REVISITING THE INVISIBLE COLLEGE: A CASE STUDY OF THE INTELLECTUAL STRUCTURE AND SOCIAL PROCESS OF SINGULARITY THEORY RESEARCH IN MATHEMATICS

by

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A thesis submitted in conformity with the requirements
for the degree of Doctor of Philosophy
Faculty of Information Studies
University of Toronto

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ABSTRACT

This study revisits the invisible college concept in order to respond to Lievrouw's (1990) question about whether it is a *structure* of scholarship measurable from outside elements (i.e., published documents) or a *social process* rooted in informal communication behaviours, perceivable only to the researchers who carry out these behaviours. Focusing on the Singularity Theory community in Mathematics, the combined research techniques of Author Co-Citation Analysis, Social Network Analysis, and Ethnography of Communication are used show that an invisible college constitutes both elements identified by Lievrouw. An invisible college is defined and observed as a multidimensional phenomenon where three factors — the subject specialty, the scientist/scholars as social actors, and the information use environment (IUE) — play interrelated roles in its orientation and growth.

ACKNOWLEDGEMENTS

A number of individuals were helpful to me during my journey towards completing my Ph.D. and I would like to take the opportunity to express my gratitude. First, I wish to acknowledge my parents, Norma and George Zuccala, my siblings Paul and Tonya and my 'kitties' Sushi and Zoë for believing in me and for standing by me through 'thick and thin.' A Ph.D. thesis is a time-consuming and at times, stressful project and I know that I could not have done it without the support of my family members.

My supervisor from the Faculty of Information Studies (FIS), Dr. Juris Dilevko, also believed in my capacity to carry out this project. He offered a tremendous amount of guidance throughout my Ph.D. program and ensured that financial resources were available during crucial research periods. With Dr. Dilevko's help I had the opportunity to travel to Drexel University in Philadelphia to acquire the skills needed to carry out my Author Co-citation Analysis (ACA) of the Singularity Theory community. Dr. Howard White and Dr. Kate McCain from Drexel took time out of their busy schedules to share their knowledge of the software and research techniques used in ACA and I will always be grateful to them for this. At the University of Toronto four additional advisors were essential to my progress. Dr Ethel Auster and Dr. Joan Cherry from the Faculty of Information studies were instrumental in helping me understand and recognize the fundamentals of quality research and scholarly writing. Dr. Barry Wellman from the Department of Sociology acted as my social network analysis/sociology of science mentor and gave me the opportunity to acquire both the software and skills needed to study the Singularity Theory community from a social network perspective. Without Dr. Edward Bierstone from the Department of Mathematics I could not have even begun to understand the Singularity Theory community; therefore I am grateful to him for acting as my key mathematics informant and for 'translating' documents that would help me acquire insight into the field's history. Several other Singularity Theory mathematicians – many of whom are named in this dissertation – provided me with insight into their work and I thank all of them for making this project a success because they agreed to participate in an interview with me. And finally, this dissertation would not have been finalized without Dr. Liwen Vaughan from the University of Western Ontario who produced a thorough research review prior to my oral defense on March 15, 2004.

I am extremely grateful to Dr. Keith Moffatt, from the Isaac Newton Institute in Cambridge, England for granting me access to the Institute environment for the ethnographic stage of my research. Dr. Bradd Hart generously granted me a leave of absence from my Publication Manager duties at the Fields Institute so that I could travel to England. Also Dr. Barbara Spronk and Neil Spronk graciously opened up their home to me so that I would have a place to live in Cambridge while I was carrying out my fieldwork.

A special thanks goes to Matthew Duncan, my friend and fellow Ph.D. student from the University of Toronto. I could always count on him to be there for me both for statistics advice and advice on how to handle the trials and tribulations of being a doctoral student. My other friends from the University of Toronto, known as the “Secret Agents” were always available for fun, laughter, and understanding when it seemed like there was no light at the end of the tunnel (i.e., A. Siegenthaler, A. Quan-Hasse, B. Morton, etc. You know who you are!). Several individuals from outside the university gave me an enormous amount of encouragement, particularly, Dr. D. Slonowsky, E. Kaufman, B. Daly, J. Williams, S., M. Zurawinski, S. Wennerstrom, J. Hayman and S. Johansen. The members of the Bibliometrics and Informetrics group (BIRG) at the University of New South Wales, Australia, headed by Dr. Connie Wilson, provided me with many rich experiences as a researcher and lecturer. I am particularly grateful to Dr. Mari Davis from BIRG for reading drafts of my thesis prior to submission. Last, but not least, my good friends from Sydney, Australia, T. Hansen, K. Hansen and K. O’Brien supported me during the final stages of my thesis writing. I will always remember enjoying the “Aussie” sun, beaches, birds and kangaroos with them when I was well on my way towards completion.

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CHAPTER 1: NTRODUCTION

1.1 Problem Context

In May 1993, Andrew Wiles gave a series of lectures entitled “Modular Forms, Elliptic Curves, and Galois Representations” at the Isaac Newton Institute in Cambridge. During his third and final lecture he astonished the members of his research community with a solution to Fermat’s Last Theorem. Ken Ribet, a visiting mathematician from Berkeley, had been following the first two lectures with anticipation. Rumors were circulating throughout the Institute and he was one of several visitors who suspected that at the third lecture “the most important mathematical announcement of the century” was about to happen:

> I came relatively early and sat in the front row along with Barry Mazur. I had my camera with me just to record the event. There was a very charged atmosphere and people were very excited. We certainly had a sense that we were participating in a historical moment. People had grins on their faces before and during the lecture. The tension had built up over the course of several days. Then there was this marvelous moment when we were coming close to a proof of Fermat’s Last Theorem.”
> (Singh, 1997, p. 248)

Towards the end of the lecture, members of the audience began to take photographs. The Institute’s director was waiting with a bottle of champagne. Wiles’ own account was that there was a “typical dignified silence” in the lecture hall leading up to the final moment when he wrote his statement on the blackboard. His last words were, “I think I’ll stop here” and immediately afterwards two hundred colleagues clapped and cheered in celebration (p. 249).

The story behind Andrew Wiles’ lecture, recounted by Simon Singh in *Fermat's Enigma*, is a fascinating portrayal of one man’s solitary quest to solve a time-honored mathematical mystery. At the age of 10, Wiles’ attention was drawn to a library book entitled *The Last Problem*. This book outlined the roots of a famous problem, which stemmed from the mathematics of ancient Greece. The problem matured over time, until an eminent seventeenth century mathematician named Pierre de Fermat part a secret so that he could pose a challenge to other mathematicians around the world.

For centuries, one mathematician after another failed to prove what became known as "Fermat's Last Theorem." Finally it was the young Wiles' who took it upon himself to be the one: "Here was a problem that I, a ten-year old, could understand and I knew from that moment that I would never let it go. I had to solve it" (p. 6).

Wiles' childhood dream was the inspiration for his mathematics education. In 1974 he received a B.A. from Merton College, Oxford and then moved to Cambridge University to begin a Ph.D. under the supervision of Professor John Coates. In 1980 he was awarded a doctorate for his mathematical work on elliptic curves. Following his graduation, he took a postdoctoral position at the Institute for Advanced Study in Princeton. When he was finally awarded the status of Professor at Princeton in 1982, he decided that it was time to begin his official journey toward solving Fermat's Last Theorem.

Wiles knew "that to have any hope of finding a proof he would first have to completely immerse himself in the problem" (Singh, p. 207). He familiarized himself with the mathematical history surrounding the Last Theorem, including past proof failures, articles on elliptic equations and modular forms, the Taniyama-Shimura conjecture and Ken Ribet's link between Fermat and Taniyama-Shimura. Wiles also abandoned all tasks that were not directly relevant to the problem, avoided unnecessary professional distractions other than his teaching responsibilities and stayed at home whenever possible to retreat to his attic study. Above all, he made a very remarkable promise to himself, and that was "to work in complete isolation and secrecy" (p. 208).

Although Wiles' isolated approach to mathematics research was highly unusual, there was an understandable motive. If he was to succeed at providing the world with a complete and flawless proof of Fermat's Last Theorem, then the glory associated with winning the most coveted prize in mathematics would be "his and his alone" (Singh, p. 4). On the other hand, his decision to keep his work a secret meant that he had never discussed or tested his ideas with other members of the mathematics community. He was putting himself at risk of making a fundamental error (p. 4).

On the day that Wiles presented his proof at the Newton Institute, he became overwhelmed with ambivalence: “I got so wrapped up in the problem that I really felt I had it all to myself, but now I was letting go. There was a feeling that I was giving up a part of me” (Singh, p. 249). Singh’s account does not elaborate on Wiles’ feeling, but it was clear that part of the "giving up" process meant that he was expected to surrender his work to peer review. Wiles and all the other members of the number theory community needed to be sure of the accuracy and correctness of his mathematics.

After a long summer of waiting for reviewer reports, Wiles received the crushing news that his solution possessed a fundamental flaw. Fortunately, his reviewers agreed to keep this flaw a secret from the rest of the mathematics community and he was given time to adopt any work strategy he needed to focus on mending the proof. Weeks and months passed and he failed to make any progress. His main concern during this tense period was that if the flawed manuscript were to be published, “he would immediately be swamped by questions and demands for clarification from would-be gap fixers.” Wiles wanted to avoid this type of distraction, since it would "destroy his own hopes of mending the proof while giving others vital clues” (Singh, p. 267). As a result, he decided to adopt another work strategy so that his seven years of devotion to solving Fermat’s Last Theorem would not end in frustration and humiliation.

Wiles realized that it was necessary to “take somebody into his confidence” – another mathematician with whom he could exchange ideas and examine other possible approaches. After careful consideration, he decided to work with his former Ph.D. student, Richard Taylor, a lecturer from Cambridge University. With Taylor, his aim was to explore the root of the problem and “try to find a way out” (Singh, p. 269). The collaborative research effort continued, but it was Wiles alone who finally gained the insight needed to repair the gap:

> I was sitting at my desk… examining the Kolyvagin-Flach method. It wasn’t that I believed I could make it work, but I thought that at least I could explain why it didn’t work. I thought I was clutching at straws, but I wanted to reassure myself. Suddenly, totally unexpectedly, I had this incredible revelation. I realized that, although the Kolyvagin-Flach method wasn’t working completely it was all I needed to make my original Iwasawa theory work..” (p. 275)

Wiles and Taylor thus proceeded to work together to write a second paper based on the gap-filling proof so that Wiles' original manuscript could at last be declared a complete and indisputable solution to Fermat's Last Theorem.

Andrew Wiles is now famous worldwide for his mathematical achievement and although his story is unique, it highlights three of the most important themes of mathematics education and research: *professional socialization, intellectual "bridge-building" and community ties*.

Mathematics education is rooted in a process of *professional socialization*. A mathematician in training participates in this process by working with a mentor. The mentor's role is to assist his/her trainee with acquiring the specialized knowledge, skills, attitudes, values and norms needed to successfully become a professional. In Wiles' case, it is possible that he might not have become an expert on elliptic curves without the professional support and guidance of his own mentor, Professor John Coates.

Coates recalls that "even as a research student [Andrew Wiles] had very deep ideas and it was always clear that he was a mathematician who would do great things" (p. 161). Yet despite Wiles' giftedness, "there was no question of any research starting work directly on Fermat's Last Theorem. It was too difficult even for a thoroughly experienced mathematician" (p. 161). Wiles himself was also aware of the impracticalities associated with working on the Last Theorem; therefore he set aside his dream for awhile and placed his trust in Coates, who gently pushed him towards a "fruitful" research direction (p. 163).

In time, Wiles' expertise in the subject of elliptic curves made it possible for him to embrace the challenge of *intellectual construction* or "*bridge building*." According to Singh, "the value of mathematical bridges is enormous. They enable communities of mathematicians who have been living on separate islands…to explore each other's creations" (p. 191).

When Wiles' began to work towards a solution to Fermat's Last Theorem, he relied on past results in number theory. The most significant result was the Taniyama-Shimura conjecture because it had made an aesthetic connection between two previously disconnected "islands" in mathematics:

the elliptic world and the modular world (p. 190). Barry Mazur equates its power to that of the Rosetta Stone: "It's as if you know one language and this Rosetta stone is going to give you an intense understanding of the other language. The conjecture has the very pleasant feature that simple intuitions in the modular world translate into deep truths in the elliptic world, and conversely" (Singh, p. 191).

For 30 years, mathematicians remained hopeful that if and when the Taniyama-Shimura conjecture could be proven there would be the development of "a whole new architecture of mathematics" (p. 194). Finally in 1986, Ken Ribet made a significant breakthrough that demonstrated the inextricable link between the Taniyama-Shimura conjecture and Fermat's Last Theorem. This link is described by Singh in terms of a "proof by contradiction" strategy:

> To prove that the Last Theorem is true, mathematicians would begin by assuming it to be false, which would imply that the Taniyama-Shimura conjecture was false. However, if Taniyama-Shimura could be proven true, then this would be incompatible with Fermat's Last Theorem being false, and therefore the Last Theorem would have to be true (p. 202).

Wiles received the news of Ribet's breakthrough during a summer evening visit at a friend's house. The information was related to him through a casual conversation and it was a moment that Wiles recalled in terms of feeling "electrified." He knew almost immediately that if he was going to prove Fermat's Last Theorem, all he had to do first was to prove the Taniyama-Shimura conjecture (p. 205).

Although Wiles' approach to problem solving was largely unprecedented, the basic elements of his work can still be compared to the every day work of others in his profession. A mathematician typically spends time learning about the ideas and research techniques developed by other colleagues. Part of this research process involves recognizing the importance of certain problems or problem areas. With a significant problem in mind, the mathematician will focus on combining his area of expertise with the results of others to develop something new. Sometimes the end result is a "bridge building" proof and at other times it is not. Nevertheless, the importance of the proof rests upon the selection of a *good* problem. As Wiles suggests, even if [the problem] is not solved, it is likely to have the effect of generating “interesting mathematics along the way" (p. 163).

Finally, the most important theme highlighted by Wiles' story, is the theme of *community ties*. In reference to Wiles, most mathematicians would agree that not only is it rare for a scholar to solve a 300-year-old problem, but the routine of doing mathematics is typically not as secretive. According to Singh, “the mathematics department of any university is least secretive of all. The community prides itself in an open and free exchange of ideas and afternoon breaks have evolved into daily rituals during which concepts are shared and explored over tea or coffee” (pp. 3-4). Ribet corroborates the view that mathematics research is mainly a social activity:

> This is probably the only case I know where someone worked for such a long time without divulging what he was doing… It’s just unprecedented…. In our community, people have always shared their ideas. Mathematicians come together at conferences, they visit each other to give seminars, they send E-mail to each other, they talk on the telephone, they ask for insights they ask for feedback – mathematicians are always in communication (p. 209).

Despite the social norm of his profession, Wiles managed to avoid discussing his research ideas with other colleagues. Remarkably, his reclusive and secretive work style did not seem to affect his community status. Wiles' colleagues at Princeton, as well as the mathematicians from the larger community of number theorists were amenable to his puzzling hiatus. More importantly, they were still willing to share information with him.

Singh notes that “after having been a recluse for almost 5 years,” Wiles decided “that it was time to get back into circulation in order to find out the latest mathematical gossip.” In the summer of 1991 he attended a major conference on elliptic equations in Boston and found himself being welcomed by his international colleagues. The other mathematicians "were delighted to see [Wiles] after such a long absence from the conference circuit since they "were “unaware of what he had been working on.” Wiles "was careful not to give away any clues” and he was fortunate because “they did not suspect his ulterior motive when he asked them the latest news” (p. 238).

In the end, Wiles' secretive work efforts proved to be worthwhile. His intellectual "bridge building" grew from his community roots; hence it was fitting that he chose to give his special lecture during the number theory workshop at the Isaac Newton Institute in Cambridge. For Wiles, Cambridge was considered to be "a wonderful place to announce the proof – it [was his] old hometown, and [he had] been a graduate student there" (p. 244). Several of the mathematicians who played an important role in his work, specifically John Coates, Ken Ribet, Barry Mazur, and Nick Katz were planning to attend the workshop and he knew that he could communicate with colleagues he trusted. At different points in his career, these same colleagues fostered his mathematical interests, protected his secret research efforts and applauded him when he presented his solution to Fermat's Last Theorem.

In sum, Simon Singh's biography is more than just the story of an academic hero because it triumphs at de-mystifying the world of mathematics by highlighting what mathematicians do and how it is that they work together to achieve a higher purpose. Andrew Wiles possessed great problem-solving abilities, yet because he was a member of a professional research community, the community of Number Theorists, there is evidence that he could not have succeeded in a state of total isolation. His professional community needed to be involved at crucial points and when his colleagues were involved, they supported him as he moved towards the final stages of his remarkable proof.

In the following chapters a new 'story' of *professional socialization, intellectual construction* and *community ties* in mathematics research will be told. This story is not about one particular mathematician, but a group of mathematicians belonging to an international research community known as the "European Singularities Network." It is about an "invisible college" and part of it also takes place at the Isaac Newton Institute, which has a reputation for bringing together the most eminent researchers in the world.

1.2 Research problem

The term “invisible college” appears often in studies of scientific communication; however, its modern reference to an *informal* communication network of scholars (revived by Price; 1963) has not been reflected well in the research literature. According to Lievrouw (1990), there is a lack of real information about invisible colleges because researchers tend to focus on products of scholarship (i.e., documents and citation data) and/or network structures rather than on the actual communication processes of people who do scholarly work (e.g., Chubin, 1985; Crane, 1972; Crawford, 1971; Lingwood, 1969; Mullins, 1968).

For example, Crane (1972) examined the growth of two specialty literatures, one in mathematics and one in sociology, and approached the issue of “informal communication” by collecting survey data on co-authorship patterns and the exchange of preprints. Her survey questions focused on gathering information about *formal* communication activities – activities that facilitate the production of documents – and Lievrouw (1990) argues that the data should not have been construed as examples of *informal* behaviour (p. 63).

Lievrouw's (1990) criticism of Crane's (1972) study and other similar studies is that the term invisible college is frequently misused or given different meanings for different purposes; therefore it has become an “ambiguous” construct. In light of this, she raises an important question: are invisible colleges *structures* of scholarship (discernable and measurable from outside elements – i.e., published documents) or are they *social processes* rooted in informal communication behaviours, perceivable only to those who carry out these behaviours? (p. 66).

To reconcile the structure versus process problem, Lievrouw (1990) recommends the following:

> If the invisible college is to be an informal social phenomenon then a revised definition can be proposed: *An invisible college is a set of informal communication relations among scholars or researchers who share a specific common interest or goal* (p. 66; original emphasis).

With this definition, there can be no assumption that an invisible college is rooted in a prerequisite formal institutional structure (p. 66).

Lievrouw's (1990) second recommendation is that future research concerned with the invisible college should be based on a new set of issues. For example: How do individuals perceive their interactions with others within, versus outside of, the invisible college? How do individual scholars use invisible colleges as resources to help fulfill their information needs? (p. 67).

Her third recommendation is that ethnographic methods of research, in addition to bibliometrics should be used in studies of scholarly communication. Lievrouw (1990) concedes that bibliometric analysis is an effective technique for producing "maps" of documents and that such maps offer a "systematic glimpse of the communication acts that produced the documents in the first place" (p. 68). On the other hand, if the goal is to understand underlying "informal" aspects of communication (i.e., mentoring, collegiality, or collaboration) she insists then that qualitative techniques of research, primarily participant observation and interviewing, have the potential to give the researcher more "interpretive and heuristic" power over a study (p. 68).

For the most part, Lievrouw's (1990) research observations and recommendations are valuable. She identifies an important problem (i.e., the *structur*e versus *social process* problem concerning the invisible college) and advises researchers to design new studies that will help solve this problem. Nevertheless, her proposed *definition* of the invisible college requires further consideration. This definition sets the invisible college apart from all other types of communication systems; particularly those rooted in formal structures, and my argument is that it may be too narrow.

Derek de Solla Price (1963; 1986) originally used the term "invisible college" to denote an informal communication network of scholars – *elite* scholars from different research affiliates who belong to an "in-group" of approximately 100 people. According to Price, the "people in such a group claim to be reasonably in touch with everyone else" and have the power to confer power and prestige on one another. With respect to how the group members stay in touch, he specified that they "meet in select conferences (usually held in rather pleasant places), they commute between one center and another, they circulate preprints and reprints to each other, and they collaborate in research" (1986, p. 119). Moreover, he stated that the members contribute "*materially*" to research in a subject area and that they not only do so "on a national scale," but also on an international scale, including "all other countries in which that specialty is strong" (1986, p. 119, emphasis added).

Price's (1963; 1986) recognition of the *material* contributions of invisible college members is significant because it implies that published documents are relevant to the invisible college phenomenon, even if the production of these documents is not entirely its sole purpose. When the participants of an invisible college network generate work for publication, and when an attempt is made to trace the links between their publications, there is an opportunity to gain insight into their shared research interests – interests that comprise their "subject specialty." As a result, the "subject specialty," rooted in documented evidence, may be viewed as the *structural* component of the invisible college, whereas the invisible college itself is the underlying *social* component. Hagstrom's (1970) understanding of the relationship between the two terms, *invisible college* and *subject specialty* provides further clarification:

> the set of scientists in a discipline who engage in research along similar lines can be called the scientific specialty. It is reasonable to believe that scientists will communicate most often and intensively with others in their specialties, exchanging preprints with them [and] citing their work… There is some evidence that "specialties" are not "invisible colleges" – or tightly knit networks of communication .. [yet] most specialties in science are quite small -- small enough so that they could be tightly knit communication networks even if they are not so in fact (p. 91-92)

Given both Price's (1963; 1986) and Hagstrom's (1970) explanations, it is clear then that an invisible college can exist within a subject specialty, but a subject specialty is not necessarily an invisible college. In effect, the *structure* versus *social process* problem also becomes clearer. There may be a lack of real information about invisible colleges, as Lievrouw (1990) suggests, because it is easier for researchers to study the specialty or *structural* component rather than the interpersonal or *social* component. Most documents associated with a subject specialty are readily available to researchers and can easily be submitted to bibliometric analyses. But, even though it easy to access documents and create bibliometric "mappings" of the intellectual structure of scholarship, it can be problematic to assume that they reveal much about underlying *informal* communication.

Conversely, if Lievrouw's (1990) proposed definition of the invisible college is adopted, with its emphasis on the "*set of informal communication relations among scholars or researchers, who share a specific common interest or goal,*" another set of problems may arise. For instance, it can be difficult to determine who is participating in an invisible college network, if the shared goal that has cultivated interpersonal relationships within that network is not identified. In other words, it is not efficient to examine how individual scholars make use of personal contacts to satisfy information needs if there is no prior understanding of the intellectual basis for their needs in the first place - their *subject specialty*.

In order to understand the true nature of the invisible college, we must first recognize that it is not a one-dimensional construct, but rather a *multi-dimensional* phenomenon. Second, we need to establish a clear definition that remains open to all of its multi-dimensional components. My proposed definition is the following:

> An invisible college is a set of interacting scholars or scientists who share similar research interests concerning a subject specialty, who often produce publications relevant to this subject specialty and who communicate both formally and informally with one another to work towards important goals in the subject, even though they may belong to geographically distant research affiliates.

With this new definition, researchers can evaluate the types of contributions that bibliometric and ethnographic modes of analysis can make to the study of invisible colleges. Also, it opens up an opportunity to superimpose sociometric data on bibliometric data as a means for creating "representations of a specialty based on citation and its variants, co-authorship, colleagueship, trusted assessorship, mentorship;" factors that "would corroborate the [invisible college's] spatial and temporal dimensions" (Chubin, 1976, p. 455).

To gain new insight into the invisible college phenomenon, I have chosen to focus on Singularity Theory research in mathematics. A *singularity* is defined as "the strange but remarkable point among anonymous non-singular points" (Trotman, 1999, p. 866). Singularity Theory research is concerned with various types of singularities and how they arise in different branches of mathematics and physics, including algebraic geometry, dynamical systems and string theory. There are approximately 100 mathematicians worldwide involved in this subject specialty (European Singularities Network, 2001).

This case study is based on the assumption that Singularity Theory is both an *intellectual structure* of communication and a *social process* of information seeking and sharing. It is a special type of research organization or "territory," which can be mapped (i.e., bibliometrically traced) and explored as a community with a social history at the same time.

1.3 Conceptual model for the research

The conceptual model for this research is derived from the theoretical work of Rosenbaum (1993, 1996). Rosenbaum was the first to develop a structurationally informed value-added model for the study of organizations. Originally his "structurational" approach was used to examine information behaviour in a management organization; however, its significance is that it can be generalized to various types of organizations, including scientific organizations.

Rosenbaum's model, illustrated in Figure 1.1, stems from a merging of Taylor's (1991) value-added perspective of Information Use Environments (IUEs) with Gidden's (1984) theory of structuration. When combined, the two theoretical perspectives create "duality of structure." Duality of structure is a more effective way of understanding "how the information behaviours [of social actors] and social environments mutually and simultaneously constitute each other" (Rosenbaum, 1993, p. 235). Specifically this means that individuals who interact with one another in social situations can be influenced by the structure of their IUE. At the same time, the IUE can also "be a product or consequence of [how individuals engage in] social action and interaction" over time (p. 79).

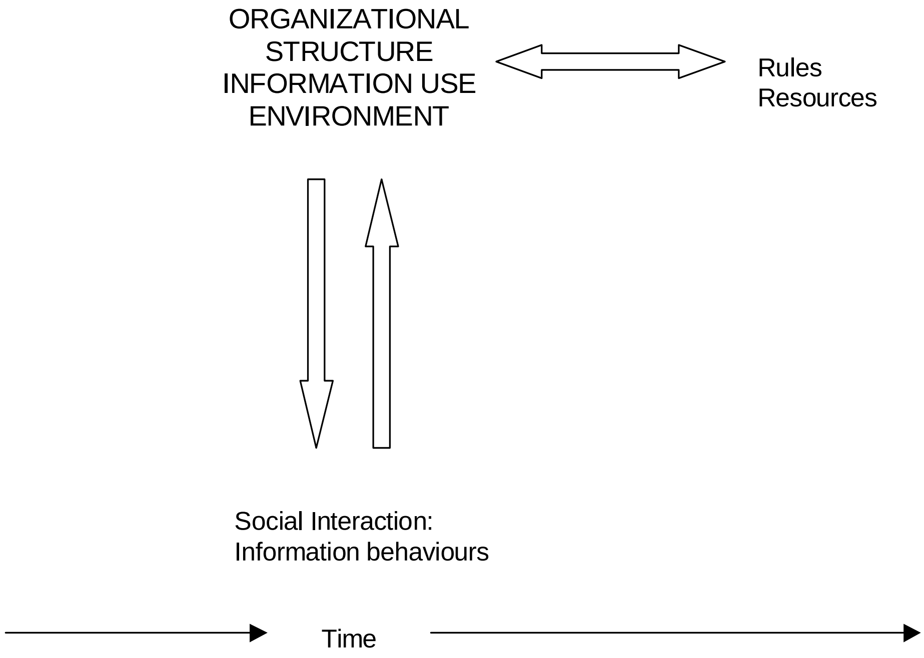

Figure 1.1. Rosenbaum's original model of a structurationally informed value-added approach to organizations (Adapted from Rosenbaum, 1996, p. 101).

For this study concerning a scientific organization, or "invisible college," I have chosen to include the key components of Rosenbaum's (1996) model, but introduce a few modifications. The "structurational" and temporal aspects of Rosenbaum's model, as well as his conceptual understanding of the dual nature of **social interaction** and the **IUE** are retained. In my model however, the **social actors** are specified as the vehicles for **social interaction** and the rules attributed to the **IUE** are shifted to another component termed the **subject specialty**. The resulting new model is shown in Figure 1.2. Three overlapping ovals have been illustrated to highlight the interrelationships between 1) the **subject specialty**, 2) the **social actor**, and d) the **IUE** (see Figure 1.2).

