colleagues and Dillman (1978), the professional status and contact information for each of the 900 participants were individually verified using web-based university directory servers. Where persons in the sample were found who no longer appeared to be affiliated with a particular institution, or who did not directly perform research activities (e.g. because they held a primarily clerical position), these persons were dropped from the sample and a replacement was drawn at random from the original pool.

Data Collection

Each participant was mailed a 9” x 12” envelope containing a letter inviting participation in the study (see Appendix F), a letter from a prominent member of each field encouraging survey participation (see Appendix F, and note that in the case of neuroscience a separate letter was not provided but a sentence was added to the main invitation letter indicating that a prominent member of the field had been consulted and encouraged response), a new five dollar bill, a postage-paid return envelope and the survey instrument. The invitation letter introduced the purpose of the study and encouraged participants to respond. It also informed them that participation was optional and gave contact information for the University of Michigan Institutional Review Board office. The survey instrument consisted of two double-sided pages and contained a grand total of 54 items (see Appendix D). Instructions on the survey form indicated operational definitions for potentially ambiguous terms, such as “field” and “collaboration.”

Response

514 surveys were returned before the final analysis commenced in August, 2004. The overall response rate was therefore 57.1%, which is an exceptionally high rate for mail surveys and surpassed my expectations. A brief profile of the respondents is provided here, with substantially more detail in Appendix G.

Demographics

Given the comparative nature of portions of this study, the proportion of the total response from each field was of critical interest. Happily, response across the three fields was almost exactly even, with the three proportions ranging from 32.8 to 33.9% of the total sample. Of these, overall 49% were faculty and the remainder consisted of research scientists, postdoctoral researchers and graduate students. Not surprisingly, 72% of respondents reported their highest degree to be a Ph.D., while 19% reported this to be a master's or M.D. Respondents reported receiving their highest degree a mean of 14.1 years ago (SD=12.3, n=381), with a range of 0 to 53 years ago. Overall, 88% of respondents indicated that their highest degree was in their current field of research, though, at 76%, this figure was substantially lower for neuroscientists.

Collaboration traits

To get a sense of the collaboration experience of respondents, they were asked to consider a specific research collaboration from which they had recently submitted results for publication. The size of these collaborations varied substantially across fields, though the two modal values were 4 and 5. Not surprisingly, collaborations in HEP were far larger and involved individuals from far more institutions than collaborations in the other two fields. Interestingly, neuroscientists reported a greater number of disciplines represented in their collaborations. Respondents were also asked to characterize the success of their collaboration based on the quality of results produced and the ability to work together effectively. Responses were overwhelmingly positive, with overall mean values of 4.4 out of 5 for both measures, and negligible differences between fields.

Data Entry and Cleaning

490 survey forms were electronically scanned by the staff at the University of Michigan Office of Evaluations and Examinations. The remaining forms had not been completed by respondents in ways that could be reliably scanned, so data from these forms were entered manually by the author. To verify scanning accuracy, a small number of paper survey forms were randomly selected and checked against the spreadsheet. No discrepancies were discovered. In addition, the scanning software automatically flagged any items to which the respondent marked more than one answer. This occurred infrequently and these cases where investigated and corrected where possible. Where correction was not possible, the response was deleted.

Using the SPSS statistical package (Version 12.0 for Windows, which was used for all statistical analyses in this study), the data set was then checked for missing values. In total, 133 incomplete cases were found and had to be discarded. This reduced the set of cases under consideration to 381.

Performance of Measures

All of the items used to measure these variables are listed in Appendix E. Note that several items included in the survey instrument (see Appendix D) did not perform as expected and had to be dropped in the course of analysis. The performance of the measures used in analysis is described in this section.

Collaboration propensity

Eight scale items were used to measure collaboration propensity. The standardized Cronbach's alpha for these items was calculated to be .78, which is just above the minimum threshold of .7 suggested by Nunally (1978) for social science research. Following the suggestion of Carmines and Zeller (1979), a factor analysis was then performed on these eight items and they were found to load significantly on a single factor.

Scientific competition

Four scale items were used to measure scientific competition and were based on items used in prior studies by Walsh and Hong (2003) and Hagstrom (1974). Cronbach's alpha was calculated to be .61. Though this is a relatively low alpha score, it is within the range of acceptable, though undesirable, scores specified by Nunally and it was determined that the prior usage of these items in several prior studies provided sufficient validation. A factor analysis was also conducted, and the four items were found to load significantly onto a single factor.

Ties to industry

Three scale items were developed as part of this study to measure ties to industry. Cronbach's alpha for these three items was calculated to be .76, which is an acceptable level of reliability. The three items were also shown in a factor analysis to load significantly onto a single factor.

Ease of collective credit attribution

Four items were developed to measure the ease of collective credit attribution. Despite reliable performance of these items in the pilot survey (alpha for a slightly larger set of very similar items was .69), alpha for these four items was .36. When two of the items were removed, alpha was .46. This is not an acceptable level of reliability, but is sufficiently close to Cronbach's minimum threshold for exploratory research that this variable was included in subsequent analyses for exploratory purposes. Additionally, a factor analysis revealed that the remaining items loaded significantly on a single factor.

Focus

Three scale items were used to measure research focus. Cronbach's alpha for these three items was calculated to be .25, which is not an acceptable level of reliability. A factor analysis, however, indicated that these three items load significantly onto one factor. Focus is included in subsequent analyses for exploratory purposes.

Resource concentration

Two scale items were used to measure resource concentration. Cronbach's alpha for these items was calculated to be .66, which is an undesirable, though acceptable, level of reliability for exploratory research such as this study. A factor analysis was performed and these three items were found to load significantly onto one factor.

Agreement on quality

Five items were used to measure agreement on quality. Cronbach's alpha for these items was calculated to be .49, which is an undesirable, though acceptable level of reliability. A factor analysis shows that these five items load significantly onto one factor.

Need for and availability of help

The four items used to measure the need for and availability of help were based on similar items used by Van De Ven, et al. (1976). Alpha for these items was .44, which would ordinarily not be an acceptable level of reliability. Because the items have been used before, however, this variable was included in subsequent analyses for exploratory purposes. In addition, a factor analysis revealed that these four items loaded significantly onto a single factor.

Descriptive statistics

Once the individual items comprising the construct variables had been summed, their distributions were examined. It should be noted that sums of the individual item scores were used to represent constructs because there was no prior theoretical reason to believe that certain items would have disproportionate influence on the overall construct score. Thus, there was no theoretical need for a weighted sum using the factor scores. Moreover, all of the variables of interest were reasonably normally distributed and had no significant outliers. Table 2 provides the descriptive statistics for these distributions, including means, standard deviations, minimum values, maxium values, skewness and kurtosis.

Table 2 Descriptive Statistics, N = 381

Variable Name	Minimum	Maximum	Mean	Std. Deviation	Skewness	Kurtosis
Collaboration Propensity[a]	23	40	32.97	3.83	-0.11	-0.68
Scientific Competition[a]	4	19	11.65	2.69	-0.06	-0.12
Proximity to Industry[a]	3	14	6.77	2.54	0.39	-0.43
Ease of Attribution[a]	2	10	7.31	1.50	-0.61	0.50
Focus[a]	4	13	7.94	1.62	0.14	-0.14
Resource concentration[a]	3	10	8.30	1.77	-.91	.03
Agreement on Quality[a]	12	25	18.72	2.07	-0.13	0.31
Need for and Availability of Help[a]	9	20	16.10	1.90	-0.60	0.88
Network-based tool usage[b]	1	7	5.31	1.77	-0.81	-0.35
Traditional Tool usage[b]	1	7	4.03	1.89	-0.13	-0.99

Notes: [a] These variables were measured using 5-point Likert scales, but different numbers of item scores were summed to create these constructs. Thus, the value ranges for these variables differ.
[b] These variables represent single items measured using 7-point Likert scales.

Summary

This chapter has described the methods used in conducting this study. Qualitative data gathering included 138 interviews in the field with researchers in the three fields being studied. Quantitative data gathering involved two pilot studies to aid in the development of a questionnaire instrument, the final version of which was deployed in a population of 900 scientists and engineers. The results from this study were then used to assess and adjust the primary measures and constructs to be used in the analyses presented in subsequent chapters.

CHAPTER 4

RESULTS

In Chapters One and Two I posed two possible theoretical explanations for collaboration propensity, and presented a series of factors and hypotheses to test in a critical comparison of these theories. This chapter presents the results from this critical comparison. It will be demonstrated through the use of ordinary least squares (OLS) linear regression models that the work-related attributes are substantially more powerful predictors of collaboration propensity than the factors related to the individual vs. collective orientation of culture that were measured here. I begin with a brief description of the analysis techniques used, and then proceed to test the relevant hypotheses. I will also use detailed qualitative description to illustrate these quantitative findings and explore them in greater depth.

Table 3 Nested Linear Regression Models Predicting Collaboration Propensity (N=381)

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Control								
Physics	.27***	.25***	.10	.11	.07	.03	.02	.01
EE	-.21***	-.21***	-.14***	-.15***	-.15***	-.12***	-.13***	-.84***
Experience	-.08*	-.10**	-.06	-.07	-.04	-.03	-.03	-.01
PhD?	..08*	.06	.07	.03	.04	.04	.04	.03
Coauthors		.05	-.04	-.05	-.06	-.05	-.05	-.05
Remote Coauthor		.09	.04	.07	.10*	.08	.08	.08
Work Success		.07	.07	.05	.01	-.01	-.01	.00
Results Success		.05	.03	.02	.02	.00	.00	-.01
Trad. Tool Use		.03	.00	.00	-.03	-.11**	-.11**	-.12**
Work-Related Attributes								
Res. Conc.			.41***	.36***	.29***	.26***	.26***	.21***
Agr. on Quality				.29***	.21***	.18***	.17***	.11
Need for Help					.34***	.36***	.36***	.36***
Net Tool Use						.20***	.20***	.18**
Focus							.01	-.07
Individual vs. Collective Culture								
Std. Attribution							.01	-.07
Sci Competition							.00	-.01
Ind. Competition							.00	.00
HEP Interaction Terms								
Res. Conc.								.09
Agr. on Quality								.06
Need for Help								.05
Focus								-.02
Net Tool Use								.04
Std. Attribution								.08
Sci Competition								-.03
Ind. Competition								-.06
EE Interaction Terms								
Res. Conc.								.03
Agr. on Quality								.28
Need for Help								-.02
Focus								.49*
Net Tool Use								.03
Std. Attribution								-.07
Sci Competition								.03
Ind. Competition								-.011
Adjusted R^2	.19***	.19***	.31***	.39***	.48***	.50***	.50***	.51***
R^2 change								
F Score	21.98***	1.56	60.77***	48.71***	63.10***	16.05***	.04	1.38

Notes:* p <= .1, ** p < .05, *** p < .01. All values presented are standardized β coefficients.

Statistical Analyses

Following steps described by Neter, et al. (1996) and detailed in Appendix G, I developed and ran a series of nested linear regression models in which the independent factors proposed in Chapter 2 were regressed on collaboration propensity. As can be seen in Table 3 I began with basic demographic variables and proceeded to add variables one at a time to measure the amount of variance explained by each successive addition. The best fitting model with the largest number of explanatory factors was Model 6, with an adjusted R^2 value of .50 ($p < .001$). Model 7 is shown in the table only to illustrate that including additional factors added no explanatory power to the model and reduced its' overall fit. Model 8 illustrates that testing the interaction effects between field of research and each of the independent factors also adds no explanatory power. As is detailed in Appendix G, data included in the model were checked for multicollinearity, unusually influential cases, and general adequacy.

Table 4 Bivariate Correlations for Variables of Interest, N = 381

	1 Coll. Prop.	2 Sci. Comp.	3 Prox. To Ind.	4 Credit Attribution	5 Focus	6 Res. Conc.	7 Agree on Qual	8 Help	9 Net. Tools	10 Trad. Tools
1	1	.09*	-.14***	.05	.20***	.52***	.36***	.53***	.41***	.22***
2		1	.19***	-.05	-.03	.10*	.16***	.07	.10*	.04
3			1	-.01	-.07	-.16**	-.04	-.08	-.14***	-.12**
4				1	.21***	-.05	.23***	.09*	-.03	-.02
5					1	.22***	.20***	.17***	.13*	.16***
6						1	.13**	.34**	.39***	.29***
7							1	.29***	.19***	.05
8								1	.17**	.22**
9									1	.55***
10										1

Notes: * p < .1, ** p < .05, *** p < .01.

Indirect/Interaction Effects

It was hypothesized in Chapter 2 that the different independent factors would operate differently in the three fields of research being studied here. In other words, we might expect a factor such as scientific competition to have a different effect on collaboration propensity in physics than it does in neuroscience, due to differences between these fields in culture and practice. To test these hypotheses quantitatively, methods suggested by Jaccard, Turrisi and Wan (1990) were used to construct Model 8 above, which is a regression model to test for interaction effects. This model included the product terms of each field indicator dummy variable (there were two: one for physics, one for earthquake engineering – neuroscience effects are given from the case where both of these are zero) and each predictor variable. The adjusted R^2 statistics for the model before (.50) and after (.51) adding the interaction terms were compared using an F test, and found not to differ significantly, $F(16, 333) = 1.38$, $p=.15$. The beta coefficient for only one interaction term is statistically significant. Thus, these quantitative results do not support Hypotheses 1C, 2D, 3C, 5C, 6B, and 7B. There is, however, preliminary support for Hypothesis 4C with regard to focus in that the beta coefficient for the interaction between EE and focus is marginally significant. This, combined with the change in the EE coefficient in Model 8, suggests that there is some attribute of earthquake engineers reporting high focus that causes them to have higher collaboration propensity than their colleagues. This is discussed further in Chapter 5.

Does Collectivist Culture Matter?

The first proposed theoretical explanation for collaboration propensity relies on a cultural understanding of research disciplines, with a specific focus on the individual vs.

collective orientation of disciplinary cultures. From this perspective, I argued in Chapter 2 that we would expect higher collaboration propensity in settings where people perceive a culture that is more oriented toward collective achievement than individual. This was measured here using the constructs of scientific competition, ties to industry, and the extent to which there are standard practices for the collective attribution of credit. As can be observed in Table 3 and as I will explain here, these factors were not strong predictors of collaboration propensity. It should also be noted that the order in which these factors are added into the OLS model does not affect their explanatory power in a statistically significant way. When the three independent values related to culture are added directly after the demographic and control variables, the R^2 value for the model is not increased by a statistically significant margin.

At the same time, however, Table 3 also shows that field of research does have some impact on collaboration propensity, even after controlling for all other factors. Specifically, the "dummy" variable for earthquake engineering remains a statistically significant predictor in all models and has a negative relationship with collaboration propensity. This suggests that there are attributes of culture that have not been measured directly here that are specific to earthquake engineering and have some influence on collaboration propensity. This is discussed further in Chapter 5.

Table 5 Comparison of Descriptive Statistics for Cultural Variables (N=381)

	Physics			EE			Neuroscience		
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N
Competition*	11.56_a	2.58	127	11.67_a	2.63	129	11.73_a	2.87	125
Ties to Industry**	5.42_a	2.33	127	7.13_b	2.38	129	7.78_c	2.32	125
Protocol Development***	7.31_a	1.67	127	7.55_b	1.35	129	7.06_c	1.43	125

Notes:
*Competition is the sum of four 5-point scale items, so all values are on a scale of 20 where higher numbers indicate greater concern about competition.
**Ties to industry is the sum of three 5-point scale items, so all values are on a scale of 15, where higher numbers indicate stronger ties.
*** Protocol Development is the sum of two 5-point scale items, so all values are on a scale of 10.
Means in the same row that do not share subscript differ at $p < .05$ in contrast tests performed within an ANOVA analysis.

Scientific Competition

Hypothesis 1 states that that there should be a negative relationship between collaboration propensity and the perceived level of scientific competition. This hypothesis is rooted in studies suggesting a link between concerns about competition and willingness to share data, and discuss or publish results. This hypothesis, however, was not supported by these data. As can be seen in Table 3, the addition of Scientific Competition in Model 7 added no explanatory power over Model 6, and the standardized beta coefficient for this variable was 0. As can be seen in Table 4, there is a slight positive bivariate relationship ($r=.09$, $p < .1$) between these variables, but this correlation is weak and only marginally statistically significant. As this was a surprising finding, I also tried running the OLS models and correlations with each of the single items comprising the overall constructs. This method yielded no stronger relationships than the aggregate approach.

Hypotheses 1A and 1B state that there should be differences between fields in their concerns about scientific competition. Specifically, concerns about competition should be higher in neuroscience than in the other fields, and lower in HEP than in the other fields. It is somewhat surprising, however, to note in Table 5 that respondents in all fields report approximately equal levels of concern with scientific competition. As can be seen in Table 5, the mean scores range between 11.56 and 11.73 out of 20. Though respondents in neuroscience report slightly higher concern with competition, as might be expected given previously mentioned accounts of strong competition in the biomedical arena, this difference is not statistically significant, $F(2, 378) = .12$, $p = .89$. Thus, Hypotheses 1A and 1B are not supported.

As I describe below, however, I believe that the general concerns about competition captured by the survey instrument do not accurately reflect more subtle differences in the way that competition operates in these fields. As we might expect, competition in all three fields is focused on the status and reputation of individual researchers. How status is achieved, however, varies somewhat. In neuroscience, it is achieved primarily by being the first to achieve important outcomes and publish these in high profile venues, whereas laboratory equipment and the ability to conduct cutting edge research are important in EE. In HEP, on the other hand, researchers involved in very large collaborative endeavors must distinguish themselves from the crowd in the competition for glamorous jobs and project assignments. These different types of competition will be described in detail below, along with the ways in which they impact collaboration propensity. The important lesson from this discussion is that, even where competition is fierce, collaboration happens when it is necessary to accomplish vital research tasks.

Competition in HEP

HEP exhibits a fascinating balance between the collective orientation described by Knorr-Cetina (1999) and the cutthroat, fierce competition for prizes and recognition described in a journalistic account of the H2 experiment at CERN by Taubes (1986). While it is true that extremely large HEP collaborations render individual reputations subservient to the experimental collective in the sense that the collective must prevail in order for any individual to achieve success, it is nonetheless true that, as several of my interview subjects pointed out, "the Nobel prize won't be given to 1,500 physicists." Thus, respondents indicated a strong need to remain alert and competitive as individuals in need of a strong reputation within a collaboration, in addition to as a collaborative group in fierce head-to-head competition with other experiments for results. As is illustrated below, a collectivist culture does not imply a lack of competition between the individuals within that collective.

Individual reputation: The only way to get recognized

High Energy Physics collaborations are massive research organizations in and of themselves, comprised of thousands of individuals from hundreds of institutions. Each of these researchers is subject to the usual academic pressures of finding research-related employment and maintaining status through the promotion and tenure review process. As in other fields, this is achieved by doing high quality research and earning a prominent reputation among one's peers. Unlike many fields, however, prominence in HEP is rarely achieved via first-author publications. Indeed, I have already noted that standard practice in the community is to alphabetically list all members of a collaboration on any paper published by any member of that group.

While the intent of this practice is to attribute credit to the collective entity for their collective achievements, the apparent reality is that published work on its own brings with it little, if any, reputational credit. Moreover, despite this policy of collective attribution and the generally cooperative dynamic that can be observed in HEP collaborations, my interview data suggest strongly that individual reputation is of central importance to physicists, and that credit attribution is currently a highly contentious topic. As one informant described the system of collective authorship:

> In a lot of ways it sort of doesn't work. You put everyone on, it sort of demoralizes some people. If there's a real creative person, you want to somehow let him get the rewards for being creative, and that's difficult because one person can do something creative but he's using the data and the work of a few thousand others (CERN03).

This highlights the interesting fact that long author lists play the dual role of being a motivating source for intense competition for reputation, while at the same time being precisely what leads many observers to characterize physicists as having a collectivist orientation, relative to other fields of research. The effect of this practice, however, is to render highly ambiguous the formal record of individual contributions. This makes it extremely difficult for individual physicists to distinguish themselves from the rest of the collaboration. Many physicists I spoke with shared personal experiences or stories about close colleagues in which this system was perceived to be unfair. One informant, for example, described a significant contribution made by an individual postdoctoral researcher to a large experiment the informant is involved with:

> One of the postdocs has made quite a lot of progress in [a specific area of a large experiment]. He did pretty much all the work by himself along with one of the associate scientists, who actually happens to work for me so I know a bit about this. He gets credit, I guess, because he gets to give the seminars about that, but any publications will be strictly alphabetical. Is that fair? Probably not. But how else do you do it? (CERN04).