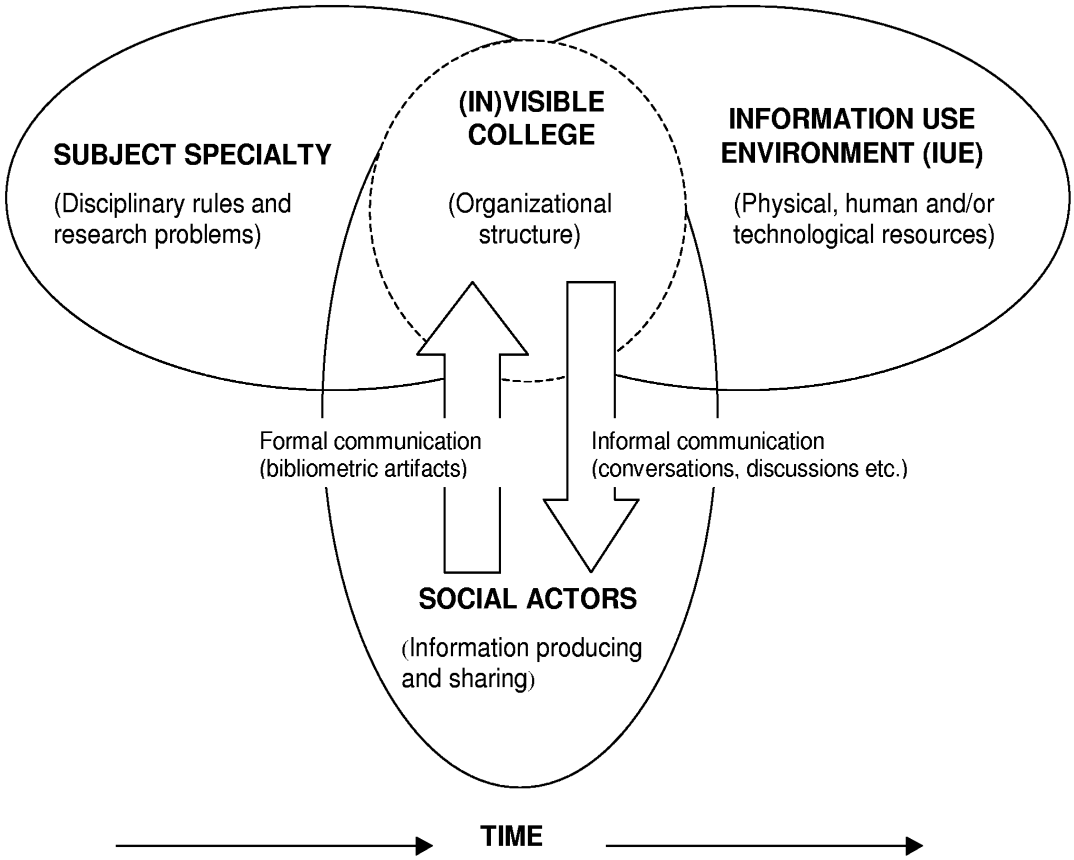

Figure 1.2. Structurationally informed value-added model for the study of scientific organizations.

The space that intersects the **IUE,** the **subject specialty** and the **social actors** produces an organizational structure termed the **(in)visible college**. An **(in)visible college** may or may not be visible, depending on its association with a particular type of **IUE**. Some **IUE's** are grounded by a physical space, while others or not; thus the **IUE** is basically "the set of elements that affect the flow and use of information messages into, within, and out of any definable entity" (Taylor, 1986, p. 3). If the **IUE** is established as a physical space, it has the potential to fortify an **(in)visible college** with the provision of human, physical and/or technological resources. In addition to the **IUE**, the **subject specialty** is important because it informs the **(in)visible college** of its disciplinary rules and research problems. The scientific researchers who understand the disciplinary rules and interact with one another to solve research problems are the **social actors**. These **social actors** make use of the **(in)visible college** to support their information seeking and sharing, but they may also reinforce or "instantiate in action" (Rosenbaum,1996, p. 112) the **(in)visible college** through the contribution of bibliographic artifacts, or evidence of scientific achievement for preservation.[1]

1.4 Research assumptions and questions

In connection with this new structurationally-informed value-added model of the (in)visible college, there are four working assumptions:

1) Social actors who interact more frequently with one another in an (in)visible college network are those who tend to share similar topic or problem area interests within a subject specialty.

2) The amount of time social actors spend together and their level of familiarity with each other has some influence on who they choose to interact with for collaborative purposes.

3) By virtue of being an *informal* communication network, an invisible college can stimulate interaction among social actors, which in turn leads to the production of bibliometric artifacts. However, even in the midst of this interaction there is no guarantee that these artifacts will appear.

4) Resources made available to social actors by a physically manifested IUE have the power to shape or influence their social activities, but in the absence of this physical IUE the invisible college may still exist and thrive.

To substantiate these four assumptions and guide my case-based investigation of the Singularity Theory community, the following research questions have also been developed:

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[1] Note that my understanding of all elements that make an invisible college more or less "visible" differs from Koku, Nazer and Wellman (2001). In their study of the Globenet scholarly network they define the "visible college" as a community based on "a shared vision, the availability of financial support, a formal selection process, defined boundaries, and mandatory group meetings" (p. 1757).

- What are the topics that comprise the intellectual structure of Singularity Theory research?
- Who are the mathematicians that have made formal (bibliographic) contributions to this subject and how are they intellectually related to one another?
- Can the formal cognitive aspects of an ACA of Singularity Theory help to uncover significant information about underlying social relationships?
- What is the co-authorship structure of Singularity Theory research and how does it relate to the intellectual structure of this subject?
- What is the structure of collegiality in Singularity Theory research and how does this structure relate to subject's intellectual structure and co-authorship structure?
- What is the role of the international mathematics research institute as a specialized information use environment?
- What types of resources are made available to the mathematicians who visit this research environment?
- How do the visiting mathematicians make use of the institute environment for information sharing, and what are their personal expectations and experiences?
- How can we measure the success of the institute environment in promoting the cross-fertilization of research techniques and ideas to create diffusion of innovation in mathematics?

With these questions in mind, the next chapter will now focus on the research literature concerning scientific communication. Specific attention will be given to the invisible college construct and research based on collaborative work in science.

CHAPTER 2: REVIEW OF THE RESEARCH LITERATURE

2.1 Scientific communication

Communication is the essence of scientific knowledge work. Scientists communicate to share ideas and information, to verify new discoveries and detail their research findings so that evidence of scientific knowledge is preserved for future reference. Communication, as Griffith (1990) suggests, can be viewed as "the only general scientific behaviour," since "other behaviours are mostly specific and technical" (p. 31).

As a general form of behaviour, the process of scientific communication is multifaceted. Scientists rely on different *modes* of communication (i.e., oral or written) for different research purposes and they also look for and share information via different communication *channels*. *Formal channels* "carry information which is public and remains in 'permanent' storage," whereas "*informal channels* carry information to restricted audiences and its storage is relatively temporary" (Garvey & Griffith, 1968, p. 131). Moreover, scientists participate in different types of *communication systems* - systems that coincide, place more or less emphasis on *formal* and *informal* information channels, and fall under the influence of cultural, and political and economic factors.

Paisley's (1968) "systems approach" to scientific communication indicates that culture plays a significant role because the scientist's *cultural system* is responsible for "awarding Nobel prizes, emphasizing priority of discovery, establishing great private foundations and supporting universities" (p. 4). *Political systems* are influential because the "the need to know" in science is often motivated by federal funding programs (p. 4). It is also common for political factors to contribute to "persistent scientific nationalism" in many research fields and to sometimes cause scientists to "largely ignore foreign research" (p. 4). Scientific communication is further affected by a broader *legal/economic system* – "the system of patents, copyrights, industrial secrecy, competitive research and development – all profoundly affecting the flow of information" (p. 6).

The scientist's *membership group*, *reference group*, *invisible college*, *formal organization* and *work team* are distinct communication systems that function as primary information sources and are "especially relevant to information use" (p. 4). When a scientist is asked, "what do you do?" (s)he is likely to name a *professional membership group*: "I'm a mathematician." Adherence to this membership group may not be strict (e.g., some theoretical physicists also refer to themselves as mathematicians); however, it will be the system that controls "the 'official' information channels of the scientist's field" (e.g., primary journals and monograph series) (p. 5).

The scientist's research identity is shaped further by a *reference group*, "which includes other scientists with similar specialization, similar training, excellence of work, or other characteristics" (p. 5). Paisley (1968) explains that a reference group "need not be contained within a membership group" given that certain specialties in science (e.g., human information-processing) are drawn from several membership groups (p. 5).

The *invisible college* or subsystem of the scientist's *reference group* is comprised of a smaller number of international researchers and is designed for "direct access." Although the scientist may save papers and reprints from researchers belonging to his/her reference group," (s)he is more likely to arrange meetings, plan joint projects and co-author works with participants of the same *invisible college* network (p. 5).

Formal organizations in science (e.g., universities, private research companies) are communication systems that emphasize "roles, lines of responsibility and products rather than people themselves" (p. 6). *Formal organization systems* also integrate scientists of different status levels at the same location (p. 6).

Paisley (1968) notes that the scientist's *work team* is the most important information system, since it documents the history of its research projects and provides the scientist "with rich, non-redundant information through conversation" (p. 6). Within the *work team*, the most subjective or personal communication system is "*the scientist within his own head*" (p. 6). This system is aptly described as the "system of motivation, of intelligence and creativity, of cognitive structure, of perceived relevance of information inputs and uses of information outputs. Ultimately, all other systems support this one" (p. 6).

Finally, the process of scientific communication would not be complete without the scientist's *formal information system*. Libraries and other types of information centers (e.g., online journals; indexing databases; specialized research institutes) constitute this system, which has become a "competing marketplace" for specialized information services (Paisley, 1968, p. 6).

To understand communication in science, studies based on one or a combination of the above information systems have been carried out by a number of researchers from both the fields of sociology and library and information science. A large selection of these studies, published in the 1960s and 70s have been reviewed by Menzel (1966), Herner and Herner (1967) Paisley (1968), Allen (1969); Lipetz (1970), Crane (1971), Lin and Garvey (1972) and Crawford (1978). More research has accrued throughout the 1980s and 1990s, and since 2000 there has been a growing interest in the role that electronic media plays in scientific work, including the social forces in science that are shaping the adoption of technological innovations (e.g., Hurd, 2000; Kling & McKim, 2000). Clearly the literature concerning scientific communication is expansive. The scope of this review will take the *invisible college* as its main focus, including a sub-section of studies relevant to the *scientist as a social actor* and *collaborative work in science*. References to research concerning "*the scientist within his own head*", *scientific work teams*, and *information use environments* also appear in different parts of this thesis. Essentially, it is the *invisible college* literature and the gap associated with this literature that is of primary interest, since this aim of this study is to identify this gap and demonstrate how it may be filled.

2.2 The scientist as a social actor

Research concerning the scientist "*within his/her own head*" typically focuses on material information resources and the cognitive-behavioural work processes carried out by the scientist in their everyday work environments (e.g., Brown, 1999; Chen, 1974; Ellis & Haugan, 1997; Hart, 1997). Much of this research is survey-based and its main advantage is that it allows researchers to compare and contrast the information needs and uses of large groups of scientists. With the insights gained from survey research, information professionals are in a better position to assess the effectiveness of current information services, and make efforts to introduce improvements or create alternative types of services for changing needs. A closer look at the information seeking literature reveals however, that the scientists do not operate solely "*within their heads*," since they are also *social actors* to a large degree. Collegial contacts play a vital role in their everyday work; therefore information seeking and use in science can be understood according to a social model.

Allen's (1996) view of a social model is that "people are always embedded in social situations." Within these situations "it is sometimes difficult to distinguish clearly between individual influences and social influences on information-seeking behaviour" (p. 73). When looking at information needs from a social perspective, this means that emphasis should be placed on the "social embeddedness" of the need and the "social factors that influence how people approach their information needs" (p. 73). According to Allen (1996), "individual knowledge structures are derived from experience," and "experience necessarily occurs in [a] society" (p. 74). Likewise, people who share information-based experiences typically possess similar "knowledge structures" and "it is precisely this social reality that makes communication possible" (p. 75).

Scientists rely on personal contact with colleagues to satisfy information needs relevant to individual tasks and personal goals. At the same time, they also *share* or *exchange* information with colleagues, locally, nationally and internationally, so that others can benefit from their personal knowledge. This sharing of information leads to *communal knowledge* within a specialized area of science and can lead to stages of innovation and growth. Crane (1972) refers to scientific growth as a kind of "contagion" process in which ideas are transmitted from person to person; hence the scientist as a "social actor" plays an active role in allowing the contagion effect to take place.

Specialized work teams and invisible college networks in science are valuable contexts for studying social information seeking and sharing. Work teams are typically small (15 to 20 people), localized social systems that provide the scientist "with rich, non-redundant information through conversation" (Paisley, 1968, p. 6). Past research concerning scientific work teams reveals that there tends to be a close relationship between interpersonal information seeking and research performance. Specifically, the more contact that scientists have with different colleagues in the laboratory setting (e.g., 15 to 20 or more individuals) and the more time spent in communication (e.g., 15 hours per week), the greater their experience is in terms of higher levels of productivity (Pelz & Andrews, 1996). Moreover, work teams have been found to solve problems more effectively as a result of higher levels of interpersonal contact (Allen, 1977).

The invisible college, by contrast, is a social system that may overlap the work team to some degree. It acts beyond the laboratory or departmental setting and encourages a similar interpersonal function, except that it allows the scientist to have access to a much broader resource of personal contacts, often international in scope. An invisible college also differs from the work team because more financial support is required to help develop and maintain interpersonal contacts that are challenged by geography. Despite some differences, however, the common element between the work team and the invisible college is that "the more strongly an individual identifies with each of these social systems, the more accountable he or she feels to the goals and norms of the system" (Paisley, 1980, p. 136).

2.3 Invisible college networks

The term “invisible college” was first used in seventeenth century Europe when the Royal Society of London was founded. Members of this early Royal Society of Scientists (many of whom were mathematicians) did not belong to a formal institution; but referred to themselves as an invisible college due to their geographic closeness and regular meetings based on shared scientific interests (Bartle, 1995; Lievrouw, 1990; Lingwood, 1969; Price 1963).

Later, Price’s (1963; 1986) bibliometric research led to the modern identification of invisible colleges as groups of elite, mutually interacting and productive scientists from geographically distant affiliates who exchange information to monitor progress in their field. While his research focused on measuring formal channels of communication, specifically the growth of scientific literatures and collaborative work as evidenced by multi-authored papers, he used the term “invisible college” to emphasize *informal* patterns of interpersonal contact among scientists.

In order to examine "the tangible results of scientific work more deeply,” Price (1993; 1986) emphasized the importance of learning “more about the social institutions of science and the psychology of the scientist” (1986, p. 56). The scientist’s character, he said, is rooted in “the basic difference that exists between creative effort in the sciences and in the arts. The artist’s creation is intensely personal, whereas that of the scientist needs recognition by his peers. The ivory tower of the artist can be a one-man cell; that of the scientist must contain many apartments so that he may be housed among his peers” (1986, p. 62).

Price’s (1986) understanding of the invisible college was that it emerged from new groups of scientists; “groups composed of [a maximum] of 100 colleagues.” These groups, he explained:

> devise mechanisms for day-to-day communication. There is an elaborate apparatus for sending out not merely reprints of publications but preprints and pre-preprints of work in progress and results about to be achieved.
>
> In addition to the mailing of preprints, ways and means are being found for physical juxtaposition of the members… For each group there exists a sort of commuting circuit of institutions, research centers, and summer schools giving them an opportunity to meet piecemeal, so that over an interval of a few years everybody

who is anybody has worked with everybody else in the same category (pp. 74-76).

Following Price's (1963; 1986) lead, the invisible college soon became an important concept for analysis. A number of researchers during the late 1960s were interested in the general role of *informal* communication in science (e.g., Garvey & Griffith, 1968; Hagstrom, 1965; Menzel, 1968; Mullins, 1968); however, the notion of an "organized" system of informal interpersonal contacts presented a significant problem. Crane's (1969; 1980) concern was that it would be difficult to prove the existence of invisible colleges, given that scientists "have many contacts with other scientists in their own research areas and in other fields; some fleeting, some lasting. If social organization exists in a research area, it is of a highly elusive and relatively unstructured variety" (1980, p. 10).

To test the invisible college hypothesis, or the idea that "science grows as a result of the diffusion of ideas that are transmitted in part by means of personal influence," Crane (1972) focused on measuring the social ties among scientists in two research specialties: sociology (i.e., the diffusion of agricultural innovations) and mathematics (i.e., the algebraic theory of finite groups).[2] A preliminary analysis of the published literature (403 papers in sociology and 305 papers in mathematics) revealed that the growth of both specialties in terms of cumulative numbers of new authors and new publications had progressed through the first three stages of the logistic curve.[3]

Crane's (1972) sample of authors (221 from sociology and 102 in mathematics) were mailed a bibliographic list of their personal publication references, along with an attached questionnaire based on the references. In the questionnaire, respondents were asked to give the names of scientists who had influenced their selection of research problems and research techniques, to name persons

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[2] Since Price's (1963) invisible college hypothesis grew from the observations of fast-moving specialties in physics, Crane (1972) decided to select a field from the social sciences, thinking that it would "grow more slowly and less efficiently than [a field from] the natural sciences" (p. 15). The mathematics field was then added to the study for comparative purposes. Crane (1972) noted that "in this case, an effort was made to select an area from a discipline in which the character of research is very different" (p. 16).

[3] Crane (1972) adopted Price's (1966) theory that the growth of science follows a logistic curve and that the exponential accumulation of the literature is the direct result of interpersonal interaction among scientists.

with whom they discussed research related to the area, and to list the names of researchers with whom they were currently collaborating.

The results of the survey revealed that interaction had occurred between the two research specialty members and that the members' knowledge of one another had in fact led to mutual influence and communication. Seventy percent of the members of the mathematics specialty were linked to one another in one large communication network, and 43 percent of the members of the rural sociology specialty also shared the same linkage. Two sociograms (communication networks) were constructed for the separate specialties based on the information given by respondents concerning the identities of persons with whom they discussed their ongoing research informally. Sixty-six percent of the mathematicians were found to be members of a single "informal discussion" network and 73 percent of the members of the rural sociology area were also linked to this type of network. When other types of ties, such as influence on the selection of research problems, student-teacher relationships, published collaborations were included in the sociometric measure, the percentage of connectedness increased to 78 in mathematics and 74 in sociology.

Further examination of the survey data indicated that there was a strong correlation between productivity and the individual scientists' present commitment to their research specialty. Crane (1972) also discovered that the most productive members of both specialties tended to have more relationships with many of the other research specialty members. As both the sociology and mathematics areas grew, the highly productive scientists played major roles in the direction of theses and seemed to be the most important sources of influence upon the selection of problems.

What interested Crane (1972) was that even though the sociologists and mathematicians reported names of personal contacts within their specialties, many of them were just as likely to have contacts with scientists who had not published in their specialties. Fifty-one percent of the rural sociologists' contact choices, based on all types of sociometric ties (except for published collaborations), pointed to outsiders. In the mathematics area, 72 percent of the respondents reported the names of persons within the specialty, while 28 percent reported the names of outsiders.

At first, Crane (1969; 1972; 1980) thought that her survey statistics, especially from the sociology specialty, were providing evidence of a *lack* of social organization. However, after she examined the data more thoroughly, she found that the majority of the outsiders named by the rural sociologists were selected only once (84%) and that a very small number (3%) were named more than five times. The same was true for the mathematicians in that the majority of outsiders were only mentioned once. Accordingly the type of social organization detected in both specialties could best be described in terms of a "social circle" (1980, p. 11).

Inside a "social circle," direct and indirect ties exist between many of the scientists, but not necessarily all of them. A social circle tends to be "not well instituted," but can become visible over time through publication patterns when a group of productive scientists are drawn together for a period of intense research on a topic (Crane, 1980; Kadushin, 1966). Crane (1969; 1972; 1980) noticed that the members of the sociology and mathematics specialties possessed a great deal of influence on one another within their respective "social circles." Still, they needed to keep in touch with outsiders for a variety of reasons; the most important being to prevent psychological over-investment in one subject area and to achieve a measure of originality in problem solving.

Crane's (1972) monograph *Invisible Colleges* was published shortly before Grannovetter (1973) introduced the "weak ties" theory; however, there are some parallels between this theory and Crane's view of how the "social circle" operates in science. The need for scientists in specialty groups to contact other scientists outside their immediate social circle demonstrates to some extent that "weak ties [are] indispensable to individuals" (Granovetter, 1973, p. 1971). According to Grannovetter, "those to whom we are weakly tied are more likely to move in circles different from our own and will thus have access to information different from that which we receive (p. 1371). It follows then, that when scientists make use of "weak ties" with other scientists or pursue "different" information from outsiders, they are more apt to "build bridges between cognate specialty areas" (Cronin, 1982, p. 227). Crane (1970) also acknowledged previously that "the most productive scientists in [a] core area have ties with the most productive scientists in other areas, thus producing the cross-fertilization of ideas which is required for innovation" (p. 39).

Research carried out by Crawford (1971) and Gaston (1969) during the late 1960s and early 70s produced findings similar to that of Crane (1969; 1972; 1980). Crawford's (1971) sociometric analysis of informal communication among sleep and dream research specialists revealed the presence of a dominant social network (73%) of active scientists. At the core of this network was a group of 33 "central scientists" who were the recipients of the most contact and who had distinguished themselves from other members of the network by their greater productivity. These central scientists also accounted for 83 percent of contacts between the various sleep and dream research centers across the United States. Likewise, Gaston (1969), using a sample of high-energy physicists from across the United Kingdom, found that the most productive and highly recognized scientists (including experimentalists and theoreticians) were connected to more members within the specialty area, both by their own choice and through the choice of others.

Crawford's (1971) research in particular supported Crane's (1969; 1972; 1980) discovery that even in the midst of social organization within a specialty, contacts with scientists on the outside still tend to play an essential role in scientific problem solving. When Crawford (1971) asked the sleep and dream researchers to name the persons they had contacted more than three times during the past year concerning their work, 58 percent of the total persons named were from the same specialty, while 42 percent were scientists from other specialties.

Contrary to Crane's (1969; 1972; 1980) findings, research has demonstrated further that a *lack* of communication among individuals in a scientific specialty can slow publication growth, and in some cases, cause the specialty itself to disappear. For example, Fisher's (1966; 1967) socio-history of "The Last Invariant Theorists" revealed how one mathematics specialty in the early 1900s eventually disappeared because the mathematicians who were contributing to this field had been working in isolation from one another across Great Britain, North America and Germany.

Fisher (1966) collected and analyzed a cumulative number of publications (N=1,342) associated with Invariant Theory (1887 to 1941) and observed a growth pattern based on a linear distribution instead of the exponential distribution typical of other thriving specialties (cited in Crane, 1972, p. 178). Following a socio-historical investigation of this specialty, he found evidence to attribute this growth pattern to three key variables: "the general *environment* in which the mathematics [was] done, the specialists' *commitment* to the theory, their relationship to their students, and the *places* in which they worked (Fisher, 1967, p. 218).

In Great Britain, there were almost no "schools of thought" across the country and all the major contributors to the field had died before 1900, "leaving no effective invariant-theoretical progeny" (p. 231). The Americans, on the other hand, faired much better than the British, in that the research conditions did in fact encourage the development of "schools." A certain number of American students were trained by specialty leaders, but the schools never did become established because the students either "left mathematics or soon became isolated because the institutional structure was not sufficiently built up to provide them with encouraging places to work" (p. 235).

In Germany, Fisher discovered that the patterns of affiliation among the mathematicians differed markedly in comparison to those in Britain and America. The research situation in this country was such that it did in fact allow for the establishment of schools of thought. Also, many of the students who were directly introduced to invariant theory by their German instructors went on to teach in universities or technical schools. But for reasons not entirely known, perhaps due to their personalities or because of the environment in which they were placed, these mathematicians "did not transmit the theory to a succeeding generation" (p. 238).

Among all the mathematicians studied by Fisher (1967), very few were found to be "completely occupied" with the study of invariant theory. "As is typical of the profession," he notes, "they all moved from topic to topic so that most of those who worked on the theory had only marginal commitment to it" (p. 243). Those mathematicians who were deeply committed to the specialty area somehow managed to transmit their commitment to their students, but these students then became the last surviving Invariant Theory practitioners.

Certain modern algebraists of the 1920s were convinced that “Invariant Theory had died when Hilbert solved its central problems in 1983:” however, Fisher’s historical analysis revealed otherwise (p. 243). A few men did continue to cultivate the theory well into the 1920s. These mathematicians who continued to work on certain problems often did so in connection with other subjects, and because they were not associated with Invariant Theory as a distinct specialty, they did not refer to themselves as Invariant Theorists. They also did not seem to have any connection with the older Invariant Theorists and were treating the new problems “in a completely different manner using new settings and different techniques” (p. 244).

In comparison to Crane’s research approach, Fisher’s (1967) study of the invisible college gives the impression of being more thorough because of the time and attention given to the subject content of Invariant Theory and the personal histories of the men identified as the “Last Invariant Theorists.” While Crane’s survey and social network approach did “enable [her] to test hypotheses about social connectedness, recruitment, and communication in two fields,” her work was criticized because it “presented almost nothing about the intellectual content or the personalities working in diffusion theory or finite groups (Hagstrom, 1973, p. 382). Perhaps Crane (1972) could have uncovered more details concerning her subject areas and the personalities of the research specialists; however one can understand that it would have been difficult due to the fact that the invisible college, by virtue of its “invisible” nature, is a complex phenomenon to study. Even with today’s advanced communication technologies, there are still obvious challenges associated with gaining first-hand insight into the work habits and social patterns of academic personalities who are dispersed across a nation or in many cases worldwide.

Among the most recent invisible college studies, Lievrouw, Rogers, Lowe and Nadel (1987), Sandstrom (1998) and Tuire and Erno (2001) demonstrate that triangulation, or “the comparison of several types of data gathered about a single social phenomenon” can be useful as a research strategy

(Lievrouw et al, 1987, p. 244). Triangulation tends to be "resource intensive" in that it requires an enormous amount of support, both in terms of financial and human capital to do adequately. The results produced through this method can be confusing and difficult to reconcile, but the key advantage in using triangulated data, though sometimes "messy," is that it can "reflect the actual 'messiness' that is typical of most social networks (Lievriouw et al, 1987, pp. 244-245).

Lievrouw et al (1987) examined a single social network or "invisible college" of biomedical scientists from the United States, specializing in lipid metabolism research. Data were collected from a) a co-word analysis of grants awarded to biomedical scientists by the National Institutes of Health (NIH); b) a factor analysis of the scientists' responses on a sociometric roster instrument; c) a co-citation analysis of their publications and c) qualitative analyses of interview and questionnaire responses.

Once analyzed, the data revealed that the lipid metabolism scientists who had been awarded grants tended to appear prominently in the research literature, and that many of the principal grant holders across the United States also exhibited a certain degree of professional affiliation and interaction as co-workers. Of interest to Lievrouw et al. (1987) was that their findings related a clear "distinction between the communication structure, or social network among scientists, and the actual content of [their work]" (p. 245). The social network cluster of the lipid metabolism scientists verified the presence of close links among the scientists based on mentoring relationships, co-authoring relationships and communication patterns. At the same time, an examination of the same scientists' published work revealed that there were "a number of different *lines* of specialized research, going on somewhat independently of one another" (p. 245, original emphasis). Two obvious sub-specialties were detected in the co-citation clusters, but Lievrouw et al. (1987) discovered that these sub-specialties were not in competition with one another intellectually because the topics were closely related and complementary. Competition did however seem to be a prominent with respect to the need for financial, human, and clinical resources. The research team concluded that the tightly-woven social network of lipid metabolism scientists most likely developed as a result of a need to share in this limited resource pool (p. 245).