This informant's account also raises the importance of alternative sources of reputational credit, such as giving seminars and talks or holding positions of responsibility within the collaboration. In discussing the evaluation of candidates for jobs and promotions, interview subjects described a system that relies not on lists of publications and formal achievements, but instead on informal word-of-mouth reports of reputation, letters of recommendation, and indicators of achievement that are internal to the collaboration, such as representing the collaboration at a conference by giving a talk or "publishing" a peer-reviewed internal note. Therefore, competition to achieve recognition through hard work or particularly novel solutions to difficult problems is reported to be fierce at times. Because there are few formal or objective mechanisms for formally recognizing individual contributions, however, this can be quite frustrating for some individuals. One subject described her experience as follows:

> It's been a very frustrating experience because I do know that I have been one of the few who has performed exceptionally well. We have done it on time and whenever there was a problem I was able to re-arrange, re-steer and adjust the problem and all that… Despite that, upper management insists on assigning someone else the responsibility for being in charge officially in the organigramme. … He was never in the lab. He doesn't know what we are doing. The only time he came to the lab was to borrow screwdrivers which he did not return. So it's been very, very frustrating, and he gets the credit officially for the work, so this has been very tough (CERN21).

Moreover, just as reputation is becoming increasingly difficult to accrue on larger and larger experiments, the job market in HEP is extremely competitive. This serves to reinforce the importance of cultivating one's own distinct reputation. One informant, when asked about his trajectory to his current position as a project team leader, described finding consistent work as a struggle:

> I didn't have great choices. You know, you look around where you get a job. … ATLAS is finally a post where I have permanent contract, but before I was on three different postdoc positions, if you like. And then when that runs out, you had to see what next. So you're not free to say 'Now I want to go and work there' because you have to find some payment for what you want to do (CERN14).

At the same time, however, most of the physicists I spoke with agreed with the rationale for attributing collective credit on publications and acknowledge the centrality of teamwork in a large collaboration. Many people told me stories of individuals who were perceived as "too competitive" by the collaboration and were marginalized by the community. They described the importance of balance, as this informant indicated: "I mean it's clear that you have to have some ambition, that you need to be a little bit competitive, but you're working as part of a team, and everyone fighting never works" (CERN17). Nonetheless, several informants expressed a desire for changes in authorship practices that make individual contributions more explicit, or requiring that authors on a paper be able to explain and defend the work that was conducted. As it stands, many physicists applying for jobs already make this distinction when listing papers on their CV. Changes in formal practice, however, would make these distinctions more legitimate.

Institutional reputation: Collaboration politics

As important as individual reputation is in the competitive dynamic of an HEP collaboration, it must be acknowledged that individuals do not typically join these endeavors alone. They join as part of a group from a particular institute, which has formally agreed to the terms of experiment membership. On joining, a group typically indicates an interest in helping to build a particular component of the detector, and also has an interest in a particular aspect of the physics analysis that will take place when the experiment comes online several years down the road.

The voluntary nature of the collaboration adds an important dimension of complexity to this situation. Because the resources supporting the collaboration come overwhelmingly from the participating institutes, the leaders of the collaborations themselves have little or no formal control over these resources and therefore have no formal authority to dictate what members of a particular institute can or cannot do. The result is a structure in which many development activities are conducted in parallel by multiple institutes until a single approach must be chosen in order to move forward, as was described by one of the leaders of the ATLAS experiment:

> So when we started to approach the situation that we have to submit a technical proposal to CERN to construct ATLAS, then we had to also launch a process of deciding 'Are we going to do this? Or are we going to do this?' And here it is very important to make these decisions in a way that people don't leave. Because at this point, you know, imagine that you are in a technical group of a university, you have applied to your funding agencies for money for developing a certain type of instrument, you have spent 2 years with a student working on it and so on. And then you get into this situation where now the collaboration decides, 'Are we going to do what I want to do or are we going to do what he wants to do?' Or

> whatever. And then, I'm sure you can imagine that this is a very tense moment. And a very painful moment for several of us (CERN24).

Different experiments handle these situations differently, but some sort of peer review process is usually involved, and a decision strategy is recommended to the leadership by the reviewers.

As this informant suggested, such decisions occasionally result in institutions withdrawing from the collaboration if their approach to a problem is not chosen, but respondents generally described a process that involves acute political sensitivity, and a willingness to take the time necessary to ensure effective compromise. One respondent, who leads a voluntary coalition of 43 institutes in the construction of a particular subdetector described this process as follows:

> You end up with something that I call 'managing by coffee,' which means that you have to convince the various people, the various institutions, to go your way….So that takes a little bit of diplomacy and quite a bit of nerve cracking, but it works at the end. It works because people are motivated and they have been part of this exchange (CERN25).

His term for this process comes from the fact that these meetings are generally held over coffee in the Building 40 café at CERN (see Figure 5 below). This beautifully illustrates the delicate balance between mentalities of collectivist compromise and fierce competition between institutes that is always present in HEP collaborations.

As the ATLAS experiment moves forward, it will be determined which research groups will be responsible for which aspects of the physics analysis process. Here a similar game of institutional reputation ensues, but rather than competing to have one's approach selected for implementation researchers are competing for high-profile or "glamorous" assignments. Indeed, though it is true to some extent that some prestige will accrue to all members of a large collaboration that makes a major research discovery, respondents suggest that it is also almost invariably true that more prestige will accrue to the members of the physics group that actually conduct the final analyses leading to the discovery. In the case of the LHC experiments, for example, being part of the physics group that does the analyses locating the Higgs boson is considered a high prestige position. It should be pointed out that it is not known precisely which data analysis run will lead to this discovery (if this were known with certainty, there would be little sense in performing other analysis tasks), but extensive Monte Carlo simulations of the experiment due give some sense of the relative probabilities of this discovery based on different parameters. Many respondents indicated that, despite being three or four years away from the ability to carry out this work, they were aware of steps that individuals had taken to make a more glamorous data analysis assignment more probable:

> For ATLAS it's the Higgs search. Everybody's looking there. It's going to be very, I mean people are already drawing lines in the sand for this. So it's probably ten percent of the analyses that are like this, but it's, you know it may be the more visible ten percent (CERN20).

This same informant later expressed his concern that the current round of physics group conveners was chosen entirely from Europe. He indicated that if this happens again, the United States might see this as a move to shut it out of physics:

> You can see how this would play in Congress. We've spent hundreds of millions of dollars to do this, and now when it comes time to do the physics America is not being involved (CERN20).

Whether or not this is really an attempt to marginalize American physicists is immaterial. The important point here is that decisions about who will be involved in key analysis tasks are being tracked extremely closely.

In understanding the relationship between competition and collaboration propensity in HEP, this discussion of individual and institutional reputation illustrates that day-to-day work in HEP is characterized by fierce competition that in many ways undermines the formally collectivist structures and practices put in place by the field. In the end, though, researchers in HEP have little choice but to collaborate if they are to do any research at all, so their real goal is accruing sufficient reputational credit, at both institutional and individual levels, to remain gainfully employed in the field.

Don't sit on the enemy side: Inter-experiment competition

In addition to individual and institutional competition within the large HEP collaborations for reputation, there is a spirit of intense, though generally friendly, competition between the collaborations as well. At CERN, this is primarily evident in the competition between the two LHC experiments: ATLAS and CMS.

The detectors being assembled by these two collaborations will sit at points 180 degrees from each other on the circular path of the LHC. Interestingly, this opposition is reflected in the CERN office space allocated to the two projects, which occupy the opposite and nearly-perfectly symmetrical halves of Building 40. This centerpiece of this building is a large, circular five-story atrium which features a small café (which is *de rigeur* for buildings at CERN, where "getting a coffee" is a ubiquitous meeting practice). Though there are no official lines of demarcation in this atrium, there is a clear tendency for the physicists from the two experiments to keep to their "own side" of the atrium—that is, the side of the building on which their experiment (ATLAS or CMS) is housed. An ATLAS physicist with whom I was visiting promptly corrected my inadvertent move toward a table on the CMS side by noting that he didn't want to, "sit on the enemy side." This was said in a friendly way, but the implied competitive spirit was clear.

Moreover, not all are happy with the division of the building along the vertical plane. One interview subject suggested that this would afford too many opportunities for physicists from the other experiment to walk down the hall and see "what we're doing." He wished the building had been divided horizontally (i.e. giving floors 1 and 2 to ATLAS, and 3 and 4 to CMS) instead. Some of this fear may stem from a well known incident of leaked information between experiments at the DESY facility in Hamburg, Germany. Here, it is still not known exactly what happened, but one respondent indicated that he thought it was because researchers from the two experiments "shared a printer." At the same time, though, this respondent also noted that he was aware of several marriages in the HEP community that cross experiment lines (e.g. where two married spouses are members of competing collaborations). In these cases, though, there reportedly must be a clear understanding between partners that they will not leak information. Thus, we see here that there is evidence of some "fear of the enemy," but at the same time a necessary implied trust that one's colleagues will not leak valuable information to outsiders. Violation of this trust is reported to be grounds for dismissal from the Collaboration.

Another critical aspect of the competition between the LHC experiments is the degree to which respondents report that they rely on each other. In the current construction phase, for example, team leaders and experiment leadership on ATLAS report regularly meeting with CMS colleagues and helping each other out:

> We have a lot of, for example, on computing, on collaborative tools, there's no reason why not to be collaborative also with the other experiment whether it's on computing or organizing conferences. At least at this moment it's okay (CERN19).

The last sentence of this quote is indicative of a general feeling among respondents, however, that such cooperation will occur much less frequently when the detectors come online and analysis begins. Respondents also indicate that the two collaborations are, fundamentally, looking for the same thing using different methodologies. Thus, whichever collaboration makes the first big discovery will hope for confirmation from the other collaboration in order to validate the discovery:

> The two experiments have come to different optimizations of what they want. You cannot say that one is right or one is wrong. The two are right, and they are just different ways of optimizing how to look, and this is what gives you some confidence that when the two experiments are going to have results, they're going to compare it and because they came at it from different means, through different systematics, different methods to the same result, then if they agree then that's the scientific truth you want (CERN25).

Finally, the experiments depend on each other for motivation. One respondent indicated that having two simultaneous experiments at Fermilab over the past several years (CDF and D0) had a major impact on the productivity of both collaborations: "I mean it was worth building D0 just for its improvement on the CDF results. Even if it never came out with a single paper it was worth doing" (CERN20).

In understanding collaboration propensity, there are two important points to raise here. The first is that the collective culture of HEP experiments does not eclipse scientific competition, and competition for reputation is rendered particularly fierce by certain aspects of this collective orientation. Second, both competition and collaboration are generally treated as facts of scientific life by these subjects. They do not collaborate because they are not competing, but because they have no choice if they are to do any work at all.

Competition in EE

In EE, evidence was observed of strong competition between universities. In some ways, this competition was more about achieving prestige via membership in the elite strata of the community than being the first to release a particularly novel discovery. This is not to suggest that there is no pressure to produce novel findings in earthquake engineering. Rather, I argue that the engineers I spoke with were less concerned about being "scooped" by those working on similar problems at other labs and more concerned about being "forced" (e.g. by funding agencies) to share their data with those who might make use of it more quickly. Put differently, there was a strong feeling that the access to high end research equipment that comes with an appointment at a top university guarantees a right to discoveries enabled by the use of this equipment. Thus, competition between laboratories was intense and, in many cases, there was a general resistance to sharing data outside collaborative groups, particularly within a few months of when an experiment is conducted.

Having the right stuff: Testing equipment in EE labs

Experimental earthquake engineering research is laboratory intensive science in more than one sense. In the first place, conducting an experiment can require a student to spend several weeks or months in the lab constructing and instrumenting large, sophisticated specimens. More importantly for our purposes here, the size of these specimens mandates large-scale test equipment and vast high-ceilinged laboratory spaces (often referred to as "high bays"). These are quite expensive to build and maintain, and are therefore not available to all researchers in the field. In many ways, a civil engineering department's laboratory space serves as a sort of "crown jewel" for their EE research group. Meetings with researchers at these sites, even when the meeting has nothing to do with experimental test equipment, almost inevitably include a tour of the laboratory. Notable dignitaries are invited to attend laboratory opening ceremonies. And photos of the equipment are prominent on department web sites and recruitment literature, as is illustrated in Figure 4. Moreover there is a strong sense of ownership that accompanies access to this equipment. Researchers are proud of their equipment and the research capabilities that it provides them with.

Figure 4 Lehigh University Engineering Research Center Web Site, January 2005

This sense of ownership becomes particularly interesting in that one of the goals of the NEES project (see Chapter 3 for a description of NEES) is that equipment provided is intended to comprise a national, distributed shared-use facility. Despite the equipment's placement at a particular site, its use is governed by an independent consortium open to the entire EE community. This notwithstanding, several of the faculty members I spoke with about the NEES equipment in their laboratories were particularly excited because it would allow them to recruit better graduate students to their own university's program.

There is also a strong sense within the community that researchers at the local equipment sites are significantly more qualified to use "their" equipment than outside users, and should therefore have priority or a right to involvement in any use, even by outsiders. One of the equipment site PIs, for example, indicated that "for some tests

[here] it can take years of expertise to develop the technology and expertise" (EF29) needed to run a test. This is further evidenced in suggestions from the community that all proposals require a co-principal investigator from the local equipment site. Other faculty members I spoke with suggested that local researchers should conduct the "real tests" with the new equipment, while outside users should have the opportunity to propose "piggy-back" experiments:

> So the PI here will say 'This is what we'll do, this is what we'll measure [in this experiment]. And you'll get that information out, so another person at a facility can say 'Could you add in a little bit more on this?' And the NSF should have a special pot of money for adding value to these tests (EF29).

The essential point here is that there is a strong sense in which researchers at elite laboratories recognize that their test equipment gives them substantial competitive leverage, and they seek to maintain this advantage.

For purposes of collaboration propensity, the importance of this is that competition is observed to be fierce, but does not impact researchers' willingness to collaborate. Rather, people at equipment sites are happy to collaborate with outsiders who might "piggyback" on their large projects and add potential visibility and funding. It is important to point out, however, that hese are not collaborations of approximate equals as we saw in HEP and will see in neuroscience. Rather, the need for collaboration is much stronger on the part of the investigator at a site without sophisticated testing equipment. And people at more peripheral institutions need to collaborate in order to have access to any equipment at all.

Insiders and outsiders: Data sharing in EE

Data sharing behavior in EE is an important indicator of competition in two respects. First, many researchers are unwilling to share their data until after they are finished with it, thus pointing out the perceived right to discovery that comes with access to test equipment and, in turn, the competitive importance of this right. Second, there is evidence that it is perceived as "better" to collect one's own data than to use that of others, pointing again to the primacy of equipment access.

When a researcher in EE designs an experiment, constructs a specimen and runs tests on that specimen, there is a strong and clear sense of ownership that pervades all aspects of the experiment. This includes what is implied to be a fundamental right to be the first to analyze those data and be the first to lay claim to potentially interesting discoveries lying within them. People also expressed a concern that they needed enough time to make sure the data were "clean" and "correct." Both cleaning and analysis take time, but there was a clear tension evident in one engineer's comments. He noted that:

> People can look at the data 3-4 months later when I know it's right, but somebody should not be able to simulate it. That's what I'm planning on doing tomorrow. So, that simulation? Say a year and a half. They should be able to look at [the data] and understand what happened, some peripheral information, but not enough internal information so they can simulate it. Otherwise the [researcher] is left in the cold because he spent the time to look at the data carefully (ER2).

We see here the acknowledgement that it is important to share data, but also a clear desire not to give away potential discoveries or future publications.

Additionally, several researchers indicated in interviews that there is a stigma in the experimental EE community associated with using data collected by others (Birnholtz & Bietz, 2003 discuss this further). One interview subject indicated that figuring out "what will make others want to use [data from our lab] is a serious issue. People seem to feel they need to collect their own data, so they get more credit" (EF11). This stigma can affect the respect one receives from one's peers and, by extension, one's likelihood of getting papers accepted for publication, tenure, funding and other reputation-related factors. This creates an interesting paradox in that, due to competitive pressures of two sorts, both the sharers and consumers of shared data are wary of engaging in data sharing activity.

For collaboration propensity, on the other hand, this sort of competition provides an increased incentive for researchers, including those who do not primarily engage in experimental work, to become involved with collaborative design and execution of experimental projects to avoid these problems. For example, we spoke with many people who engage primarily in numerical modeling work who collaborate with experimentalists in order to gain access to data for model validation. This relationship will be revisited, and also begins to set up one of the key differences between data sharing and collaboration that will be discussed later.

Competition in neuroscience

As was expected at the outset, neuroscience was the area where immediate concern about being anticipated in the release of results was most evident as a constant driving force. Rather than discourage all collaboration, however, competition has two observable effects that are demonstrated below. First, the competition for results constrains the set of available resources and collaborators. Second, competition for reputation encourages researchers to have an independent project on which they can rely for first-author publications, and a large collaborative project for the bulk of their funding.

Fear of anticipation: A constant concern

The vast majority of subjects considered their area of research to be competitive, though some indicated that this was primarily true for the human component (i.e. the study of humans) of their research program and less so for their animal studies. One subject indicated that "It's constant. Like, the first couple times you get scooped on something it hurts. But eventually you get past it and you realize that, well, there's just no way not to get scooped" (N2). This same informant went on to say that this competitive spirit motivates some researchers' behavior at professional meetings:

> They go to meetings, see what other people, their competitors, are doing. They see somebody is getting close to something they're working on and they come back and they push harder on that project to get it out so they don't get scooped. I think that's a constant concern in the field (N2).

Another subject talked about competition inhibiting some scientists' willingness to share results and data for a community database she was assembling:

> There are many related unpublished findings that we would love to include in the database, because it strengthens the resource tremendously, but many people will not put their findings into that database until the publication is out. And some of those individuals are close enough to retirement that this is looking like an impossibility (N7).

One effect of this competitive spirit is to constrain the set of possible collaborators to a few known and trusted colleagues. Within a collaboration, people otherwise quite concerned about being anticipated are very open and sharing. This was summed up well by the same subject:

> Collaborations are investigator initiated. And investigators aren't going to collaborate with people they think are going to stab them in the back, okay? (N2)

Another subject confirmed this and described his own process for choosing the individuals with whom he is willing to share data and results:

> So my approach is I sort of assess the person and then assess their character, their moral character. And then you sort of know if this is going to be a good collaboration or not. There are some people I won't collaborate with because I don't feel I could trust them (N12).

Thus, we see that there is strong pressure to compete and be the first to make a discovery, but at the same time there appears to be a recognition that there are benefits to collaboration for reasons I discuss below. The effect of competition on collaboration here is that it mandates a careful screening of potential collaborators.

Competition for reputation

Even though all interview subjects indicated that there was significant value in collaboration, the vast majority also thought it was important to take explicit steps to build up or maintain their individual reputation. One reason for this is that hiring and promotion committees tend to look primarily at single- or first-author publications in top tier journals:

> So I'm on a search committee right now, and we're looking to hire an investigator in [our institute]. You just take the whole CV, you just flip though it all and you don't care about anything else. And in the search committee meetings you just ask "How many first author papers?" or "Oh, he's got three of them, but they're not in *Neuron*, *Science* or *Nature*" so that one just gets put off to the side. And that's how, you know, it's important, it's critical (N12).

Though two respondents reported limited experience with committees that were starting to look more favorably on collaborative work, most people expressed concern about their ability to accrue sufficient reputational credit via collaborative work. This is further reflected in the fact that the laboratories in this field tend strongly to be named after the senior investigator in the lab, and in that even in large collaborative projects people spoke of contributions in terms of labs named for individual investigators (e.g. the "Smith" or "Johnson" labs).

With this spirit in mind, I observed three strategies for building individual reputation while simultaneously engaging in collaborative work. Two postdoctoral researchers I spoke with reported having smaller scale projects that they work on alone and can therefore be virtually guaranteed first author publications. One of these postdocs nicely summed up the tension between his two projects:

> The people that I've been exposed to with this [large collaboration] are all very well known, highly respected people that have been in the field for a long time and so there's a lot to learn from these people. But at the same time I recognize the importance of independence in this field. You're always pushed to be an independent researcher which is, of course, the role of the smaller project that I work on (N10).

Thus, the first observed strategy is to exploit the benefits of collaboration on a large project, while at the same time demonstrating independence and remaining competitive via smaller, independent projects.

Another strategy I observed was a postdoctoral researcher who is involved in a consulting capacity in the data analysis phase of a wide range of projects. She reported being uninterested in managing an entire research group, and enjoyed the freedom of not having to learn techniques or purchase equipment for data gathering. This subject acknowledged that she was unlikely to advance to an independent academic position under these conditions, but was hopeful that she could persist in her current role on many subsequent projects.

The final strategy that I observed was to be involved only with collaborative projects, but to get a large number of first author publications from these. This frequently requires significant effort at the start of the collaboration to define specific areas of research for participants, and also formally discuss what the author list composition will be for papers on different topics. These formal discussions are sometimes met with hostility when the discussions take place early on, but as one subject indicated:

> If…you're contributing the bulk of the intellectual horsepower, then your student has to be first author. Period. And if you can't agree on that up front, then you probably shouldn't do it (N12).

Other subjects involved in large projects also reported that early discussion of these issues can help avoid significant conflicts. Thus, for those who are willing to fight early on for intellectual "turf" and authorship, this third strategy in competing to establish reputation can be effective.

This has important implications for the present discussion of collaboration propensity. What is reported here represents a fascinating split between the two theoretical approaches to collaboration being compared here. Competition for reputation does seem to be encouraging researchers to work on independent projects likely to yield first-author publications. It is not, however, simultaneously discouraging collaboration in cases where collaboration is valuable in answering key questions. Rather, we see that researchers either engage in multiple simultaneous projects, or they negotiate a collaborative condition in which they can remain competitive.

Proximity to Industry

Hypothesis 2 states that there should be a negative relationship between perceived proximity to industry and collaboration propensity. This is based on findings from prior studies suggesting that researchers were less willing to publish results and adopt information and communication technologies in fields with closer ties to industry. Hypothesis 2, however, was not supported by these results. As can be seen in Table 3, the addition of Proximity to Industry in Model 7 added no explanatory power over Model 6, and the standardized beta coefficient for this variable was 0. As is illustrated in Table 4 there is a slight negative bivariate relationship between Proximity to Industry and Collaboration Propensity. This suggests that the variable does have some limited explanatory power when other factors are not controlled for, but this power is eclipsed by the other factors under consideration here in the regression models.

Hypotheses 2A, 2B and 2C state that there should be differences between fields in their proximity to industrial and commercial organizations. Specifically, HEP should be farthest from industry, followed in turn by EE and neuroscience. As is shown in Table 5, the data support these hypotheses. Differences between groups are statistically significant $F(2, 378) = 34.22$, $p < .001$ and the hypothesized contrasts are significant at the $p < .05$ level. What is particularly noteworthy, though not surprising, in this finding is the magnitude of the difference between HEP (5.42) and the other two fields (7.13 and 7.78, respectively).

As we might expect, these results do correlate with expectations based on previous characterizations of these fields as individually vs. collectively oriented cultures. What I will show below, however, is that proximity to industry does not change the value of collaboration or the propensity to collaborate of individual researchers. I will show instances where concerns about private intellectual property demonstrably slow the research process down and make the initial phases of collaboration difficult, but this does not change the need for collaboration in achieving desired results. This begins to set up an important and emerging distinction between collaboration and public data sharing, and also sheds light on the lack of a quantitative relationship between proximity to industry and collaboration propensity.

Proximity to industry in HEP

HEP is a field that is strongly rooted in basic research. Though it is arguable that much of the physics research spurred in the World War II era had applied goals, it is nonetheless the case that this work is overwhelmingly sponsored by governments, and that there are very few industrial partnerships. This is evidenced by the low reported "ties to industry" values for HEP presented in Table 5, and further supported by interview respondents. Though they mentioned a few cases of partnerships with industry to build detector components, or even to commercialize certain detector technologies, no respondents mentioned interest in commercializing the actual research discoveries in HEP. In some cases, international collaborations have been formed explicitly to avoid having to partner with industry for certain construction tasks. This is the case in an interesting relationship that his emerged between the Israeli and Japanese institutes:

> Japan is a very powerful country from the research point of view, but there are some special rules that apply there that make it very hard to do R&D. That is, there are no strong groups with engineers and technicians that can develop something. There is more writing specifications to go out to industry and build things. And in the face of R&D, industry is not a very good partner. It's actually a very bad partner. So in that sense we had a very good complementarity with our Japanese colleagues in that we could have their input to do R&D mainly in Israel, where there is quite a bit more freedom in the way you handle funds, and based on a fairly good technological infrastructure (CERN25).

Thus, there appears to be little, if any, relationship between collaboration propensity and ties to industry in HEP, save for a few collaborations to build certain detector components.

Proximity to industry in EE

Sims (1999) notes that EE is a field that has its roots in applied research. It was largely born out of the desire of the California highway department (CalTrans) to mitigate risk of future earthquake damage to highways and bridges. This interest spread to those building various sorts of structures, and also to regulators wishing to amend building safety codes to minimize risks posed by the threat of possible future earthquakes. One engineer currently working on scale models of the new San Francisco Bay Bridge noted, for example, that the construction code for steel bridges is only half a page long, and they are working to learn more. Experiments underway in the laboratories I visited were a mix of projects funded by private firms and those funded by public agencies. Within this latter subset, many engineers also make an important distinction between applied tests for agencies like CalTrans and more basic tests sponsored by agencies like the National Science Foundation or the military.

In many cases, it is the funding received to conduct applied tests, and particularly those for corporate or military sponsors, that is responsible for funding laboratory construction and test equipment acquisition. Subjects also reported that much of the data generated by these tests is kept confidential. As one informant indicated:

> We do have sponsors. Who owns that data is not me, but the sponsor. They have some ownership of that data. If I was them, I'd want the ability to use that data before it gets broadcast all over the world. They've paid for that, so they want it (ER5).

Another informant told a story of two competing commercial firms planning to build similar special-purpose industrial structures in the same general geographic area. Both independently approached this lab, and independently sponsored separate, but essentially identical, tests of separate, but essentially identical, structures.

At the same time, however, researchers asked about the typical sequence of an experimental investigation are quick to point out that there is a clear separation in their minds between "basic" and "applied" projects, and that these are frequently conducted differently. This suggests that there may be a different ethic at work with regard to secrecy, and the fact that "basic" investigations are more likely to yield publications further reinforces this. There also appear to be some laboratories that conduct more basic work, whereas others conduct more applied work. One engineer I spoke with referred to another lab as a "contract shop" in a somewhat derisive way, implying that they do a lot of applied tests for paying clients.

Moreover, unlike HEP and neuroscience, it is quite common for graduate students in EE to be involved in a research project as a master's student, and then go work for a commercial engineering firm. One of the goals of the NEES project, in fact, is to speed the field application of lessons learned in research laboratories. Thus, this is the only field under examination here that explicitly trains its' graduate students for work in commercial organizations.

The importance of all this for our discussion of collaboration propensity is that the ties to industry demonstrated here do not appear to constrain collaborative behavior. Rather, collaborations occur between commercial organizations and academic laboratories, and between academic laboratories on larger, publicly funded basic research projects.

Proximity to industry in neuroscience

Certain parts of the neuroscience community have strong ties to industry and strong interest in commercialization, while others have much weaker ties. The strongest ties appear to be in human-centered research (as opposed to primates or rodents, for example), where there is strong interest on the part of pharmaceutical companies. This is not to suggest, however, that there are no industrial ties in animal research, as shall be shown below. Specifically, the evidence I observed can be divided into two closely related categories.

First, there is evidence that some universities and private (but nonprofit) funding organizations are strongly concerned about intellectual property rights and the potential for profits from research discoveries. One project requires that all presentations and reports be reviewed by lawyers before they are released from the collaboration, which can be frustrating to participants:

> It's frustrating as a scientist because we can't get through a meeting without talking business. And that, of course, is really interesting, but it often interferes with the science aspect. I mean as a scientist you want to share your data and you want other people's opinion… And as a business person you shouldn't [do that], which makes sense, but it's their concern (N11).

Another subject talked about working with colleagues who had patented their laboratory mice:

> I have one collaborator who's at [another institution], and they're psychotic about their, you know, intellectual property. And so we managed to [share mice], but it took months longer than it should have…As soon as the lawyers get involved it just goes downhill (N12).

We see here that collaboration is still possible where there are strong ties to industry, but it can be more complicated. These complications caused this subject to resist pressures from his institution to patent his own mice.

The second aspect of ties to industry was evidence of pharmaceutical and other commercial biomedical firms being interested in commercializing research findings. One laboratory I spoke with, for example, specializes in the creation of reagents that allow for the fluorescent tagging of particular proteins. One tag created in this laboratory has been successfully manufactured and sold by a commercial firm. Another subject indicated that pharmaceutical research firms were very interested in the results from his work, which related to aging in the brain. Thus, the race to protect intellectual property appears to be warranted in that there is strong interest in commercializing discoveries in this area.

For our discussion of collaboration propensity, the primary implication of proximity to industry in neuroscience is that proximity can complicate collaboration by introducing additional legal hassles, but it does not change its value to individual researchers in answering key research questions.

Ease of Collective Credit Attribution

Hypothesis 3 states that we would expect a positive relationship between the ease of collective credit attribution and collaboration propensity. This is based on several prior studies suggesting that standard practices that simplify collective credit attribution can impact collaboration and research behavior more generally. In this study, however, the results did not support this hypothesis. As can be seen in Table 3, the addition of Standard Attribution Practices in Model 7 added no explanatory power over Model 6, and the standardized beta coefficient was 0. Table 4 shows, additionally, that there does not appear to be a bivariate relationship between Standardized Attribution Practices and Collaboration Propensity. It is important to bear in mind, however, that the reliability score for this construct was relatively low ($\alpha = .46$). Thus, this quantitative result is exploratory in nature, though it is supported by the qualitative results presented below.

Hypotheses 3A and 3B state that there should be differences between fields in the extent to which standardized practices exist for the attribution of credit on collaborative projects. Specifically, it was expected that these would exist to the greatest extent in HEP (H3A) and to the least extent in EE (H3B). As is illustrated in Table 5, however, these hypotheses are not supported. Though the differences in standard practice development are small, they are statistically significant, $F(2, 378) = 3.38$, $p < .05$, as are the contrasts at the $p < .01$ level. Thus, it may be concluded that neuroscience has the lowest level of standard practice development, while EE has the highest. This is particularly surprising in the case of HEP, where the protocol for credit attribution is quite clear and widespread: include everybody.

As I shall illustrate below, however, the ease with which credit can be attributed on a collaborative project often depends more on the relationship between the individuals involved than on the development of formal protocols and standard practices. Moreover, I will show that respondents did not generally express a strong concern about formal protocols for credit attribution where there were none, and were generally dissatisfied with the formalized practice of "massive inclusion" in HEP. What did emerge as important to people entering into collaborations was a recognition that through some means, formal or informal, they would receive credit.

Ease of collective credit attribution in HEP

In HEP, on the other hand, the large number of people involved in a typical collaboration mandates formalized means of credit attribution and assignment of authorships. As I noted in my earlier discussion of competition, the most visible form of this is the practice of having extremely long author lists on papers. In HEP, all papers published using data from a given collaboration include all members of that collaboration as authors. This is a strong point of pride for many of the subjects I spoke with. As one subject put it:

> So every piece which is there has somebody who has thought about, has given a year of his life to make sure that a bolt is in the right place and has the right effect. Not that guy at the end [doing the analysis] who does not know that the bolt is absorbing part of the noise. … So I think that it is important that everybody who has worked there, even left or even died, every year people die on these collaborations, it is very bad if this memory is gone. … I like the idea of authorship extended (CERN24).

Nonetheless, the composition of the author list is a highly contentious topic among collaborators. Indeed, I experienced this first hand when I visited CERN. As a visitor affiliated with the ATLAS group at the University of Michigan, I applied for a CERN ID card and computing account. One section of the application was dedicated to whether or not I would be included in the ATLAS author list (for which I was clearly not a candidate), and required a rationale and series of signatures to authorize this inclusion. In addition to these bureaucratic requirements, interview subjects mentioned that some amount of "service work" to the collaboration was also required, in order to prevent people from "bypassing" the hard work of the design and construction phases of the experiment and joining just in time to participate in the more glamorous physics analysis.

Despite these measures for restricting who gets credit, the author lists are nonetheless extremely long and, as was discussed in the competition section above, this creates ambiguity about who actually made the most significant contributions to research efforts. Moreover, it causes people to question whether or not their colleagues "really belong" on the author list:

> People have views that vary all over the field. So an engineer who did some work on a special part of the apparatus, should he be in the author list? Or even a physicist who's in a group, but never even set foot in the experiment. Should his name be there? (CERN05).

This confusion likely explains the seeming paradox in the quantitative results. We see in the quantitative results presented here that HEP is actually *lower* than the other two fields on this dimension, despite an extensive formal protocol for attributing credit. The problem appears to be that the collectivist nature of the protocol does not mesh with the competitive, reputation-focused arena of scientific research. Thus, as was illustrated earlier, HEP interview respondents did not always feel that they received adequate credit for research discoveries, and many indicated that they were not sure how they would actually receive credit for their contributions to current experiments, if they received any credit at all. The important implication of this confusion is that the formal protocol is not widely perceived to be effective, does not resolve initial ambiguities about how credit will be received and therefore does not impact collaboration propensity.

Ease of collective credit attribution in EE

In EE, there did not appear to be formal procedures for attributing credit in collaborative projects, but respondents nonetheless reported that it was generally easy to determine whom their co-authors should be on a project and that it was obvious at the start of a project how they would receive credit. The likely explanation for this ease is the generally small size and simple structure of EE collaborations. Most of these investigations involve a single faculty member and one or two graduate students. In some cases, additional faculty members are brought in for their expertise in a particular area, such as numerical modeling or instrumentation. The apparent implication of this arrangement is that all members of these collaborations are included as authors on publications, and there is therefore little ambiguity in inclusion. When I asked people about the order of authors, a variety of practices were indicated. One graduate student, for example, indicated that most of his work is done in collaboration with another of his advisor's graduate students:

> [He] is good at electronics, like when things break. I control the shaking during the test, and [he] controls the P-Wave hammer. It takes two people to run a test and it's good to get him involved. When we write papers, we switch off on who is the primary author (ES7).

Others I spoke with indicated similar practices, though there was enough variation that it was clear that no formal standards are in place. The important element of this story, though, is that the lack of standards for attributing credit does not seem to constrain collaboration propensity or be related to it in any way.

Ease of collective credit attribution in neuroscience

The neuroscientists I spoke with suggested that neuroscience projects typically involve several individuals from a single lab, and may also include a smaller number of people from a second lab. Occasionally there will also be larger projects that may involve up to a total of fifty collaborators at five or six institutions. The typical author list on a neuroscience paper for the people I spoke with ranged from five or six up to a maximum of ten to fifteen for one researcher. He referred to this as a "very long" author list for the field. There is widespread agreement that the first author is always the person who contributed the bulk of the intellectual effort on the paper, and the last author position is reserved for the "senior" author, or principal investigator in the lab. There is little agreement, however, on who else gets included in the list or on when or how inclusion in an author list should be negotiated.

Let us first consider author list inclusion. Members of the large research collaboration that I spoke with indicated that it was standard practice in this group to put the PIs from all of the participating institutions as senior authors, and major local contributors as early authors. One member of this group described the process of constructing an author list as follows:

> And you know, the PIs fill in from the back end and the people working on the project fill in from the front end. We've been inclusive in our authorships. I mean our papers, even though there's a clear primary author and a clear senior author, all five PIs are going on at the end. All of the data analysis group is somewhere in the middle, plus another one or two people who may have contributed to the project (N10).

It is unclear, however, how much value one actually accrues from being a part of a paper with a long author list. Another informant said that

> If you were the last author, the fourteenth author, the senior author then it really doesn't matter for you whether there are ten other people on it or four other people But if you're one of those fourteen other people or one of three other people, it makes a difference (N5).

The point here is that being one of the "middle" authors on a paper with few authors may be better in some sense on a paper with a short author list than on a paper with a long author list, thus calling into question the value of generous inclusive practices observed in some labs (i.e. it may be theoretically better to negotiate an alternative arrangement of alternating names into shorter author lists, but this was not discussed by or with subjects).

One subject reported a strong desire to include students and technicians in his laboratory on his papers. This is not always well received by the community:

> I've published papers that have, you know, five, six, seven, eight, nine authors. And I get flak for it sometimes. People say, you know, 'Well, who really did the work?' And I say, 'Well, that's a ridiculous question' (N1).

Given the prestige of the first and senior author positions, substantial negotiation takes place on many projects to determine not only who will be included on the author list, but also where their names will fall. One researcher who is involved in smaller collaborations noted that she typically arranges things so that one laboratory can name the senior author and the other lab can name the first author. This obviously becomes problematic in larger collaborations, however, and this was acknowledged. There is also disagreement about when these discussions should take place. Several researchers indicated a desire to work this out at the start of a project, sometimes because of negative past experience. One subject said that she's felt like she's had to say things like "I'll only expand my role if I'm guaranteed some publication credit" because "sometimes I've been left off things where I've done a lot of consulting and design work" (N7). Others, however, see this early negotiation as premature both because it is difficult to know at the start what the magnitude of each individual's contribution will be, and because the makeup of the collaboration itself may change:

> I once had a case where basically before this collaboration was started someone really wrote down all the rules like that and it becomes difficult because actually what happened in that case is that a third person got involved in a lot of the work, but our collaboration agreement stated certain things about authorships and then suddenly you have to change the agreement, so this is not an easy thing to do (N5).

Thus, it can be seen that, in strong contrast to the rigid inclusive protocol evident in HEP, there is much disagreement in neuroscience about who should be included in author lists and when this should be determined. This does not, however, seem to impede people's desire to collaborate when they think it will be useful to them.

It's All About The Work

The results presented for Hypotheses 1 – 3 suggest that factors related to individual versus collective orientation of disciplinary cultures were not powerful predictors of collaboration propensity in the fields studied and as these attributes were measured here. This leaves open the question of whether or not factors based on the work-related attributes approach outlined in Chapter 2 can explain collaboration propensity more effectively. In this section it will be illustrated that these factors do appear to be more powerful predictors. Specifically, resource concentration, agreement on quality, the need for and availability of help, and the usage of network-based collaboration tools appear to be the most significant factors.

Table 6 Comparison of Descriptive Statistics for Work-Related Attributes (N=381)

	Physics			EE			Neuroscience		
Variable	Mean	SD	N	Mean	SD	N	Mean	SD	N
Focus*	8.78_a	1.6	127	7.57_b	1.50	129	7.46_b	1.42	125
Resource concentration **	9.52_a	1.03	127	7.30_b	1.70	129	8.01_c	1.72	125
Structure***	18.77	1.94	127	18.70	2.12	129	18.72	2.17	125
Coupling****	16.76_a	1.67	127	15.62_b	1.91	129	15.94_b	1.92	125
Internet-Based Tool Use	6.50_a	1.09	127	4.47_b	1.78	129	4.98_c	1.70	125

Notes:
*Focus is the sum of three 5-point scale items, so all values are on a scale of 15.
**Resource concentration is the sum of three 5-point scale items, so all values are on a scale of 15.
***Structure is the sum of five 5-point scale items, so all values are on a scale of 25.
****Coupling is the sum of four 5-point scale items, so all values are on a scale of 20.
Means in the same row that do not share a subscript differ at $p < .001$ in contrast tests performed within an ANOVA analysis.

Focus

Hypothesis 4 states that there should be a positive relationship between the perceived degree of focus in a field and collaboration propensity. This is based on suggestions from prior theoretical work that researchers in highly focused fields are more likely to have similar worldviews and methodological approaches. Thus, it stands to reason that their collaboration propensity would be higher. There was mixed support for Hypothesis 4 in these results. The bivariate correlations in Table 4 do show a moderate positive correlation between Focus and Collaboration Propensity, $r=.20$, $p < .01$. As is shown in Table 3, though, adding Focus in Model 7 provides no additional explanatory power over Model 6 and the standardized beta coefficient is not statistically significant. It must also be considered, however, that the low reliability score for this construct ($\alpha = .44$) suggests possible issues with both the measures used for this variable in this study and the theoretical consistency of the concepts underlying this variable.

Hypotheses 4A and 4B stated that there should be differences between fields in terms of their level of focus. Specifically, HEP was expected to have a higher level of focus than the other two fields (H4A), and neuroscience was expected to have a lower level of focus than the other fields (H4B). As is illustrated in Table 6, Hypothesis 4A was supported by the data, but Hypothesis 4B was not. HEP does have a higher level of focus than the other two fields, $F(2, 378) = 18.0$, $p < .001$, with the contrast significant at the $p < .001$ level, but there were no other statistically significant differences. That the difference between HEP and the other fields was in the expected direction provides some validation for the Focus scale, despite its low reliability score (see Chapter 3). As was illustrated in Chapter 4, however, Focus was not found to be a significant predictor of collaboration propensity. I will illustrate here that despite varying levels of focus in the fields studied here, respondents reported no troubles in finding like-minded collaborators or working effectively together.

Consensus by constraint: Focus in HEP

Not surprisingly, I found substantial evidence to suggest that focus is high in the HEP community relative to the other fields studied here. This was observed in two primary ways. First, there is widespread agreement in the field on what the important research questions are and, at a general level, how to solve them. Second, there is typically a very small number of active experiments in the HEP community, and within these experiments there is a delicate sort of consensus about how to conduct the work.

In the first place, there is an important divide in HEP between theorists and experimentalists. Despite this division, the two communities have an important symbiotic relationship. Theorists push the boundaries of theoretical models for understanding physics, and depend on the experimentalists for data and results. Experimentalists, in turn, depend on the theorists for input on what the important next steps are in the evolution of the field. These two communities frequently meet informally, but also get together at formal sessions such as a periodic meeting in Snowmass, Colorado that seeks to understand the state of the field and chart its future.

Theory provides one important input in determining what to explore experimentally, but one interview subject described the decision to conduct a particular experiment as a consensus that's driven by theory, technology and available resources. For example, in a world of unconstrained resources, "we would probably be building a VLHC machine. There are designs on the table for that. It's a ring which is as wide as Illinois" (CERN01). And in a world of unconstrained technology:

> People have talked about muon colliders where you bypass this argument that you can't accelerate the particles any more in a circular machine… The problem is that the technology is…at least fifteen years away (CERN01).