Sandstrom's (1998) work concerning an invisible college in human behavioural ecology, also based on the combined use of bibliometric, questionnaire, and interview data, led to the development of an "optimal foraging model" for understanding how scholars seek and use information while creating new knowledge.[4] The optimal foraging model explores why it is that information seekers choose to exploit certain information resources while ignoring others or why they choose to shift attention away from one line of inquiry in favor of another (Sandstrom, 1994, p. 414). The model's key elements of investigation include: a) the *actor*, or person responsible for "choos[ing] among [information] alternatives, b) the *currency* by which the actor measures costs and benefits of these alternatives, c) the set of *constraints* beyond the actor's control that limit his or her behaviour, and d) the *strategy* set specifying the actor's range of available options (Smith, 1983; cited in Sandstrom, 1994, p. 416, original emphasis)

To create a "topography" or bibliometric mapping of the human behavioral ecology specialty (1988-1995), Sandstrom selected a sample of 63 authors – all major contributors to the specialty, but from different fields (e.g., anthropology; biology; psychology) – and correlated the author's *oeuvres* (first-author bodies of work) in an author co-citation analysis.[5] A structural examination of the "map," or two-dimension scatterplot of correlated author nodes was then carried out to determine the "intellectual and style-of-work dimensions, the clusters of authors' names and the boundaries between them" (p. 324).

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[4] The term optimal foraging originated in the ethological studies of searching behaviour and prey selection among animals and has been used in recent years by anthropologists to "analyze dietary choices, habitat usage, group size and settlement, and time allocation among human hunter-gatherers (Sandstrom, 1994, p. 415).

[5] Author Co-citation Analysis (ACA) is a research technique originating from the co-citation studies of Small (1973), Small & Griffith (1974) and developed more fully by White & Griffith (1982) and McCain (1986; 1990a). Details concerning the use of this technique and related issues are highlighted in the methodology section of this dissertation. Sandstrom's (1998) own definition of ACA is that by "depicting the intellectual structure of scholarship, this set of techniques (involving multidimensional scaling, cluster analysis and principle components analysis) was developed to retrieve and analyze cited author data from citation indexes published by the Institute for Scientific Information" (p. 139).

What Sandstrom found was a high connectivity ratio (93%) among the mapped authors, which she interpreted as being “evidence of cohesion and multiple cross-boundary associations among the names selected” (p. 325). In addition to this remarkably high connectivity value, certain other patterns were emphasized. Author nodes scattered about the north end of the vertical axis (style-of-work dimension) were identified from the fields of anthropology and psychology, whereas the author nodes located at the south end of the same axis were identified as biologists and primatologists. The horizontal axis of the map (intellectual dimension), provided further information about the authors’ research interests, which focused on either the “genetic factors/reproductive strategies or the “ecological factors /subsistence strategies” of human behavioural ecology (p. 221).

Based on a cluster analysis of the co-cited author data, Sandstrom identified 9 key author groups associated with the following sub-topics: 1) *evolutionary psychology*, 2) *biological evolutionary theory,* 3) *evolutionary culture theory,* 4) *human behavioural ecology – reproductive strategies*, 5) *primatology /ethology*, 6) *comparative evolutionary biology/archaeology*, 7) *human behavioural ecology – subsistence strategies*, 8) *behavioral ecology*, and 9) *evolutionary ecology* (p. 221). Relative to each topic, the authors were classified yet again according to “three center-periphery zones: the contributors own cluster, other core clusters and omitted clusters” (p. 7).

Of interest to Sandstrom was the style in which the author nodes scattered around the peripheral and core zones of her bibliometric map; therefore, a second qualitative research phase was included to contribute to a more in-depth analysis. Questionnaire and interview data were collected from a purposive sample of five key author informants, all from the field of anthropology. The informants’ initial role was to assist in validating the overall structure of the map, to critique its features and to offer advice pertaining to necessary amendments (e.g., the re-labeling of clusters; adding and deleting the names of particular authors) (p. 204). With a specific line of questioning, Sandstrom then looked more closely at how the anthropologist informants “became aware of (searched) and incorporated (handled) both the works of highly-clustered and peripheral authors, in the hope of explaining the social origins of the *currency* for information seeking and use” (p. 182).

Evidence was found to confirm that the bibliometric artifacts as well as the authors' invisible college "identity" both relate strongly to information foraging behaviour. Sandstrom's (1998) data revealed that the anthropologist authors had "attempt[ed] to maximize benefits and cut costs in pursuing useful information, analogous to the way that human and animal foragers search for and process food resources in unpredictable environments" (p. 7). Specifically, the reports she gathered of her informants' reading habits, browsing habits and other forms of deliberate solitary information seeking were associated with the gathering of "peripheral" resources, or "first time references, previously unfamiliar to citing authors" (p. 8). By contrast, resources attributed to the core zones of the research field were all collected by her informants through alternative habits, mostly through socially mediated activities (e.g., colleague recommendation and graduate training).

Overall, Sandstrom's (1998) understanding of a "center-periphery model" was that the core-scatter bibliometric distributions of authors were descriptive of the "likelihood of encounter with given pairs of authors in a given bibliometric environment" (p. 8). She concluded that the "repeated co-citation of the work of other authors is one mechanism whereby scholars create and maintain boundaries that facilitate the rejection of irrelevant information. Such boundaries constitute invisible colleges" (p. 8).

Tuire and Erno's (2001) study of an educational research community in Finland focused on the invisible college construct, specifying an equal attention to both formal and informal aspects of communication. This study, based solely on a social networking approach, included 104 participants: professors from eight different universities across Finland. Data were collected by questionnaire and citation counts to create three matrices: an *information* matrix (i.e., personal contacts through meetings, calls, e-mail etc in one year), a *collaboration* matrix (co-operational research under the same funding, co-authorship or exchange of papers for comments) and a *citation* matrix (p. 500, original emphasis).

The *collaboration* network among the Finnish education scholars was "very thin" (i.e. a low density value of 0.04) and existed primarily within the university departments as opposed to between them. The *information* network produced a slightly higher density value (0.12), but demonstrated once again a much greater flow of information inside the individual universities. With the structuring of the *citation* matrix, the results clearly differed, showing more cross-boundary relationships between the universities. Only a few central scientists were receiving the majority of citations, while several other scientists were hardly cited at all. The scientists belonging to the center of the citation network were not, however, central to the collaboration network. When Tuire and Erno examined the citation and collaboration networks in unison, they found that educational research could be divided into three invisible colleges: research on learning, research on teaching and the sociology of education.

In sum, most of the research concerning the invisible college suggests that it is a fairly organized, coherent social system for scientists and that a certain degree of predictable behaviour can be found within this system. Invisible colleges mark the presence of loose communication networks, but they also indicate the presence of high levels of communication or organization where a radical conceptual re-organization is taking place within a specialty area (Griffith & Mullins, 1980).

Still, one of the main problems associated with the invisible college construct is that it can be difficult to describe and has already been assigned too many definitions. Clusters of interacting scientists with mutual research interests have previously been characterized as "the hierarchical elite resulting from an expectable inequality, and number about the square root of the total population of people in that area of research front" (Price, 1971, p. 75). Some researchers claim that invisible colleges are just simply "innovation cliques" (Van Rossum, 1973) or "social circles" made up of smaller, fragmented "schools" (Crane, 1972; 1980; Kadushin, 1966). Others believe that an invisible college is a tightly meshed community - "it selects its own society, then shuts the door" (Paisley, 1968, p. 6). A more modern view, expressed by Cronin (1982) is that the invisible college is disadvantaged as an informal communication system because it is at risk of being unstable, short-lived, expensive to maintain and resistant to institutionalization. In a similar vein, Mulkay, Gilbert and Woolgar (1975) believe that invisible colleges are "concentrations of research ties without clear boundaries … amorphous social groupings … in a state of constant flux, partly due to overlapping personnel and to migration" (p. 190).

Despite these varied perspectives, there is some reassurance that the invisible college "is likely to remain a pivotal feature of the scientific communication system for the foreseeable future" (Cronin, 1982. p. 232). According to Cronin, "advances in communications technology, coupled with the likely growth in interdisciplinary research suggest that the management and promotion of invisible colleges throughout science could prove to be an area warranting careful thought and investigation" (p. 232). So far research concerning the use of advanced technology reveals that the "electronic worlds" of invisible college researchers does not "diminish [their] desire to come together to meet" (Brunn & Lear, 1999, p. 299). Participants in Brunn and Lear's survey of the Human Dimensions of Global Change (HDGC) community were frequent users of e-mail, but still acknowledged "the value of face-to-face meetings, formally presenting ideas at conferences, exchanging views with old and new colleagues, taking field trips, and having fun" (p. 299).

2.4 Collaborative work in science

Scientists as *social actors* not only communicate research findings and share information with work team members and members of their invisible college network, "they also co-produce and co-report research results and information to each other – in short they both communicate and collaborate" (Melin & Persson, 1996, p. 363).

Collaborative work is a form of interpersonal behaviour, which draws upon the personal knowledge structure of the individual scientist, but provides that scientist with a unique learning experience situated in a co-operative context. A collaborative project between two or more scientists influences the perception of an information need by minimizing the private intellect of the individual in favor of focusing on the social aspect of the need. Research situations involving collaborative work often result in the publication of a co-authored paper, though not necessarily in all cases.

According to Katz and Martin (1997) “the idea of a collaboration is far from simple” because it “can take many forms” (p. 2). These forms range from the offering of insights and general advice to the active participation of two or more researchers in the production of a specific piece of work. The role of the collaborating partner can vary in terms of making a substantial contribution to a research project or a contribution that is minimal or maybe even negligible (p. 3). Moreover, a collaborative partnership can include the pairing of a teacher and pupil, supervisor and assistant, researcher and consultant, colleague and colleague, or the pairing of research teams from different organizations or nations in either an inter-organizational or international collaboration (Subramanyam, 1983, p 34). Thus to fully understand the social dynamics of collaborative work, Subramanyam suggests that consideration should be given to the complex nature of the human interaction taking place as well as the likelihood of the nature and magnitude of the collaboration changing during the course of a research project (p 2.).

Laudel’s (2001) qualitative-quantitative study asserts that “many variants of research collaboration are not covered by co-authorships” and that the “bibliometric indicator ‘co-authorship’ is systematically biased against some collaborative practices” (p. 369). In order to develop a ‘micro-theory’ of collaboration, Laudel interviewed 101 scientific research group leaders and research group members (including postdoctoral fellows and PhD students) and analyzed the groups’ publication data extracted from the Science Citation Index. The analysis of the qualitative data in particular led to the discovery of 6 main types of collaborative work: 1) *collaboration involving the division of labor* (DOL), 2) *service collaboration* (SER), 3) the *provision of access to research equipment* (ARE), 4) the *transmission of know-how*, and 5) *mutual stimulation* (MUS) and 6), *trusted assessorship* (TRA) (pp. 374-375, original emphasis).

Collaboration involving a division of labor (DOL) was identified by Laudel (2001) in terms of a “shared research goal and a division of creative labor between the collaborators” (p. 374). This type occurred in situations where more than one research group was needed to carry out both theoretical-conceptual and experimental work. The members of the different research groups initiated the collaboration with the joint formulation of a research problem. Distinct tasks were then carried out by individual scientists, but communication was maintained by all workers to ensure that collaboration "spanned all phases of the research" and that "their activities were closely linked" (p. 374).

In a *service collaboration* (SER), the research goal for the collaborative project was typically set by "one of the collaborators alone" and it was this person who performed all the creative work, while other collaborators took on the more "routine" contributions. One example of this collaboration was when the partner responsible for the creative work could not "learn the methods required to solve the problem because the learning process would be too time-consuming or because he or she lacked the necessary knowledge to learn the method" (p. 374).

The weakest type of collaboration, defined as *the provision of access to research equipment* (ARE), was present when at least one collaborator was "willing to provide access to the necessary research equipment in his or her laboratory" (p. 374). While the "provisional" collaborator did not play a role in carrying out any of the routine research work, he or she remained available to give "an introduction to the equipment" and provided "ongoing assistance" in the use of this equipment when necessary. Laudel (2001) notes that "in contrast to DOL and SER the whole research process for this form of collaboration, including the goals and tasks was "concentrated in one research group" (p. 374).

The *transmission of know-how* (TKH), or fourth type of collaboration, was found when one collaborator agreed to transmit "procedural knowledge" (i.e., knowledge about features of a research object or the application of methods etc.) to another researcher. The researcher seeking out the collaboration was typically the one who needed to learn from this particular colleague in order "to efficiently solve problems which suddenly occurred in experimental work" (p. 374).

The fifth type of collaboration, characterized as *mutual stimulation* (MUS), was observed when two or more scientists communicated in a way that stimulated them to "think about unsolved problems in their field, about possible new research projects, about the interpretation of older data etc.” (p. 375). The difference between this type of collaboration and all others was that it was characterized by “a single research process and [did] not contain an exchange of clearly defined contributions” (p. 375).

Finally, Laudel (2001) points to *trusted assessorship* (TRA) as a slightly different form of collaboration; a form not having to do with knowledge production, but rather the process of publishing results. “*Trusted assessorship*” was a term originally used by Mullins (1973) and re-asserted by Laudel because it aptly describes the role of friendly and accepting colleagues who support the publication process by acting as research critics (p. 375).

Although Laudel (2001) defines separately the six different collaboration types, she specifies further that some of them “often accompanied each other.” For example, in some situations where a research group was given *access to research equipment* (ARE) in another laboratory, *service collaboration* (SER) became important if the equipment involved was complicated (p. 375).

Kraut, Galegher and Egido (1988) have shown that most collaborative projects in science can be viewed in terms of a *task-relationship-stage* process. There are least three key stages that collaborators go through from the moment a joint information need is perceived to the presentation of a final knowledge product. Figure 2.1 illustrates specifically the different processes that can occur at the *task level* and *relationship level* of the *initiation*, *execution* and *public presentation stages* of a research project (see Figure 2.1).

According to Kraut et al. (1988), a relationship between collaborating scientists is established when two or more researchers recognize that they are suitable work partners. This suitability tends to involve similar work styles and personalities, and a view that all the collaborating partners are smart enough and responsible enough to do a share of the work. Institutional norms will also have an affect on the establishment of collaborative relationships, since some social systems more than others create opportunities for collaborative work and often even require “particular people to make contact with particular others.” (p. 745). Although it is true that “people with interests in common are often geographically clustered," collaborators from different geographic locations still manage to "get connected" in other ways, such as through conference activities (pp. 745-747).

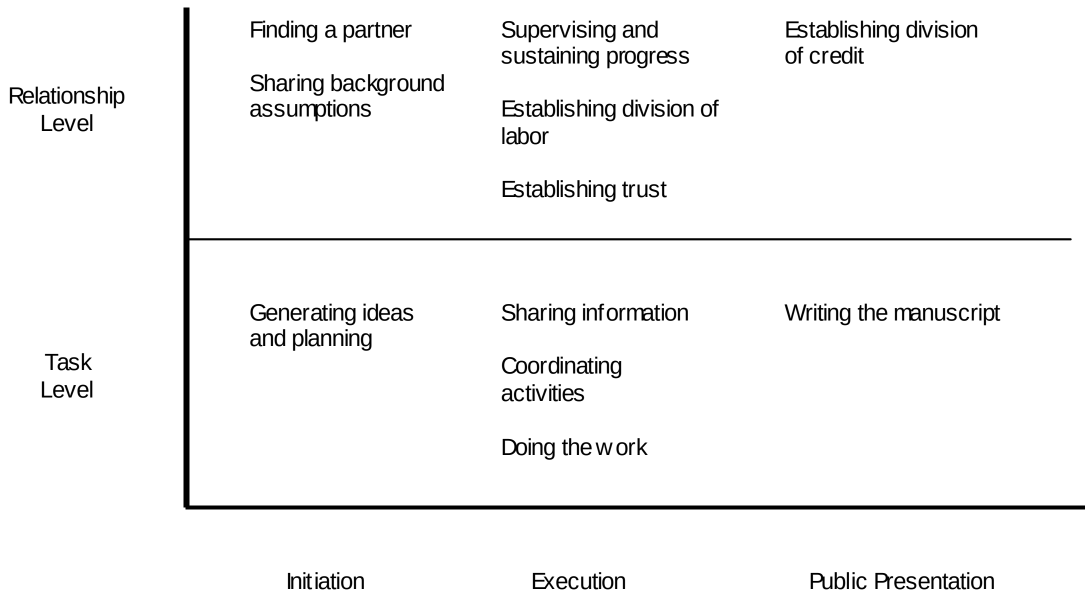

Figure 2.1. Model of research collaboration (Source: Kraut, Galegher, & Egido, 1988, p. 263).

At the *task level* of the *initiation stage*, the blending of relationships and tasks begins. Kraut et al. (1988) observed that collaborators engage in multiple face-to-face discussions over a long period (i.e., weeks and sometimes months) to generate shared ideas. These types of discussions are typically informal and are usually considered to be "the most intellectually exciting and rewarding aspect of collaborative work" (p. 749). The idea generation process may be spontaneous and can include talking, arguing, interrupting and the writing of equations or drawing of sketches. Certain technologies and physical locales support the exchange of ideas, including offices, conference rooms, paper, pencils, and blackboards. When enough ideas are formulated to set a specific research plan, further ideas are discussed concerning how the research will be executed (pp. 740-750).

The *execution stage* of a collaborative project is rooted in the assumption that a general plan about a topic and a work goal has been set by the collaborative partners. Kraut et al. (1988) observed that the collaborators who develop a solid work plan and remain committed to the joint project are usually able to overcome practical barriers, deal with periods of sloth, and maintain a congenial personal relationship when faced with stressful circumstances (p. 751).

At the *task level* of the project's execution phase the primary concerns are activity co-ordination and the sharing of information. According to Kraut et al. (1988), "information that would have remained implicit throughout a solo research project must become explicit so that it can be communicated to a research partner" (p. 752). In order to make the information sharing process easier, collaborating researchers sometimes divide tasks among themselves and/or "encapsulate" pieces of the project for presentation to other group members. Kraut and his colleagues describe encapsulated work in terms of a "complete sub-unit" of work that is comprehensible to all group members but does not contain detailed aspects of the "process by which the sub-unit was produced" (p. 753).

The *relationship level* at the *execution stage* of a project requires collaborators to balance the workload among all researchers and recognize the amount of time and effort put forth by the project contributors (p. 757). A feeling of trust is also important to the collaborative relationship because the members of the work team must rely on each other's good sense, noble motives and potential to influence the outcome of the joint work (p. 758*)*.

At the *public presentation stage*, scientific collaboration typically ends with a documented or published piece of work, but it may also include formal and informal discussions of the work during a seminar or group meeting (Kraut et al., 1988, p. 759). Scientists involved in collaborative relationships (*relationship level*) must decide how to publicize the results of a joint project, divide credit among (or between) themselves for completed work and control how outsiders view the each collaborator's relative contributions (p. 759). Certain professional norms or rules will influence the ordering of authorship on a published document. Some scientific groups adhere to a rule of making "authorship strictly alphabetical," whereas other groups determine ordering of authorship on the basis of who contributed the most original ideas, or who carried out the most significant amount of work (p. 760).

The *task-level* of the *public presentation* stage generally involves an intensive focus on the writing process. Kraut et al. found that most collaborators did not divide the writing task by document section. Their study revealed that, in collaborative teams of two persons, it was more common for one member to "assume responsibility for writing a first draft" while the "second collaborator took on the role of editor and reviewer" (p. 762). Communication during the documentation stage tends to be limited and thus distance was not found to affect the writing process. Certain communication technologies, such as e-mail and word processing software can make long distance editing and revising efficient and inexpensive, and in many cases it was found to improve face-to-face meetings (p. 763). Moreover, collaborators seemed to appreciate having someone else help to improve the writing and help scrutinize the documented work in order to develop their arguments more clearly (p. 762).

Despite recent efforts to characterize the social nature of collaborative work, less attention has been given to the dynamics of human interaction, and more on the measurement of co-authorship patterns in the scientific literature (e.g., Harsanyi, 1993; Kretchmer, 1997; Melin & Person, 1996; Newman, 2000). Persistent is the assumption that "multiple-authorship and collaboration are synonymous" (Katz & Martin, 1997, p. 4). Given this assumption, Katz and Martin argue that "in some instances, not all those named on a paper are responsible for the work and should not share the credit accorded to it" (p. 4).

On the other hand, a test of the research collaboration-co-authorship connection has proven that "there is hardly a tendency for collaboration to be underrepresented when studying co-authorships" (Melin & Persson, 1996, p. 365). In a small-scale study on the subject, Melin and Persson (1995) found that only 5% of authors had experienced situations in which collaboration did not result in co-authored papers. Relative to this finding, the two researchers caution that when using co-authorship data to study collaboration, it should be taken as "as a rough indicator" and that other complementary types of data should be gathered also "to reduce various kinds of uncertainties involved" (p. 365).

The advantage in using bibliometric or co-authorship data to study collaboration is that the data collection process is fairly easy and can be replicated if other researchers have access to the same data set (Katz & Martin, 1997). Unlike other types of data collection methods, bibliometric measures are relatively unobtrusive and can lead to the examination of historical trends (Harsanyi, 1993). Bibliometric measures also tend to be reliable in the sense that the co-authorship connection is "a reasonable definition of scientific acquaintance: most people who have written a paper together will know one another quite well" (Newman, 2000, p. 3). Likewise Edge (1979) confirms that "the most obvious form of mutual influence among scientists is active collaboration leading to a shared authorship of research papers (p. 109). In other words, "co-authorship does reflect an active mutual influence" (p. 121).

In the early 1960s, Price (1963; 1980) observed a growing trend of multi-author papers in the physical sciences literature, with the average number of co-authors increasing rapidly. Based on this observation, he predicted that "by 1980 the single author paper [would] be extinct and that scholarly publications would "move steadily toward an infinity of authors per paper" (1986; p. 79). One year later, Clarke (1964) collected data concerning co-authorship in the biomedical sciences (1934-1969) and proved that the average number of authors per paper, calculated at 2.3, had remained steady throughout a 35-year period. Price's forecast thus seemed dramatic; however, studies concerning co-authorship patterns continued and research now shows that his outlook on the future of collaborative work was not too far from the truth. The number of co-authored papers in both the sciences and social sciences has in fact increased steadily over time (e.g., Beaver & Rosen, 1979a; Bird, 1997; Cronin, 2001; Endersby, 1996; Grossman & Ion, 1995; Gupta, 1993; Pao, 1992; Wagner-Dopler, 2001). In the field of mathematics, for instance, Grossman and Ion (1995) reported that "over 90% of all the papers published during the 1940s were the work of just one mathematician." By the year 1995, "scarcely more than half of them [were found to be] solo works" (p. 129).

Some of the factors associated with the general increase in multi-authored publications include: the early movement toward the "professionalization" of science (Beaver & Rosen, 1978; 1979b); research specialization and technical sophistication, and the need for scientists' to pool their knowledge (Cronin, 2001); the proliferation of scholarly meetings, electronic communication, and the 'publish or perish' pressure on faculty (Grossman & Ion, 1995); and scientists' need to increase their visibility, gain recognition and establish higher measures of formal productivity (Beaver & Rosen, 1979a; Pao, 1992).

Beaver and Rosen's (1978) essay concerning the professional origins of co-authorship relates the term "professionalization" to "the dynamic organizational process which led to a revolutionary re-structuring of what had been a loose group of amateur and full-time scientists into a scientific community." They note that "'professionalization' redefined how science was done, who did it, where it was done, who paid for it, what its practitioners wanted, and how they became scientists" (p. 66). Moreover, 'it continues to affect structural changes within the scientific community as well as the community's relationship with the outside society." (p. 66). The authors explain also that:

> professionalization can best be viewed as a process which organizes a group of individuals along a set of attributes – attributes which are both inclusive and exclusive. That is, professionalization defines the rules, rights, and rites of access to the group, what holds the members of the group together, and what sets them apart from other individuals in the larger society. Furthermore, professionalization structures the obligations and benefits of the group's members, while defining their relationships with outsiders (pp. 66-67).

The important relationship that professional scientists have with the outside society is the "ability to lay claim to support and society's ability to provide it." A continued reliance on the outside society thus "implies a need to justify such support" (Beaver, & Rosen, 1978, p. 67). Scientists are expected to convince outside community members of the benefits of their work and then fulfill a promise to deliver those benefits. Consequently, within the scientific community there is also a need for representatives – scientists who have reached the status of "acknowledged leaders" – to act as liaisons to the larger society (p. 67).

Beaver and Rosen (1978) point to the establishment of the National Academy of Sciences (1863, U.S.A.) as an example of how "the relationship between a professionalizing scientific community and the outside society may, with increasing social support, lead to increased centralization of authority within the scientific community" (p. 67). Recognition or the acknowledgement that one was a "winner" was essential to one's status within the U.S. National Academy. Member scientists during this period were in need of some type of mechanism for achieving and sustaining higher levels of recognition, and that mechanism, according to Beaver and Rosen, was collaboration. Collaboration "provid[ed] a means of demonstrating one's ability to those already in a position to 'recognize' others as well as keeping up one's output from such a position" (p. 69). Throughout the seventeenth and eighteenth centuries the "logical place to look for [origins of collaboration] was in the new organs of communication created in the scientific revolution: scientific journals and their contents, that is, scientific papers" (p. 72). Today, scientists still look to professional recognition as a reason for participating in collaborative work, but because collaborative projects require a lot of time, recognition is not the *only* motivating factor. Two additional factors, namely disciplinary similarity and spatial proximity (factors of extrinsic value to the collaborative process) have also been known to play a significant role.

Qin, Lancaster and Allen (1997) recently determined that "most collaboration occurs among scientists from the same department or discipline" and that "vocabulary barriers grow larger" as scientists from different disciplines try to work together. They found that within-disciplinary collaboration is generally favorable because it "minimizes the difficulties brought about by technical jargons and hence increases the chances of success in communication" (p. 912).

Kretchmer's (1997) study of an invisible college network of physicists confirmed that "birds of a feather flock together" – the co-authorship patterns that she observed were found to be similar to those patterns found in non-scientific populations (p. 590). Kretchmer's (1997) understanding of the "birds of a feather" phenomenon stems from a general theory that "persons are guided more or less by a deliberate search for persons and this search is mainly controlled by the similarity of characteristics" - *a socio-psychological approach to collaboration*. Moreover, "the type and degree of similarity of a relation is substantially dependent upon the context into which this relation is embedded:" hence, "characteristics of social relations [arise] preferably from occasional patterns of social contacts" - *a socio-structural approach to collaboration* (p. 581, emphasis added).

In comparison, other research has shown that disciplinary similarity and geographic proximity are not always essential to the development of a collaborative research project. Collaborative work does occur from time to time between scientists from different research disciplines (e.g., Crow, Levine & Nager, 1992; Heffner, 1981; Qin, Lancaster & Allen, 1997), as does intra-national and international scientific collaboration (e.g., De Lange & Glanzel, 1997; Katz, 1994; Glanzel & De Lange1997; Glanzel, 1995; Glanzel, Shubert & Czerwon, 1999; Glanzel, 2001; Luukonen, Tussen, Persson & Sivertsen, 1993).

One of the most comprehensive studies concerning interdisciplinary collaboration has shown that "the levels and types of interdisciplinarity," specifically *scientist-scientist interaction, scientist-information interaction, information-information interaction*, vary "with a general trend toward the highest measures of interaction in biology and the medical sciences (Qin et. al., 1997, original emphasis).[6] With respect to international collaboration, Glanzel and Schubert (2001) explain that it:

> tends to be reflective of individual mobility or the interests of individual scientists as well as the economic and/or political dependence of a country or geopolitical region, for instance, in different forms and degrees of neo-colonial ties; but international collaboration can also be conditioned by large or special equipment such as CERN in Switzerland, the observatories in Spain or Chile, which are often shared in large multinational projects, or is conditioned by biological factors, for instance, if a new virus has to be studied in the environment where it first occurred (p. 200).

[6] A *scientist-scientist interaction*, according to Qin et al. (1997) places emphasis on the personal interactions that occur between two or more interdisciplinary scientists. A *scientist-information interaction* can be measured in terms of "the total amount of information used or absorbed in a research project and the discipline incorporated in a research project" (p. 913). Since *information-information interactions* tend to be "subtle and intangible," the researchers specify that this form of interdisciplinary collaboration may be measured by looking at how many different disciplines (i.e., as measured by the journal cited) have been integrated into a new source of information or knowledge (p. 913).

In the absence of special economic and political interests or biological factors, Katz (1994) discovered, through an investigation of intra-national university-university collaboration among scientists in Canada, Australia and the United Kingdom, that research cooperation decreases exponentially with the distance separating the collaborative partners. This means that geographical proximity can influence collaboration choices and there is evidence to suggest that situations involving informal, face-to-face contact may be an essential ingredient (p. 40).

In fact, opportunities for face-to-face contact are important. Among scientists at the same university, research data has shown that pairs co-located in the same department were more likely to work together (52% of 294 pairs) than those in different departments (7% of the 3, 984 pairs) (Kraut, Egido & Galegher, 1990, p. 158). Kraut et al. note that there is the possibility that researchers whose offices are close together are more likely to share common research interests (i.e., subspecialties within a department often allocate offices on the same floor or corridor). By holding constant organizational proximity and research similarity, the authors were able to prove that proximity or collocation from a departmental perspective does have an independent effect on research collaboration (p. 158).

As a whole, past research has been successful at examining the reasons for and multiple factors that contribute to the co-production and co-reporting of scientific knowledge. Studies concerning collaborative work are likely to continue given the new opportunities that scientists now have to maintain contact with each other regularly online through e-mail and other advanced communications technology. This study does not focus on the impact of such technology, but it is still important to acknowledge the power of the Internet today and its ability to reduce communication constraints associated with time and distance: "the Internet should profoundly affect scholarly relations because the scholarly life is rarely silent, lonely, or contemplative .... Scholars have a message to get out to the world, or at least to their corner of it." (Koku, Nazer & Wellman, 2001, p. 1753).

2.5 A new invisible college study

The purpose of this study is to examine a new case of an invisible college and gain more insight into the structural and social aspects of the 'invisible college' phenomenon in science. It is designed to analyze the interrelationship between the intellectual identities of scientists, their co-authorship patterns and general collaboration habits, and their collegial connections to one another as international conference participants. A new conceptual model has been proposed to fill the gap identified in previous research. Certainly the invisible college has been defined or discussed extensively as a construct (e.g., Chubin, 1985; Crane, 1972; Cronin, 1982; Lievrouw, 1990; Price, 1963), but there has never been a study prior to this based on a specific framework for systematically analyzing its multidimensional components. The components that I view as being most important to the invisible college are the following: the subject specialty, the social actors and the "Information Use Environment" or IUE.

Since a myriad of difficult-to-trace activities can take place within an invisible college network (i.e., information sharing; collaborative work), a wide range of research techniques will be used to elucidate both its structural and social components. The main techniques are author co-citation analysis, social network analysis, one-to-one interviews, participant observation and the content analysis of public documents.

Author co-citation analysis has been used by a number of scholars to map the intellectual structure of subject specialties in the sciences, social sciences and humanities (e.g., Bayer, Smart & McLaughlin, 1990; McCain, 1983; McCain, 1990b; Perry & Rice, 1998; Sandstrom, 1998; White, 1983; White & Griffith, 1981b; White & McCain, 1998). My research is the first to map co-cited authors from a subject specialty in mathematics known as Singularity Theory.

Singularity Theory is the chosen specialty area because it has experienced a significant period of development throughout the past 60 years. Trotman (1999) details the history of this specialty in *Le Dictionnaire d'Histoire et Philosophie des Sciences*. Mathematicians first began to study curves with singularities at the School of Alexandria in ancient Greece; however, Singularity Theory as it is known today is largely based on the major contributions of Milnor, Arnold, Hironaka, Thom and Lojasiewicz during the 1960s and 1970s. The classification of Singularities was only just completed as recently as 1979 (Isaac Newton Institute Annual Report, 2001, p. 24). Also, Singularities research has emerged recently as a distinct class code within the 1991 and 2000 American Mathematical Society Classification system. For example: *Singularities (*32Sxx; 1991- now*)* and the *Theory of singularities and catastrophe theory* (58Kxx, 2000-now). Due to its presence within the AMS Classification we know that it is a mature specialty, but it is still obviously young in comparison to the main cognitive underpinnings of mathematics (e.g., 14-XX – 1940-now – ALGEBRAIC GEOMETRY) (shown in Appendix E). With respect to the invisible college phenomenon however, Singularity Theory is a more appropriate subject for analysis. The broad cognitive areas are simply too large in terms of the numbers of mathematicians involved and too ancient to be examined from a socio-cogntive perspective. As a relatively small, but mature subject then, Singularity Theory possesses certain characteristics that suggest it has been functioning as an invisible college. For instance, a European Singularities network site is now posted on the World Wide Web and at this site the names of mathematicians worldwide (approximately 100) working in this specialty area are listed. Links to preprints are posted and available for downloading, and there is also a list of past and future Singularity Theory conferences, which are important events for the mathematicians to meet personally and share new research information (European Singularities Network, 2000).

With Singularity Theory as the chosen subject, this study develops a unique overlapping approach to analyzing bibliometric and social network data to determine how the intellectual identities of the mathematicians (as measured by citations) reflect upon or is reflective of their social relationships as co-authors and conference participants. The different measures, when analyzed together, can provide different types and levels of information concerning communication within the invisible college and reinforce the value of using Author Co-citation (ACA) and social network maps jointly as navigational tools when exploring underlying “informal” work processes.

This study also advances citation analysis as a research method by demonstrating how co-cited author statistics and co-authorship statistics may be used to evaluate specialized research programs designed to foster international collaboration and the diffusion of innovation. Specialized institute environments designed for mathematics research have become important workspaces for mathematicians since the latter part of the 20th century, yet their role in fostering invisible college activity has not previously been examined.

Griffith (1990) suggests that, if the goal of studying a science is to learn more about human behaviour, communication must be examined as a social process because "communication is the only general scientific behaviour" (p. 31). On the other hand, a communication system is not complete without its bibliographic representations, since these "are its principal artifacts" and preserve the cognitive aspects of scientific achievement. In keeping with Griffith's perspective, the significance of this study is that it gives equal attention to both the bibliometric representations of mathematics and the social process of communication in mathematics. This equal representation is what White (1990) refers to as the "aerial" view (cognitive map) and "ground level" view of the subject specialty or invisible college.

CHAPTER 3: RESEARCH METHODS

This case study of Singularity Theory research in mathematics is based on a quantitative-qualitative mixed-methods research design. Citation data, social network data and ethnographic data collected during fieldwork at the Newton Institute are analyzed to produce a complementary or "triangulated" plan of analysis whereby "overlapping and different facets" of the invisible college "may emerge like 'peeling the layers of an onion'" (Creswell, 1994, p. 175). The assumptions, practices and issues associated with *citation analysis, social network analysis* and *ethnography of communication* are outlined in the following sections, including a summary of why they are used together in the same study. Although each approach to data collection and analysis is distinct, their use is based on the idea that any weaknesses associated with one method may be compensated by the strengths of another method (Jick, 1979). And finally, this study is also not without certain delimitations and limitations and these are noted to clarify the scope of the research and potential research-related shortcomings.

3.1 Citation analysis

A citation is a bibliometric artifact that reflects to a certain degree past information use and the influence that a cited document has on a given research field. According to Edge (1979), citations may be viewed as "visible traces of past communication; the 'paying of intellectual debts.' Patterns developed in citations are therefore interpreted as patterns of communication; groups of inter-citing authors are held to influence each other within an 'active research network'" (p. 103).

Citation analysis in Library and Information Science (LIS) can be traced back to the work of Small (e.g., Small, 1973; 1981; 1986; 1993; 1999; Small & Crane, 1979; Small & Greenlee, 1980; Small & Griffith, 1974; Small & Koenig, 1977), Garfield (e.g., Garfield, 1972; 1975; 1977; 1979a,b; 1992; 1993; 1997a,b; 1998; Garfield & Welljams-Dorof, 1992) and Griffith (e.g., Griffith, 1979;

1988; Griffith, Drott & Small, 1977; Griffith & Mullins, 1980; Griffith, Servi, Anker & Drott, 1979b; White & Griffith, 1982). Small (1973) introduced the technique of document co-citation analysis and used this technique with Griffith (Small & Griffith, 1974) to demonstrate how it can be used to map the structure of scientific specialties. Garfield (1977; 1998a,b) the founder of the Institute for Scientific Information (ISI), created *Current Contents*® and the *Science Citation Index* (*SCI*) and is also credited for advancing "citationology" - the theory and practice of citation analysis. White and Griffith (1981a,b; 1982) enhanced the practice of co-citation analysis by using co-cited authors to create visual maps of prominent authors in selected subject areas of scholarship. Co-citation analysis has subsequently been used by other researchers to map research specialties (Bayer, Smart & McLaughlin, 1990; Ding, Chowdhury & Foo, 1999; Karki, 1996; McCain, 1984; 1985; 1986a,b; 1990a,b; Perry & Rice, 1998; White & McCain, 1998). Moreover, White's (1986) and McCain's (1990a) articles concentrate on the technical aspects of author co-citation analysis, specifically the retrieval of co-cited author data and the statistical procedures used to map co-cited authors in "intellectual space."

Citation analysis in general focuses on the relationship between cited and citing documents. The units of analyses that mark a relationship may be cited articles, books, journals, authors, research organizations or universities. Two of the most common techniques of citation analysis are *bibliometric coupling* and *co-citation analysis*. *Bibliometric coupling* identifies a link between documents that cite the same earlier documents. In *co-citation analysis*, documents or authors are said to be co-cited when they jointly appear in the citation lists of one or more subsequently published documents (Small & Griffith, 1974; Smith, 1981). An author co-citation analysis generates co-citation pairs of author *oeuvres* (i.e., sets of documents by authors, rather than single articles or books) to determine authors' perceived similarity within a research field (White, 1990). The association made between documents in *bibliometric coupling* is intrinsic, whereas *co-citation analysis* examines links that are extrinsic – the links between authors or documents are dynamic and valid only as long as they are co-cited (Garfield, Malin & Small, 1978).

The main assumption behind citation analysis is that the citation is a measure of bibliographic influence or impact; therefore citation counts can be used to determine the most important authors, publications, or research organizations. Researchers have questioned this assumption, including the notion that all citations are essential; that the citing of a document implies its use by the citing author, and that the cited document is related in content to the citing document (Chubin, 1980; Edge, 1977; 1979; Folly Hajtman, Nagy & Ruff, 1981; Kostoff, 1998; MacRoberts & MacRoberts, 1989; Pierce, 1990; Prabha, 1985; Smith, 1981).

Edge (1977; 1979), a self-proclaimed "non co-citationist" specifically argued that citation analysis is limited because it deals only with formal levels of scientific communication and omits measures of influence or impact related to informal variants of communication. Yet in terms of formal "intellectual" influences in research, there are known instances where an important publication has not been included in a citation list. Cole and Cole (1972) note that sometimes "a crucial intellectual forebear to a paper is not cited" even though "the omission is rarely due to direct malice on the part of the author" (p. 370). On the whole, Garfield (1980) claims that "the vast majority of papers do properly cite the earlier literature" (p. 217).

The problem of excessive self-citing among authors has been explored (Tagliacozzo, 1977), as well as the issue of all citations not being equal: some tend to be negative and some are positive (Chubin & Moitra, 1975; Moravcsik & Murugesan, 1975). Garfield and Welljams-Dorof (1992b) state that citations in the hard sciences

> generally tend to be positive, representing the formal acknowledgement of prior sources that contributed to the citing author's research. Of course, there are occasional exceptions, such as the cold fusion controversy, but these are well known and obvious. In the social sciences critical citations are more common. Thus, raw citation counts may not be indicative of an author's paper or paper's positive impact in the social sciences, and the *context* and *content* should be examined (p. 325).

Lindsey (1980; 1982), Long and McGinnis (1982), Harsanyi (1993) and Persson (2001) have also considered the problem of first author citations studies, which fail to allocate credit to secondary authors of multi-authored papers. Persson (2001) recently tested the difference between using first author citation counts verses all author citation counts in an author co-citation analysis of Information Science (1986-1996). A new search strategy was developed for this study, and one obvious difference in the resulting maps was the inclusion/exclusion of author names.[7] Certain names from the first-author map "had to leave" and others were added to the second all-author map because they had gained more citation counts (p. 343). Some of the newly included authors to the all-author map went directly from zero to a substantial citation count. With respect to the overall structure of the information science field, Persson admits, however, that there was no "dramatic change." The sub-field clusters in both maps were quite similar: "bibliometrics at the bottom and information retrieval and searching at the top" (p. 343).

Case and Higgins' (2000) recent review of citation behaviour draws attention to one of the most important topics in "citationology:" the reasons and motivations of authors for citing other authors. To address this topic, past research has focused on the content and context of citations, observations of citing behaviour and anecdotal evidence provided by citing authors (e.g., Chubin & Moitra, 1975; Garfield, 1977; Lipetz, 1965; Rice & Crawford, 1993). Merton's (1957) initial observation was that authors cite other authors as a form of social recognition, or to show acknowledgement of new ideas. Citer self-interest has also been considered a motivating factor. For example, Ziman (1968) and Leopold (1973) both indicate that authors cite the works of recognized authority figures merely to persuade their audience of the importance of their research and new claim of knowledge. Garfield's (1977) anecdotal study produced a list of 15 reasons for citing, with the top three listed as: 1) to pay homage to pioneers; 2) to give credit for related work; and 3) to identify methods and equipment. Another survey developed by Brooks (1985) questioned authors' motivations for citing and found that the "persuasiveness" factor was the most important reason, even though the list of reasons seemed to vary among authors from different disciplines. Shadish, Tolliver,

[7] Persson (2001) built a search key based on a list of source items from the *Social Sciences Citation Index* CDE editions 1986-1996 (e.g., AU – Leggate P; Dyer H) that included "the following subparts: first author's last name, publication year, volume number and starting page" (p. 340).

Gray and Gupta's (1995) survey of citer motivation produced responses that were once again varied, but the general conclusion was that highly-cited articles were those authors thought of as exemplary cases of research in their field. Case and Higgins (2000) most recent survey in the field of communications, indicates that authors cite most often because: 1) the work was novel, well-known and a concept-marker; 2) citing the work might promote the authority of one's own work, and 3) the work deserved criticism (p. 643). Although the content-context analyses of citations can be valuable, Case and Higgins (2000) are not convinced that citation typologies are adequate enough to support a theory of citer motivation. They recognize that "investigating the motivations for citations does indeed pose epistemological and methodological problems:" therefore, "if the motivations of authors are to be understood, then asking authors directly about their motivations … is a logical place to approach the issue of citer motivation" (p. 636).

Despite some of the criticism directed towards citation studies, research has produced evidence to suggest that citation analysis is a valid and reliable research method (Barlup, 1969; Virgo, 1977; Garfield; 1997a,b; White; 1990). When Barlup (1969) asked authors in the field of medicine to assess the degree of relatedness of citations to their work, he found that 72% perceived the citations to be definitely related and only 5% were judged as not related. Virgo (1977) tested the hypothesis that more important papers, on average, tend to be cited more frequently in the published literature of science and discovered that citation frequency alone is a good indicator of the perceived importance of a scientific journal article. Garfield (1979a; 1997a,b) and White (1990) have also been instrumental in dispelling the myths associated with citation analysis. Both scholars argue that it is an objective, unobtrusive method for visualizing the intellectual structure of a research field, but acknowledge that there are some limitations and caution researchers to apply the method with care.

To advance the practice of citation analysis, Cronin (1994) indicates that a more flexible theory is needed – one that focuses on the nature of "tiered citations" (p. 537). With this kind of theory, the perceived lack of similarity between pairs of cited and citing documents does not have to be a problem if researchers consider "the notion that there are levels or gradations of citation" (p.

538). Pichappan (1996) agrees with Cronin and introduces a new classification scheme based on the strength of cited paper – citing paper relevancy. According to this new scheme, *umbral-umbral* papers, classified as type one are high in relevance. The type two relationship, labeled *penumbral-umbral*, presupposes "that part of the cited paper forms the body of the discussion … of the cited paper." With the *umbral-penumbral* pair, or type three relationship, there is a partial link. And finally the type four pairing of *penumbral-penumbral* papers are said to be low in relevance (p. 651).

Smith's (1981) review recalls the time when citations had to be gathered from lists of references in journal articles and manipulated by hand. Throughout the past three decades the development of computerized indexes has improved the research situation and broadened the scope of analysis. Today, the information scientist can retrieve raw citation data from highly complex and comprehensive electronic databases containing millions of bibliographic references. The Institute for Scientific Information (ISI) maintains several databases that are useful for gathering citation data, including *the Arts & Humanities Citation Index; Biosciences Citation Index; ChemSciences Citation Index*; *Index to Scientific and Technical Proceedings*; and *Science Citation Index* (available at: http://www.isinet.com/ products/products.html). An expanded version of *SciSearch* (1974-present), also produced by the Institute for Scientific Information (ISI), is available online through the Dialog™ Corporation. *SciSearch* is the multidisciplinary index to the literature of science, technology, biomedicine, and related disciplines, and includes all significant items from major scientific and technical journals, for example, articles, reviews, meeting abstracts (available at: http://library.Dialog.com/bluesheets/html/b10034.html).

3.2 Social network analysis

Social network analysis involves the study of *social environments, social structure and social actors*. Wasserman and Faust (1994) state that the *social environment* "can be expressed as patterns or regularities in relationships among interacting units." *Social structure* is defined as "the presence of regular patterns in relationship” between interacting units (pp. 3-4). Although a frequently studied unit of interaction is the *social actor*, other interacting units may be utilized as structural variables in a social network analysis. Haythornthwaite (1996) indicates that the network analyst can study the interactivity of “organization charts, family trees, archival records or publications ” (p. 424).

Moreno (1934), Bavelas (1948), Newcombe (1953) and Cartwright and Harary (1956) were among the first researchers to carry out social network studies. Some of the key terms used in social network analysis, such as *sociometry* and *sociogram* originated from Moreno’s early research. Bavelas, Cartwright and Harary (a mathematician) pioneered the application of graph theory to group behaviour (cited in Scott, 1992, p. 12). Freeman (1983; 2000) advanced the graphing techniques used in the visualization of social networks, while other network analysts focused on developing software tools for analyzing and displaying network data (e.g., Borgatti, Everett & Freeman, 1992; Krackhardt, Blythe & McGrath, 1995). As social network analysis evolved into a refined research approach, various books and articles have been produced to outline its principal data collection and analysis techniques, and promote a range of applications (e.g., Freeman, White & Romney, 1989; Haythornthwaite, 1996; Scott, 1992; Wasserman & Faust, 1994; Wellman & Berkowitz 1997).

The underlying assumption of social network analysis is that social interaction is both recognizable and relevant to many topics in the social and behavioural sciences. For example, socio-structural issues associated with obtaining and keeping a job, job satisfaction and getting ahead in employment situations have been the focus of many social network analysts (e.g., Burt, 1992; Granovetter, 1974; Kilduff & Krackhardt, 1994; Lai, Lin & Leung, 1998; Wegener, 1991). Wellman and Frank (2001) examined sources of social capital in personal community networks and Moore (1990) carried out a social network analysis of the unique personal networks of men and women to understand their structural determinants. In recent years, the rapid advancement of computer-mediated communication has prompted an interest the use of social network analysis to study the structure of online communities or “computer-supported social networks” (e.g., Rice, 1994; Garton, Haythornthwaite & Wellman, 1997). Much of the research activity associated with social network analysis confirms that it is a distinct research approach based on a solid foundation of theories, techniques and applications. Today, the International Network for Social Network Analysis, established by Barry Wellman in 1976, continues to advance the multi-disciplinary features of social network analysis and facilitate the research interests of a dynamic group of scholars.

To understand the structure of a social network, it is important to examine patterns of relationships and relationship ties. The first detail or “unit” of analysis is the content of a relationship, or the type of resources (e.g., information) shared or that have been shared, exchanged or delivered within the studied relationship. Another unit of analysis is the direction of the relationship. Some relationships involve the direct flow of resources from one person to another (e.g., a mentor offering advice to a student), while others are distinctly non-directional (e.g., scholars belonging to the same research association). The strength of a relationship is usually measured in terms of its presence or absence and the quality or frequency of interaction between the relating units. Tie strength extends the meaning of strength between a pair of interacting units to the strength of weakness of their tie relevant to all other relationships in an entire group of interacting units (Haythornthwaite, 1996, pp. 326-327).

Relationships in a social network can occur as a *dyad*, a *triad*, a *subgroup* or *group*. A *dyad* is the relationship that establishes a tie between two actors. A *triad* is a subset of three actors and the possible tie or ties between two of the actors or among all three of the participating actors. A *subgroup* focuses on the subset of actors from a larger group and all ties among them. The most important relationship model in a social network analysis is the *group*. In one group there is a finite set of actors that belong together because of a denoted conceptual, theoretical or empirical boundary. A group is examined in terms of the system of ties established among all its members (Wasserman & Faust, 1994, pp. 18-19).

Social networks can occur in the form of *egocentric networks*, or *whole networks*. The *egocentric network* focuses on one individual actor, or "ego" and the ties that (s)he maintains with others from a selected network. For example, research concerning the issue of "social support" has focused on the egocentric network to gain insight into how personal relationships aid the health or well being, occupational success, or information needs of an individual (e.g., Wellman & Wortley, 1990). By contrast, the *whole network* is a term used to describe the ties that all members of an environment maintain with each other in that same environment. With the *whole network* perspective, the researcher can examine the interactions of all members of a group or community who engage in similar activities and to focus on the key players in resource provision (Haythornthwaite, 1996, pp. 328-329).

Wasserman and Faust (1994) also describe the difference between the *one-mode network* and the *two-mode network*. A network "mode" is defined as "the number of sets of entities on which structural variables are measured" (p. 35). With the *one-mode network* only a single set of actors is studied and viewed as representing a specific connection to one another. The set of actors can vary in type in that the set may be people, subgroups, organizations or "collectives" in the sense of a community, state or nation. A *two-mode network* differs from the one-mode network in that it examines the tie(s) between one set of actors and another second set of actors, or it may focus on one set of actors and one set of events (p. 39). The second explanation of a *two-mode network* is often referred to as an affiliation network because it arises when "one set of actors is measured with respect to attendance at or affiliation with, a set of events or activities" (p. 40).

The relational properties of a network and positional properties of its interconnected actors are examined in connection with the following five principles: *cohesion*, *structural equivalence*, *prominence*, *range*, and *brokerage*. A group's level of *cohesion* is represented by the strength of the socializing relationships among the networking members. Specific measures of *cohesion*, namely density and centralization, determine the extent to which all members interact with all other members in the same group (Haythornthwaite, 1996, p. 332). On a network map or *sociogram*, a cohesive or highly interconnected subgroup can appear as a "cluster" or "clique" (p. 332).

Structural equivalence is the term used to identify actors in a network who share a similar role (Haythornthwaite, 1996, p. 334). Two actors, professors for example, may fill the same role with respect to members of the same network in that they are mentors to the same, or to different, sets of graduate students.

The power or degree of influence that an actor in a network has on other actors is based on a measure of *prominence*. Often a social actor's *prominence* is assessed in terms of his or her degree of centrality in a network. On a sociometric map, a node representing the actor and many lines emanating from the node connecting to other actor nodes illustrates centrality. The central actor is termed the network star. On the other hand, the map may also include at least one isolate actor who has no lines connecting to other actors in the network (Haythornthwaite, 1996, p. 334).

Range is an additional concept referring to the collection of sources that an actor has access to in a social network. One actor's *range* is determined by the size of his or her network. Generally if the actor's immediate network is large in size, or there is a bridging tie to another network, (s)he is said to have access to a wide range of social resources. The principle idea is that "the more networks an actor is connected to, the more [resources] the actor will have access to and the more [these resources] will be of different kinds" (Haythornthwaite, 1996, p. 335).

The term *brokerage* denotes a measure of "betweenness," or the extent to which an actor assumes a central position in a network as an intermediary. The actor with a brokerage role is not usually directly connected to many actors in the network; however, he or she retains control of the resource sharing as a kind of "gatekeeper" (Haythornthwaite, 1996, p. 335).

A variety of different data gathering techniques are associated with social network analysis. Wasserman and Faust (1994) specify the use of *questionnaires*, *interviews*, *observations*, *archival records*, and *experiments*. If the social actors selected for a study are people, then the *questionnaire* can be a valuable tool for gathering information about who informants like or respect, who they go to for advice, or who they seek out for information in a social network. Also, *interviews*, whether they are face to face, or by telephone may be used to gather the same type of information where it is not feasible to utilize a questionnaire. *Observation* is often part of a social network analysis as it allows the researcher to focus in on small groups of people (i.e., dyads, triads or subgroups) who are engaged in face-to-face interaction. The benefit of observation is that it can be used with people who cannot respond to questionnaires or interviews. *Archival records* are useful for the analysis of past interactions (e.g., newspaper reports, court records, published citations of one scholar by another). *Experimental data* is obtained when the researcher opts to record and analyze interactions among social actors in a controlled situation (e.g., a group problem-solving experiment).

3.3 Ethnography of communication

Ethnography of communication has rarely been specified in the Library and Information Science (LIS) literature; however, there is evidence to suggest that ethnographic research in general is becoming more important to the field. Julien's (1996) analysis of the information needs and uses literature (1990 to 1994) indicates that 4 percent of past studies have used ethnography to investigate information-seeking behaviour. Specific research examples include Chatman's (1990; 1992) investigations into the information-seeking behaviour of janitors and the information world of retired women. Rice-Lively (1994) also used ethnography to study the actions and events of an online learning community. Shultz' (1996) ethnographic research focused on the use of medical jargon between health professionals in a hospital setting and more recently, Solomon (1997a,b,c) named ethnography of communication in a study of the routine patterns of communication and information sense-making of employees in a work planning unit of a public agency.

Ethnography of communication, as a specific form of ethnography, focuses on *communicative situations*, *events* and *acts* and their underlying social meanings (Saville-Troike, 1989). Originally this research approach was rooted in linguistic anthropology, where Dell Hymes (1962; 1972) was the first to develop and use the "ethnography of speaking" to study the interaction between language and social setting. Often in this research tradition attention is directed to "ways of speaking:" however, Saville-Troike clarifies that specific references to the "ethnography of speaking" can cause terminological confusion when in fact the method “usually includes a much broader range of communicative behavior than merely speech” (p. 23).

The product of ethnography of communication is typically a thick description of ways in which people use speech and other communication channels to share resources or information within their “culture” or “information culture.” Mellon (1990) notes that “culture” in general may be defined in terms of “shared knowledge: what people in a society learn that allows them to behave, and to interpret other people’s behaviour, appropriately” (p. 7). The term “information culture” therefore refers to the shared knowledge structure and commonsense understanding of a group of people, who determine appropriate information-seeking and dissemination activities relative to their cultural setting.

In ethnography of communication, a *communicative situation* provides the context within which communication occurs (Saville-Troike, 1989). The detailed types of activities that occur within a communicative situation may be diverse, but the general ecological configuration of activities usually remains consistent. A *communicative event* is the basic unit examined by the ethnographer for descriptive purposes. The event may be defined according to the purpose of the communication, the general topic and the participants. Of additional importance is the setting of the event (e.g., a lecture hall), the tone of the event (e.g., serious and scholarly) and rules for interaction (e.g., the speaker is the only person who talks, until the lecture is finished and there is a question and answer period). An event terminates when there is a shift in the focus of attention or if there is a change in the number of participants or in the participant’s relations with one another. The boundary of a communicative event may be recognized when there is a conspicuous period of silence or change of position in the body/bodies of the participant(s). Periods of discontinuity, such as short interruptions to a conversation may occur at some points of a communicative event, but the ethnographer can continue to examine the event if it resumes without a change in major components. A *communicative act* is based on one primary function of interaction (i.e., a referential statement, a command or a request) and may be verbal or non-verbal. A verbal request for information can take various forms (e.g., *Do* *you know where I can find the Canadian Mathematical Society directory? Where is the Canadian Mathematical Society directory? Can you help me find the Canadian Mathematical Society directory?*). In events categorized by non-verbal expression, the ethnographer might observe the raising of eyebrows, a "questioning" facial expression or a telling hand gesture. During communicative events silence is even considered to be a communicative act if it is intentional.