With constraints, however, the community decided to build the LHC at CERN: "the cheaper, logical option is to use the existing civil engineering, so that's the LEP tunnel, and put in the highest energy machine you can" (CERN01).

Interestingly, not all informants agreed that this consensus in the field is positive. One indicated his concern that physicists are currently too enmeshed in the details of the Higgs mechanism to assess whether the whole thing is correct or not:

> Most of us are so absorbed in these highly evolved models that we have a hard time looking beyond that and taking a step back, ok, so because you learn all this complicated math and theory and then you're told about the Higgs mechanism, and so it must be true. Ummm, I wouldn't be surprised to find out that it's not correct (CERN01).

Thus, we see that one possible effect of a high level of focus in a field of research can be a lack of high-level perspective and dissent about important problems. In a field that also exhibits high resource concentration, like HEP, this can mean that vast amounts of resources are channeled to a very narrow set of problems that may or may not be the right ones to address.

The second dimension of focus is the consensus that exists within the experiments themselves. In discussing this, it is first important to note that this type of consensus is not a matter of getting, for example, the 1,500 ATLAS physicists together at once and getting them to agree on an experimental strategy. Rather, it is the result of a complex aggregation of interested parties that occurs over several years. One subject described this as follows:

> It's not a perfect democracy. Usually what happens is it starts rather small. A group of interested people come together and make a letter of intention to do a certain experiment with certain physics in view. At LHC it was Higgs. And more and more people join and at some point the room is not big enough for everybody any more, and so what you do is form boards. There's an executive board, collaboration board… (CERN02).

The decision to join an experiment is typically made at the institute level. Few, if any, institutes have groups involved in the two large LHC experiments, and most respondents said that they were expected to work on the experiment chosen by their research group. Thus, focus operates here both at the experiment and institute levels of analysis.

In considering the relationship between focus and collaboration propensity in HEP, the important point to take away is that collaboration here is not the result of a high degree of focus. At the same time, however, a high degree of focus is not the result of a need for collaboration though it may appear this way on the surface. Rather, there is a complex relationship here where, to some extent, it can be argued that the present level of focus reflects a historical aggregation of collaborations that have brought ever-larger groups of researchers together around problems of common interest. This is further supported by the building of consensus via formal meetings to chart the course of the field and allocate scarce experimental resources.

Structured heterogeneity: Focus in EE

While not focused on a narrow set of research questions or methods as a field, earthquake engineers are united around an interest in improving understanding of the seismic performance of various structures. They can generally be classified neatly according to research interests into the categories of structural and geotechnical engineering. Structural engineers work with built environments, such as bridges and buildings. Geotechnical engineers study primarily soils and soil-based structures, such as retaining walls and foundations. Within these areas, researchers can be further characterized by the experimental equipment and methods that they tend to use. These include five categories: shaking tables, reaction walls and frames, field equipment, centrifuges and numerical models. Within each of these groups, in turn, there are more divisions including such factors as the scale of the research specimens, the types of structures or soils under investigation, the types of sensors used, and so forth. The essential point here is that there is a heterogeneous set of problems and approaches in EE, but these can be characterized in a relatively structured way and are unified around a common theme. This is illustrated nicely by the publishing aspirations of my interview subjects. When asked, virtually all indicated a desire to publish in *Earthquake Spectra*, the journal of their major professional association. Most also, however, reported a desire to publish in a journal more closely related to their specific research interests such as *ASCE Structures* or AISC's (the American Institute for Steel Construction) *Engineering Journal*.

Not surprisingly given this structure, focus increases somewhat when considering specific research sub-communities. In other words, there is evidence of agreement on important research methods and questions between groups and facilities that work with similar equipment. I visited two geotechnical centrifuge laboratories, for example, that were geographically quite disparate (one in New York and the other in California), but both reported using sand from precisely the same location in Nevada for filling their soil box models so that their results can be compared. Furthermore, I asked informants in many of the labs that I visited how difficult it would be for them to explain their setup or even conduct their research at another facility with similar equipment. A graduate student in a centrifuge lab indicated that "it would not be hard at all" (ES7) to explain his experimental setup to a student at another centrifuge lab. Another student in a structures lab indicated that "going someplace else, it would be hard to find tools and work with the lab techs at first" (ES1), but beyond these low-level differences it would not be hard to do research at another site.

Another interesting aspect of focus in EE work, particularly in contrast with examples presented below in my discussion of fierce disagreement about neuroscience methods, is that the engineers I spoke with viewed generally alternative approaches to their research questions as complementary, and not competing. For example, geotechnical researchers at one laboratory that uses very large soil boxes on shaking tables indicated their desire to collaborate with researchers at a centrifuge facility because

> some tests here might take weeks to a month for testing…[small scale] centrifuge technologies can be used to simulate large scale tests in advance of the physical tests here (EF29).

These small scale simulations can be useful both in validating findings at both sites, and in determining in advance potential configuration problems for very large, and very expensive, tests on the shaking table. This was also generally true for those who study structures via reaction walls or shaking tables. Each apparatus has strengths and weaknesses that are acknowledged.

All of this demonstrates for our purposes that despite focus being relatively low at the level of the field (at which the survey instrument captured it in this study), there is a structured heterogeneity in EE within which there is strong evidence of sub-communities that are quite highly focused in terms of their general approaches. Within these pockets, there is evidence of collaboration potential. Thus, focus does not impact collaboration propensity at the field level, but it does appear to do so within research sub-communities.

There's room for everyone: Focus in neuroscience

Despite agreement on several important high level research questions, I found substantial evidence of low focus in the neuroscience community, relative to the other fields being studied here. One subject summed this up in noting that:

> Is there agreement? Sure, you can get 25 people together in a room and they'll tell you, for example, that neural coding is a critical question. But if you actually sit down with each of those 25 people individually and ask them what they mean by "neural coding" they'll come up with 25 different answers (N1).

Other informants reported being involved in or knowing about heated disagreements on how to appropriately address key questions. For example, one person indicated that his lab uses transgenic animals to study learning memory, but acknowledged that there is a large group of researchers "out there who think this is just crazy, that we really don't know enough to knock out or mutate genes and they think we're never going to learn anything" (N12).

At the same time, however, these subjects indicated that this lack of agreement does not impede their ability to find like-minded collaborators. There seem to be two main reasons for this. One is an acknowledgement of the importance of bringing multiple perspectives to bear on hard problems. One informant noted that collaborations form when:

> People aggregate towards their own fields of interest, strike up conversations. One person has an expertise in molecular biology, one person in anatomy, one in electrophysiology or something else. And they can't each do it all so they talk to each other about collaborative projects (N10).

Interestingly, this can operate at multiple levels. In describing her involvement in a large collaborative project, another informant indicated the principal investigators (PIs) on the project determined what overall questions were to be addressed and what procedures would be used, but for her work on the genetics component she could "use whatever [procedures] I want because people don't understand what I'm doing there anyway" (N5). In other words, she was brought in as a specialist in genetics and her agreement with the high level questions and methods specified by the PIs did not impede her freedom to use whatever procedures she wished in her own specialty work on the project.

The second reason is that the field is sufficiently large and diverse, and the work is on a sufficiently small scale (in contrast to, say, HEP) that there is room for competing approaches. One subject indicated that he had no trouble finding collaborators "because there's plenty of people on both sides of the fence of any argument" (N12). Other subjects tended to agree that the field was large enough to support diverse approaches to problems and that widespread agreement was not necessary.

Within pockets of the community, however, there is evidence to suggest that there is interest in standardizing certain aspects of research work. In some cases, this took the form of emerging widespread agreement on a particular methodological approach, as was the case for the informant who noted that "I think it's agreed that gene microarrays are the way to go and that using these postmortem samples is the way to go and so there's general agreement within the field" (N11).

In two other cases, informants were directly involved in the development of particular research methodologies and techniques, and indicated their desire to spread these methods and, indirectly, increase focus in the field somewhat. One of them has been involved in the development of computational simulations of brain activity, and noted that the development of simulations forces researchers to get together and concretely define key parameters and procedures. This agreement, he said, will enable the field to become organized around a set of fundamental methods and questions, which he believes to be crucial to understanding the brain more completely: “We’re going to have to adopt a similar structure [to physics] whether people like it or not. And, of course, they don’t because it threatens the priests” (N1). The latter part of this comment refers to resistance he has encountered from researchers who are focused on single-investigator projects and individual reputational augmentation, which he referred to derisively as “stamp collecting.”

The second case was a researcher in a lab that develops reagents for usage in a particular type of gene expression research. He indicated current involvement in over 40 research projects on a consulting basis, about 37 of which are located at remote sites. One of his primary responsibilities is to understand how these methods are being used elsewhere, so that local researchers can make improvements; and to help still more researchers learn to use these methods. To facilitate this learning, it is common for scientists to spend anywhere from several days to several months at the lab learning to use these techniques. What we see here is that the development of a new research method has spawned a more highly focused research community that deals with a certain class of problems in a certain way, thus increasing focus for this component of the overall neuroscience community in ways that were not common in the past. Another informant described nicely how he explained this process to one of his students:

> And one of my students commented on it and said ‘He just does that?’ And I said, ‘Yeah that’s his whole gig, you know he makes these really cool biologically useful reagents. And he doesn’t do much biology. He’s a chemist, but you know if it wasn’t for him a lot of us, we wouldn’t be able to advance the questions we’re interested in’ (N12).

This explanation suggests one of the many ways that neuroscientists depend on other researchers in order to answer questions that are of interest, as is described below.

Resource Concentration

Hypothesis 5 states that there should be a positive relationship between the perceived level of resource concentration and propensity to collaborate. This is based on theories of science and organizations which suggest that people are more likely to work together when they see themselves as dependent on each other for essential resources. Hypothesis 5 was strongly supported by these results, as is shown in Table 3. Adding resource concentration in Model 3 boosts the explanatory power of the model (the R^2 value) from .19 to .31, a statistically significant difference, $F(1,356) = 60.77$, $p < .001$. Moreover, the standardized β coefficient is positive and statistically significant (with p consistently < .01) in all models where this variable is present, and is among the strongest three predictor variables. Thus, there does appear to be a positive relationship between the perceived level of resource concentration and collaboration propensity.

Hypotheses 5A and 5B stated that there should be differences between fields in terms of their degree of resource concentration. Specifically, HEP was expected to have a greater degree of resource concentration than the other two fields (H5A) and EE was expected to have greater resource concentration than neuroscience (H5B). As is illustrated in Table 6, Hypothesis 5A was supported by the data, but Hypothesis 5B was not. HEP does have a higher level of resource concentration than the other two fields, $F(2,378) = 73.06$, $p < .001$ with this contrast significant at the $p < .001$ level. Resource concentration in neuroscience, however, was greater than in EE (contrast significant at $p < .001$) than in neuroscience.

I will show here that there are several reasons for this, basically related to the fact that when resources are highly concentrated, researchers are more likely to collaborate. Different fields, however, exhibit different sorts of concentration. In HEP and, to a lesser extent, EE research the sheer scale and expense of equipment limits the number of locations where research can be carried out and forces people to depend on each other for access to this equipment. In neuroscience, work is on a smaller scale but researchers depend on each other for other scarce and concentrated resources, such as difficult to collect data sets or human brain tissue. Regardless of where the impetus for concentration stems from, however, resource concentration clearly has a strong influence on collaboration propensity.

No other way: Resource concentration in HEP

The nature of experimental HEP research requires massive, extremely sophisticated apparatus that can cost hundreds of millions, if not billions, of dollars to construct and maintain. This means that HEP researchers are, by necessity, highly dependent on each other and must pool resources in order to do any research at all, and are therefore highly likely to collaborate. Resources in this field are thus highly concentrated relative to the other fields being studied here and one must be aligned with this concentration of resources to do experimental research work.

Moreover, the increasingly global nature of HEP collaborations, at CERN in particular, is changing the nature of this resource concentration. It was once the case, for example, that most American institutes joined experiments at American laboratory facilities (e.g. Fermilab, SLAC, etc.) and were funded by American funding agencies (e.g. Department of Energy and National Science Foundation). The same was more or less true for European institutes at CERN and Deutsches Elektronen-Synchrotron (DESY), Japanese institutes at the High Energy Accelerator Research Organization (KEK), and so on. One important consequence of this arrangement is that the vast majority of resources used by an experiment were controlled by a single (or small number of) funding sources. These resources were channeled through the hierarchical leadership structure of the experiment. In the new experiments, this is no longer the case.

As was mentioned above, modern HEP experiments are voluntary organizations of institutes which, in turn, receive funding from their own governments and government agencies, universities, private foundations, and so forth. Thus, it is only through the contribution of these resources by the participating institutes toward the goal of the collective collaboration that the resources become concentrated. Several members of the ATLAS leadership team indicated that ATLAS leadership has formal control over about 25% of the experiment's total resources. The important effect here in contrast with the funding scenario described above is that the leadership of the experiment no longer has control over the resources of the experiment. As one informant described:

> It's more difficult because we don't have a single hierarchical structure that governs and funds it. So the funding is completely different. So you have to convince someone to do something in a way that he doesn't completely want to do it even though you're not his boss. So you have no control over what he does (CERN03).

The important point here is that in this new scenario "resource concentration" refers not to a centralized concentration of resources controlled by a single source, but a voluntary commitment of financial resources and effort by all collaborators to complete tasks according to the plan put into place.

The way this concentration differs from that found in EE is nontrivial, and in the case of ATLAS revolves largely around a document called the "Memorandum of Understanding." This document was written in 1994 and all participating institutes must agree to its terms and sign it. It is also true, though, there is no formal enforcement authority and that institutes have signed this agreement and then withdrawn from the collaboration. Nonetheless, one member of the experiment leadership team described the Memorandum as follows:

> It is sort of like the ATLAS bible. It tells you exactly who's doing what, and in terms of deliverables. It's like a high tech picnic party, you know, where everybody brings to CERN certain stuff and then we put it together and we hope it works. And that's ATLAS (CERN18).

The essential point captured here is the dependence on the good faith efforts of all the collaborating institutes in assembling a detector over which no person, institute or funding agency has complete control.

Growing experiments: Resource concentration in EE

As was noted in my discussion of focus in EE, research in this field can vary widely in scale and substance. For some researchers, it is sufficient to run simple tests of novel materials, such as combinations of concrete and fiber reinforcements, using basic testing equipment available in most civil engineering laboratories. Increasingly, however, EE research involves expensive large scale earthquake simulations that require highly specialized and sophisticated equipment. For example, a single experimental specimen in one of the laboratories I visited can take up to 6 months to build and easily cost $250,000. There is a small number of laboratories in the world that are equipped to do research on this scale, and a small number of researchers capable of doing this work. One effect of this steady increase in research scale is that the sheer size of these projects frequently mandates collaboration due to the magnitude of the construction and data analysis tasks. This is in stark contrast to the single-investigator model that dominated smaller scale earlier research in this field. Thus, resource concentration in this area of EE research is increasing as the scale of the research increases.

One important aspect of the EE community to consider here is the NEES project. As mentioned above, this project provides the entire community with a set of large scale, shared use experimental facilities. In one sense, this can be seen to reduce the concentration of resources, or at least the access to those resources that are necessary in conducting one's work. I spoke with a faculty member, for example, who conducts large scale experimental work and was getting ready to move to a lab with only small scale equipment. She looked forward to the opportunity to use NEES facilities from her new laboratory, indicating that "NEES helps in making me not have to think about totally giving up large scale testing" (EF30). In this sense, NEES changes resource concentration by making this researcher less dependent on being at a specific laboratory or collaborating with specific colleagues if she wants to conduct large scale experiments.

At the same time, however, there are two ways in which NEES arguably increases resource concentration for all researchers in EE. In the first place, NEES equipment facilities are sufficiently sophisticated that they raise the proverbial bar for what is considered cutting edge research in the field. Researchers at institutions that did not receive funding for NEES equipment are rightfully concerned that they will be marginalized if they cannot successfully access and utilize the NEES equipment sites. Second, NEES provides the capacity to link physical specimens at separate sites together as if they comprised a single specimen. In the MOST experiment (Spencer et al., 2004) run in 2003, for example, a bridge frame was tested. Part of this frame was located at the University of Colorado and another part at the University of Illinois. These were linked by a computational model of the bridge, and the experiment was run as if the two physical models were connected. The advantage of this approach is that more sophisticated structures can be simulated. It is also possible to test a building at one site that focuses on structural research, and its foundation at another site that focuses on geotechnical work. For example, one geotechnical researcher I spoke with indicated that "we've outlined experiments with the Berkeley shake table where Berkeley has the structure and we have the foundation" (EF11).

All of this has two key potential implications for resource concentration. First, it increases the sites' dependence on each other in order to do cutting edge research. In other words, even researchers who are already at equipment sites and wish to conduct multi-site tests must now depend on the other sites for their ability to do this. Second, laboratory space and time on the equipment are already considered to be scarce resources, and are projected to become more scarce under the NEES shared use scenarios. By increasing the quantity of these resources required to do a single experiment, multi-site tests theoretically make the equipment even less available to potential principal investigators at peripheral institutions in the community. Thus, these peripheral researchers must somehow align themselves with these large experiments in a more minor role, suggesting a collaboration model more akin to HEP in a culture that still resonates with the prestige of being a single investigator. As NEES became operational only recently, it is too soon to tell how this will play out. The important point here, though, is that it is clear from these examples that as resource concentration increases, so too does collaboration propensity.

Nobody can do it alone: Resource concentration in neuroscience

Resource concentration varies in neuroscience research. I have already mentioned strong pressure that some researchers feel to develop an independent research program and have a laboratory of their own. At the same time, however, one subject noted (and most acknowledged) that "nobody's going to figure out the brain by themselves, despite what some people think. It's not going to happen" (N1). As I will illustrate here, interviews suggest widespread dependence on colleagues for expertise and small-scale experimental resources, as contrasted with the very large scale resources that are shared in EE and HEP research. These resources were not explicitly considered by the survey administered here, but they are nonetheless concentrated and important—and they will be discussed here.

In the first place, neuroscientists depend on each other for expertise, particularly during different phases in the sequence of a complicated study. For example, I spoke with one expert in functional magnetic resonance imaging (fMRI) whose primary area of research is the processing of these images to generate accurate statistical representations of brain activity. He therefore depends entirely on other researchers, whom in his case are at other institutions, to collect fMRI images and analyze them. As he describes it, "My focus is on getting data sets generated in [two large US cities] and…trying to optimize these processing chains" (N4). Because he is working with human brain images that are considered medical records, he also described an array of legal hurdles involved in transferring these from one institution to another.

Another researcher I spoke with conducts research on mouse and rat brains, and depends on collaborators at another lab to run the animals through a testing protocol before the brain tissue is harvested and analyzed. As he described it:

> We're more interested in the expression of gene X in condition A versus condition B. Well, I'm not set up to do all the condition A versus condition B for those rats, but Dr. [Smith] is. So he runs the rats through the paradigm and then gives me the tissue (N2).

I observed many additional cases of these sorts of symbiotic collaborations in which both researchers are interested in different questions, but have complementary expertise.

Other neuroscientists I spoke with depend on their colleagues for access to experimental or computational resources. For the most part, these were not large physical apparatus that people must travel to access, but rather were small, but difficult to develop, "tools" for conducting research. I place tool in quotation marks because the community uses a broad definition for this term that includes, for example, certain varieties of specially bred (and often patented) mice. Frequently these tools are shared among a group of collaborators.

Similarly, I spoke with several members of one large collaborative project who noted that they are dependent on one of the investigators at another institution who is providing access to a set of human brains that he has worked with the local coroner's office to collect over a span of many years. The brains are stored in a freezer and are then divided up among the different members of the collaboration, based on their interests. One subject described this as follows: "another one of the collaborators is an anatomist who's very careful in, sort of, how to cut the brains in the right way to get the right regions of the brain isolated" (N5). Thus, the collection of human brains in this project serves in some ways serves as a scarce, expensive resource that brings researchers together, as with large laboratory equipment in EE labs, and forces them to depend on each other. In another similar case a subject described the cost of generating good quality FMRI data sets for analysis in his research as one of the reasons he was collaborating with a remote group of researchers spread across the globe:

> It's very expensive to generate a high quality data set…. It's going to be a unique data set in which the subjects performed multiple tasks under a very rigid protocol that took several years to develop, so it's sort of rare data (N4).

Agreement on Quality

Hypothesis 6 states that there should be a positive relationship between the perceived level of agreement on what constitutes quality research and collaboration propensity. This is based on the argument that widespread agreement on what constitutes quality research allows people to find collaboration partners with whom they will be able to work successfully. Hypothesis 6 was supported by these results. As can be seen in Table 3, adding agreement on quality in Model 4 boosts the explanatory power of the model from .31 to .39, which is a statistically significant difference, $F(1, 355) = 48.71$, $p < .001$). In addition, the β coefficient is positive and statistically significant (with p consistently $< .01$) in all models where this variable is present. Indeed, there does appear to be positive relationship between the perceived level of structure and collaboration propensity.