The principal concerns of ethnography of communication are the *nature of the speech community*, *patterns and functions of communication*, the *social structure of the speech community*, the *categories of talk* and *routines or rituals of speech* (Saville-Troike, 1989). In the scholarly culture of mathematics, for example, the *nature of a speech community* is reflected in the shared language form that sets the boundary of a specialized area of research. To examine mathematicians' *patterns and functions of communication*, it is necessary to focus on the expressive function and directive function of a communicative act. Communicative functions are related to the mathematicians' research purposes and information needs. Each member of the research community will have a *social identity* (e.g. professor; graduate student; postdoctoral fellow); therefore, it is also necessary to examine how social identities define and constrain interpersonal interaction and the flow of information. The *categories of talk* specific to the group under investigation may include a "brain-storming" session, a one-to-one conversation, or a formal lecture. Shared knowledge is the factor that influences the development routines or rituals of speech. The mathematicians' in one specialty area may have a distinct way of talking about mathematics; a manner that other researchers involved in the subject can understand and take for granted (Saville-Troike, 1989, pp. 2-48).

Saville-Troike (1989) believes that the contribution of ethnography of communication "to the description and understanding of culturally constituted patterns of communication will be limited if its methods and findings are not integrated with other descriptive and analytical approaches" (p. 10). Ethnography of communication is therefore viewed as being holistic. This is particularly valuable to LIS researchers because it means that it is flexible enough to characterize the established theories of this field and disciplinary orientation towards the collection of data. Since there is "no single best method" of collecting data, the choice of appropriate data collection techniques depends on the relationship between the ethnographer, the scholarly research community and the situation in which the fieldwork is conducted (Saville-Troike, 1989, p. 117).

Fieldwork, which is common to all ethnographies, encompasses a repertoire of data collection methods including document analysis, passive or participant observation, and in-depth interviewing. The analysis of written documents includes the use of published books and articles, autobiographies, diaries, e-mail messages, journals, letters and memos written by subjects or about subjects in a research investigation. In ethnographic research there are periods where the fieldworker may need to immerse him/herself in selected documents "to identify the dimensions or themes that seem meaningful to the producers of each message" (Berg, 1998, p. 230). Certain message forms, for example, research articles or monographs published by a scientific community could be used to learn more about the subject interests of the scholars and their style of written communication about these interests. Work setting memos, which may "contain strictly work-related information or other casual communications" can help the researcher reflect on the "tone and atmosphere of the work setting" and the "research aspects of the workplace culture or work folkways. Also, they may contain information relevant to understanding the general organizational communications network used in the setting, the leadership hierarchy, various roles present in the setting, and other structural elements" (Berg, 1998, p. 215).

The ethnographer or field researcher as both passive and participant observer usually gains entry to the field setting as an outsider. Sanday (1979) explains that the rationale for this position "is based on the assumption that one comes to understand something by seeing it as an outsider" (p. 528). Hammersley (1981) adds that "in order to be able to understand what [(s)he] sees and hears, the researcher must learn the culture of those [(s)he] is studying" as though it were "anthropologically strange" (p. 210). This learning process is essentially a process of discovery. The researcher should discover what is happening and not just focus on the "documentation of physical behaviour," but acquire enough insight to attribute the "intentions, motives and perspectives" of the individuals who are being observed (p. 210).

Field notes are considered to be a central component of the passive and/or active observation process. In the midst of tracking, observing, eavesdropping and asking questions, the researcher must find time to keep a record of field notes for in-depth analysis. Berg's (1998) recommendation is that field notes should be completed immediately following every excursion into the field; however, Burgess (1991) suggests that "note-taking is a personal activity that depends upon the research context, the objectives of the research, and the relationship with informants" (p. 192). Perhaps the best advice overall is that the researcher establishes a regular time for writing up notes and duplicates them for safety reasons (Berg, 1998).

The ethnographer as an interviewer may choose to talk with informants in the field setting for the purpose of gathering information. Prior to the interview process, consideration is usually given to the type of interview that will be carried out, particularly in terms of how structured it will be, how the questions will be formulated and how much time it will take. Scholars who have reflected upon the interview process explain that it can be formal or informal, structured or unstructured, or also standardized, non-standardized, or semi-standardized (e.g., Babbie, 1995; Berg, 1998; Rubin & Rubin, 1995). The standardized interview tends to be formal and structured with a list of predetermined questions. Interview participants are asked to respond to each question, and all are given the same set of questions. This technique of interviewing is used by researchers who have solid ideas about what they want to uncover in the interview and by those who expect (ideally) compatible responses (Berg, 1998, p. 60). In an ethnographic approach to research, the non-standardized or semi-standardized interview tends to be more common. The non-standardized interview is informal or non-directive and used "during the course of research to augment field observations" and to "establish rapport" with informants (p. 61). Semi-standardized interviews involve the preparation of predetermined questions and/or special topics. The questions are posed to the interviewee in order, but the researcher conducting the interview is allowed the freedom to probe beyond the answers to prepared questions to draw out elaborated responses (p. 63).

Chatman's (1984) understanding of fieldwork is of interest, given that she is one of few LIS researchers to spend a significant amount of time doing research in a naturalistic setting. The wealth of experience she has gained has contributed to specific insights concerning the process of *gaining entry to the field, developing a research role, overcoming anxiety, developing a rapport with informants, empathizing with them and establishing a relationship with them based on reciprocity*. In order to gain entry to the field, she advises that the best strategy is to request an interview with the person identified as the "gatekeeper" (i.e., the person in a formal position to protect the people, setting or institution). When meeting this gatekeeper it is important to explain the nature of the research honestly, particularly the issue of confidentiality (p. 429).

Once the field researcher does gain entry to a chosen setting, it then becomes important to assume "a characteristic posture" in relation to the respondents so as to "permit intimate observation of every day life in the field" (p. 429). The role of researcher-participant, or total researcher is discussed as possible options; however, Chatman herself has adopted the role of "student-researcher." She indicates that "as a graduate student, [she] found that most of the people encountered had no difficulty in accepting the reality that graduate students do research" (p. 429).

Anxiety is sometimes experiences by the field researcher when certain problems are encountered. The problem of gaining entry, for example, can be a source of anxiety, particularly in cases where field gatekeepers negatively interfere with the research process or subjects refuse to participate. The researcher who can manage anxiety will eventually realize that it need not hinder the process as this personal experience may help "one to gain insight into the factors affecting the research" (p. 430). According to Chatman (1984), even the most experienced fieldworker may be placed in anxiety producing situations and must rely on a combination of prepared knowledge about the setting in which the study will take place and common-sense knowledge.

Chatman (1984) also writes about the importance of establishing a rapport with the informants so that they will be more likely to feel comfortable while being observed and allow communication to take place in a more unguarded manner. Reciprocity empathy and maturity are also significant terms related to the researchers role. Reciprocal processes allow the researcher to show some form of obligation or appreciation toward informants (p. 433). Empathy is closely related to reciprocity, as it may be one way of giving to informants but it is typically a personal decision on the part of the researcher to remain empathetically open to "seeing the social reality from the view of the client" (p. 435). Moreover, the researcher who adopts a "mature" outlook will likely recognize that it is possible to overcome difficulties related to field research. As one becomes more practiced in ethnographic research, Chatman suggests that there are many opportunities for personal satisfaction and joy associated with meeting and understanding new people (p. 435).

3.4 "Triangulating" citation, social network and ethnographic data

The quantitative-qualitative design of this study involves "triangulating" a specific set of research techniques associated with citation analysis, social network analysis and ethnography of communication. When used together, "bibliometric, sociometric, cognitive and communication data provide the researcher with overlapping, incompletely congruent views of structure in a chosen research specialty" (McCain, 1990b, p. 212). Certain incongruous results cannot always be explained; therefore McCain suggests that "the researcher should not strain to account for all anomalous observations" given that "an exploration of these differences can highlight the particular advantages of one approach vis-à-vis another" (p. 212).

Citation analysis, social network analysis and ethnography of communication have unique methodological histories and have been used more frequently in specific research fields – Library and Information Science, Sociology, and Linguistic Anthropology respectively – yet the common element is that they all focus on relational systems. In citation analysis, the bibliometric artifact is the unit of analysis taken from a relational system that concentrates on the proliferation of intellectual ideas. Social network analysis is concerned with the relational system of social actors and the impact this system has on the individual's behaviour or the functioning of the social network as a whole. Ethnography of communication examines the role of culture in a relational system where a group of people with a shared language and knowledge structure can be observed interacting with one other in communicative situations, events and acts. By combining these methodological approaches, this study will develop new insight into how mathematicians as social actors maintain a relational system dedicated to the sharing of information and the preservation of a research culture that provides them with a unique environment for doing mathematics.

3.5 Delimitations and limitations of the study

As a case study, this research is designed to systematically gather information about a particular group of people and social setting to permit some degree of understanding of how they operate or function (Berg, 1998). The research scope has thus been narrowed to one group of scholars from the field of mathematics, the Singularity Theory community, and no other group outside this subject specialty. Attention is given to the Singularity Theorists' co-citation patterns, co-authorship patterns, patterns of attendance at worldwide conferences and personal thoughts associated with being members of an international research community and working in specialized research environments. In some parts of the dissertation, information conveyed by 'outside' mathematicians (i.e., those not identified as Singularity Theorists) is incorporated for discussion purposes and to enhance the interpretation of other data. A slight expansion of the scope in this sense is important, given that the overall intent is to produce a case study that is *instrumental* in nature. The expected benefit of focusing on the Singularity Theory community then is that the findings will not only lead to new insight into the invisible college, but also lead to a generalized understanding of similar groups of mathematicians.

The Isaac Newton Institute for Research in Mathematics (Newton Institute) located in Cambridge, England is the setting chosen for the field research. The Newton Institute is one example of many specialized research environments designed to bring international mathematicians together for intense periods of work. Other research institutes around the world, for example, the Mathematics and Sciences Research Institute (MSRI) in Berkeley, California and The Fields Institute for Research in Mathematical Sciences (Fields Institute), in Toronto, Canada offer similar programs.

Prior to gaining entry to the Newton Institute, I was employed at the Fields Institute as the Scientific Program Coordinator (1996-1999) and Publications Manager (1999-2002). My work at the Fields led to the plan of this study and inspired my interest in the institute environment as a setting for ethnographic fieldwork. During my employment term, I had the opportunity to gain some preliminary knowledge of the field setting; knowledge that is considered valuable to the fieldwork process. As Hammersley and Atkinson (1989) indicate, "it helps to 'case' the setting beforehand, for example by contacting people with knowledge of it or of other settings of a similar type" (p. 64). When it was time to initiate the actual fieldwork, however, it was the Newton Institute that I chose for the ethnography, because I wanted to enter the field as a complete outsider. I recognized that a conflict of interest would occur at the Fields Institute if I chose to carry out my fieldwork in this 'home' environment and carry on with my employment obligations at the same time. Moreover, in January 2000, I discovered that the members of the European Singularity Theory community were in the process of organizing a program term at the Newton Institute (August to December 2000). The timing of this upcoming program and the plan of my study coincided very well, so it became even more essential for me to transfer to this environment for the ethnographic period. In certain parts of the study, comparisons are noted between the Fields Institute and the Newton Institute, but this does not mean that the research constitutes a formal comparative case study of the two research environments. My background knowledge of the mathematics culture at the Fields Institute is simply used in this study to contribute relevant details and to enhance the interpretive process.

Concerning research-related limitations, one associated with the use of author co-citation analysis requires clarification. Author co-citation analysis, which is used to "map" co-cited authors in "intellectual space," begins with the retrieval of co-cited author statistics from an Institute for Scientific Information (ISI) database. The Dialog™ *SciSearch* database was used in this research, but due to the design of this indexing system, the collection of all author co-citations for a large sample of author pairs is very difficult. Harsanyi (1993) notes that "complete counts for a given author require obtaining a complete bibliography of that individual's work and adding citations found under various first authors to the count for which the individual is the first or only author. At best this is tedious, and at worst (if a bibliography is not available) it is impossible" (p. 329). Likewise, Persson (2001) indicates that "a full scale test [of co-citedness] is unrealistic since it means tracing all authors of several thousand cited documents" (p. 339). Consequently, only first author co-citation counts are used in this study, but this is standard practice in the use ACA. First-author counts have been used by many other researchers (e.g., Bayer et al., 1990; McCain, 1990b; White & Griffith, 1982), who understand that the general "aim is not to rank authors but rather to identify research themes" (Persson, 2001, p. 343). This analysis and mapping procedure is therefore best at illustrating "overall trends or dimensions in scholars' approach toward research" and providing a "general historical view of the intellectual structure of a research area" (McCain, 1990b, p. 213).

Another research-related limitation is that it can be difficult to combine the use of quantitative and qualitative data effectively in one mixed-methods study. With a "mixed methods" design, both quantitative data (results of statistical analyses) and qualitative data (themes generated from interviews and observations) are presented together in certain parts of the study. According to Creswell (1994), the quantitative-qualitative combination is sometimes not "pragmatic" because of the "extensive time required to use both paradigms adequately, expertise needed by the researcher, and the desire to limit the scope of a study and the lengthy reporting requirements" (p. 173).[8] Nevertheless, there is a certain advantage in using research methods in combination, particularly if the researcher aims "to better understand a concept being tested or explored" (p. 177).

In this study, the invisible college is the concept that is being explored. Because the invisible college is multifaceted, a mixed-methods design was chosen with the understanding that it would generate particular challenges for me as a researcher. First, there was the challenge of choosing the techniques for collecting and analyzing the data and the second, I could see that it would be important to combine them in a suitable manner to convey different but interrelated aspects of the same phenomenon. Overall, the challenges associated with this study were worth pursuing: the mixed-methods design opened up the opportunity for me to "elaborate on results, use one method to inform another, discover paradox or contradiction, and extend the breadth of the inquiry" (Creswell, 1998, p. 185).

[8] Creswell's (1994) explanation of the difference between quantitative and qualitative research as "differing paradigms" is that the "quantitative approach holds that the researcher should remain distant and independent of that being researched. By comparison, "the qualitative stance is different: researchers interact with those they study, whether this interaction assumes from living with or observing informants over a prolonged period of time, or actual collaboration" (p. 6).

CHAPTER 4: THE INTELLECTUAL STRUCTURE OF SINGULARITY THEORY

Singularity Theory research as an intellectual structure can be examined visually with the Author Co-citation Analysis (ACA) technique developed by White and Griffith (1982). This technique makes use of the individual author's *oeuvre* (single or co-authored document(s) of which (s)he is the first author) and assumes that two authors are "intellectually" related to one another if they are co-cited frequently together in many documents (White, 1990). A complete ACA involves the selection of author names, the retrieval of (first) author co-citation frequencies, the compilation of a raw data matrix and conversion to a correlation matrix, and the multivariate analysis of the correlation matrix for interpretation and validation (McCain, 1990). Details of this procedure are outlined in the next paragraphs, including the questions answered by this study and a discussion of the results relative to the conceptual model introduced in Chapter 1.

4.1 Author co-citation analysis pilot

In preparation for this case study ACA, a pilot ACA was carried out at the College of Information Science and Technology of Drexel University in March 2000 with Professors Howard White and Kate McCain. The main objective of the pilot was to learn how to use this research technique and determine if a preliminary set of authors from the Singularities specialty would produce high enough citation and co-citation counts for a large-scale co-citation map. According to White (1986), a successful ACA depends on "choosing good names on which to search. They must be authors prominent enough to have been cited by other writers in journals recorded in *SciSearch* or Social *SciSearch*. The authors must also be related enough in other writers' eyes to be cited together (co-cited) with some frequency" (p. 94).

At the time of the pilot, there was a concern about whether or not the works of the Russian Singularity Theorists would be indexed in the Dialog™ *SciSearch* database. White had indicated that the Institute for Scientific Information (ISI) databases were biased toward American and English-Language research and suggested that it might not be possible to proceed with an ACA if the Russian works were not indexed (personal communication, March 1, 2000). It was reasonable to expect that they would not be included since there was a significant period before 1990 when the Russians faced travel restrictions and were unfamiliar with the mathematics that was being done in other parts of the world. Likewise, the North American, European and Asian researchers were unfamiliar with the Russian mathematics because it was published only in Russian language journals. To examine the extent of the problem, a preliminary search for author citation and co-citation counts was performed and the results confirmed that the *SciSearch* database does index the works of Russian authors, in English or English translation. Relatively "healthy" citation counts and co-citation counts were found for a small selection of international authors (N=24) from various countries, including Russia; therefore we established that it would be feasible to continue with a large scale ACA.

For the pilot study, 24 Singularity Theory authors were selected only on the basis of high citation and co-citation frequencies. Appendix A provides the raw co-citation and correlation matrices, the hierarchical cluster analysis and the iteration history for the two-dimensional scaling solution. A list of the 24 authors' names and the summary statistics appear in Tables 4.1 and 4.2 below. The resulting cluster-enhanced two-dimensional pilot map is shown in Figure 4.1.

Table 4.1. Co-cited author list (n=24) for the pilot ACA.

ARNOLD, Vladimir Igorevich	LOJASIEWICZ, Stanislaw Sr.	THOM, René
BIERSTONE, Edward	MALGRANGE, Bernard	TROTMAN, David
BRIESKORN, Egbert	MATHER, John N.	VARCHENKO, Alexander
BRUCE, James W.	MILNOR, John W.	VASSILIEV, Victor
DAMON, James	MOND, David M. Q.	WALL, Charles T. C.
GORYUNOV, Victor V.	SAITO, Kyoji	WHITNEY, Hassler
GUSEIN-ZADE, Sabir	SIERSMA, Dirk	WILSON, Leslie Charles
HIRONAKA, Heisuke	TEISSIER, Bernard	ZARISKI, Oscar

Table 4.2. Summary statistics for pilot ACA.

Number unique author pairs over 24 authors' names	24(24-1)/2= 276
Number of unique author:	
pairings never made	18
pairings made only once	25
Connectivity ratio	258/276 = 94%
Mean co-citation rate (over 75 authors)	38.26
Range of raw co-citation counts	0 - 584 (Wall & Milnor)
Range of mean co-citation rates	5.71 (Varchenko) – 120 (Milnor)

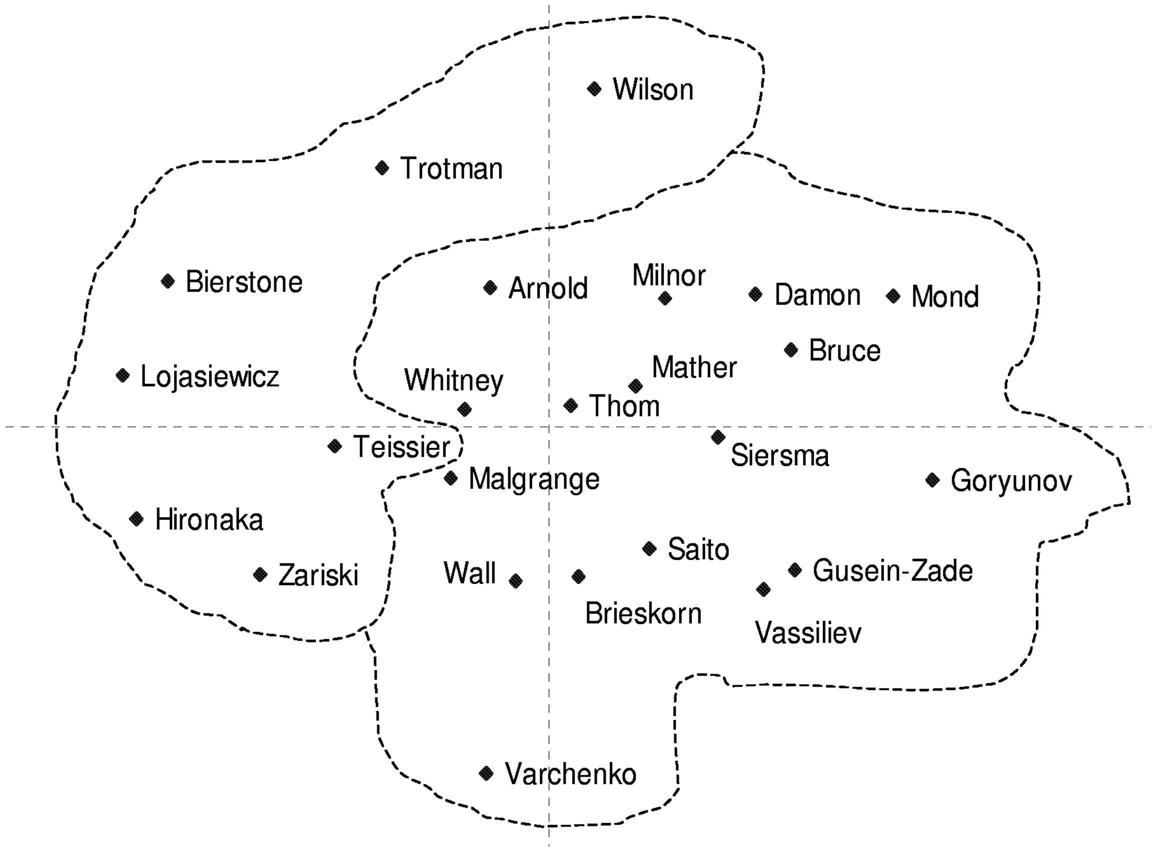

Figure 4.1. ACA pilot map of 24 Singularity Theory authors (1974-2000).

On the pilot ACA map, attention was given to the positioning of the authors so that a few preliminary interpretations could be made as part of a practice analysis. Due to my work at the Fields Institute and previous conversations with the visiting mathematicians, I was not surprised to see that TROTMAN and WILSON were, relative to the other author nodes on the pilot map, adjacent to one another. I knew that they had traveled to each other's affiliates (Hawaii and Marseilles respectively) for research purposes and I was aware of the fact that they were friends. The "intellectual" similarity of the two authors, as seen by other writers from the subject specialty, led me to reflect upon their underlying *social* relationship. This view corroborates White's (1990) understanding that "the use of authors as the unit of analysis opens the possibility of exploring questions concerning both perceived cognitive structure and perceived social structure of science. If co-citation analysis shows close relationships between *oeuvres*, is it the case that the persons who wrote them are socially connected? Anecdotal evidence points to a qualified "yes", but the matter has received less systematic attention than it deserves" (p. 86). Consequently, this study is designed to give more systematic attention to ACA, not only to determine what an ACA can reveal about the intellectual structure of Singularity Theory, but also to discover how it can be used with other research techniques to generate insights into other underlying social relationships.

4.2 Selection of the final ACA author set

To prepare a large-scale map of Singularity Theory research – one that would convey further insight into the specialty's structure – a starting list of approximately 150 authors' names was collected from four information sources: 1) the mathematical literature (i.e., published journal articles, conference proceedings, and monographs), 2) conference participant lists posted on the World Wide Web, 3) the European Singularities Network web site, and 4) conversations with Singularity Theorists who agreed to name important or influential colleagues. Based on a pre-test in the Dialog™ *SciSearch* database (1974-2001) for high citation and high co-citation rates, the starting list was then reduced to a narrower set of approximately 60 names.

In keeping with idea that the invisible college is both an intellectual structure and a social process, certain social factors were also taken into account during the author selection process. Thirty-six of the authors were chosen purposefully, regardless of their co-citation rates, because they were posted on the Newton Institute's web site as confirmed visitors for the Singularity Theory program (August to December 2000). Some names produced high co-citation counts with every other author on the list (e.g., SAITO's average was 16) and others much lower counts (e.g., KAZARIAN's average was 1), yet all were added to the sample because they represented a measure of "social involvement" in the specialty, as conference participants. The 36 "Newton Institute" authors, listed with an asterix (*) in Table 4.3 were targeted as potential interview informants for the ethnographic phase of the study. All represented a range of statuses within the international research community in that some were recognized as being senior (e.g., WALL) and others junior (e.g., TIBAR) to the field.

During the four-month Singularity Theory program at the Newton Institute, 17 of the visiting authors agreed to participate in one-to-one interviews. All interviewees were shown a preliminary version of the ACA map and asked to name colleagues that they thought should be included or deleted from the final map. Their suggestions were taken into account: a few names were added and others were removed. Overall, an effort was made to include authors from a variety of countries, particularly those where Singularity Theory research has become a significant part of the mathematics research culture (e.g., Japan, England, U.S.A., Canada, France, Germany, the Netherlands). The final diversified list of 75 author names used in the study is presented in Table 4.3 (see Table 4.3). According to McCain (1990a), a diversified list of authors is critical for the examination of the overall structure of a subject specialty, because "it <u>defines</u> the scholarly landscape being mapped. If the authors are not chosen to capture the full range of variability [e.g., research topics, methodologies, or political/national orientations, etc.] … these aspects of structure cannot be demonstrated" (p. 433, original emphasis).

Table 4.3. Co-cited author list for Singularity Theory (drawn from the *MathSciNet* Author Index).

ARNOLD, Vladimir Igorevich	GREUEL, Gert-Martin *	MOND, David M. Q. *†
AROCA HERNÁNDEZ-ROS, José M.	GUSEIN-ZADE, Sabir	OKA, Mutsuo *
ARTAL-BARTOLO, Enrique *	HAMM, Helmut A. Hamm	PARUSINSKI, Adam *
BIERSTONE, Edward	HERTLING, Clause *	PELLIKAAN, Ruud
BRASSELET, Jean-Paul	HIRONAKA, Heisuke	PHAM, Frédéric
BRIANCON, Joël	ISHIKAWA, Goo *	PORTEOUS, Ian
BRIESKORN, Egbert	IZUMIYA, Shyuchi *†	SABBAH, Claude
BRUCE, James W.	JANECZKO, Stanislaw *	SAITO, Kyoji *†
BRYLINKSI, Jean-Luc	KAZARIAN, Maxim E. *†	SEDYKH, Vacheslav *†
CAMPILLO LOPEZ, Antonio	KHOVANSKII, Askold	SIERSMA, Dirk *†
CHILLINGWORTH, David R. J. *	KOIKE, Satoshi	SLODOWY, Peter *†
DAMON, James *†	KUO, Tzee-Char	STEENBRINK, Joseph H. M. *
DIMCA, Alexandru *	KURDYKA, Krzysztof *	SUWA, Tatsuo
DUPLESSIS, Andrew *†	LE DUNG TRANG *†	TEISSIER, Bernard
EBELING, Wolfgang *	LEJEUNE-JALABERT, Monique	THOM, René
FUKUDA, Takuo	LOJASIEWICZ, Stanislaw Sr.	TIBAR, Mihai *
FUKUI, Toshizumi *	LOOIJENGA, Eduard *†	TROTMAN, David *†
GABRIELOV, Andrei M.	LUENGO VELASCO, Ignacio*	VAN STRATEN, Duco *†
GAFFNEY, Terence *†	MACPHERSON, Robert D.	VARCHENKO, Alexander *
GALLIGO, André	MAISONOBE, Philippe	VASSILIEV, Victor *†
GIBLIN, Peter *	MALGRANGE, Bernard	WALL, Charles T. C. *†
GIBSON, Christopher	MATHER, John N.	WHITNEY, Hassler
GIVENTAL, Alexander	MERLE, Michel	WILSON, Leslie Charles *
GORYUNOV, Victor V.*	MILMAN, Pierre	ZAKALYUKIN, Vladimir *†
GRANGER, Jean-Michel	MILNOR, John W.	ZARISKI, Oscar

* Participant of the Singularity Theory program at the Isaac Newton Institute (August to December, 2000)
† Interview Informant

4.3 Pretest for name-forms, truncation effects and surname variations

With the purposeful author set (n=75) chosen for the ACA, a pre-test for name forms was carried out to determine if precise author names could be used in the data collection process or if the first initial had to be truncated. Complete name forms for each of the Singularity Theory authors were verified in *MathSciNet*, the web-based Mathematical Reviews Index of the American Mathematical Society (1940-present inclusive).[9] A search in the *MathSciNet* Author database, based on the input of a surname and first name initial (e.g., WILSON, L*) was performed to view the list of indexed names, including homonymous authors, name form variations, and links to the individual author's complete set of reviewed works. In some cases homonymous author names did appear, for example: WILSON, LC; WILSON, LB; WILSON, LD; WILSON, LG; WILSON, LH, WILSON, LJ, WILSON, LT; WILSON, LW. To find out which "WILSON, L" was involved in Singularity Theory research, a review of each author's complete body of works was carried out. In this particular case, all publication titles subject key words and abstracts associated with WILSON, LC or WILSON, Leslie Charles clarified that this mathematician was the Singularity Theorist.

To search for WILSON's co-citation counts in the *SciSearch* database it was necessary then to attach the truncation symbol (?) to his first initial or use his precise name with the second initial: S CA=WILSON L? OR CA=WILSON LC. The "truncation symbol (?)" allows the co-citationist to "generalize the request to cite any works by [a named author] to retrieve any papers that cite any works by [the author] whether or not citers use [a] second initial" (McCain, 1990a, p. 434). More often than not, this symbol was required for an author search in this case, so I opted to use it consistently, even if the author did not possess a second initial. With the name SIERSMA, D., for instance, the truncation or non-truncation of his only initial "D(?)" yielded the same citation results.

9 *MathSciNet* and the equivalent *MathSci* DIALOG™ database covers published journal articles, monographs and conference proceedings.

Due to the international nature of Singularity Theory research, a few variations in surname were found in the *MathSciNet* Author database. For three Russian surnames, citers had used either the English or Russian spelling: KAZARIAN or KAZARYAN; GORYUNOV or GORJUNOV; VASSILIEV or VASSILYEV. In some instances, only the first part of a hyphenated surname was cited instead of both parts, for example, LEJEUNE rather than LEJEUNE-JALABERT and ARTAL rather than ARTAL-BARTOLO (Note: the use of the hyphenated surname GUSEIN-ZADE was always used in full form). For the Vietnamese name LE DUNG TRANG, citation inconsistencies resulted in the appearance of LE, DT or TRANG, LD. Three compound surnames, AROCA HERNANDEZ, CAMPILLO LOPEZ and LUENGO VELASCO were expected to vary in terms of use; however a pre-test in both *MathSciNet* and *SciSearch* revealed that for all authors, the first part of the compound surname was always cited. In every instance where a surname did vary, the second name form was included in the search strategy with the operator "OR" to ensure that all relevant citation counts were retrieved (e.g., S CA=GORYUNOV V? OR CA=GORJUNOV V?).

4.4 Retrieval of author co-citation frequencies

The author co-citation frequencies for this case study of Singularity Theory were extracted from the Dialog™ *SciSearch* database files 434 (1974-1989) and 34 (1990-2000). In performing the co-cited author search, two author names were specified with the intention of retrieving any paper that cites both. For example: "S CA= TROTMAN D? AND CA=BIERSTONE E?" With this command, a search is carried out for all writers that cite anything by the first author David TROTMAN and anything by the first author Edward BIERSTONE.

Given the number of author names selected for an ACA, the combination of all possible pairs can reach a maximum number of N(N-1)/2. For practical reasons, White advises that "it is best to keep the input set of authors relatively small" since it is obvious that "the more pairs one starts with, the more pairs one must enter (if all combinations are to be formed)" (p. 95). In this study, a total of

75(75-1)/2=2,775 co-citation pairs was expected; therefore a mechanized search strategy was used to avoid the frequent typing of individual search commands. To satisfy this requirement, all search commands were pre-composed in Microsoft Word, then cut and pasted into the DialogLink™ module, which contains a type-ahead scroll window set for automatic input to *SciSearch*. The start command involved searching for the first author (e.g., S CA=ARNOLD V?) and pairing the citation results (S1) 74 times with every other author from the cited author list. This cycle was repeated for each author (i.e., S1 x 73,72,71 etc.) until all of the authors' co-citation profiles were complete (see Appendix B for sample search strategy).

Dialog™ *SciSearch* is a comprehensive database for all articles, reviews, and proceedings papers published in the fields of mathematics, chemistry, the engineering sciences and physics. Because of the broad coverage of this database the pairing of "homonymous" author names – homonyms from the same specialty or other research specialties – could lead to irrelevant co-citation counts. Most co-citation analysts agree that it is unlikely that two or more authors with the same surname and first name initial will be active in the same research specialty, or have "twins" paired together as co-cited authors in another scientific specialty (Harter, 1986; Sandstrom, 1988; White, 1986). Harter (1986) claims that the number of such pairings is "exceedingly small" (p. 187). Likewise, White (1986) has found that "homonym discrimination breaks down only in rare cases" and provides an example search in LIS (CR=WILSON P?) which has the effect of retrieving papers citing either Patrick or Pauline Wilson in LIS (p. 95). Sandstrom (1988) recognizes also that "significant skewing can conceivably occur when two <u>very common names</u> are combined, but this question is one that should be addressed empirically" (p. 164, original emphasis).

With respect to the Singularity Theory author set, it was difficult to determine the common nature of certain surnames, since names understood as not common in one culture (e.g., U.S.) may in fact be common in another (e.g., China). To avoid any incorrect assumptions, an author search in *MathSciNet* was carried out to determine exactly how many other authors in mathematics had the same name. The search revealed that the most common surnames were the following:

WILSON=217; WALL=185; KUO=124; SAITO=114; ISHIKAWA=58; ARNOLD=64; FUKUDA=47; OKA=26; WHITNEY=22; FUKUI=20; MILMAN=14. Among these surnames, some also shared the same first initial: WILSON L=10; WALL C=4; KUO T=10; SAITO K=15; FUKUDA T=11; FUKUI T=6; OKA M=5; MILMAN P=2.

Based on the high rate of homonymous names indexed in *MathSciNet*, it seemed reasonable to expect that further homonyms would be generated from the all research fields covered by *SciSearch*. They did appear, but the inclusion of each author's first initial in the search strategy ensured that many were omitted in the co-citation counts. The pairing of two authors' names with the “AND” operator in the search strategy also excluded some unwanted names. In addition, I could not discount the possibility that two completely homonymous authors (i.e., authors with the same surname and initial, but from a different research discipline) might be paired together in a citation list. In instances where a high co-citation count was retrieved an effort was made then to verify that this was not the case. The verification procedure simply involved examining the list of articles associated with overall the co-citation count and subtracting unwanted counts associated with non-mathematics articles.

4.5 Construction of the raw data matrix and Cosine proximity matrix

As all co-citation counts for the paired Singularity Theory authors were gathered, each count was added to the cells of a 75 x 75 matrix with identically ordered author names (alphabetical) placed next to the rows and the columns. Column averages were calculated and inserted into the cells along the diagonal. This option was essentially arbitrary based on the fact that in previous ACAs McCain (1990) “experimented with an alternate approach – treating the diagonal cells as missing data (for example, see McCain 1989)” (p. 435). Upon examining the results she found “little difference, in mapping, clustering and factor analysis, between scaling the diagonal values (a la White & Griffith) and treating them as missing data” (p. 435).

At the next stage of the ACA, the raw data were converted to Cosine proximity values and added to a new 75 X 75 matrix. Converted data helps to determine the overall similarity of two authors in a matrix relative to every other author pair and to remove differences in scale between authors who produce much higher co-citation counts than others (McCain, 1990a). In McCain's (1990a) technical overview of ACA, Pearson's *r* is noted as the recommended conversion measure; however, in the main co-citation analysis of this study (i.e., not the Pilot study) Cosine was used as an alternative. Ahlgren, Jarneving and Rousseau (2003) explain that Pearson's *r* is "not an optimal choice of a similarity measure in ACA" because it "does not satisfy some natural requirements" (p. 550). One requirement is that "a similarity or association measure between two data vectors x and y (of equal length)" should "map this ordered pair of vectors to a real number such that s (X, Y) = s (Y, X) and such that it attains its maximum value (often 1) when X=Y" (p. 551). Essentially, the similarity measure should be non-negative. In a hypothetical example, the authors show that when two authors A and B, with a high Pearson's *r* correlation value of 1 (perfect similarity) are part of a matrix incorporating added zero co-citation counts, the r value unexpectedly diminishes to 0.91. This effect, according to Ahlgren et al. (2003) is an "absurd situation" given that "two 'perfectly similar' authors showing the same behaviour with respect to a group of new authors are not 'perfectly similar' anymore" (p. 553).

A portion of the 75 x 75 raw matrix and the converted Cosine proximity matrix is illustrated in Tables 4.4 and 4.5 (see Tables 4.4 and 4.5). The higher the proximity measure between two authors, the more similar they are perceived to be by their citing colleagues due to shared subject interests.[10]

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[10] In some fields of scholarship, for example, the humanities and social sciences, this measure of similarity is associated with the idea that two or more authors are co-cited frequently because they contradict or oppose one another in their presentation of different theories or arguments concerning a controversial topic or theme.

Table 4.4. Partial matrix of raw co-citation counts. Singularity Theory (1974-2000).

	ARNOLD	AROCA	ARTALBARTOLO	BIERSTONE	BRASSELET	BRIANCON	BRIESKORN	BRUCE	BRYLINSKI	CAMPILLO	CHILLINGWORTH	DAMON	DIMCA	DUPLESSIS	EBELING	FUKUDA	FUKUI	GABRIELOV	GAFFNEY	GALLIGO	GIBLIN
ARNOLD	56	1	7	35	1	24	189	161	14	1	13	92	40	23	59	13	9	49	43	8	17
AROCA	1	1	0	9	0	3	0	0	0	0	0	0	0	0	0	0	1	4	0	3	0
ARTALBARTOLO	7	0	2	0	0	0	6	0	0	0	0	1	12	0	0	0	0	0	0	0	0
BIERSTONE	35	9	0	10	2	10	3	8	2	0	3	15	3	5	0	3	2	36	8	11	0
BRASSELET	1	0	0	2	3	5	1	0	12	0	0	0	1	0	0	0	0	0	1	0	0
BRIANCON	24	3	0	10	5	12	10	8	12	0	0	20	10	6	1	6	6	3	19	47	1
BRIESKORN	189	0	6	3	1	10	21	14	8	2	0	12	18	0	24	1	0	21	3	6	2
BRUCE	161	0	0	8	0	8	14	15	0	0	1	52	22	18	3	3	1	2	38	4	35
BRYLINSKI	14	0	0	2	12	12	8	0	6	1	0	1	4	0	0	0	0	2	0	9	0
CAMPILLO	1	0	0	0	0	0	2	0	1	1	0	0	0	0	0	1	0	0	0	0	0
CHILLINGWORTH	13	0	0	3	0	0	0	1	0	0	1	0	0	0	0	0	0	0	0	0	0
DAMON	92	0	1	15	0	20	12	52	1	0	0	14	15	23	5	7	2	2	42	12	3
DIMCA	40	0	12	3	1	10	18	22	4	0	0	15	8	10	7	0	0	3	8	6	1
DUPLESSIS	23	0	0	5	0	6	0	18	0	0	0	23	10	5	0	9	0	1	25	4	0
EBELING	59	0	0	0	0	1	24	3	0	0	0	5	7	0	5	0	0	16	0	1	0
FUKUDA	13	0	0	3	0	6	1	3	0	1	0	7	0	9	0	4	9	0	12	2	0
FUKUI	9	1	0	2	0	6	0	1	0	0	0	2	0	0	0	9	1	0	1	0	0
GABRIELOV	49	4	0	36	0	3	21	2	2	0	0	2	3	1	16	0	0	8	1	1	0
GAFFNEY	43	0	0	8	1	19	3	38	0	0	0	42	8	25	0	12	1	1	10	10	4
GALLIGO	8	3	0	11	0	47	6	4	9	0	0	12	6	4	1	2	0	1	10	5	1
GIBLIN	17	0	0	0	0	1	2	35	0	0	0	3	1	0	0	0	0	0	4	1	2

Table 4.5. Partial matrix of Cosine proximity values. Singularity Theory (1974-2000).

	ARNOLD	AROCA	ARTALBARTOLO	BIERSTONE	BRASSELET	BRIANCON	BRIESKORN	BRUCE	BRYLINSKI	CAMPILLO	CHILLINGWORTH	DAMON	DIMCA	DUPLESSIS	EBELING	FUKUDA	FUKUI	GABRIELOV	GAFFNEY	GALLIGO	GIBLIN
ARNOLD																					
AROCA	0.26																				
ARTALBARTOLO	0.477	0.115																			
BIERSTONE	0.537	0.752	0.239																		
BRASSELET	0.486	0.321	0.304	0.441																	
BRIANCON	0.581	0.557	0.527	0.69	0.546																
BRIESKORN	0.686	0.333	0.634	0.506	0.382	0.64															
BRUCE	0.635	0.195	0.373	0.472	0.277	0.494	0.717														
BRYLINSKI	0.561	0.431	0.449	0.568	0.736	0.708	0.655	0.348													
CAMPILLO	0.152	0.321	0.474	0.219	0.204	0.511	0.36	0.111	0.193												
CHILLINGWORTH	0.65	0.178	0.309	0.371	0.38	0.335	0.49	0.628	0.304	0.037											
DAMON	0.736	0.265	0.385	0.546	0.356	0.622	0.676	0.905	0.414	0.167	0.605										
DIMCA	0.753	0.264	0.696	0.493	0.443	0.688	0.835	0.75	0.602	0.297	0.489	0.803									
DUPLESSIS	0.648	0.183	0.26	0.493	0.301	0.553	0.449	0.831	0.27	0.127	0.537	0.905	0.656								
EBELING	0.65	0.183	0.546	0.397	0.288	0.456	0.852	0.832	0.443	0.184	0.614	0.735	0.782	0.556							
FUKUDA	0.745	0.286	0.29	0.553	0.421	0.586	0.524	0.698	0.377	0.105	0.668	0.781	0.614	0.77	0.547						
FUKUI	0.428	0.319	0.244	0.476	0.257	0.462	0.487	0.505	0.33	0.106	0.391	0.533	0.444	0.489	0.466	0.664					
GABRIELOV	0.607	0.695	0.497	0.813	0.5	0.69	0.762	0.566	0.654	0.286	0.442	0.587	0.685	0.406	0.651	0.495	0.486				
GAFFNEY	0.701	0.251	0.27	0.543	0.353	0.605	0.528	0.83	0.349	0.138	0.497	0.915	0.716	0.932	0.603	0.781	0.532	0.483			
GALLIGO	0.381	0.606	0.264	0.625	0.429	0.701	0.404	0.352	0.622	0.286	0.191	0.484	0.469	0.449	0.267	0.418	0.413	0.56	0.501		
GIBLIN	0.463	0.092	0.214	0.294	0.157	0.286	0.43	0.525	0.216	0.039	0.371	0.619	0.532	0.53	0.452	0.347	0.312	0.338	0.544	0.2	

4.6 Multivariate analysis and mapping of co-citation clusters

Once the Cosine values were calculated from the raw data matrix, they were then used as input to the *SPSS* programs CLUSTER and ALSCAL for a cluster analysis and multidimensional scaling routine. In keeping with the tradition of ACA research, a hierarchical agglomerative approach to the cluster analysis was carried out. Although there are “150 specific methods” to choose from in CLUSTER and each may be used “to group objects, people, countries, or other entities on the basis of shared attributes,” McCain (1990a) notes that “ACA research has tended to use the agglomerative clustering approach” (p. 437). With an agglomerative routine, there is a “bottom-up building” of clusters that joins individuals and/or groups of authors together gradually to represent smaller clusters and then the authors are subsequently joined again to create larger author clusters, until there is a complete linkage (p. 437). The resulting *SPSS* display may be a histogram icicle plot or a dendrogram, or both. In Appendix C, a figure of the complete linkage dendrogram is presented to illustrate the bottom-up building of the author clusters in Singularity Theory. Appendix C also presents the full iteration history for the two dimensional ACA solution (see Appendix C).

A multidimensional scaling (MDS) routine, which typically follows the hierarchical cluster analysis, requires as input to *SPSS* the same correlation matrix of proximity values. Specifically, MDS is “a set of techniques used to create visual displays – maps – from proximity matrices, so that the underlying structure within a set of objects can be studied” (McCain 1990a, p. 437). Cocitationists rely on MDS to “provide an information-rich display of the co-citation linkages and to identify the salient dimensions underlying their placement” (p. 437). The major output of an MDS routine in ALSCAL is a scatterplot display of points mapped in two or three-dimensional space. Each point, representing an individual author, is placed on the map according to their original proximity values of similarities or dissimilarities. According to McCain (1990a), the “points representing authors with high similarities [are] placed close together in ‘intellectual space,’ while points representing authors with high dissimilarities [are] placed farther apart (p. 438).

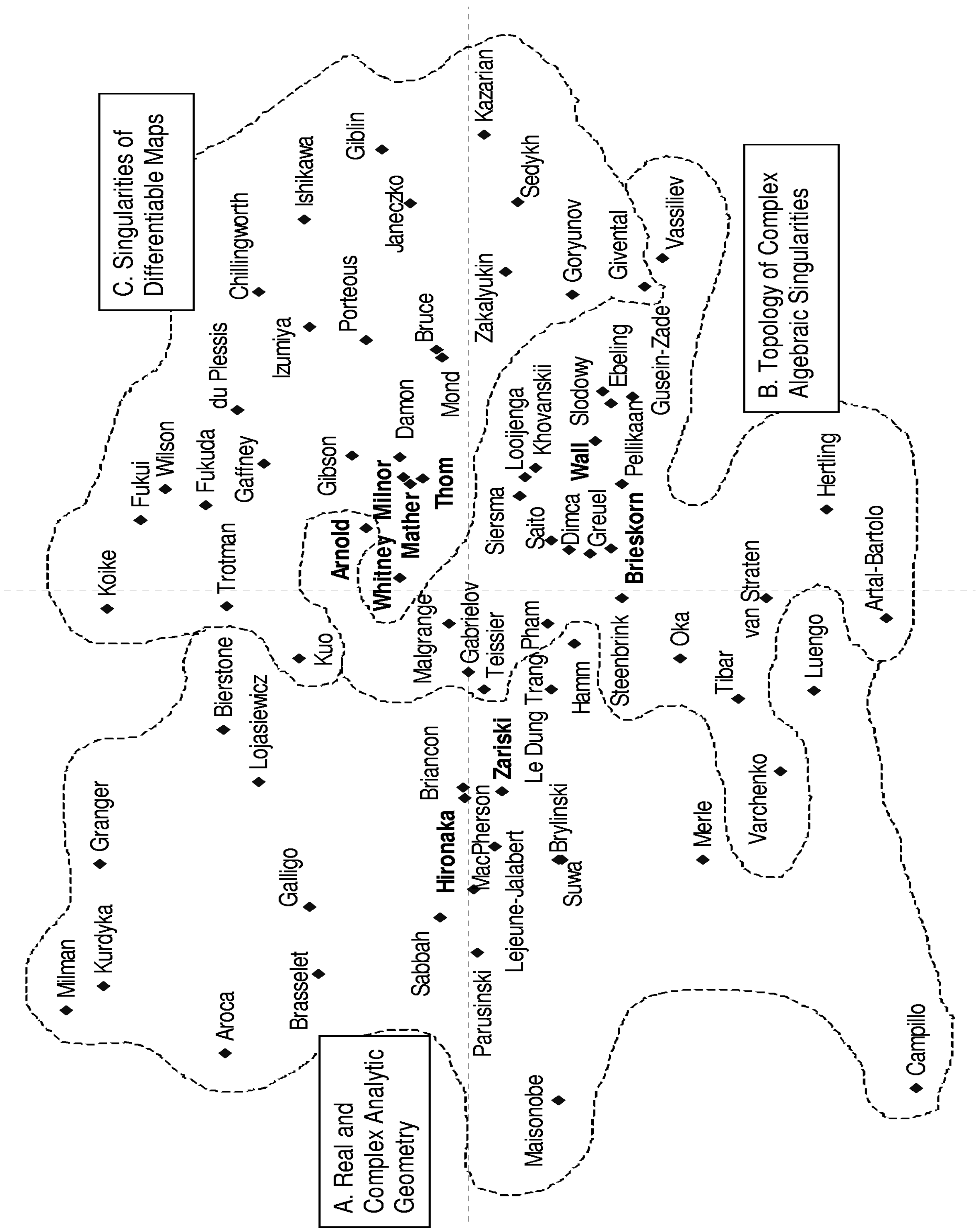

Figure 4.2. ACA map of 75 Singularity Theory authors (1974-2000).

As part of the MDS output, ALSCAL also produces a table of spatial coordinates – two co-ordinates for a two-dimensional mapping, three orthogonal projections for a three-dimensional mapping, etc. The general aim of ACA is to “capture as much of the original data as possible in only two or three dimensions” for simplification purposes (McCain, 1990a, p. 438). However, one drawback is that the data may be “distorted” somewhat and the map “cannot account for all the variance in the proximity matrix” (p. 438). In *SPSS*, the ALSCAL program provides a report on this level of distortion with a statistic called the “stress” measure and the RSQ value, or the percentage of the total variance explained. The stress measure is useful because it acts as a “criterion for determining the ‘best fit’ between the original input matrix ‘distances’ and the estimated ‘distances’ in the chosen low-dimensional solution. The algorithm stops when improvement in stress falls below some minimum value” (McCain, 1990a, p. 438).

To establish a salient view of the intellectual structure of Singularity Theory, co-cited author maps in both two and three dimensions were generated in *SPSS* and compared on the basis of reported RSQ and stress values. McCain’s advice was that if the RSQ improved with the three-dimensional solution by 8 or more percentage points, the three-dimensional mapping could be considered the best fit for the data (personal communication, Oct. 13, 2000). In this study, the three-dimensional solution yielded a 5 percent improvement in the proportion of variance (error) explained (i.e., RSQ). Specifically, the RSQ for the two-dimensional author co-citation solution was .84367, with an acceptable stress value of .18157 (i.e., .18157 < .20000). By comparison, the three-dimensional solution resulted in an improved RSQ value of .89528 and a decrease in stress at .13052 (see Appendix D for comparative results associated with the two and three-dimensional ACA solutions). Normally, if an RSQ value is closer to 1, the better the outcome in the multidimensional scaling routine. Also, the smaller the stress, the better the data fit the model. Because the RSQ improvement for the three-dimensional solution was minimal, the two-dimensional solution, which provided a much clearer visual output, was chosen for interpretation and validation. Figure 4.2 displays the final cluster enhanced mapping of the 75 Singularity Theory authors. Three main author groups are indicated by dotted lines and have been labeled as a result of the interpretative process detailed in section 4.9 (see Figure 4.2).

4.7 Co-cited author connectedness

The connectivity ratio for the 75(75-1)/2=2,775 unique author pairs in Singularity Theory was 63%. 1024 author pairings were not made and 332 were made only once. The lowest co-citation count was zero and the highest count was 584 (MILNOR and WALL). The most highly integrated authors in the set were ARNOLD (co-cited with 74 others), MILNOR (72), MATHER (71), WHITNEY (70), THOM (69), BRIESKORN (68), WALL (68), HIRONAKA (67) and ZARISKI (54).

4.8 Factor analysis

In addition to the clustering and MDS routines, a principal components factor analysis was carried out to help explain certain interrelationships among the Singularity Theory authors. Factor analyses in general allow researchers to "study the correlations among a large number of interrelated quantitative variables by grouping the variables into a few factors" (*SPSS* Base 9.0 Application Guide, 1999, p. 317). After grouping, the variables within each factor (i.e., authors) "are more highly correlated with variables in that factor than with variables in other factors" and each factor is subsequently "interpreted according to the meaning of the variables" (p. 317). A principle components analysis (i.e., the most common form of factor analysis), seeks a linear combination of variables in order to "characterize or account for the variation (spread) of each dimension in a multivariate space" (p. 319). The resulting factors are orthogonal, or uncorrelated.

In ACA, an exploratory factor analysis can be used to complement a clustering analysis, given that the cluster routine cannot illustrate multiple associations between authors – when the author is clustered, (s)he stays in one position. With a factor analysis, every author used as input "loads on (contributes) to every factor, and the interpretation or definition of each new factor is based on those authors with high loadings. Only authors with loadings greater than ± 0.7 are likely to be useful in interpreting the factor, and only loadings above ±0.4 or ±0.5 are likely to be reported" (McCain, 1990a, p. 440).

A varimax or orthogonal rotation of the factor axes was specifically used in this study to maximize the variance of the squared loadings of each factor (column) on all variables (rows) in the factor matrix. The result is the minimization of the number of variables that have high loadings on any one given factor. McCain (1990a) notes that the varimax rotation method is most commonly used in ACA: it produces a "simple structure" of uncorrelated factors compared to the oblique rotation method, which provides a "matrix of factor intercorrelations" (p. 440). The varimax rotation assists in determining "specific facts about an author's breadth" within a subject area. Singularity Theory is known to be a fairly cohesive subject; therefore in selecting the varimax method the relationship between the authors was considered the outcome of interest and not the degree to which there was any "subject-related linkage *above the author level*" (McCain, 1990a, p. 440, original emphasis).

Table 4.6 presents all component scores (in rows) for each given factor (in columns) generated from the SPSS FACTOR routine. A cut-off point of .40 was set for the author loadings so that the higher correlation values would be prominent (note: an exception was made for BRASSELET, since his highest loading was a value of .392) (see Table 4.6). The scree plot, which served as the criterion for selecting the number of factors is shown in Figure 6, with an arrow pointing to the elbow on the curve where the eigenvalue slope starts to level at component 5 (see Figure 4.3). Table 4.7, following the scree plot, is also added to show the eigenvalues (> 1) up to and including component five and the percentage of variance explained as each factor was extracted (see Table 4.7).

Note from Table 4.6 that 36 of the Singularity Theory authors load specifically on one factor. The names of the authors with significant single factor loadings (i.e., above .80) are GIVENTAL,

GORYUNOV, ZAKALYUKIN, JANECZKO, KAZARIAN, SEDYKH, VASSILIEV, LE DUNG TRANG, HAMM, OKA, WILSON, GAFFNEY, DUPLESSIS, FUKUDA, BIERSTONE, KURDYKA, LOJASIEWICZ, MILMAN, BIERSTONE. Thirty-seven of the other authors load on 2 factors or more, but SIERSMA, LOOIENGA, LEJEUNE-JALABERT, and WHITNEY are notable because they load on 3 factors above the .40 cut-off.

Table 4.6. Factor analysis of 75 Singularity Theory authors (1974-2000).

Rotated Component Matrix

	FACTOR				
	1) Singularities of Differentiable Maps: Hamiltonian & Lagrangian Systems; Symplectic Geometry	**2) Singularities - Algebraic Geometry; Several Complex Variables and Analytic Spaces**	**3) Equisingularity (Topological and Analytic)**	**4) Semi-Analytic Sets and Sub-Analytic Sets**	**5)Analytic Algebras and Generalizations, Preparation Theorems; Sheaves of Differential Operators and their Modules**
Givental	**0.927**				
Goryunov	**0.893**				
Zakalyukin	**0.892**				
Sedykh	**0.866**				
Gusein-Zade	**0.856**	0.409			
Janeczko	**0.854**				
Kazarian	**0.837**				
Vassiliev	**0.832**				
Ebeling	**0.748**	0.456			
Ishikawa	**0.731**				
Khovanskii	**0.726**				
Bruce	**0.723**		0.58		
Siersma	**0.698**	0.499	0.408		
Slodowy	**0.679**				
Mather	**0.673**		0.528		
Izumiya	**0.664**		0.609		
Saito	**0.638**	0.594			
Looijenga	**0.634**	0.499	0.437		
Thom	**0.566**		0.503		
Milnor	**0.544**		0.487		
Hertling	**0.512**	0.487			
Giblin	**0.457**				

	FACTOR				
	1) Singularities of Differentiable Maps: Hamiltonian & Lagrangian Systems; Symplectic Geometry	**2) Singularities - Algebraic Geometry; Several Complex Variables and Analytic Spaces**	**3) Equisingularity (Topological and Analytic)**	**4) Semi-Analytic Sets and Sub-Analytic Sets**	**5)Analytic Algebras and Generalizations, Preparation Theorems; Sheaves of Differential Operators and their Modules**
Le Dung Trang		**0.84**			
Hamm		**0.829**			
Oka		**0.823**			
Artal-Bartolo		**0.743**			
Greuel	0.471	**0.724**			
Merle		**0.718**			
Dimca	0.426	**0.707**			
Steenbrink	0.549	**0.702**			
Pham	0.485	**0.687**			
Teissier		**0.68**			
Brieskorn	0.588	**0.677**			
Tibar		**0.676**			
Luengo		**0.673**			
van Straten		**0.647**			
Zariski		**0.637**		0.539	
Lejeune-Jalabert		**0.609**		0.504	0.425
Wall		**0.559**	0.42		
MacPherson		**0.554**		0.48	
Briancon		**0.553**			0.479
Brylinski		**0.543**		0.468	
Pellikaan	0.468	**0.527**			
Suwa		**0.524**			
Varchenko		**0.474**			
Campillo		**0.465**			
Wilson			**0.886**		
Gaffney			**0.857**		
Duplessis			**0.856**		
Fukuda			**0.839**		
Trotman			**0.798**		
Gibson	0.515		**0.791**		
Kuo			**0.752**	0.403	
Damon	0.546		**0.735**		
Porteous	0.518		**0.724**		
Koike			**0.691**		
Mond	0.51		**0.669**		
Arnold		0.48	**0.612**		
Whitney	0.405	0.402	**0.544**	0.491	
Chillingworth	0.44		**0.481**		
Fukui			**0.46**		

	FACTOR				
	1) Singularities of Differentiable Maps: Hamiltonian & Lagrangian Systems; Symplectic Geometry	**2) Singularities - Algebraic Geometry; Several Complex Variables and Analytic Spaces**	**3) Equisingularity (Topological and Analytic)**	**4) Semi-Analytic Sets and Sub-Analytic Sets**	**5)Analytic Algebras and Generalizations, Preparation Theorems; Sheaves of Differential Operators and their Modules**
Kurdyka				**0.865**	
Lojasiewicz				**0.848**	
Milman				**0.828**	
Bierstone				**0.806**	
Aroca				**0.77**	
Gabrielov	0.443			**0.723**	
Malgrange	0.505	0.417		**0.556**	
Parusinski		0.45		**0.548**	
Hironaka		0.524		**0.547**	
Brasselet				***0.392****	
Galligo				0.423	**0.739**
Granger					**0.72**
Maisonobe					**0.693**
Sabbah		0.405		0.486	**0.554**

Extraction Method: Principal Component Analysis.
Rotation Method: Varimax with Kaiser Normalization.
Rotation converged in 9 iterations.