Hypothesis 6A states that agreement on quality will be higher in HEP than in the other two fields. As is shown in Table 6, however, the data do not support this hypothesis. No statistically significant differences were found on this dimension, $F(2,378) = .05$, $p = .95$. This indicates that respondents feel about the same about whether or not there are widely agreed upon standards for what constitutes good research in their area, which is not surprising given the characterization by Hargens (1975) that was discussed in Chapter 2. Despite this lack of differences, however, agreement on quality was shown in Chapter 4 to be an important predictor of collaboration propensity. I will illustrate below that this is because agreement on what constitutes good research allows collaborators to find each other more easily and work together more effectively.

Agreement by design: Agreement on quality in HEP

In HEP, agreement on quality is quite extensive in that there are a very small number of methods used for research, and agreement about what constitutes good research is widespread. Moreover, the nature of the large collaborations is such that one cannot publish a result from these experiments until the results and analysis have been "blessed" by the collaboration leadership and reviewed by all members of the collaboration, as described by one informant:

> There is a publication committee that reviews every prospective paper and circulates it to all the collaboration and everybody has a chance to look at it and, you know, express his opinion before it goes out to the journals (CERN05).

This is strictly enforced. A member of the support staff for one of the CERN collaborations indicated to me that her office was now in charge of all journal submissions from the entire collaboration. When asked why this was necessary, she responded that, "because they thought that otherwise people would just go off anywhere even if it had not been approved. It's kind of just to keep an eye over the whole thing" (CERN12). Thus, it can be seen that not only is there agreement on what constitutes good quality research, but much research is not even formally submitted for review until there is agreement that it is good.

In addition, there is an evident hierarchy of institutes in the field. Undergraduate and graduate students I spoke with informally at CERN, but in particular those from the United States, almost invariably knew the ranking of their university's program and that of close collaborators on their projects. Though this hierarchy fades somewhat in the ostensibly egalitarian collaborations at CERN, there are subtle status cues that can be observed. Wealthier and more prestigious institutes, for example, can afford to have more full-time faculty forego teaching responsibilities and remain at CERN for extended periods of time. They can also afford to hire more research staff at CERN, and their graduate students can travel there more frequently. For these reasons, these institutes often have more office space at CERN, which is an extremely scarce resource, and can often have more impact on the experiment by virtue of all this proximity. The important point here, though, is that some institutes are perceived as "better" than others and these distinctions are generally agreed upon. The implication of this agreement on both of these points for collaboration propensity is that without this agreement, it would be difficult for 1,500 physicists to sign their names to a single paper. Collaboration would be more difficult, and perhaps less likely.

A common framework: Agreement on quality in EE

There is evidence of some agreement on what constitutes good research and on what constitutes the hierarchy of journals and institutions in EE. Moreover, the level of agreement appears to be increasing as the community moves toward investigations of increasing scale. Universities that have very advanced labs are consistently highly ranked by the National Research Council, and the leaders of these laboratories are consistently named as leaders in the field when respondents were asked. Respondents also tended to name the same top journals and conferences (in EE) when asked where they tried to publish their work.

In terms of assessing research work, all of EE has its foundations in civil engineering. Thus, all earthquake engineers have similar background and training, and all rely heavily, though admittedly to varying degrees, on the ability to model and demonstrate their findings using the rigorous and universal language of mathematics. This suggests adherence to a high-level set of principles and general agreement. At a more detailed level, however, there is far less evidence of agreement as evidenced by the methodological diversity in the field explained above. In terms of collaboration propensity, there can be little doubt that the common reference frame and evaluation standards furnished by a shared background in civil engineering renders collaboration propensity higher when structure is also perceived to be higher.

Agreement on quality in neuroscience

Agreement on quality in neuroscience can be considered along two dimensions. In the first place, I have already documented that there is widespread disagreement over what methods and techniques are the appropriate ones to be using in addressing specific types of problems. At the same time, however, there is evidence of agreement that the most prestigious and prominent place to publish results is in the journals *Science* and *Nature*. There is not, however, evidence of widespread agreement that all of the work that gets published in these top journals is considered to be of uniformly good quality. One informant said, for example, that "the garbage that gets published in *Science* and *Nature* by single-author zealots is not worth reading most of the time, in my opinion" (N1). Agreement on quality therefore is arguably less important than other factors in neuroscience, despite the lack of a statistically significant interaction effect (see above).

Need For and Availability of Help

Hypothesis 7 states that there should be a positive relationship between the need for and availability of help and collaboration propensity. This is based on the argument that collaboration should be more likely for people who frequently turn to others for help with their work. Hypothesis 7 was strongly supported by these results, as is illustrated in Table 3. Adding the need for and availability of help in Model 5 boosts the explanatory power of the model from .39 to .48, which is a statistically significant difference, $F(1,354) = 63.10$, $p < .001$) and is the largest difference between any pair of models. This indicates that the need for and availability of help explains a larger fraction of the observed variance than any of the other factors. Moreover, the standardized beta coefficient is positive and statistically significant (with p consistently $< .01$) in all of the models where this variable is present. Thus, there does appear to be a positive relationship between ease of coupling and collaboration propensity.

Hypothesis 7A states that the need for and availability of help in HEP should be higher than in the other two fields. As is shown in Table 6, the data support this hypothesis. Ease of coupling in HEP does differ from the other two fields in a statistically significant way $F(2,378) = 12.98$, $p < .001$, but the other two fields do not differ from each other. As was demonstrated in Chapter 4, the need for and availability of help was shown to be an important predictor of collaboration propensity. I will illustrate here that this is because the nature of work in some fields is better suited to frequent interaction and because the nature and configuration of certain laboratories foster interactions that lead to collaboration.

Need for and availability of help in HEP

In HEP, there are two levels of analysis at which help-seeking interactions might be considered: the entire experiment and the smaller workgroups that comprise it. Interactions at both levels are important in the day-to-day work of physicists and, to some extent, the level of analysis at which one seeks help reflects one's status in the experiment hierarchy. Workgroup leaders and experiment management will spend more time getting information from other workgroup leaders than the physicists more directly engaged in carrying out software and analysis tasks. There are also two types of interaction at these levels of analysis that can be considered: formal and informal. By formal interaction, I mean the acknowledged and scheduled meetings and conversations that must take place in order for the experiment to move forward, such as workgroup meetings at CERN or 'collaboration weeks,' in which many members of a collaboration gather for a global status report.

Figure 5 Cafe area of Building 40 at CERN

Informal interaction, on the other hand, largely involves conversations of the sort described by Kraut, Egido and Gallegher (1990). On HEP experiments this can range from running into somebody in the CERN cafeteria or a Building 40 corridor, to phone calls or email dialogues between colleagues that are geographically separated. Most subjects I spoke with, however, indicated that the most valuable informal interaction occurred at CERN. For example, all physicists I spoke with described the importance of visiting CERN on a regular basis in order to make contact with their colleagues and discuss problems and potential issues. As one informant indicated:

> I always tell my wife that coming here [to CERN] means working like a graduate student again. So you work 20 hour days, and then you fly home and work from 9 to 5 again. It's quite a bit like that, and it's not as if I don't get anything done when I'm at home. But here there's meetings and you still have some work to do. … You have an enormous amount of information exchange and you know you're going to go back home and there's a hole (CERN02).

Figure 5 shows the café area in Building 40, where many of these interactions take place. Table 7 provides examples of both formal and informal interaction at both of these levels of analysis.

Table 7 Examples of interaction types and levels

	Experiment-Level	Workgroup-Level
Formal	"Collaboration Weeks"	Weekly group meetings
Informal	Interaction in the CERN cafeteria	Water cooler and hallway interactions

First, consider interaction at the experiment level. HEP experiments, particularly in the planning and construction phases, typically are organized according to the components of the detectors with which they work, with groups organized around and focused on "naturally separate" parts of the detector, such as the transition radiation tracker (TRT) or the muon detection subsystem. Research groups from institutes align themselves with one of these workgroups early on and typically take responsibility for a certain part of the detector. The University of Michigan, for example, is involved in the construction and testing of several thousand muon drift tubes. At the experiment level of analysis, most of the important interaction that occurs is between these groups. As was noted earlier, it is imperative that these groups communicate with each other to ensure that the hardware and software systems that are being developed will function properly. Some of this communication takes place through technical reports and drawings that provide formal documentation of standards and decisions that have been made. Physicists involved at the lower hierarchical levels of the experiment do not report a lot of informal contact with people outside their immediate workgroup, but they do report referring to these formal reports on a regular basis. One subject indicated that "in most cases, it's less than ten people who are interested in specific things. Beyond that group you don't interact much" (CERN6). When these subjects do get updated information from other groups, it is typically in formally scheduled meetings. For example, the project leader of the ATLAS TRT system indicated that he sees it as his job to "keep the engines going and make the [sub]collaboration as a whole communicate and get together and exchange, present things so that the information flows…" (CERN14). When asked how he accomplishes this specifically, he indicated that it was mostly via meetings at which workgroups presented their progress and problems to other workgroups within the TRT sub-collaboration.

For those involved as project team leaders and in the experiment leadership, on the other hand, there is also a great deal of important informal interaction at the experiment level. While all leaders I spoke with at various levels indicated spending a great deal of time in formal meetings, many also indicated the importance of the more private, less formal conversations that can help lead to consensus. The spokesman for one of the LHC experiments noted that, "bringing people together and maybe talk among a few people, key people, is maybe better than talking only in a public meeting room" (CERN19). He also indicated the importance of brief conversations with leaders of various subcomponents to have a sense of what is going on: "I feel it's important to get the temperature…by talking to people where there is not really a problem, even if it is only very short and artificial" (CERN19). Indeed, respondents report that it is often in these informal conversations that actual decisions are made, where more formal meetings serve as forums to officially enact and discuss these decisions. One informant, who is a member of the US leadership team for ATLAS, indicated that he generally spends about five hours per day on the telephone, between conference calls and informal conversations.

At the workgroup level of analysis, most help-seeking interaction occurs between people involved in the same workgroup. Some of these interactions are formal, such as workgroup meetings and conference calls, whereas others are more sporadic, such as meeting somebody in the hallway or getting coffee. Most physicists I spoke with indicated that they have meetings with their immediate workgroup at least weekly, and frequently more than that. These meetings, like those discussed earlier at the experiment level, generally serve as forums to provide updates on tasks and discuss any potential problems. Another function that these meetings appear to serve, particularly for more junior members of the collaboration, is to provide information about who is working on what. When asked how he finds people to talk to when he runs into trouble, one first year graduate student I spoke with said:

> when there's a meeting you can see who has prepared something and what they have prepared. Um, you will very quickly see on the email lists who are quick to respond to problems and offer solutions. That's the way it works, I think (CERN23).

Thus, these formal meetings provide opportunities for information exchange and interaction, in addition to providing a springboard for informal, sporadic interactions.

With regard to these sporadic interactions, virtually all subjects reported that conversations with colleagues were extremely important when they ran into trouble or in keeping abreast of developments in their workgroup. Though all respondents reported frequent use of email for sporadic interactions, most also indicated a strong preference for face to face interaction, particularly when at CERN. In some cases, email and the telephone are simply not effective. For example, in hardware development this subject noted:

> I think that's one of the key points in hardware, what I do for this detector. You want somebody looking at the thing. It's not like when you have a software problem and you can solve it remotely, you can send the code. No. Here you really have to go there, take the oscilloscope, try to see this or that. You know? You need somebody on site helping you (CERN9).

Another subject reported that traveling to CERN gave his interactions with colleagues there a certain urgency that email was unable to capture:

> It's easier to keep pushing things off when you're not, you know, physically in the same room and that sense of urgency, I think, is probably

> worth coming out here for. You can actually finish jobs. It's really easy to start a job in physics. The thing that separates the sheep from the goats is the ability to end one (CERN20).

Thus, we see here the importance of informal interaction in troubleshooting and in catalyzing projects that involve dependence on colleagues.

Overall, this discussion suggests that the nature of work in HEP renders the need for and amount of help-seeking interactions high relative to the other fields being considered here. Physicists have a tremendous amount of technical and social infrastructure in place to support regular interaction and information exchange. Large collaborations on relatively uniform projects also mandate standardization and interaction. Moreover, physicists have a history of collaboration and a physical environment at CERN that encourages turning to others for help by virtue of concentrating so many physicists in a small space and providing so many opportunities for sporadic encounters between colleagues from around the world.

Scale brings people together: Need for and availability of help in EE

The scale and laboratory configuration of some EE research invite frequent calls for assistance and ensure that such assistance is readily available. Consider first the configuration of earthquake engineering research facilities that contain large scale test equipment. This equipment is typically located in a very large, open space that is shared by multiple concurrent experiments being conducted by different researchers. Walking through the laboratory, it is nearly impossible not to notice what colleagues are working on. It is common for students working on projects to seek each other out for advice and, indeed, in most laboratories it is forbidden to work alone for safety reasons – so students from different experiments sometimes make arrangements to work late together. In addition, during all of the large scale experiments I observed that graduate students in the department, but not involved with the experiment being observed, showed up as volunteers to help construct and troubleshoot the test underway. When asked, all students said this was a fairly standard practice. This serves to enforce some standardization of procedures, so that students are able to help each other out, and also forces some interaction between the students and laboratory staff (see Figure 6). Both of these arguably serve to increase the need for and availability of help.

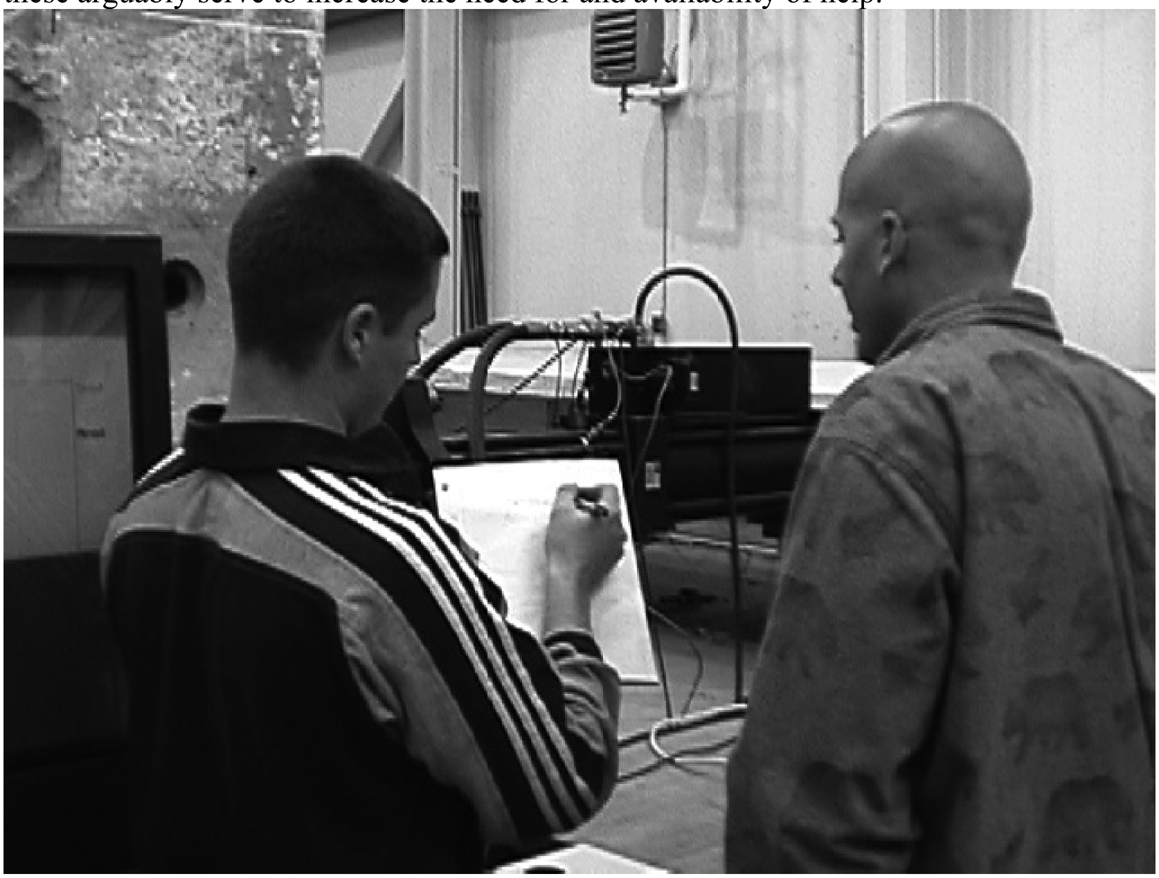

Figure 6 Graduate student and lab technician discussing results in a structural EE lab.

It is also important to note that this configuration stands in stark contrast to conditions I observed at smaller scale research sites. In particular, I spoke with one graduate student at a small scale research site who described spending days and nights alone in the model preparation area, troubleshooting problems for which he had a great deal of difficulty finding answers. This laboratory also could not afford to hire a technician, which also significantly impedes the informal standardization of research methods. Without a technician and with few opportunities for forced interactions between students, it is extremely difficult for students and faculty alike to know what others are doing. An interview with a faculty member who did his graduate work in this laboratory also suggests that this impacts collaboration propensity in that he reported doing most of his research alone without even the assistance of a graduate student.

All of this suggests the importance of the need for and availability of help in EE within collocated laboratory groups in predicting collaboration propensity. The scale of a structural engineering laboratory provides a function similar to that which will be illustrated below for CERN, but on a more localized level, in that it is a common focal point that serves to attract local colleagues during preparations for testing. Moreover, this structure serves to create an environment in which people are comfortable and expected to turn to others for help and advice. At the same time, however, there are sufficient site-specific differences that interaction across projects would be difficult, particularly if different laboratories are involved.

Need for and availability of help in neuroscience

The discussion of neuroscience as presented in this chapter so far suggests that there is little overall agreement on important research questions, how they are to be solved, or standards for attributing credit and sharing data. At the same time, however, there is a growing recognition of dependence on other researchers for access to expertise and experimental resources in order to do effective research. This has important consequences for help-seeking interactions in this community in that these observed pockets of collaboration and standardization are settings in which interaction is likely to be quite easy, and deliberate efforts to further simplify this have been observed, such as the laboratory that produces reagents and invites people to spend sabbatical time in their lab learning how to use these reagents. Thus, as is illustrated below, it can be said that the need for and availability of help in neuroscience exists within these emergent pockets, but not at the level of the overall community as in HEP.

In the first place, consider the nature of the daily work of neuroscience research, particularly as contrasted with HEP and EE. In EE work, laboratories typically consist of very large open spaces that typically house multiple research projects, thus affording many opportunities for interaction and observation between people working on separate, but similar projects. HEP is a more extreme version of this in which people from many institutes are sporadically collocated at a large facility like CERN or Fermilab. In neuroscience, on the other hand, PIs almost invariably have their own lab space, in which their own staff of students and postdocs conduct their work. While these students and postdocs (and, less frequently, the PI) certainly have opportunities for unplanned interactions in this laboratory space, there are fewer opportunities for this sort of interaction with other colleagues while conducting actual lab work. To be sure, there are many opportunities for interaction in hallways, offices and near proverbial water coolers, but this difference in research scale does appear to play a sort of governing role in determining likely interaction patterns and, by extension, the ease of coupling.

With this in mind, I asked neuroscientists about the nature of their interaction with colleagues and their responses indicated that, even in collaborative projects, most tightly coupled work is done separately in local laboratories. The collaborators come together early in the project to design the research project, then do separate work in their individual labs, and then get together again to discuss analysis and results. This process is similar whether the collaborations involve colleagues who are local or remote. As one subject put it:

> The nuts and bolts of it are about the same, whether I'm in the institution or away. What lacks is, um, in these sort of cross-country or international collaborations is the moments where you're just talking, and you sort of get these 'a ha!' moments where you've both been thinking about something that's been rambling around in your head (N12).

Other subjects reported compensating for this lack of opportunities for informal interaction with remote colleagues by, for example, setting up a conference call number where a group of collaborators can set up an impromptu conference any time. The more important point here, though, is that these interactions are not taking place in the laboratory, whether the collaborators are local or remote. Thus, ease of coupling here is less a function of how tightly coupled the lab work might be considered, and more about the ability to speak a common language and have opportunities and venues for informal interaction.

This ability to understand each other and achieve common ground, in turn, depends on a shared understanding of the work being done, in terms of the methods and tools being used and desired outcomes. Frequently this understanding need only be achieved at a relatively high level of granularity, as was the case with the neuroscientist mentioned earlier who could use whatever methods she wanted in doing her genetics work, because nobody understood what she was doing at a fine level of detail. In a similar case, one subject who does research on mice mentioned his collaboration with researchers who study humans:

> They usually send me, you know, graphs and they'll say 'Look, this is what we found and this is what it means.' And you know I just take their word for it because I don't know how to do neuropsychological exams on people. I know how to do them on mice (N12).

Even at this higher level, though, a detailed understanding of the problem at hand and the general nature of the methods being used is required in order to make the dialogue useful. This can be found, as was discussed earlier, in the pockets of agreement that form around particular problems and methods.

Network-Based Tool Usage

Based on limited evidence from prior studies suggesting that the usage of email can impact scientific productivity, Hypothesis 8 stated that the frequency with which network-based collaboration tools are used should have a positive relationship with collaboration propensity. Indeed, such tools make it potentially easier to talk with more people and juggle more simultaneous projects with remote collaborators. As can be see in Table 3, adding the frequency of network tool usage in Model 6 provides a slight, but statistically significant, boost in explanatory power over Model 5, $F (1, 353) = 16.05$, $p < .001$). In addition, the standardized beta coefficient is positive and statistically significant ($p < .01$). Thus, there does appear to be a positive relationship between the frequency of usage of Internet-based tool usage and collaboration propensity. Hypothesis 8 is supported by these results.

Moreover, Hypothesis 8A suggests that there should be variation in the frequency with which Internet-based collaboration tools are used in different fields. As is shown in Table 7, the data strongly support this hypothesis, as the mean usage is 6.5 on a 7 point scale in HEP, compared with 4.5 and 4.98 in the other fields, respectively. Neuroscientists also report slightly more frequent use than EE respondents, and this difference was found to be statistically significant. For traditional tools, the difference between physicists and the other two fields was statistically significant, but there was no significant difference between EE and neuroscience.

The Role of Demographic and Control Factors

Does Field Matter?

One important question in assessing the validity of these models is ensuring that the observed variance in collaboration propensity cannot be explained by field of research alone, and that the significant independent predictors are still effective even when field is controlled for. The direct effects of field of research on collaboration propensity can be seen in Table 3. As the table shows, the two field variables (two of the three are used because these are binary coded dummy variables) are the most powerful predictors in Model 1, but this model explains a relatively small amount of the observed variance when compared with later models (R^2 for Model 1 = .19 vs. .50 for Model 6).

Nonetheless, there is a consistently negative and statistically significant relationship between earthquake engineering and collaboration propensity. As is mentioned elsewhere this suggests that, despite the poor predictive power of the specific cultural attributes measured here, there may be some unmeasured properties of the EE field that inhibit collaboration.

Moreover, the early models (1-3) also suggest a relationship between HEP and collaboration propensity, but this relationship weakens substantially (and eventually fades almost completely) as more powerful explanatory variables are added. This weakening raises the possibility that these explanatory variables merely describe HEP, which previous studies suggest has higher collaboration requirements than the other two fields, and that HEP is actually explaining most of the observed variance in this data set. There are two important reasons why this is not the case, however. In the first place, comparing the R-square values for Model 1 (.19) with that of Model 6 (.47) clearly illustrates that that Resource concentration, Coupling and Structure explain substantially more variance than HEP alone, when all other factors are controlled for. In addition, I ran the same regression models on a reduced data set that excluded all HEP respondents. Results were largely similar, though the models were somewhat less powerful, possibly due in part to the substantial reduction in the number of cases under consideration.

Individual Collaboration Experience

As was noted in Chapter 2, it stands to reason that quality and frequency of prior collaboration experience could impact collaboration propensity. The effect of whether or not a researcher has recent prior collaboration experience, which is how frequency is here operationalized, is described in the next paragraph. Quality of experience was included in early regression models, but was found not to contribute in a useful way. This may be due in part to the low level of variance on this measure, which is illustrated in Appendix G.

With regard to whether or not a researcher has prior collaboration experience, recall that respondents were asked whether they had participated in both local and remote collaborations within the last five years (where collaboration is defined as publishing a paper with co-authors). I will first consider local collaboration experience. As can be seen in Table 3, adding this variable in Model 2 adds no additional explanatory power over Model 1. Moreover, the standardized beta coefficient is 0. Thus, there does not appear to be a relationship between local collaboration experience and collaboration propensity.

With regard to publishing a paper with remote co-authors, it can be seen in Table 3 that adding this variable in Model 2 also provides no significant boost in explanatory power over Model 1. In addition, the standardized beta coefficient is positive and marginally significant. Iin Model 5 there does appear to be a positive, though limited, relationship between Remote Collaboration Experience and Collaboration Propensity. It is not entirely clear why this should be true, and this is a possible subject for further study. What is important for our immediate purposes, however, is that this relationship is controlled for in the results presented above.

Traditional Tool Experience

Table 8 Descriptive Statistics for Frequencies of Internet-Based and Traditional Collaboration and Communication Tools (N=381)

Variable	Physics			EE			Neuroscience		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Traditional Tool Use	5.24_a	1.58	127	3.43_b	1.65	129	3.40_b	1.81	125

Note:
Means in the same row that do not share a subscript differ at $p < .05$ in contrast tests performed within an ANOVA analysis.

With regard to traditional tool usage, it is noteworthy in Model 6 in Table 3 that there appears to be a slight, negative relationship between the frequency of traditional tool usage and collaboration propensity. This relationship was not hypothesized in Chapter 2, but is nonetheless quite surprising and interesting. It is difficult to explore this finding further with the data collected here, but further study is warranted and suggested in Chapter 5. Additionally, there was a statistically significant difference between fields in their usage of traditional tools, as can be seen in Table 8. This, of course, makes intuitive sense given the large collaborations comprised of individuals at many institutions in HEP.

Field Tenure

It was suggested in Chapter 2 that the number of years a researcher has spent in his or her field could impact collaboration propensity, particularly in fields where collaboration is a new approach to problems that have traditionally been tackled using a single investigator model. As can be seen in Table 3, field tenure was a marginally significant and weak negative predictor of collaboration propensity in Models 1 – 3, but becomes insignificant in the more powerful models as additional independent factors are added. Thus, field tenure does not appear to be an important factor here, but it must nonetheless be controlled for to demonstrate this.

Summary

This chapter presented a critical comparison of two potential theoretical approaches to the prediction of collaboration propensity. It was shown that a disciplinary culture approach to this problem, which relies on competition, industrial ties and collectivist credit attribution practices as its primary indepdendent factors, provided little explanatory power in linear regression models. Rather, an approach focused on attributes of work was shown to be substantially more powerful. In particular, resource concentration, agreement on quality and the need for and availability of help were shown to be the most important factors in this perspective. These results suggest strongly that, when considering the likelihood of collaboration, this latter perspective is more valuable. In some ways, this is a surprising finding in that it suggests a strong willingness to overcome obstacles to collaboration that are presented by research cultures strongly focused on individuals. This is in opposition to many of the studies of data sharing and secrecy cited in Chapter 2.

CHAPTER 5

DISCUSSION

As was illustrated in the previous chapter, the results of this study suggest that the attributes of work measured here were more useful in predicting collaboration propensity than the specific attributes of culture that were studied. I will argue in this chapter that important differences between collaboration and other activities in science that are often discussed in concert with collaboration, such as the free sharing of data, play a key role in these results. If we are to better understand collaboration and collaboration propensity, I argue that there is a need for more directed social studies of scientists, with a focus on the nature of their work and institutional context, to complement the value gained from more broadly construed cultural and social studies of science.

Generally speaking, this work has important implications for the theoretical understanding of collaboration and scientific work. It also suggests means for designers and policy makers to assess the need for collaboration tools, and some preliminary approaches to their development. These implications will be presented in detail later in this chapter, followed by directions for future research will be presented that will enhance our understanding of collaboration propensity, collaboration success and effectiveness, and how these relate specifically to ICT.

Discussion of Cultural Results

From a cultural standpoint, two important high-level findings were presented in Chapter 4. First, It was demonstrated that the specific cultural factors measured here that are related to individual versus collective orientation were not useful in predicting collaboration propensity. Nonetheless, it was also shown that field of research does have some influence on collaboration propensity, which suggests that there may be elements of culture not measured here that are important in determining collaboration propensity. Thus, these results do not necessarily cast doubt on the broad set of cultural attributes that might be measured – just those specific factors that are examined here. I will discuss the results for these factors here, followed by a general discussion of this theoretical approach.

Constraint, but not Impediment: Scientific Competition

In fields where individual researchers are worried about being "scooped" by competitors and consumed with augmenting their own reputations via single-author publications and awards, it stands to reason that they would also be more wary about turning to others for help or sharing their resources openly with a collaborator. Contrary to these expectations, however, the qualitative and quantitative data presented here show little, if any, evidence of a relationship between scientific competition and collaboration propensity. In other words, the presence of scientific competition neither encourages nor discourages collaboration in the fields studied here.

The examination of qualitative data shed some light on this result in two ways. First of all, there was evidence of substantial scientific competition in all fields studied – even in HEP, which has been previously characterized as being highly collectivist and collaborative in its culture. This leads to the second point, which is that respondents indicated that the presence of scientific competition was a fact of life that constrains their choice of collaborators to trusted colleagues and affects their willingness to share more openly, but does not impede their willingness or desire to collaborate. Where collaboration is useful and trusted colleagues are present it occurs, competition notwithstanding.

Proximity to Industry

As with scientific competition, a cultural approach to collaboration would suggest that perceived close ties to industry should discourage collaboration. Where there is strong interest in commercializing and patenting research discoveries, there are likely to be increased concerns with secrecy and information privacy. It seems reasonable that this should also reduce researchers' propensity to collaborate. As was shown in Chapter 4, however, the data presented here do not support this explanation. No relationship was found between proximity to industry and collaboration propensity.

As was illustrated in Chapter 4, the fundamental explanation for this seems to be similar to what occurred with scientific competition. Specifically, proximity to industry and concerns about intellectual property and secrecy did emerge as roadblocks to collaborations, but they were not insurmountable when collaboration was useful or necessary in answering questions of interest. Rather, there was evidence of substantial effort to make collaborations work despite these roadblocks. Respondents reported being frustrated by the involvement of lawyers and elaborate intellectual property agreements in their collaborative endeavors, but most also acknowledged the rationale behind this.

Another factor to consider is that the pressures for commercialization and intellectual property are coming from pharmaceutical companies, private funders and universities seeking to profit from intellectual property. I observed no cases where the researchers themselves were seeking to profit directly from their discoveries. In this way, proximity to industry is a constraint on scientists' behavior, but it is not their chief concern. They are concerned with solving problems of interest and augmenting their reputations and take the steps necessary to accomplish these goals, within the constraints established by interested third parties.

Ease of Collaborative Credit Attribution

A disciplinary cultural approach to collaboration would suggest that on projects where it is easy to determine whom to include as one's co-authors and when it is clear how one will receive credit for involvement in collaborative projects we should see an increased propensity to collaborate. This is because the uncertainty frequently associated with collaboration should be reduced. As was illustrated above, however, the data presented here do not support this result. No relationship was found between the perception of standard practices for collective credit attribution and propensity to collaborate.

There appeared to be two reasons for this. In the first place, it is interesting to note that people in the fields that do not have collaborative credit attribution practices (EE and neuroscience) were generally satisfied with working this out on a case by case basis, and those who were in the field that does have an elaborate credit attribution protocol (HEP) generally complained about this system, indicating that its actual effect is to dissolve the value of any credit that it attributes by virtue of its extremely collective nature. Moreover, researchers' feelings in EE and neuroscience varied widely about how formalized credit attribution agreements should be, and whether this should be negotiated before or after the completion of a project. While some researchers I spoke with would almost certainly be somewhat more likely to collaborate if this uncertainty was reduced at the outset of a project, this was not universally true. Attribution standards development did not, however, emerge as an important factor in qualitatively understanding propensity to collaborate. It appears to be the case that both the presence and absence of such standards can complicate collaboration in some cases, depending on the specific circumstances.

Why Does This Theory Work Elsewhere?

I have argued that the attributes of culture measured here are not useful in predicting collaboration propensity, but that I am not trying to overturn the established literature suggesting that a cultural approach to collaboration can be informative in some situations. Essentially there are two types of cases that must be considered in this discussion. First are the cases where culture seems important to collaboration propensity, but the important attributes of culture are not those measured in this study. Second are the cases where the specific attributes of culture measured here are useful, but collaboration propensity is not the variable of interest.

In the first place, I have mentioned above that field of research does seem to play some role in determining collaboration propensity, particularly in EE where the dummy variable remains negative and statistically significant in all the OLS models presented in Table 3. Though the precise reasons for this cannot be discerned from the data collected here, it is likely the result of a strong (arguably cultural) tradition of single-investigator research in EE laboratories and, as was also mentioned in Chapter 4, a significant value placed on collecting and analyzing one's own data. This suggests a need for a more careful examination of the relationship between collaboration propensity and specific attributes of a broader construction of culture. In other words, this study operationalizes, measures and examines one specific dimension of disciplinary culture and its relationship to collaboration propensity. These results give reason to believe that other aspects of culture may be playing an important role here, but more work is needed to isolate, operationalize and study these specific attributes.

Additionally, the marginal significance of the focus interaction term in Model 8 suggests preliminarily that when earthquake engineers report a high level of focus on similar problems and methods, their collaboration propensity is higher than that of their EE peers. This suggests there may be value in looking more closely at the earthquake engineers who reported higher focus to better understand why they reported higher focus and how their collaboration behavior differs from that of their peers.

Second, we must address the question of what is different about the present situation from other settings where the attributes of culture studied here have been more useful in the past, such as the usage of data sharing systems and communication technologies. There are two possible answers to this question, and they are not mutually exclusive. One is that collaboration is different in fundamental ways from other activities, such as data sharing, that have been studied in the literature. Another is that many other studies have not been comparative in nature or did not use quantitative measures, and it is therefore difficult to gauge the relative explanatory power of a cultural approach in these settings.

With regard to the first explanation, I contend that collaboration is different in important ways from data sharing and other more collectively oriented activities. At a fundamental level, success in science is rewarded and accrued largely at the individual level. Achievement requires control—or access to control, at some level–over the data and other experimental resources necessary to make and publish research discoveries. For many scientists, activities that threaten this control (or access to control) without compensating for this threat in some way are not considered desirable activities. Thus, concerns about competition, secrecy and credit attribution can be seen in some ways to be a proxy for the level of threat to control that a researcher is willing to tolerate. Public data sharing by individual or small team investigators, because it involves sacrificing exclusive rights to one's data, are strongly affected by these concerns because it involves a high degree of threat to control. Sharing one's data publicly gives anybody access to those data – and the potential discoveries that lie within. Indeed, where public data sharing has been successful it has largely been in cases where there is some sort of compensation (in the form of publication credit, for example).

Collaboration, on the other hand, does not threaten the control of individual investigators. Rather, as my interview subjects indicated, it involves the sharing of work between colleagues who have negotiated both a trusting relationship and the specific terms of the collaboration, be they formal or informal. Moreover, my informants further demonstrated that collaborative relationships are typically symbiotic—both parties get something they would not otherwise have, and the whole is frequently greater than the sum of its parts. Thus, collaboration propensity is less sensitive than data sharing to concerns about the cultural factors presented here.

Another important factor to bear in mind is that existing studies have shown that there is a negative relationship between these cultural concerns, particularly with regard to competition and proximity to industry, and willingness to share data. There have been no studies I am aware of, however, that demonstrate a positive relationship between a lack of concern about competition and a willingness to share data. Even in HEP, widely characterized as highly collectivist in orientation, we see strong evidence of competition and secrecy between experiments and even workgroups within experiments. Thus, while an individually focused culture can predict individually focused behavior, one wonders if there currently exists a sufficiently collectivist culture within science in which we would expect to find evidence of truly collectivist behavior.

Liabilities of a cultural approach

This last point leads to an important liability of a cultural approach to understanding science more generally, particularly with regard to cross-disciplinary comparisons. While it is undoubtedly true, as is persuasively illustrated by Knorr-Cetina

(1999), Collins (1998) and others that there are cultural differences within science, it is difficult to say concretely how much these differences matter in a system that is, in all fields, fundamentally based on a reward system at the individual level. There does seem to be some impact, but the results presented here illustrate the need for a more careful examination of the extent to which these cultural distinctions actually make a real difference and the specific attributes that are important in understanding and predicting the individual behavior of scientists. The relative strengths of these effects, in other words, must be more systematically assessed.

Discussion of Work Related Attributes

I showed in Chapter 4 that factors presented here based on the day-to-day work being performed proved quite effective in explaining collaboration propensity. Results for the most important of these factors will be discussed here, followed by a general discussion of this theoretical approach.

Resource concentration

It is not surprising that resource concentration proves to be valuable in predicting collaboration propensity. Where researchers perceive dependence on each other for access to critical experimental and financial resources, we would expect them to have a higher propensity to collaborate. As was illustrated in the previous chapters, the data presented here support this claim. This was further illustrated by qualitative data suggesting that the nature of resource concentration varies across the fields studied here. In neuroscience, the focus is on small-scale experimental resources and expertise, whereas in EE and HEP the focus is more on access to scarce and very large experimental apparatus. It must be mentioned, of course, that the apparatus in HEP clearly dwarf by many orders of magnitude the nonetheless-large equipment used in EE laboratories.

Despite these differences, however, there do not appear to be differences in the fundamental ways that resource concentration impacts collaboration propensity.

Agreement on Quality

Where there is perceived agreement on assessment criteria and agreement on what institutions are doing good research, we would also expect to see greater collaboration propensity because it will be easier for researchers to work together. As was illustrated above, this claim is also supported strongly by the data presented here. It was also illustrated qualitatively in Chapter 4 that the nature of this agreement varies by field somewhat. Earthquake engineers all have common roots in civil engineering, whereas neuroscientists are a diverse set of researchers who nonetheless agree on the important venues for research publication. Both of these are in contrast to HEP, where collaborations have an elaborate structure for approving results that get submitted for publication in order to institutionally ensure agreement.

Need for and Availability of Help

When researchers are accustomed to turning to each other for help, and where such help is available, we would again expect to see higher collaboration propensity. As was illustrated above, this claim too is supported by the data. I also showed in Chapter 4 via qualitative data that the scale of research may play a role in this. Large scale equipment has the effect of aggregating researchers around it, as is the case in the extreme at CERN and to a lesser extent in structural EE laboratories, thus encouraging interaction and implicit standardization of research methods. There is also some theoretical chance that this then encourages the aggregation of resources, development of larger experiments, and increased resource concentration, but exploring this speculation is beyond the scope of this study. On the other hand, where research is conducted at smaller scales, I found some limited evidence to suggest that there is less interaction, less standardization and less interest in collaboration.

Network-Based Tool Usage

It was expected that increased frequency of network-based collaboration tool usage would correlate positively with collaboration propensity, and this was shown to be the case. This is interesting in that it suggests preliminarily that frequent use of network-based communication and collaboration tools may increase the likelihood of collaboration. In cases where collaboration is desirable, this is a potentially valuable finding for developers and funders of these network-based technologies. At the same time, however, it must be remembered that these results cannot demonstrate a definitive causal relationship between these factors (see below), and that the measure used does not allow for isolation of specific communication and collaboration tools. Thus, more study is needed to better understand this preliminary, but promising, finding.

Covariate and Control Variables

While less powerful than the factors just mentioned in predicting collaboration propensity, frequent use of traditional collaboration tools does emerge as a statistically significant predictor, though the relationship is negative. This was particularly true for those who had remote collaboration experience. More study is needed to fully understand this finding. Oddly, the relationship between traditional tool usage and collaboration propensity was negative and statistically significant. This is puzzling and I could find little reason for it in reviewing and analyzing the qualitative data.

Why Does This Theory Work Here?

Fundamentally, the reason this work-focused theoretical approach works well in explaining collaboration propensity is the same as the reason the cultural approach used here does not. Even though promotion and recognition in science generally occurs at the individual level, achieving these outcomes requires scientific outcomes. And where collaboration is perceived to be a useful, expedient or essential way to reach high quality and/or novel scientific outcomes, researchers will collaborate. Indeed, my qualitative results depict a wide range of settings in which collaboration occurs, and for a wide range of reasons. As illustrated above, they also depict researchers willing to work around a variety of roadblocks in order to achieve collaborative goals.

Theoretical Implications

The broader theoretical implication of this finding is that there is a general need to look more closely at the individual attributes of scientific work that impact broader observed phenomena. In other words, there is a need for a subtle but important shift from “social studies of science” to “social studies of scientists.” Such a shift has value both in considering the work attributes studied here more carefully and in providing a forum for identifying and isolating components of culture that may be important to collaboration but that were not specifically measured here. In some ways, this general need echoes claims previously made by Vaughan (1999), who called for a more careful consideration of the organizational setting of knowledge creation. By moving from high-level studies of science to more detailed and systematic studies of scientific work, we can draw on an extensive literature of individual motivations and group/organizational behavior and information processing. Such an approach, particularly as collaboration and the development of collaboration tools, become increasingly important will arguably allow for a more valuable and nuanced understanding of collaboration that is demonstrated here to be more powerful.

I am not, of course, suggesting that the factors studied here are the only ones that matter in understanding science and collaboration. I note below, in fact, that one important area of future research is further developing and sharpening these and other important attributes of collaboration propensity. Rather, what I am suggesting is that we change the level and nature of analysis of scientific work. These results suggest that differences between disciplines may in fact turn out to be less important than differences between the work styles and attributes of individual researchers within these fields. As I suggest in the next section, this has important implications for design and policy.

In this light, the major theoretical implication of this study is that it demonstrates the effectiveness of this approach, and provides the framework for a preliminary theory of collaboration propensity. It also suggests the need to combine work-based analysis as presented here with other methods, such as social network analyses that consider the roles of particular social relationships and experiences in predicting collaboration behavior. If we are to understand collaboration more completely, we must consider both of these approaches.