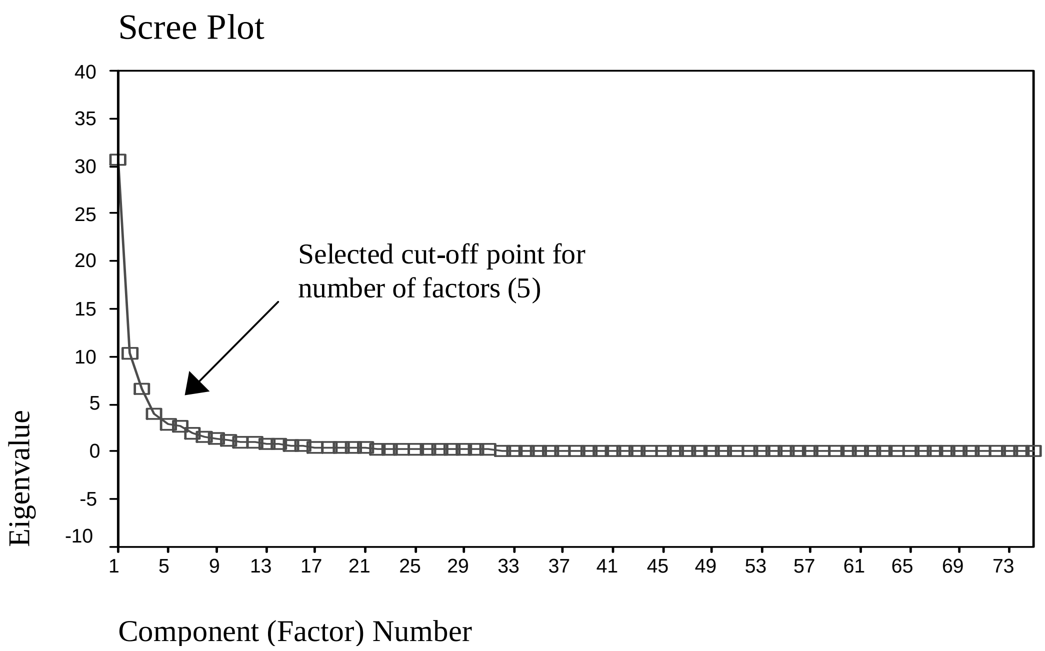

Figure 4.3. Factor analysis scree plot.

Table 4.7. SPSS FACTOR output of the total variance explained.

Total Variance Explained					
Extraction Sums of Squared Loadings			Rotation Sums of Squared Loadings		
Total	% of Variance	Cumulative %	Total	% of Variance	Cumulative %
30.727	40.970	40.970	16.194	21.593	21.593
10.253	13.671	54.641	14.384	19.179	40.772
6.693	8.924	63.565	12.057	16.076	56.848
4.055	5.407	68.972	8.391	11.189	68.036
2.833	3.777	72.749	3.535	4.713	72.749

4.9 Interpretation and validation

The interpretation and validation of the MDS map and factor analysis is the final stage of the ACA process. At this stage, the research focus shifts towards discovering "what the author clusters, factors, and map dimensions represent in terms of scholarly contributions, institutional or geographic ties, intellectual associations, and the like" (McCain, 1990a, p. 441). In this case study of the Singularity Theory community, the same types of discoveries are sought, including answers to the following research questions: What are the research topics that comprise the intellectual structure of this subject specialty in mathematics? Who are the mathematicians that have contributed to this research specialty and how are they intellectually related to one another? Can the formal cognitive aspects of an ACA help to uncover significant information about underlying "social" relationships? According to McCain (1990), a number of ACA studies "have been more or less intuitive accounts of the researchers' own subject areas, and interpretations based on personal knowledge" (p. 441).[11]

[11] An example is the ACA study carried out by White and McCain (1998) concerning authors from the field Information Science. Both authors are Information Scientists (bibliometricians) and relied on their personal knowledge of the field to interpret their ACA results.

As an outsider to Singularities research and to the general field of mathematics, my situation concerning the interpretative process is admittedly more challenging. I have acquired some personal knowledge of the Singularity Theory community due to my work at the Fields Institute and to some extent this background experience is valuable. Nevertheless, most of my interpretations rely on information gleaned from mathematics publications, review articles and data collected from the *MathSciNet* database. My interpretations are also informed to a large degree by the comments of various interviewees: Singularity Theorists who intuitively understand the intellectual structure as insiders and who agreed to talk with me about this structure during my ethnographic research period at the Isaac Newton Institute.

On the two-dimensional map of Singularity Theory research, shown in Figure 4.2, a core-periphery structure is evident in relation to the x and y map axes. Certain author nodes are located near the center (e.g., MALGRANGE, WHITNEY, THOM) and others are scattered towards the periphery (e.g., ARTAL-BARTOLO, KOIKE, MAISONOBE). This structure is both familiar and expected since most "author maps" have been successful at "showing who is central and who is peripheral to a field" (White, 1990, p. 103). Authors occupying central positions are relatively well cited and those who are located around the periphery tend to have low co-citation counts and are not as well integrated into the specialty.

Axes placed on ACA maps, particularly two-dimensional maps, are often interpreted in terms of a "primary subject dimension" (i.e., the vertical axes) and a "perceived style of work dimension" (i.e., the horizontal axes) (White, 1983, p. 311). In studies based on a "soft/hard" contrast to the research literature, or a "quantification distinction" (e.g., the literature of the social sciences), White (1983) explains that this approach is common (p. 311). Among the Singularity Theorists, it was clear that the axes were not particularly useful for distinguishing a soft versus hard or theoretical versus practical distinction. All of the authors share a similar technical approach to solving mathematical problems and all have contributed to the same general subject area. As a result, the axes shown in Figure 4.2 have not been interpreted and labeled, and have simply been added to orient the eye towards the map's central and peripheral areas. It is my understanding, based on White (1983), that "the dimensions of intellectual space as given by automatic co-citation mapping are not limited to these two [categories]" and may not be subjected to any categories at all. For this reason, attention is given in the discussion section of this chapter to the arrangement of mathematics "schools" around the axes, rather than to the axes themselves (p. 311).

The map covers the citing literature from 1974 through to 2000. The view presented is historical in nature – a "snapshot" of how this research specialty has developed up to a particular point in time and it is not representative of current mathematical developments. Yet within this specified time frame, the piling up of co-citations is significant enough to determine author relationships. As White (1990) suggests, the relationships can be observed in terms of intellectual influence, but "many relationships other than intellectual influence are reflected by citations, and some influences are not captured by citations at all" (p. 94). Often it is the case that "social structure is revealed, in the sense that authors belonging to the same organization or with personal ties will be placed close together on the map" (White, 1983, p. 311).

The first clue to the presence of inter-author relationships, based on the citation patterns alone, can be obtained from an examination of the ACA clusters. On the Singularity Theory map, shown in Figure 4.2, three cluster boundaries have been drawn with dashed lines. Each line was added according to the output of the hierarchical cluster analysis detailed in section 4.6. In most ACA studies, researchers choose "a single level for detailed analysis," therefore a cut-off point at a "high level of [the cluster] aggregation" was selected in order to present a "general image" (McCain 1990, p. 473).

Each cluster group derived from the hierarchical clustering procedure can be "interpreted as and translated into subject terms for naming or labeling purposes" (White, 1990, p. 104). Within the Singularity Theory specialty there are many subject areas of interest and all are included in the American Mathematical Society (AMS) Classification system (see AMS class code structure in Appendix E). My aim in this study was not to mirror the full AMS classification system by determining the ‘true’ number of clusters in Singularities research, but to present a general image based on its three main (broad) subjects. Figure 4.4 illustrates how the AMS system was used to help determine and label the subject areas, so that the authors attributed to the clusters may be identified as the key topic area contributors (see Figure 4.4).

Papers that are published and submitted to the AMS review are assigned primary and secondary classification codes by the individual authors. In *MathSci* the classification codes are indexed in a tagged field and can be ranked with the “OR” command in Dialog™. As shown in Figure 4.4, the 22 authors grouped in cluster A were used as input to Dialog™ *MathSci* (AU=MILMAN, P OR AU=GRANGER, J? etc). A ranking of the AMS descriptors (RANK DC) associated with this list of 22 names generated an output of the following top 3 codes: 1) 14B05 (Singularities--Algebraic Geometry), 2) 32B20 (Semianalytic and Subanalytic Sets--Several Complex Variables and Analytic Spaces), and 3) 32C40 (Singularities-- Several Complex Variables and Analytic Spaces). Note that for each author group, the RANK command produced overlapping results: Cluster A and B share codes 14B05 and 32C40 and Clusters B and C share the code 58C27. A certain degree of subject overlap was expected, given that the cluster boundaries fit together somewhat like a puzzle.

With the help of a mathematician informant (TROTMAN), the descriptor codes were then verified and translated into broad topic labels: Cluster A – REAL AND COMPLEX ANALYTIC GEOMETRY, Cluster B – TOPOLOGY OF COMPLEX ALGEBRAIC SINGULARITIES and Cluster C – SINGULARITIES OF DIFFERENTIABLE MAPS. The authors belonging to one of the three clusters, A, B, and C, are fixed inside one cluster boundary, even though it is understood that they may also have cross-boundary interests. Dashed lines have been used specifically to convey the idea that the boundaries are permeable. The authors enclosed within a cluster are considered to be “intellectually similar” because they are recognized as having contributed more to the named topic than in any other and have also been cited in association with that topic and neighbouring authors more often by their colleagues.

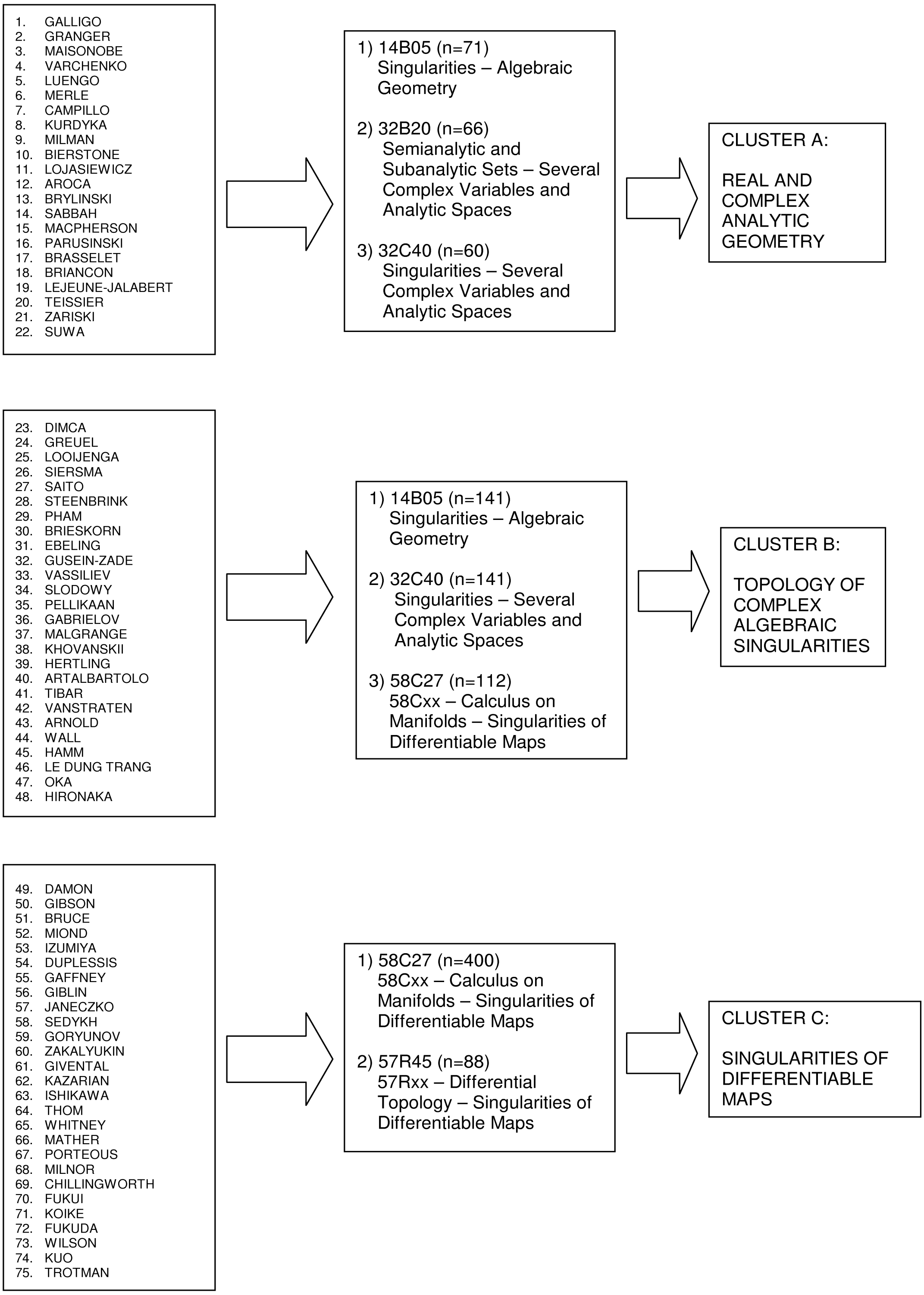

Figure 4.4. Cluster interpretation using AMS Classification.

BRUCE, MOND, GIBLIN, GIBSON, PORTEOUS and DAMON, for example, are intellectually similar because their common research interests are relevant to the topic “Singularities of differentiable maps.” All have been recognized for their contributions to this subject due to the piling up of co-citations made by their colleagues. TROTMAN, WILSON, FUKUI, KUO and KOIKE are also perceived as being intellectually similar, due to their work on stratified sets. There is yet another grouping of authors on the left-hand side of the map, which includes SABBAH, MAISONOBE, BRASSELET, BRIANCON and MACPHERSON. These authors are linked by their contributions to the study of sheaves of differential operators and their modules, stratifications, constructible sheaves and intersection cohomology.

For the most part, the subject groupings make sense; however, a few inaccuracies appear with respect to the proximity of the author nodes. HIRONAKA and BRIANCON, for instance, are mapped in close proximity (in fact their nodes seem to almost intersect) and at a glance it would be easy to assume their “closeness” is more significant than that of every other paired author on the map. This is not the case. With these two author nodes, there is evidence that the map has been distorted a little by the two-dimensional solution. On a three dimensional map, their true relationship to one another might have been better represented in that BRIANCON would have been pulled upward or outward into three-dimensional space, placing him in a more peripheral position apart from HIRONAKA.

At the core of the Singularity Theory map, near the crossing of the x and y axes, nine authors have been highlighted in bold font due to their relatively high co-citation rates: ARNOLD, MILNOR, MATHER, WHITNEY, THOM, BRIESKORN, WALL, HIRONAKA and ZARISKI. These authors, from both the perspectives of a bibliometrician and a citer, are viewed as central figures. A review of their individual research profiles helps to elucidate the meaning behind their intellectual positions and explain why the other Singularity theorists tend to cite them often.

According to Durfee (1999), BRIESKORN’s and MILNOR’s contributions are rooted in the “wonderful interaction of topology and singularity theory, which began to flower in the 1960s” (p.

417). In 1965, BRIESKORN "made the astonishing discovery that certain simple equations define sets of complex zeros whose intersection with a sphere around a singular point is one of the exotic spheres of John MILNOR – topologically a sphere, but with a different differentiable structure" (Trotman, 1999, p.867). MILNOR later proved "a fibration theorem" that was considered "fundamental for much subsequent work" (Durfee, 1999, p. 427). Durfee notes that "this theorem together with its consequences first appeared in [an] unpublished preprint, which dealt exclusively with isolated singularities. (A full account of this work was later published in the book [*Singular Points of Complex Hypersurfaces*], where results were generalized to non-isolated singularities)" (p. 427).

Oscar ZARISKI is recognized for developing "an algebraic theory of singularities of complex algebraic varieties" and for proposing "a theory of equisingularity" (Trotman, 1999, p. 866). Trotman explains that equisingularity "concerns the comparison of different notions that make precise when two singularities are 'the same' (p. 866). HIRONAKA's significant contribution, based on ZARISKI's influence was to "generalize what ZARISKI had done to prove for dimension =3 the theorem concerning the resolution of singularities on an algebraic variety. HIRONAKA proved the results in any dimension," and was awarded the Fields Medal for his mathematical achievement (Fields Medal and Rolf Nevanlinna Prizes, 2002).

Due to the research of WHITNEY and THOM the concept of "stratification" has become "central to Singularity Theory: one decomposes an algebraic variety into smooth varieties, defined by algebraic invariants, around which the topology remains locally constant" (Trotman, 1999, p. 866). WHITNEY was senior to THOM and was close to the end of his career when THOM's career was beginning; however, there was a certain amount of research influence between them. For example, in 1955, WHITNEY, "proved that for differentiable mappings of the plane into the plane there are only two types of stable singularities, the fold and the cusp, and he showed that a generic mapping has only singularities of these types concerning the singularities of differentiable applications" (p. 866). A year later, THOM "proposed a theory of singularities of differentiable mappings between manifolds. He emphasized the notions of genericity and stability, defined and exploited transversality and studied the homology of critical sets in order to obtain global topological invariants" (Trotman, p. 866).

MATHER's research, which was influential during the late 1960s and early 1970s in the U.S., England and in Japan, led to the solution of a particular problem arising from the work of THOM. MATHER "proved that almost all mappings in all dimensions are topologically stable. In order to do this, he developed a rigorous version of the theory of stratified sets and mappings of THOM, proving in particular that the local topology remains constant along the strata of a WHITNEY stratification" (Trotman, 1999, p. 867).

Vladimir ARNOLD is recognized for having "pursued the classification of singularities of mappings" and for making "prodigious calculations in a series of articles from 1968 to 1984" (Trotman, 1999, p. 866). With his students in Moscow, he "obtained an impressive collection of results, using singularity theory to generalize the caustics of Huygens and Newton, as well as the mathematics of Lagrange's mechanics. They produced lists of various types of singularities, for example, singularities of functions and generic projections of surfaces" (p. 866).

WALL, another author occupying a central position on the Singularity Theory map, has been involved in this specialty for many years. Although recognized mainly for his work in the area of geometric topology, J. J. O'Connor and E. F. Robertson indicate that "he has made substantial contributions to the study of singularities, especially isolated singularities, of differentiable maps and algebraic varieties" (The MacTutor History of Mathematics archive. Charles Terence Clegg Wall. 2002)

Among all the authors included on the map, MILNOR, HIRONAKA, and THOM, are distinctive because they have one achievement in common: all were awarded the prestigious Fields

Medal.[12] THOM received the award in 1958 for inventing and developing the theory of cobordism in algebraic topology. MILNOR was a medal recipient in 1962 for "proving that a 7-dimensional sphere can have several different structures" (note: this led to the creation of the field of differential topology). As noted previously, HIRONAKA also won a medal in 1970 for "generalizing the work of Zariski and proving a theorem concerning the resolution of singularities on an algebraic variety" (Fields Medal and Rolf Nevanlinna Prizes, 2002). Singularity Theory research has therefore benefited from the work of great mathematicians – mathematicians who have not only made significant contributions to this specialty but to other related topics in algebraic geometry, topology and catastrophe theory, and to mathematics research in general.

To complement the results of the CLUSTER map, the FACTOR procedure provides yet another type of 'birds-eye view' of Singularity theory – a view without (cluster) boundaries and one that indicates which of the mathematicians have 'traveled' frequently to different regions inside their own 'country.' The country metaphor is introduced here given that one of my interview informants indicated to me that the Singularity Theory research specialty "is so large... I don't know how...[to explain it]. It's like if you are in a big country and you change direction in that country, but you are still in that same country" (LE DUNG TRANG).[13]

Traditionally, the results of an ACA FACTOR procedure (i.e., author loadings) are used to name a factor according to a specific topic or subject. In the case of Singularity Theory research, it was difficult to accurately label (interpret) each factor because most of the authors generated low correlation values (i.e., between .4 and .5). In light of this, McCain's (1990a) technical advice was taken into consideration and a decision was made to focus on the higher correlation values (i.e., values closer to .7 and .8) for the labeling process. Each factor label, shown in Table 6 was

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[12] The Fields Medal was named in honour of Professor J. C. Fields, a Canadian Mathematician who was the secretary of the first International Congress of Mathematicians held at the University of Toronto, Ontario in 1924.

[13] In all instances where an author is identified with respect to a quotation or piece of transcript, I have been granted permission by that author to use his/her name.

determined as a result of an author search (AU=A or B or C etc.) and a ranked classification code search (RANK DC) in Dialog™ *MathSci* for the research topics that the grouped authors have in common with one another. The top codes that came up as a result of the ranking process were used specifically for labeling. Given the changing history of the AMS class code system it is important to note that authors who published predominantly during the 1950s and 1960s may have assigned older codes that differ from the ones used in more recent years. This means that the labels were biased in favor of the more current coding system.

To some degree, the factor labels are reasonably descriptive. For instance, the link between KURDYKA, LOJASIWEICZ, BIERSTONE, MILMAN, etc. (i.e., all authors loading under Factor 3) is meaningful because the topic "Semi-analytic sets and Sub-analytic sets" appropriately describes the grouped authors' shared research interests. On the other hand, there is some uncertainty as to whether or not the label for Factor 1 is fitting: "Singularities of differentiable maps." The grouped authors associated with this factor are not entirely similar, and may not be considered closely connected to one another on the basis of this topic definition alone.[14] Perhaps the topic label is too general or vague as a descriptor, but it is also likely that there is another underlying factor altogether that is contributing to their grouping. With this type of exploratory analysis it is difficult to know for certain what the underlying factor is.

THOM and WHITNEY, who are well recognized in terms of their shared research interests and known for their influence on one another, do not appear together under one specific factor. THOM's name loads predominantly on Factor 1(value=0.566), while WHITNEY is associated predominantly with Factor 3 (value=0.544). Nevertheless, the two mathematicians still share a common link to one another with correlation measures under Factor 1 and Factor 2. Overall, the correlation coefficients for both authors are not particularly high (ie., ranging in values from .402 to

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[14] This information was related to me by my key informant (BIERSTONE), who has a more intuitive understanding of who the mathematicians are and the potential relationships between them because of the work that they have done over the years.

.566). What the measures seem to suggest is that the two mathematicians are similar in terms of their general impact on the development of the overall research specialty, as opposed to being closely aligned with one another at the juncture of one subject. During a conversation with one of my informants (BIERSTONE), I was told that WHITNEY and THOM (mainly due to his theoretical contributions) could be considered the most important mathematicians to the development of Singularities research. Based on this comment, my interpretation is that the factor loadings seem to be 'mirroring' to some degree the two authors' widely recognized influential positions.

SIERSMA and LEJUENE-JALABERT, like WHITNEY and THOM, are additional authors with more than one factor loading. Yet based on the history of their research, which is not as significant as WHITNEY's or THOM's, the interpretation of their positions is likely that they have not developed fixed cognitive identities. These authors are not closely aligned with one particular topic, thus may be viewed as 'territory travelers': mathematicians who have taken an interest in a variety of topics within the whole of Singularity Theory research. During a one-to-one interview with SIERSMA, I specifically asked:

> *I. Is it true that you have focused on the same research area… that you have done most of your research in Singularity Theory?*
>
> R. In my case, I have focused on this [re: isolated singularities] and I've been in the field… uhm… I've also of made of course some choices of what to do in the field, not always on that…
>
> *I. Yes, but would you say that your work builds on what you have done on the past?*
>
> R. Yes, of course, but at certain moments you get some new ideas and you focus on other things…

By contrast, other mathematicians, like LE DUNG TRANG, HAMM, WILSON, and GAFFNEY (to name a few) have very high correlation values associated with one factor (over .80). LE DUNG TRANG's profile in the factoring routine, for example, indicates that he is perhaps one of the more cognitively 'identifiable' authors in Singularity Theory. By this, I mean that his colleagues tend to cite his work frequently in association with topics linked to the Singularities of differentiable maps. He seems to be perceived by his colleagues as having demonstrated over time a fairly focused, in depth style of contribution. When I spoke with LE DUNG TRANG during our interview, he indicated that he has been following a “classical line” of research. He also mentioned that he has been involved in “other things,” but when I asked specifically whether he would identify himself mainly as a Singularity Theorist, his response was the following: “Well ya. Surely. I’ve made most of my career in this field, from the start.”

In sum, the factor analysis is useful in that it provides a general picture of who the research ‘travelers’ are within the intellectual structure of Singularity Theory and who are not. Some authors appear to have crystallized cognitive identities in relation to one topic area, while others have been cited in connection with a few different topics. A factor analysis provides certain clues about the relationships among the authors; but, because it is exploratory in nature an important question is still left open for discussion: Do the author groupings seen in both the factor and cluster results reflect only a measure of subject similarity, or is there also another “factor,” perhaps a social factor influencing the observed patterns?

4.10 Discussion

The question of whether or not bibliometric studies can reveal information about underlying social relationships among authors is not new and has been raised previously by Whitley (1974), Edge (1977), White (1990), Lievrouw (1990), and more recently by White, Nazer and Wellman (2004). Whitley’s (1974) idea was that “there are two types of institutionalization in science: two aspects of scientific activity which are patterned to varying degrees, the cognitive and the social” (p. 71). The two types, he explained, need not be separated since it can be “fruitful to analyse the differences in the extent of coherence and cohesion between intellectual products, their mode of production, and the social circumstances surrounding their production, evaluation and revision” (p. 71). Whitley was not a co-citationist; however, his view of the two types of institutionalization in science is still of interest to scholars, particularly among those who use bibliometric and sociometric techniques to examine both the social and intellectual structure of scholarly communities. White, Wellman and Nazer (2004) for instance, propose that "citers and citees often have interpersonal as well as intellectual ties" even though "evidence for this belief has been rather meager" in past years (p. 111). One of the main reasons for this is that "social networks researchers have lacked bibliometric data (e.g., pairwise citation counts from online databases) and citation analysts have lacked sociometric data (e.g., pairwise measures of acquaintanceship)" (p. 111). Recently, the joint authors asked: "does citation reflect social structure?" in a study that explored the links between the inter-citation data and social and communication data of a group of 16 international human development scholars from different disciplines. One of their findings was that "scholars who cited each other (interciters) communicated more than non-interciters" (p. 111).