Policy Implications

This work has several important policy implications. First, the lack of explanatory power for cultural factors and the distinction between collaboration and sharing provide some indication that when considering collaboration tools and their adoption, concerns about scientific competition and industrial proximity as impediments may be unwarranted. This is, of course, not the case for the development of community data sharing systems. For collaboration tools, though, a focus on support for existing collaborative groups and respect for their privacy (as detailed below) may be enough to overcome perceived potential cultural barriers. Researchers to whom collaboration is valuable will overcome these barriers on their own.

In addition, these results suggest that collaborations are not being constrained by distance. Indeed, there was a positive relationship between having a remote co-author and collaboration propensity. This, combined with other recent findings increases the imperative for the development of effective collaboration tools for distributed research collaborations. Moreover, the importance of work attributes and the lack of significant interaction effects between field of research and these factors suggest that it may be more useful to invest resources in tools for specific types of work, rather than in tools for specific disciplinary communities. Indeed, the regression models in Chapter 4 clearly show that the important distinctions between research styles for the purposes of collaboration propensity do not exist along disciplinary boundaries, but within the disciplines. Thus, developing collaboration tools for researchers engaged in work that has specific attributes may be a more valuable approach than attempting to develop tools that whole communities of researchers, who may be engaged in disparate forms of research with varying degrees of collaboration propensity, are expected to use.

Design Implications

The major design implication for this work is that in assessing the readiness and needs of prospective users of collaboration tools, it is critical to look at the nature of the work that the researchers are engaged in. Regardless of the culture of their discipline, these results suggest that there are attributes of work that correlate with a higher propensity to collaborate. This has implications for assessment of tool readiness, but also for tool design. More studies are needed to determine the exact nature of user needs based on work attributes, but these results suggest that there is utility in careful examination of scientific work before designing collaboration tools.

In a sense, these design implications have a sort of "if you build it, they will come" feel to them. If we carefully understand the nature of people's work and use this understanding to develop tools in areas where collaboration propensity is high, it is likely that people will adopt the tools and collaborate. It is important to bear in mind, however, that these results also provide some limited evidence suggesting that such an approach will not always work. The consistently negative relationship between being an earthquake engineer and collaboration propensity indicates that, at least in EE, there may be some as yet unmeasured components of culture that might inhibit the earthquake engineers from "coming if it is built."

Limitations

There are several limitations that should be considered in interpreting this work:

Threats to internal validity

As was noted in Chapter 3, the undesirably low Cronbach's alpha scores for several measures used in this study point to potential issues with construct validity. This was particularly true for the need for and availability of help, focus, collective credit attribution standards and agreement on quality. At the same time, many of these measures were developed as part of this exploratory study, and cannot reasonably be held to the same reliability standards as stable, established scales for measuring more clearly defined constructs. Thus, more work is necessary in understanding and operationalizing the latent variables that underlie these constructs, and interpretation of these results should be done in light of the study's exploratory nature.

Another potential issue is that collaboration propensity was defined and operationalized here as an attitudinal measure. In other words, it reflects respondents' reported attitudes about possible future collaboration and does not reflect actual collaborative behavior. Additional work is needed to determine both the extent to which collaboration propensity maps onto actual collaboration and how to measure collaboration, while controlling for confounding factors such as funding opportunities. In other words, it is possible that an individual reported here to have low collaboration propensity could engage in future collaborative work for reasons unrelated to the utility of collaboration in answering research questions of interest (e.g. funding opportunities that require collaboration). This would not necessarily be an example of the propensity scale not functioning correctly in a predictive capacity, but rather a potential case of a confounding factor that must be controlled for.

Threats to external validity

Because limited data were available about the researchers in the initial sample, there was no way to formally assess whether or not the respondents differed in any meaningful way from the sample along demographic or other lines. There is no evidence to suggest that the sample was not representative, but this must nonetheless be considered as a possibility. It is also possible that respondents with an interest in collaboration or collaboration tools were more likely to respond to the survey, and thus could bias the results somewhat. Finally, it must be considered that the sample, though widely accepted sampling techniques were used, is not absolutely random in that there are clearly members of the fields studied that do not work on the projects or belong to the associations from which the sample was drawn.

Plausible alternative explanations

Caution must be used in interpreting the causal nature of these results. On the one hand, cross-sectional quantitative data are presented from which it is theoretically not appropriate to draw causal relations (Asher, 1983). In other words, both the factors leading to collaboration and the propensity to collaborate were measured at the same time. It is therefore difficult to demonstrate using these data that the factors studied are *causes* of increased collaboration. Rather, it can be said that they correlate with an increased or decreased reported propensity to collaborate. An alternative plausible explanation, for example, is that resource concentration, agreement on quality and the need for and availability of help are the result of collaboration, rather than the other way around.

On the other hand, there are some design elements of this study that do provide for some confidence in drawing causal inferences. First, survey respondents were asked to predict their future collaboration behavior which introduces an indirect time lag of sorts. Second, the qualitative data presented do provide for some insights into understanding the causal links between variables.

Directions for Future Research

Given the exploratory nature of this work, there are countless possible directions for future research in this area. Three of these emerge as particularly important and are described in this section.

Validating and refining the model of collaboration propensity

The model presented here is preliminary. Additional work is necessary to validate and refine this model in several ways. First, the measurement and analysis of several factors under examination here were constrained by the low reliability of the questionnaire scale items in this context. One key area for future work is therefore the development of more robust measurement instruments for these factors. Given that several of these factors are not well understood in the context of science and engineering research, studies that focus deeply on improving our understanding of specific factors would be useful in improving instrument design. Second, an improved measurement instrument must be deployed in additional contexts to validate both the measures used and the model itself. And finally, it may be necessary to identify and consider additional constructs in the process of refining the model.

Understanding the role of individual experience

These results preliminarily suggest a relationship between collaboration propensity and both prior collaboration experience and the frequency of collaboration tool usage. The qualitative data gathered here, however, did little to clarify the nature of this relationship. More data are needed to validate the existence of this relationship, and at a finer level of granularity. Specifically, it would be useful to know about people's specific experiences in prior collaborations, and which Internet-based collaboration tools they are using that correlate with increased collaboration propensity. Such data would be useful both in assessing various types of tools, and in better understanding how ICT usage impacts scientific productivity and collaboration propensity.

Moreover, the nature of the measures used in this study make it necessary to treat collaboration with a particular colleague as identical to collaboration with any other colleague or group of colleagues. In a more detailed examination of collaboration, however, it would be useful to understand the impact of specific characteristics of interpersonal relationships that influence people's choices about whether and with whom to collaborate. Some small groups of individuals, for example, have particularly complementary work styles and habits, while others do not work well together at all. While it is true in these results that prior collaboration experience seems to lead to future collaboration experience, it is also true that there were very few "non-collaborator" or "poor collaboration experience" respondents, so more study of these individuals would be useful in this regard as well.

Understanding collaboration success and sustenance

This study deliberately did not assess the effectiveness, productivity or impact of the collaborations and researchers studied. Assessing all of these elements in a valid, reliable was simply not feasible in the time window available for this study. Nonetheless, there is a legitimate interest on the part of funding agencies, researchers, policy analysts and others in the real impact of collaboration and collaboration technologies on the quality, quantity and impact of the research that is enabled. An interesting possible study would be one of how the factors examined here influence the quality and impact of research produced. One important element of this, of course, is finding a suitable way to operationalize success. This concept has been operationalized in many ways in the past (e.g. citation impact, publication or funding success, self-reported success, etc.), though there is little agreement among researchers on how this "should" be measured. This is another area for additional research on effective and reliable measurement.

The purpose of this study was to understand factors that motivate collaboration. Equally important, however, in the design and development of collaboration tools is an understanding of factors that sustain collaborations and enable them to move successfully (however one chooses to define success) through the phases of research and arrive at useful results and conclusions. Without such an understanding, we know only what factors make it likely that people will want to collaborate, but not what they will need in order to do so in a sustained and useful way. Thus, another interesting study that would provide answers to important questions sparked here would be a large, exploratory study of this nature would be that focuses on factors that sustain collaboration through multiple phases of research.

APPENDICES

APPENDIX A

PHYSICS INTERVIEW PROTOCOL

- In my quantitative data, physicists generally agree that there is agreement in the field about what the important problems are and how they should be solved. Talk about how this agreement is reached.
    - Is there consensus?
    - What was your role?
    - Do you agree?
- On a large collaboration like D0, CDF, ATLAS or CMS, the efforts of a huge number of people are involved. Talk about how you think research discoveries are made in such a large environment? Who is really responsible for doing the investigative work?
    - Are there differences between internal and external reporting of results?
    - Whom do you talk to when you have novel results? Who would you not talk to?
- You work on a very large project. How many of your collaborators would you recognize if you saw them on the street?
    - How many do you interact with on a regular basis? What is the nature of these interactions?

        - Where are these people?
    - What is the difference between a meeting, say in a conference room in Building 40 that is listed on the CERN Agenda server, and a meeting for coffee in one of the CERN cafes? What other types of meetings are there?
        - Do you tend to schedule your meetings or rely on "opportunistic" encounters?
- Talk about why you travel to CERN and how you try to spend your time when you are here.
    - What is it like participating in a CERN experiment when you are based at a US Institute (if, in fact, you are based at a US institute)?
- Is there anything I haven't asked you about that you feel is important?
- Are there other people you know who are here now who you think might be willing to talk with me?

APPENDIX B

EARTHQUAKE ENGINEERING INTERVIEW PROTOCOL

- How do you typically spend your time (Eg research, classes, model construction, etc.)
- How often do you conduct experiments/tests?
- Please describe the steps in a typical test, what is involved in them, who typically is responsible for each one, and how long each one takes
- Do these things happen at the same time or in sequence? Can you draw a map of the tasks and dependencies?
- How did you learn how to do your part of the tests? Are there manuals? Who showed you? Would it be easy or hard for you to explain it to me?
- Do you work with others on their tests? Do they work with you?
- - How similar would you say your work is to <equipment type> work being done at other sites. Could you easily explain to a researcher there how you set up a test? Could you do it well enough that they could replicate your test?
- Let’s talk a little bit about when things don’t go as you expect them to.
- How do you know when something is or might go wrong?
- Who is paying attention to things that might go wrong?
- What is this person looking at, specifically?

- Who decides that an experiment might need to be stopped?
- Who do you ask for advice in these situations?
- Talk about how you manage the data from an experiment
- What data are saved, Where do you put them?, How long are they kept?, How are they organized?
- Are there other important things you think I should have asked you about?

APPENDIX C

NEUROSCIENCE INTERVIEW PROTOCOL

- Your position is _______. What does that mean your day-to-day work involves?
- In my quantitative data, neuroscience appears to be the most interdisciplinary of the fields that I studied.
    - Why do you think that is?
    - What disciplines are involved in your work?
    - How does this help you get your job done?
- Do you rely on people with different expertise/resources in your work? How is this important?
- How competitive would you say your field of research is? Are you under pressure not to discuss results?
    - What is the procedure for publishing a result in your lab?
    - What about commercial pressures and commercial funding?
- To what extent is there agreement on what the important problems are in neuroscience?
    - How much dependence is there on others to solve these problems
        - Where are these people?

  - How much do people agree on what methods to use and how to assess work?
- Do you work with people at other institutions?
  - What difficulties are there in this relationship?
  - How often do you travel, and what do you try to accomplish when you do travel?
- Talk about how you get credit for your contributions to research projects. How do you assign credit to others who work with you?
- Is your project a part of the Human Brain Project?
  - What is your role in this project?
  - How do you contribute?
  - How much do you interact with other people on this large project?
- Is there anything else that I haven't asked you about that you think I should?

APPENDIX D

FINAL SURVEY INSTRUMENT

Science & Engineering Collaboration
University of Michigan

Use a No. 2 pencil or a pen with dark blue or black ink to mark your answers. Fill in the selected circles completely.

RIGHT | WRONG

Please indicate the extent to which you agree with the following statements with regard to your research work. Skip any items that do not apply to your research work. ***When items refer to your "field," please answer in reference to your specific area of research (high energy physics, earthquake engineering, or neuroscience). Also, for the purposes of this study, "collaboration" refers to any research work in which more than one person is involved in the collection and analysis of a particular data set. The collaborators are the individuals who participate directly in these activities.***

	Strongly Agree	*Agree*	*Neutral*	*Disagree*	*Strongly Disagree*
Doing research in my field requires access to unique and/or expensive equipment.	○	○	○	○	○
Collaboration with researchers whom I have not already collaborated with would benefit my career.	○	○	○	○	○
My field is sufficiently diverse that there are researchers in my field to whom it would be difficult to explain my research.	○	○	○	○	○
I come across specific, difficult problems in my work that I do not know how to solve alone.	○	○	○	○	○
In doing my day-to-day research work, I use a standard set of technical methods or tools that could also be applied to other problems or tasks.	○	○	○	○	○
Producing quality research in my field requires access to an amount of funds that it might be difficult for a single investigator to secure.	○	○	○	○	○
When I participate in a research project, it is clear at the start of the project how I will receive credit for my contribution to the work (i.e., via an authorship on a publication, etc.).	○	○	○	○	○
Research facilities for experimental research in my field are controlled by a few investigators.	○	○	○	○	○
When I assess the work of my peers, I use the same specific standards that they use in assessing my work.	○	○	○	○	○
I feel safe in discussing my current or pending results with other persons doing similar work (other than my collaborators).	○	○	○	○	○
Collaboration with other researchers allows me to access experimental apparatus that I could not otherwise use.	○	○	○	○	○
When I encounter a difficult problem in my work, I seek the advice of a colleague or mentor.	○	○	○	○	○
I am concerned that the results of my current research might be anticipated or "scooped" by other scientists working on similar problems.	○	○	○	○	○
In my field there are standard formats (i.e., standard units, etc.) for exchanging analyzed experimental data with colleagues who work on other projects.	○	○	○	○	○
Collaboration allows me to access people with expertise that is helpful to me.	○	○	○	○	○
Other researchers in my field use techniques or methods similar to the ones that I use.	○	○	○	○	○
Reearchers in my field are working to answer a well-focused and specific set of research questions.	○	○	○	○	○

	Strongly Agree	Agree	Neutral	Disagree	Strongly Disagree
It would be difficult for interested scientists outside of my field to read and understand the last academic paper I wrote or published.	○	○	○	○	○
There is only one set of technical methods and techniques used for valid research in my field.	○	○	○	○	○
There are methods used by some prominent researchers in my field that I do not believe yield valid results even when they are used correctly.	○	○	○	○	○
When I formally assess the merits of a peer's research (such as for publication or funding review), my assessment is generally in agreement with other reviewers of the same work.	○	○	○	○	○
Other researchers in my field who do collaborative work are successful in their careers.	○	○	○	○	○
When I write an academic paper, it is easy to determine whom to include as co-authors on the work.	○	○	○	○	○
In my field, it is common for researchers to commercialize their research discoveries.	○	○	○	○	○
Collaboration is necessary in my field.	○	○	○	○	○
Collaboration is useful in solving problems that are of interest to me.	○	○	○	○	○
When I make a discovery in my field, I typically seek to patent or otherwise (i.e., via licensing agreements, etc.) keep some aspect of that discovery private or proprietary.	○	○	○	○	○
There is widespread agreement in my field about what the important research questions are.	○	○	○	○	○
It is important for me to receive competitive prizes or widespread recognition.	○	○	○	○	○
I plan to engage in collaborative research in the future.	○	○	○	○	○
There is a clear hierarchy of journals in my field, with leading journals that are generally agreed upon.	○	○	○	○	○
In the past 5 years, the results of my research have been anticipated or "scooped" by other scientists working on similar problems.	○	○	○	○	○
Collaboration allows me to access data sets that I could not otherwise use.	○	○	○	○	○
When my work is reviewed by my peers, I generally agree with their assessment.	○	○	○	○	○
Others are interested in working to commercialize my research discoveries.	○	○	○	○	○
In order to do research in my field, it is necessary to have an advanced degree in this field (and not from another field).	○	○	○	○	○
There is a clear hierarchy of institutions in my field, with prominent institutions that are generally agreed upon by most researchers.	○	○	○	○	○
I am aware of successful research collaborations in my field.	○	○	○	○	○
It is difficult for me to explain my research to interested researchers in a related, but separate, field.	○	○	○	○	○

Your collaboration experience

For this next set of questions, please think of ***a specific research collaboration*** *from which you recently submitted (or will soon submit) results for publication or presentation.* ***Answer these questions with regard to the group of people with whom you worked most closely on this collaboration.***

About how many colleagues are involved in this collaboration or workgroup?

0	0
1	1
2	2
3	3
4	4
5	5
6	6
7	7
8	8
9	9

About how many institutions are involved in this collaboration or workgroup?

0	0
1	1
2	2
3	3
4	4
5	5
6	6
7	7
8	8
9	9

About how many academic disciplines are represented in this collaboration?

0	0
1	1
2	2
3	3
4	4
5	5
6	6
7	7
8	8
9	9

Of the colleagues involved in this collaboration (including faculty and graduate students), how many work primarily in your building or within a few miles of your office?

0	0
1	1
2	2
3	3
4	4
5	5
6	6
7	7
8	8
9	9

On this project, how frequently did you use Internet-based communication and coordination tools (e.g., email, videoconferencing, data conferencing)?

- ○ Several times daily
- ○ Daily
- ○ Several times per week
- ○ Weekly
- ○ Several times per month
- ○ Monthly
- ○ Less often than monthly

On this project, how frequently did you use traditional (non-Internet) communication and coordination tools (e.g., telephone or telephone conference, face-to-face contact, etc.)?

- ○ Several times daily
- ○ Daily
- ○ Several times per week
- ○ Weekly
- ○ Several times per month
- ○ Monthly
- ○ Less often than monthly

Overall, how successful do you think this project was in terms of the results produced?

- ○ Very successful
- ○ Moderately successful
- ○ Not very successful
- ○ Not at all successful
- ○ Don't know

Overall, how successful do you think this project was in terms of working collaboratively?

- ○ Very successful
- ○ Moderately successful
- ○ Not very successful
- ○ Not at all successful
- ○ Don't know

Demographics

Which of the following best describes your current professional status?

- ○ Faculty/Lecturer
- ○ Clinician
- ○ Senior Researcher/Research Scientist
- ○ Junior Researcher/Research Scientist
- ○ Postdoc
- ○ Graduate student
- ○ Undergraduate student
- ○ Research Assistant
- ○ Other (Specify:

______________________________)

What is the highest academic degree you have obtained?

- ○ Bachelor's
- ○ Master's
- ○ Ph.D.
- ○ M.D.
- ○ Other (Specify:

______________________________)

Is your highest degree in the field in which you currently do research (or will it be, if you are a student)?

○ Yes ○ No

In what year did you obtain your highest degree?

0	0	0	0
1	1	1	1
2	2	2	2
3	3	3	3
4	4	4	4
5	5	5	5
6	6	6	6
7	7	7	7
8	8	8	8
9	9	9	9

Within the last 5 years, have you published an academic paper in a peer-reviewed journal or conference proceedings?

○ Yes ○ No

Within the last 5 years, have you published an academic paper with co-authors in a peer-reviewed journal or conference proceedings?

○ Yes ○ No

Within the last 5 years, have you published an academic paper in a peer-reviewed journal or conference proceedings with co-authors not at your institution?

○ Yes ○ No

○ Please check the box at left if you would NOT be willing to participate in a 15-20 minute telephone or in-person interview to aid in the interpretation of these research results. (Note: 30-40 persons will be selected to participate in these interviews out of approximately 1,200 total participants. Answering 'Yes' to this item does not necessarily mean you will be selected.)

If you would like to receive information about the results of this survey, please PRINT an email address at which we can contact you:

____

Please provide any additional comments on this work or your experience with collaboration.

For office use only:

0	0	0	0
1	1	1	1
2	2	2	2
3	3	3	3
4	4	4	4
5	5	5	5
6	6	6	6
7	7	7	7
8	8	8	8
9	9	9	9

APPENDIX E

VARIABLES AND CORRESPONDING ITEMS

Final Variable	Item Wording	Item ID
Agreement on Quality	When I assess the work of my peers, I use the same standards that they use in assessing my work	5557
Agreement on Quality	When I assess the merits of a peer's research, my assessment is generally in agreement with my peers.	5558
Agreement on Quality	When my work is reviewed by my peers, I generally agree with their assessment.	5556
Agreement on Quality	There is a clear hierarchy of journals in my field, with leaders that are generally agreed upon throughout the field	5561
Agreement on Quality	There is a clear hierarchy of universities in my field, with leaders that are generally agreed upon by most researchers	5562
Availability of and Need for Help	I frequently come across specific, difficult problems in my work that I do not know how to solve alone.	5583
Availability of and Need for Help	In doing my day-to-day research work, I use a standard set of methods that could also be applied to other problems or tasks.	5588
Availability of and Need for Help	When I encounter a difficult problem in my work, I seek the advice of a colleague or mentor	5584
Availability of and Need for Help	Most other researchers in my field use techniques or methods similar to the ones that I use	5589

Collaboration Propensity	Collaboration with other researchers would benefit my career.	5614
Collaboration Propensity	Other researchers in my field who do collaborative work are successful.	5615
Collaboration Propensity	I plan to engage in collaborative research in the future.	5611
Collaboration Propensity	Collaboration allows me to access instruments that I could not otherwise use	5619
Collaboration Propensity	Collaboration allows me to access people with expertise that is helpful to me	5620
Collaboration Propensity	Collaboration is necessary in my field	5618
Collaboration Propensity	Collaboration is useful in solving problems that are of interest to me	5617
Collaboration Propensity	Collaboration allows me to access data sets that I could not otherwise use	5621
Commercial/Industrial Proximity	In my field, it is common for researchers to commercialize their research discoveries.	5547
Commercial/Industrial Proximity	When I make a discovery in my field, I typically seek to patent or otherwise (i.e. via licensing agreements, etc.) keep some aspect of that discovery private or proprietary.	5541
Commercial/Industrial Proximity	Others are interested in working to commercialize my research discoveries.	5546
Credit Attribution Practices	When I participate in a research project, it is clear at the start of the project how I will receive credit for my contribution to the work (i.e. via an authorship on a publication, etc.)	5574
Credit Attribution Practices	When I publish an academic paper, it is easy to determine whom to include as co-authors on the work	5577
Focus	The methods I use in my research are the only methods used for legitimate research in my field	5548
Focus	There are methods used by some prominent researchers in my field that I do not believe yield valid results even when they are used correctly	5551

Focus	There is widespread agreement in my field about what the important research questions are	5550
Resource Concentration	Doing cutting edge research in my field requires access to rare and expensive equipment	5560
Resource Concentration	Producing quality research in my field requires access to an amount of funds that it might be difficult for a single investigator to secure	5563
Scientific Competition	I feel safe in discussing my current work with other persons doing similar work (other than my collaborators)	5607
Scientific Competition	I am concerned that the results of my current research might be anticipated or "scooped" by other scientists working on similar problems.	5605
Scientific Competition	The competition for prizes or widespread recognition in my field is intense	5608
Scientific Competition	In the past 5 years, the results of my research have been anticipated or ""scooped"" by other scientists working on similar problems	5606

APPENDIX F

INVITATION LETTERS

April 21, 2004

«first» «last»
«department»
«address1»
«address2»
«city», «state» «zip»

Dear Colleague:

You have been selected to participate in a University of Michigan study of collaboration in science and engineering. We would greatly appreciate your taking 10-15 minutes to respond.

This study is part of a larger effort, funded by the National Science Foundation, to better understand scientific work and the design of collaboration technologies. The results of this effort will help drive the development of future collaboration tools and other "cyberinfrastructure"-based technologies to support research in your field and others. It is not possible for us to understand the relevant factors without responses from individuals engaged in a range of activities, which means that **your response is very important to us.**

As a token of our appreciation for your efforts, please accept the enclosed cash gift. If you choose to participate, you can also sign up to receive a summary of the results via email. If possible, please complete and return the questionnaire by May 5.

Your participation in completing this questionnaire is voluntary. You may skip questions that make you uncomfortable and are free to withdraw at any point. Your responses will be used for research purposes only and will be kept in secure locations at the University of Michigan. Only primary members of the research team at the University of Michigan will have access to these data. The information you provide in this survey will be kept confidential. Furthermore, all personal information will be presented only in an aggregate form in reports and publications. Individual responses will not be identifiable. If you have any questions regarding your rights as a participant in this research, please contact Kate M. Keever, Administrator, Human Subjects Protection Office, (734) 926-0933, *IRB-Behavsci-Health@umich.edu*.

Thank you in advance for taking the time to complete this important questionnaire. More information about this and related studies is available online at: http://www.si.umich.edu/~collabstudy/moreinfo.html If you have additional questions or concerns, please contact us via email at collabstudy@umich.edu or by calling 734-764-1858.

Sincerely,

Thomas A. Finholt, Ph. D.
Director,
Collaboratory for Research on Electronic Work

Jeremy P. Birnholtz
Doctoral Candidate

HOMER A. NEAL
Samuel A. Goudsmit Distinguished University Professor of Physics
Interim President Emeritus

The University of Michigan
Department of Physics
500 East University Avenue
Ann Arbor, MI 48109-1120
Tel: 734 764 4375 FAX: 734 936 6529
haneal@umich.edu

April 16, 2004

Dear Colleague,

I am serving on the Ph.D. committee of Jeremy Birnholtz, a student in the UM School of Information who is studying how research collaborations function in different disciplines. He can explore the key issues only through the response of active scientists engaged in a range of collaborative activities. I urge you to take 10-15 minutes to complete the enclosed survey.

Jeremy has worked with the School of Information "Science of Collaboratories" Project, the only NSF ITR project in the nation involved in the study of collaboratories. He visited us at CERN last year, and will be spending additional time there this coming summer. The study Jeremy is proposing was thought by his committee to be valuable in determining the factors that make collaborations productive and efficient and, indeed, the outcome of his study may have direct relevance to our own field.

Again, I hope you will take time from your busy schedule to support Jeremy's efforts and complete this important survey.

Regards,

Homer A. Neal

Homer A. Neal

UNIVERSITY OF ILLINOIS AT URBANA-CHAMPAIGN
Department of Civil and Environmental Engineering

B. F. Spencer, Jr.
Nathan M. and Anne M. Newmark Endowed Chair in Civil Engineering

PHONE: (217) 333-8630

FAX: (443) 646-0675

E-MAIL: bfs@uiuc.edu

WWW: http://cee.uiuc.edu/sstl/

April 3, 2004

Dear Colleague:

I want to bring to your attention a study being conducted by Jeremy Birnholtz, a doctoral candidate in the UM School of Information. Jeremy is studying a series of factors that influence how research collaborations form and function in three different disciplines, and is administering this survey in a population of 1000 scientists and engineers across the United States. I would strongly encourage you to take 10-15 minutes and complete this survey.

This study is supported in part by the NSF-funded 'Science of Collaboratories' Project at the University of Michigan and by University of Michigan faculty involved in NEESgrid and other National Science Foundation efforts to develop our nation's cyberinfrastructure capacities.

The results will directly impact these efforts and may have important implications for our own field as well.

Again, I would appreciate it if you could take time to support Jeremy's efforts and complete this important study.

Sincerely yours,

B.F. Spencer, Jr.

B.F. Spencer, Jr.
Newmark Endowed Chair

205 North Matthews Ave · Urbana, IL 61801

APPENDIX G

RESPONSE PROFILE

In order to better understand the respondents and the data, frequency and descriptive statistics were first run and reviewed for demographic and collaboration description variables. This section of the chapter describes these results.

Demographics

Field of research

Given the comparative nature of portions of this study, the response rate from each field was of critical interest. As Table 8 shows, response across the three fields was almost exactly even. It was therefore deemed unnecessary to run a Chi Square goodness of fit analysis, given the extremely high likelihood of a strong fit.

Table 8
Response frequencies by field of research

Field	Frequency	Percent
High Energy Physics	127	33.3
Earthquake Engineering	129	33.9
Neuroscience	125	32.8

Academic Status

Respondents were asked to report their academic status. As Table 9 shows, faculty, research scientists, postdoctoral researchers and graduate students were the most common categories, accounting for 93% of the total. There are some differences along field lines, however, that merit attention. In all three cases, faculty/lecturer is the most common category, though it is most common in EE (62%), and substantially less so in HEP (38.6%). This makes some intuitive sense in that it is extremely common for recent Ph.D. program graduates to work as a postdoc in physics before applying for junior faculty jobs, where this is less common in engineering. In other words, there are fewer postdocs in the overall population of engineers, so it is not surprising that this number is lower when a random sample was drawn. The academic status of respondents was not known prior to mailing the survey, so it is not possible to formally test for representativeness via Chi Square analysis, but there seems to be little informal evidence of systematic bias in any field or group.

Table 9
Summary of Respondent Status (N=381)

	Physics		EE		Neuroscience		Total	
	Frequency	Percent	Frequency	Percent	Frequency	Percent	Frequency	Percent
Faculty/Lecturer	49	38.6	80	62.0	58	46.4	187	49.2
Research Scientist	34	26.7	7	5.5	27	21.6	68	17.8
Postdoc	20	15.7	2	1.6	16	12.8	38	9.9
Graduate Student	17	13.4	32	24.8	12	9.6	61	16.0
Other	7	5.5	8	6.2	12	9.6	27	7.1

Highest Academic Degree

Table 10 shows the highest academic degrees earned by field of research. It should be noted that those reporting a Bachelor’s as the highest degree earned are likely graduate students working on a higher degree. Also of interest is that the fraction of EE respondents with Master’s degrees is nearly triple the fraction in the other two fields. This is likely due to the fact that it is extremely common to receive a Master’s degree in EE before pursuing a Ph.D., where this is rare in the other two fields. Beyond these differences, there appears to be no evidence of systematic bias here.

Table 10
Summary of Respondents' Highest Degree Earned by Field (N=370)

	Physics		EE		Neuroscience		Total	
	Frequency	Percent	Frequency	Percent	Frequency	Percent	Frequency	Percent
Bachelor's	11	8.7	4	3.1	15	13.0	30	8.1
Master's	10	7.9	33	25.8	10	8.7	53	14.3
Ph.D.	103	81.1	91	71.1	73	63.5	267	72.2
M.D.	1	0.8	0	0	15	13.0	16	4.3
Other	2	1.6	0	0	2	1.7	4	1.1

Respondents were also asked whether or not their highest degree was in the field in which they currently do research. Of the 368 respondents who answered this item (this item was not used in subsequent analyses, so incomplete cases were not dropped from the data set), 322 or 87.5% reported that their highest degree was in their current field of research. Despite this overwhelming majority, it bears mentioning that the number of 'No' responses was substantially greater in neuroscience (23% vs. 7.3% in the other two fields). This is likely because neuroscience is a multidisciplinary field that draws on many conventional fields, such as psychiatry, medicine, bioinformatics, and others. HEP and EE on the other hand draw primarily on their own field, but do involve some computer scientists in their work for computational modeling and analysis.

Table 11
Summary of Whether Highest Degree Earned is in Field of Current Research (N=368)

	Physics		EE		Neuroscience		Total	
	Frequency	Percent	Frequency	Percent	Frequency	Percent	Frequency	Percent
Yes	115	92.7	114	92.7	93	76.9	322	87.5
No	9	7.3	9	7.3	28	23.1	46	12.5

Collaboration traits

Respondents were asked to consider a specific research collaboration from which they had recently submitted results for publication. They answered a set of general questions about the group of people with whom they worked most closely on this project.

Size

Respondents were asked how many colleagues were involved in their collaboration or workgroup. Responses ranged from 0 to 99, with an overall mean of 20.0 (n=367, SD=30.75). This mean is likely influenced by the very large collaborations reported by high energy physicists, so the two modal values (together accounting for 25% of the responses) of 4 and 5 likely provide a better indicator of typical collaboration size. As Table 12 shows, over half of the collaborations are reported to involve 7 or fewer colleagues.

What is most striking, though not surprising, when fields are compared here is the difference between HEP and the other two fields. The mean HEP collaboration (see Table 13) is 5 times larger than the mean neuroscience collaboration and 8 times larger than the mean EE collaboration. Moreover, over 50% of HEP collaborations report having 26 or more colleagues. Given the large size of HEP collaborations as described in Chapter 1, this is as expected.

Also noteworthy here is that 12 of the 14 nonresponders for this item (from the 381 cases used for the main analyses) are from HEP. These questionnaire forms were examined to better understand this. It was discovered from free-response comments written on these forms that some respondents felt overly constrained by the two-digit space provided for responding to this item. They indicated that their collaborations were larger than this, and they therefore did not respond to this item. In actuality, however, it is more likely that respondents did not read the instructions indicating that the item was asking about the group of colleagues with whom they worked most closely on this project. Qualitative data gathered in this study suggest that it is highly unlikely that a physicist would work closely with more than 99 people on a project. The typical number reported in interviews ranged from 5 to 50.

Table 12
Summary of Collaboration Size Frequencies by Field (N=367)

	Physics		EE		Neuroscience		Total	
	Frequency	Percent	Frequency	Percent	Frequency	Percent	Frequency	Percent
3 or fewer	9	7.8	54	42.2	28	22.6	91	24.8
4	1	.9	23	18.0	22	17.7	46	12.5
5-7	15	13.0	28	21.9	39	31.5	82	22.3
8-25	27	23.5	21	16.4	27	21.8	75	20.4
26 or more	63	54.8	2	1.6	8	6.5	73	19.9

Table 13
Mean Collaboration Sizes by Field (N=367)

Field	Mean	SD	n
Physics	48.49	40.76	115
EE	5.34	5.07	128
Neuroscience	8.69	11.65	124

Institutions

Respondents were asked to report how many institutions were involved in their collaboration or workgroup. Responses ranged from 0 to 99, with a mean of 11.4 (n=377, SD=21.64). Again, physics likely influences the mean, so the modal value of 2 institutions provides a better indicator of typical collaboration size. As Table 14 shows, nearly 80% of the collaborations were reported to consist of fewer than 10 institutions.

When fields are compared, it is evident here again that HEP collaborations are qualitatively different from the collaborations in the other two fields, with over 50% of HEP collaborations involving 10 or more institutions, compared with 1.6% and 3.2% in the other fields, respectively. It is evident that most collaborations in EE and neuroscience involve individuals at 1-3 institutions, whereas this is the case for only 20% of HEP collaborations. Since all HEP respondents were selected because of their involvement in very large collaborations, it is likely that respondents were referring to the number of institutions involved in their working group on the larger project.

It must also be noted that a very small number of respondents indicated that their collaboration involved individuals at zero institutions. It is assumed that these respondents meant zero institutions in addition to their own, so these responses are grouped with the '1 institution' responses in these results.

Table 14
Summary of the Number of Institutions Per Collaboration (N=377)

Number of Institutions	Physics Frequency	Percent	EE Frequency	Percent	Neuroscience Frequency	Percent	Total Frequency	Percent
1 or fewer	8	6.4	41	32.0	36	29.0	85	22.5
2	14	11.2	43	33.6	38	30.6	95	25.2
3	5	4.0	22	17.2	22	17.7	49	13.0
4-9	25	20.0	20	15.6	24	19.4	69	18.3
10 or more	73	58.4	2	1.6	4	3.2	79	21.0

Table 15
Mean Number of Institutions Per Collaboration by Field
(N=377)

Field	Mean	SD	n
Physics	28.78	30.73	125
EE	2.41	1.74	128
Neuroscience	3.21	4.28	124

Local Colleagues

Respondents were asked to report how many colleagues involved in this project work primarily "in your building or within a few miles of your office." Reported values were then divided by the total number of reported colleagues to obtain a percentage of collaborators who are reported to be local. The number of cases here is significantly lower than the other variables (349 out of 381 total cases) because it depended on respondents completing both items used in the calculation. Valid values ranged from 0 to 1, with a mean of .59 (n=349, SD=.94) and mode of 1.0. As Table 16 shows, however, fewer than half of the respondents reported having more than 40% of their colleagues present on site.

HEP respondents generally appear somewhat more likely to have fewer local colleagues, with 57% of HEP collaborations having less than 40% local colleagues, as compared with 36% and 30% in the other fields, respectively. The mean percentage (see

Table 17) is also lower for HEP. Similarly, EE and neuroscience appear more likely than HEP to have a very large fraction (92-100%) of local colleagues, which was only the case in 17% of the HEP collaborations.

Table 16
Summary of the Percentage of Local Colleagues Per Collaboration, Frequencies by Field (N=349)

% of Local Colleagues	Physics Frequency	Percent	EE Frequency	Percent	Neuroscience Frequency	Percent	Total Frequency	Percent
0 – 13%	38	35.5	21	16.9	12	10.2	71	20.3
14 – 39%	23	21.5	24	19.4	23	19.5	70	20.1
40 – 64%	19	17.8	25	20.2	29	24.6	73	20.9
67 – 91%	9	8.4	22	17.7	21	17.8	52	14.9
92 – 100%	18	16.8	32	25.8	33	28.0	83	23.8

Table 17
Mean Percentage of Local Colleagues per Collaboration by Field (N=349)

Field	Mean	SD	n
Physics	.39	.36	107
EE	.54	.35	124
Neuroscience	.58	.34	118

Disciplines

Respondents were asked to report how many academic disciplines were represented in their collaboration or workgroup. Responses ranged from 0 to 50, with a mean of 2.75 (n=369, SD=3.95) and mode of 1.0. As Table 18 shows, collaborations reporting one or two disciplines accounted for nearly two thirds of the responses. When fields are compared there is an interesting contrast. Where HEP and EE are dominated by collaborations with 2 or fewer disciplines, neuroscience appears to have more collaborations with 3 or more disciplines. Moreover, the mean number of disciplines (see Table 19) is greater in neuroscience. This makes some intuitive sense in light of the earlier observation the neuroscience draws on several fields, where HEP and EE tend to do so less.

Table 18
Summary of the Number of Disciplines Per Collaboration, Frequencies by Field (N=369)

Number of Disciplines	Physics Frequency	Percent	EE Frequency	Percent	Neuroscience Frequency	Percent	Total Frequency	Percent
1 or Fewer*	64	53.8	51	39.8	17	13.9	132	35.8
2	23	19.3	44	34.4	36	29.5	103	27.9
3	15	12.6	19	14.8	40	32.8	74	20.1
4 or more	17	14.3	14	10.9	29	23.8	60	16.3

Note: As above, it is assumed that the 2 respondents who answered '0' for this item meant that there were no disciplines in addition to their own.

Table 19
Mean Number of Disciplines Per Collaboration by Field (N=369)

Field	Mean	SD	n
Physics	2.96	5.91	119
EE	2.16	2.05	128
Neuroscience	3.17	2.88	122

Success

Respondents were asked to characterize the success of this collaboration or workgroup along two dimensions: 1) the quality of the results produced by the collaboration, and 2) the ability of people within the collaboration to work together effectively. For both items, a five-point scale was used with the choices "Very successful," "moderately successful," "Not very successful," "Not at all successful," or "Don't Know."

For the "quality of results" item, the overall mean response was 4.36 (n=380, SD=.93) and the mode was 5. This indicates that respondents were generally pleased with the results produced by the chosen collaboration.

For the "effectiveness in working together" item, the mean response was 4.43 (n=381, SD=.73) and the mode was 5. Again, this indicates that respondents were generally pleased with the effectiveness of their collaboration.

There are very small differences between fields on these dimensions (see Table 20), but these differences are not statistically significant. Thus, it appears that respondents' experience with the chosen collaboration was overwhelmingly positive, and that collaborations in one field are no more or less likely to be successful than the other two fields studied here.

Table 20
Descriptive Statistics for Measures of Collaboration Success

	Physics			EE			Neuroscience		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Results	4.36	.96	127	4.35	.97	129	4.38	.87	124
Working Together	4.39	.79	127	4.40	.68	129	4.50	.71	125

APPENDIX H

STATISTICAL METHODS

Neter, et al. (1996) provide detailed guidelines for the construction and evaluation of linear regression models in observational studies such as this one. These guidelines were followed in developing a model for analysis here, as is described in this section.

Evaluating the models

Several steps were taken to check and evaluate the regression models discussed in Chapter 4:

Model adequacy

To test the adequacy of the model, various residual plots were examined to detect evidence of systematic deviation from the response plane. The residuals were plotted against the predicted Y (dependent variable) values, in addition to against each of the predictor variables. No evidence of systematic deviation was observed. In addition, a normal probability plot was generated that plotted the ordered residuals against their expected values under normality. The plot produced a nearly perfectly linear result, which is a good visual indicator of the strong correlation between these values, as is desirable in demonstrating that the error terms are terms are reasonably normal in their distribution.

Multicollinearity

The Variance Inflation Factor (VIF) computed by SPSS was used to check for multicollinearity in this model. The VIF is an indicator of "how much the variances of the estimated regression coefficients are inflated as compared to when the predictor variables are not linearly related" (Neter et al., 1996). The authors suggest that a maximum VIF greater than 10, or a mean VIF value significantly greater than 1 are indicators of multicollinearity. The maximum VIF value in this model was 2.1 and the mean was 1.4. Thus, it can be reasonably concluded that there is no problem with multicollinearity.

Influential cases

To test for influential cases, Cook's distance statistic was computed for each case. Cook's distance considers the influence of a particular case on all of the fitted values. According to Chatterjee and Hadi (1988), an observation where Cook's distance (D) is larger than 4/n-q (where q is the number of independent parameters) merits further investigation as a potentially influential observation. Here q = 17 and n=382, so D for each case must be less than .01. In all cases, D was .01 or less so there do not appear to be any unusually influential cases.

Removing potentially troublesome items

It was observed that two of the scale items used to measure collaboration propensity (items #5619 and 5620 in Appendix E) could be seen as too similar to other items used to measure resource concentration and the need for and availability of help. Such correlations could affect the validity of the results, so these two items were temporarily removed from this construct temporarily. It was found, however, that this removal did not impact the regression results in a noticeable (or statistically significant) way. The items were therefore added back into the model.

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