In the present study, the extent to which the Singularity Theorists are both intellectually and socially connected is also of interest. The aim, based on the conceptual framework, illustrated in Chapter 1, is to demonstrate that this mathematics subject is not just a specialized area of research (an intellectual structure), but also an invisible college (a social process). Author co-citation analysis is used to help investigate the meaning of this concept.

Edge (1977; 1979) a noted "non co-citationist" has argued that the main drawback of citation studies (including ACA) is that they deal only with *formal* levels of scientific communication and omit other aspects of influence or impact related to *informal* communication. According to Edge (1977), one should not assume that "the citing of B and C together by A implies that in *A's perspective, the work of B and of C are related*" because "strictly speaking" a co-citation map only reflects the "*perceptions of authors*" (p. 242, original emphasis). His strict advice to co-citationists also was to not act "as if co-citations B and C were evidence that *B and C are related by communication ties*" or assume that "the authors 'clustered' by co-citation are *interacting groups*" (p. 242, original emphasis).

In defense of ACA, White (1990) explains that the "a co-citationist would not hold that the work of B and C are necessarily related merely because A cites them in the same work. It is the *piling* up of co-citations—the fact that their count over time exceeds a certain threshold that indicates a relationship" (p. 96, original emphasis). Moreover, co-citationists do not generally make a habit of assuming communication ties or the existence of personal interaction among clustered authors: "no one believes that cited authors in clusters are necessarily in personal communication", but "as it turns out, heavily co-cited authors often do have personal communication ties" (p. 96). In effect, the conscientious researcher who uses ACA is one who also "incorporates traditional subjective methods by looking for communication ties among authors after clustering them—for example, by interviewing the real people involved" (p. 98).

Lievrouw's (1990) article provides further support for White's (1990) defense. She agrees that "bibliometric techniques are important because they give analysts the ability to crystallize abstract ideas into concrete forms..…By allowing [researchers] to construct 'maps' of documents, bibliometrics gives a systematic glimpse of the communication acts that produced the documents in the first place" (pp. 67-68). A "systematic glimpse" refers to the idea that different levels of informal communication may be brought to light through further investigation. ACA maps are most useful then if bibliometricians consider them to be navigational tools. As Lievrouw indicates: "we know that [a] territory cannot easily be navigated without a map and that [a] map is meaningless unless the traveler can interpret it" (p. 69). With a bibliometric map in hand, the researcher can set out to explore underlying facts concerning past and present relationships within a research community. The map can be a traveling aid in that scholarly territory, where "fieldwork techniques [i.e., interviews; participant observation] typical of ethnographic studies of communication" may be used to gain more "interpretive power" (Lievrouw, p. 68).

Throughout the ethnographic period of this study, my own ACA map of Singularity Theory became an important navigational tool. While I was interviewing the mathematicians at the Isaac Newton Institute, I presented each informant with a copy and invited them to make comments. Most of the interviewees were genuinely interested and provided me with detailed responses. Some were even curious about the bibliometric procedure and wanted to discuss the nature of citations in mathematics. LE DUNG TRANG, for example, said the following:

> the only thing which I would agree with is that when you cite people it could be either because they are similar... or because they cover roughly speaking disjointed ideas... So its a big danger if you do it like that because you just mixed up two precisely opposite situations of their similarities.

He also put forth the idea that

> citations are flat… flat [as in] without real weight. Of course it is easier [to use them], but it makes a distortion of the real importance of things.

As we proceeded to discuss his idea of citation “flatness," he explained further that MILNOR’s work published in the mid-1960s was cited frequently and still is today because it is foundational:

> [When a mathematician writes about] the Milnor number [it usually becomes] an obvious thing to quote Milnor, [but in a sense] it is not so meaningful like something else…. It is good to record the foundations and the ideas come from there and I don’t say the contrary, but I think you have to be careful... A reference to the foundations is different from a reference to a deeper link [that occurs between the citing and cited work and between other cited documents in a bibliography].

LE DUNG TRANG’s comments open up an important point of discussion given their obvious connection to the argument that was made by Edge (1977). Again, it was Edge (1977) who said that one should not assume that “the citing of B and C together by A implies that *in A’s perspective, the work of B and of C are related*" (p. 242, original emphasis). If we focus on the fact that LE DUNG TRANG’s explanation refers to MILNOR and the repeated citing of MILNOR’s works, an argument can be made that fits more in line with White’s (1990) view of ACA. Over time, a piling up of citations to MILNOR has occurred because mathematicians agree that it is important to recognize the foundational aspect of his research. As a result, MILNOR occupies a central position on the Singularity Theory map. This means that MILNOR’s position does not have to be interpreted in terms of a deep intellectual connection to the work of other authors in proximity – the extent to which MILNOR and author A or MILNOR and author B are similar is not considered here. The ACA interpretative process in this study gives more consideration to the background history of

MILNOR's research in Singularity Theory and his overall contribution, which has made him a well-recognized mathematician.

With respect to my interview with LE DUNG TRANG, it is interesting to note also that when I referred him to his author position (by pointing to the map), he indicated a nod of general agreement and gave the following statement of recognition:

All the names I see beside me... I know who they are.

His recognition led to additional comments about his collegial relationships, both intellectual and social:

> I would surely be linked to Teissier … and Hamm… actually we've worked together... A third of my work is really intimately linked to Milnor... and the other part is .... [pause]… for instance, I have used quite a lot of Whitney theory. I didn't prove anything in Whitney theory, but I have used it ever since I have worked with Teissier.

In another interview with MOND, the ACA map initiated a conversation about a past working relationship with DAMON:

> *I. I've got you clustered with these names here, sort of in the top left corner of the map, and I'm wondering if you see yourself or your work as being intellectually similar to these other people?*
>
> R. Well, with Damon certainly….I've recently worked with him… I'm very much influenced by Damon, by some joint work we did together, and I guess that we wrote a paper together which must have come out about 1991, which I think was influential to both of us….
>
> *I. So where did you first meet one another?*
>
> R. In Liverpool. I was a graduate student from '79 - '82, and he visited Liverpool for a year during my first year there. It was unfortunately the year in which I couldn't do anything, so I didn't get very much done, except that we kind of made friends…. He's a very easy guy to get along with….

Likewise, my interview with SLODOWY provided additional insight into the history of his intellectual and social connections to THOM, BRIESKORN, SAITO and EBELING:

> *I. So who was your Ph.D. thesis advisor?*
>
> R. Oh yes... that's interesting. The point is that when I was studying I was somewhat independent. The German system was very free, so we could choose what we wanted to do. Well, you had to do some examinations at times, but otherwise you were rather free. So I did a diploma in group theory with [supervisor name]...

maybe you know the name? So that is where I learned group theory. On the other hand, at university I got interested in the applications of Singularity and so I got to learn the work of Arnold and Brieskorn... and there was Thom... and I just studied it for myself. I taught myself... that subject to myself and I saw that I could work in the field.

I. So you read the books of Brieskorn and Thom?

R. Yes... yes.. yes... I did this on my own for example.

I. And you had never met them or had contact with them?

R. No, no. But then after the diploma... [supervisor] was a very open person and he said well if you don't want to go on in group theory, why don't you go and visit Thom!

I. Ah... Ok...

R. So he organized a one year stay at IHES... for me to be near Thom. So I got to know Thom ... to talk to him on a very personal level... not very technical and it was interesting because it was more on a philosophical level, but it also taught me ... [pause] ….well I cannot follow Thom's ideas because they are too philosophical.. He was too much of an abstract thinker... meta meta you know… so that is why I found it at this time in Paris very interesting to see the diagrams of the Singularity side and the group theory side and I said... Oh... I don't understand that, and there were not many people understanding that so that is why I wanted to understand [it]. Fortunately I got an assistant position in Germany in Southern Germany and these people were not interested to tell me: 'Do this as a PhD.' They said: 'Maybe this guy can do it himself,' so I in some sense, from the developments of the time I followed the subject myself to work on Brieskorn's theorem……

I. And did you have some occasion to meet with Brieskorn?

R. At the end, when the main steps of the proof were through he invited me to Bonn and I gave a talk and he said: 'Oh! You should continue that!,'and that was a sign to my home university in Southern Germany. Then they said: 'Oh you do your PHD on that!' So they gave the green light. Before they had no idea of what I should work on...[pause] .. well,` I shouldn't say no idea, they just left me thinking about my problems and then when Brieskorn said 'Oh that's very interesting' and I couldn't tell anymore about how to do it ... and you should do it...... you should do it.... and that's how it became my PhD.. So in that sense I didn't have a direct supervisor of a PhD

I. But you had people who were obviously influential, namely Brieskorn...

R. Yes and the other one, that guy called [name] ... gave me the freedom to work on this...

I. So he was a kind of advisor?

R. Yeah he was the formal advisor, but finally Brieskorn was the external referee.

I. It is interesting that Brieskorn had this influence on you. Do you agree then with your position on the map?

R. Let me see that again….

I. Your name is here and Professor Brieskorn is here. Saito is here…

R. Yes. I mentioned Saito because he is much interested in similar questions nowadays and also at that time…Where is Arnold?

I. Further up…

R. Well yes there are different interests….but on the other hand….[pause]…Where am I?

I. Brieskorn is in this cluster and you are here…

R. Oh… there… there… Ebeling… Ebeling is a student of Brieskorn and we had much contact. I would say that I've referred later to Ebeling, but maybe conversely Ebeling has referred more to me. Definitely he was at the Brieskorn school and after… after Southern Germany and after the PhD, I got an assistant position at Bonn University. Not under Brieskorn but in the neighborhood of Brieskorn, so I was participating in Brieskorn seminars then in 1978, so I had contact for 5 years with Ebeling.

I. So this [the mapped position] makes sense?

R. It makes sense, yes.

Clearly, what this transcribed interview data shows is that even if the static and formal quality of an ACA map is not suitable for making assumptions about informal communication, it can still be used to uncover facts about social histories: facts that might otherwise not have been considered. Just as a sailor uses a chart to navigate the ocean, the interviewer too can rely on a map to navigate his/her way across a sea of background stories. With respect to my own interview experiences and the stories related to me by my mathematician informants, I found that the details uncovered tended to coincide well with the observed patterns of co-citedness. The Singularity Theorists who were linked together by their citing colleagues (i.e., the piling up of co-citations) often commented on more significant relations with each other beyond the citation level: relations based on informal interactions as research advisors, university colleagues and friends.

Some of the social connections among the Singularity Theorists were developed during international meetings (e.g., MOND and DAMON met during DAMON's visit to Liverpool in the late 1970s), but many were also developed within the context of nationally based "schools." A "school" in mathematics can be difficult to pinpoint, but if given a definition it may be described as a group of mathematicians who work within the same mathematics department and/or cluster of university departments located near one another in the same country. Members of a school have the same research interests, attend research seminars and engage in frequent informal meetings (e.g., casual discussions in front of a blackboard) because they live and work in close proximity. A "school" is typically formed due to the research influences of a formidable leader, but if not, it may include at least one significant senior researcher and work processes based on an identifiable intellectual style.

In conversation with TROTMAN, I discovered that the international Singularity Theory community is rooted in a few national groups that might be considered mathematics "schools." For instance, there is a "French School," which developed due to the influence of TEISSIER, LE DUNG TRANG, PHAM, HIRONAKA and THOM. This school currently includes PHAM's students, BRIANCON, MAISONOBE, GALLIGO and GRANGER. There is also a Russian School, known mainly as the "Arnold School" in Moscow, which is quite large and has extended to Paris France, where ARNOLD currently has another appointment. ARNOLD'S students include GORYUNOV, VASSILIEV, GUSEIN-ZADE and GIVENTAL. In the United States, the mathematicians influenced by MATHER were GAFFNEY, DAMON and WILSON. The "German School" descended from BRIESKORN, and included HAMM, GREUEL, EBELING and SLODOWY. The pupils of Nicolas Kuiper in Holland were SIERSMA, LOOIJENGA, STEENBRINK, VAN STRATEN and PELLIKAAN. Currently, there is a "Japanese School" in Real Singularities that is thriving and has descended from the work of FUKUDA who was in Paris with THOM in the early 70's. KOIKE, FUKUI, IZUMIYA are part of this Japanese group. In England, WALL has had a big influence, and some of his students were DUPLESSIS, TROTMAN, BRUCE, GIBLIN, and MOND. Also, the "Krakow School" in Poland was led by LOJASIEWICZ and involved quite a few students, including

DIMCA (personal communication, April 11, 2000). Figure 4.5 marks the approximate location of these mathematics “schools” relative to the axes on the ACA map (see Figure 4.5).

Various “schools” in mathematics tend to be strongly affected by the national research culture in which they were developed. The French school in mathematics, for instance, has a different meaning than the Russian school. BIERSTONE explained that

> when you speak about Arnold’s School [in Russia], then it’s really a school in the traditional sense of the word school: a group of pupils who learn under the master. But when you talk about the French school, you don’t mean that at all. What you really mean is the French “style” of doing mathematics.

In particular,

> Arnold’s school is distinguished by the fact that he himself was such a powerful personality, not only a powerful intellectual figure in mathematics, but a powerful social personality in the organization of Russian mathematics…. and he ran a seminar which had a huge participation and brought many people together every week to spend, you know, six or seven hours to talk about specific problems … so that’s a school that was given a structure by the force of his own personality.

BIERSTONE’s further reflection on the term ‘school’ was that he could not be sure

> it would be really correct to speak about the Dutch school of Singularity Theorists, because it was too small a group to really constitute a kind of identifiable intellectual style, like you could attribute to the French.

My comment following this statement was that perhaps in Singularity Theory there is really only one traditional school, namely the “Arnold school.” I also commented on the notion of social circles in mathematics: “suppose we use this term instead to refer to some of the nationally based research groups?” BIERSTONE agreed that the term social circle might be more meaningful, because in the traditional sense of the word “school” there is usually a central figure who represents the main intellectual focus.

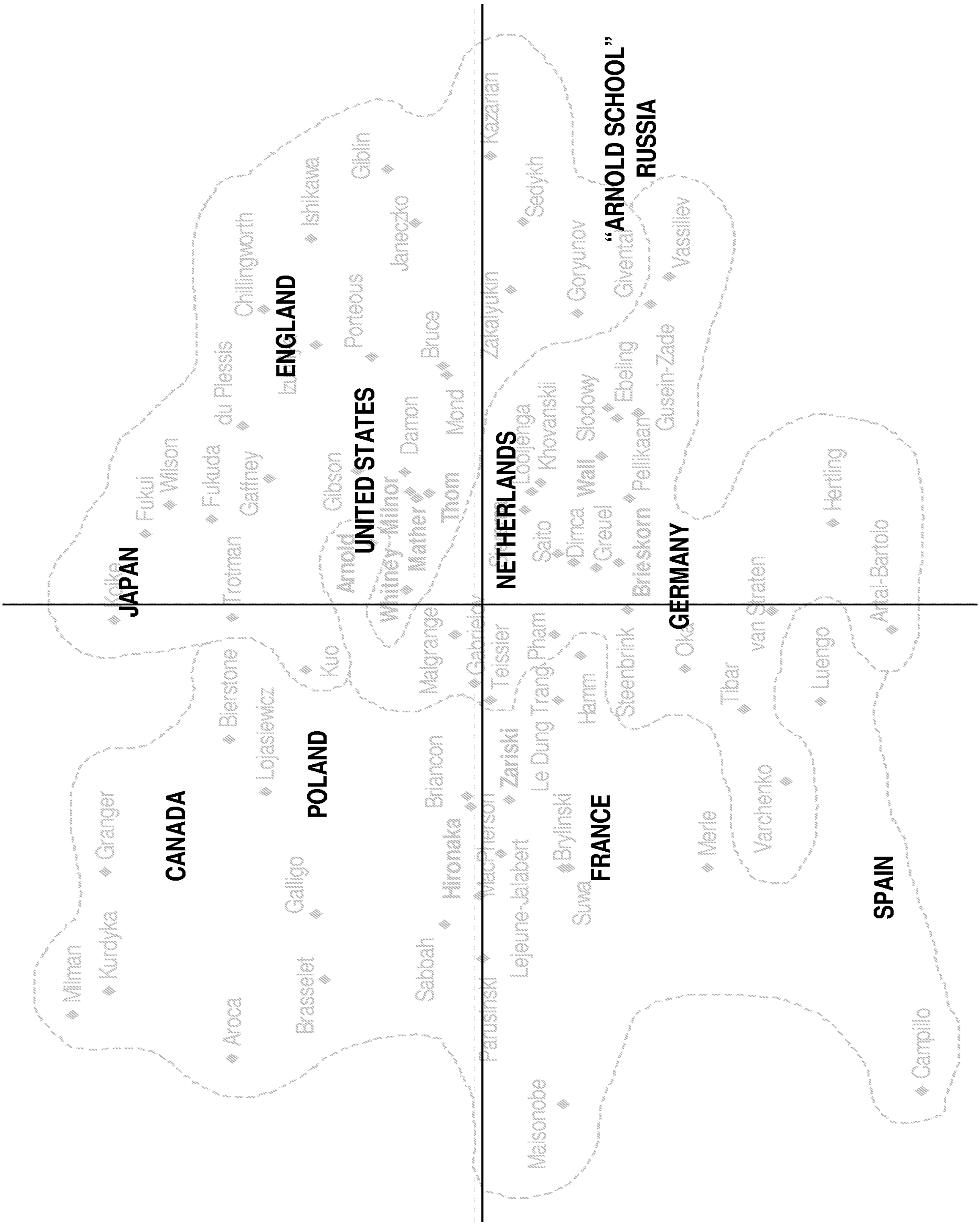

Figure 4.5. National research groups or "schools" in Singularity Theory (1974-2000).

WALL’s relationship to the social circle of mathematicians that he worked with is of interest here since he

> influenced a lot of young in England to go into Singularity Theory, even though [his] main contributions, his own main contributions are not in Singularity Theory. They were in topology, before he went into Singularity Theory. But he did influence a lot of young English people to get into the subject and those were the people that formed these Singularity groups in places like Liverpool. But certainly it was not a school again with the same kind of, you know… both intellectual and social structure that Arnold’s school had [BIERSTONE].

In my interview with VASSILIEV, a former student of ARNOLD, I obtained some valuable insight into ARNOLD’s personality and the role that he played as a school leader in Moscow:

> *I. Arnold has a very close group of people, a school of people….*
>
> R. A very great school! About 50 people… Many people here are direct students of Arnold … but now he also has another school in France. …
>
> [*VASSILIEV names several mathematicians*]
>
> *I. What are the qualities or characteristics that have contributed to him becoming a leader?*
>
> R. Qualities?
>
> *I. He must have influenced many people…*
>
> R. Yes, yes very much! It’s a fine question. Arnold is a very influential person and he knows very much. He's interested very much actually in everything. I think that when he sees that somebody says some words that he doesn't understand, then he considers himself challenged and tries to understand. [If there are some] people who can explain [what he wants to understand] he invites them to a seminar and so he tries to understand it.
>
> *I. Yes. Are his lectures very lively?*
>
> R. Yes. He is of course, but even if somebody else is delivering [the lecture] he will sit in, … and then if there is a situation which nobody does understand, immediately he begins to say "What does it mean?" and "What form would [inaudible]?"
>
> *I. So even if he's not the speaker he's very interactive?*
>
> R. Yes, yes, yes, very much. His presence is very important… even if he doesn't speak himself. Also he writes books and texts which everybody must read and ….comment on…. [inaudible] ….He has written interesting problems but didn't answer…. [inaudible]… What is happening in his absence I would

consider as a sign of an energy crisis…

[*VASSILIEV comments on Arnold's absence from the 2000 Singularity Theory program at the Newton Institute*]

I. *There's an energetic crisis? A crisis in energy with him being absent right now?*

R. Yes, yes, yes. He is absent... if he is absent … [inaudible]

I. *Your perception is that if Arnold were here, he would participate in the seminars?*

R. Yes, yes. And he... I think he would join them and establish good relations …

I. *He would be promoting interaction?*

R. Yes.

KAZARIAN, another former student of ARNOLD, was also instrumental in describing what it was like to be part of the Moscow school:

I. … *maybe you could tell me something about his [Arnold's] philosophy of teaching and philosophy of doing mathematics. Is there something distinct or special about doing mathematics, within the Russian community in comparison to other mathematicians?…*

R. Yes, well I suppose you read some of Arnold's papers on this subject … about mathematics.

I. *Yes.. I know he has written some papers on teaching mathematics….*

R. Yes, yes…you certainly should read it. I could not repeat it, but some ideas are that the relation between the advisor and the student, it's more close in Russia.

I. *Yes?*

R. They are …you're really friends.

I. *Yes?*

R. And the situation is not formal, it's extremely informal… and so from the very beginning, the first year students who attend the seminars, ….they ask questions and they discuss, there is no problem to ask some professor about some silly question and the professor is ready to explain something and this is very usual. Students begin to work very early. And the first papers appear when they are in the second or third year.

I. *They publish papers in the second and third year?*

R. Yes, yes. They publish papers, they give talks on seminars … just like all other participants.

I. *Does Arnold co-author papers with his students?*

R. Usually not.

I. *Not usually?*

R. Usually not, but I would say that Arnold… when I was a student at that time, and some people around me, he had more time to spend on his students. Now he is busier, but at that time it was like a first paper written by the students was completely re-written by Arnold. I would claim that Arnold wrote every word of my first paper.

I. *Why was this? Was it because he helped you, or would work closely with you?*

R. Well … I proved some theorem, I got some results, I explained this to Arnold and I made the talk, but when I began to write the paper, it was awful. I didn't know how to write the paper.

I. *And you were writing in English or in Russian?*

R. Russian. Russian.

I. *So he helps you organize…?*

R. Yes the problem is not the language. The problem is to organize the thoughts.

I. *Yes. So you would do the mathematics and he would help you to organize the thoughts on paper?*

R. Yes, yes.

I. *And… but then who would have received credit? So the paper would be yours or it would be Arnold's…in the final publication?*

R. No, no. The paper is my paper.

I. *It's published with your name only?*

R. It's published with my name, of course I put some acknowledgements to Arnold, but still it's my paper. But it was really completely written by Arnold. Not exactly, I brought some draft to him, and then he wrote on the back, paper, on the back side of the paper, his comments. And the amount of these comments were 2 times longer than the volume of the paper. And his comments were about some ideas how to write papers. Some citations of some Russian writers, some jokes and so on, and so on…. informal. Then I re-wrote the paper according to his remarks and then he made the same things. So it was several times, about 3 times I re-wrote the paper and he was still not satisfied.

I. *So, in…*

R. And this was like that, this is like that, it happens in many schools in Moscow. The same thing told by Arnold is that his first paper was written by Kolmogorov.

I. *And Kolmogorov was his advisor?*

R. Kolmogorov was his advisor and so on.

I. *So it's a kind of tradition… it's common in the Russian school. So then what happened to you afterwards? Then you learned from him how to construct a paper, and…*

R. Yes, he..

I. *….and now do you still send him your papers and discuss?*

R. Yes, yes, sure, but yes, he told me that he usually makes things like this with his students, but only once.

ARNOLD's energetic personality as a seminar leader and research supervisor became evident to me in my separate interviews with VASSILIEV and KAZARIAN. Unfortunately, I did not have the opportunity talk with ARNOLD or observe his interactive work style during the Singularity Theory program at the Newton Institute. However, during my employment term as a Program Coordinator at the Fields Institute, I was a member of the scientific support committee that helped to organize the Arnoldfest conference in honour of his 60^{th} birthday. It was then that I recall attending one of his lectures and noticing his engaging seminar style. The lecture hall was completely filled (i.e., standing room only) with people eager to hear him talk, including many of his former students who had traveled from Russia and parts of Europe and North America to celebrate his mathematical contributions. Following the Arnoldfest conference, a proceedings volume was published jointly by the Fields Institute and the American Mathematical Society, which included a picture of the participants as well as some images of Vladimir Arnold, who was photographed 'in action' at the front of the lecture hall.

To conclude this discussion, it is essential now to focus on my broader understanding of Singularity Theory and its relationship to the invisible college construct illustrated in Chapter 1. The overall purpose of carrying out an ACA, including the complementary FACTOR procedure, was to establish a reasonable view of the intellectual structure of Singularity Theory: What are the topics that comprise this research area? Who are the mathematician contributors and how are they intellectually related to one another?

In terms of discovering answers to the above questions, the ACA procedure was effective, because it generated a historical ‘snapshot’ for analysis. On the ACA map we can see: a) who the central figures are (the ‘intellectual stars’) to Singularities research, b) how Singularities research has evolved into a reasonably cohesive structure (63% connectivity ratio across 2,775 unique pairs) and c) clusters of identifiable topics that have been developed by frequently recognized (co-cited) contributors. Since Singularity Theory is a fairly cohesive structure, one element that would make it an invisible college is certain: we know that it is a mature and evolving subject specialty. Nevertheless, it was noted previously that an invisible college can exist within a subject specialty, but a subject specialty is not necessarily an invisible college. How do we know then that the Singularity Theory specialty functions as an invisible college? Here the third research question becomes relevant: Can the formal cognitive aspects of an ACA help to uncover significant information about underlying informal relationships?

ACA cannot in and of itself provide a sufficient basis for assuming underlying informal social interaction; however, it can provide clues as to where the researcher might look for these details. An ACA mapping of Singularity Theory is therefore a good starting point for exploring the individual histories that make up the inner workings of an invisible college. It is a technique that can be used with other research techniques, particularly interviews, participant observation, and social network analysis, to ‘peel away’ the layers. To continue with this ‘peeling away’ process, the next chapter will now examine the patterns of collaboration in Singularity Theory and compare these patterns with the ACA. Collaborative work, both in terms of its formal and informal manifestations (i.e., co-authored publications and casual discussions) is vital to mathematics research and can provide further insight into how the Singularity Theory community functions as an invisible college.

CHAPTER 5: CO-AUTHORSHIP AND COLLABORATION IN SINGULARITY THEORY

In this chapter, a social network approach to bibliometric data is used in connection with ACA to determine if there is a relationship between the intellectual organization of Singularities research and the mathematicians' international co-authorship (collaboration) patterns. The term social network is defined as "the presence of regular patterns in relationship" between interacting units (Wasserman & Faust, 1994, p. 3). The mathematician as a social actor is examined as the key unit of interaction based on the number of times s/he has co-authored a paper with other authors from the international network. Mathematicians have different ways of collaborating with one another and not all forms of collaboration result in a co-authored paper. Consequently, information concerning collaborative behaviour among the Singularity Theorists and the types of conditions or situations leading to collaboration are also discussed in this chapter. However, co-authorship is still included as both a predominant and adequate measure of collaboration because it is "a reasonable definition of scientific acquaintance: most people who have written a paper together will know one another quite well" (Newman, 2000, p. 3).

5.1 Retrieval of co-authorship frequencies and social network illustration

Raw co-author counts for the 75(75-1)/2=2,775 Singularity Theorist pairs were retrieved from the *MathSciNet* database of the American Mathematical Society and assembled crosswise in an adjacency matrix or *sociomatrix* (Wasserman, 1994, p. 150). A portion of this matrix is illustrated in Table 5.1 (see Table 5.1). All counts were associated with either a journal article, monograph or conference paper published by members of the Singularity author set during the period of 1974 to 2000. The final count for each co-author pair was based on an incremental binary measure so that one instance of co-authorship was given a value of 1 and any subsequent instances involving the same two authors were given values of 2,3,4 etc. A non-paired result was given a value of 0.

The procedure for collecting the co-authorship data required searching first for the exact Singularity Theory author's name in the *MathSciNet* Author Database (note: this database distinguishes between homonymous names) and retrieving a full count of all publications for that author. The resulting list of published items were then examined individually for the names of co-authors to be sure that each and every instance of a co-authorship count was viewed directly in relation to a given monograph or article title. For instance, a search for the name BIERSTONE in *MathSciNet* produced a list of 38 items published from the period of 1974 to 2000 (More were before this time period, as well as after). A review of this publication list revealed that MILMAN has co-authored a paper with BIERSTONE 27 times. A count of 27 was then added to the cell of the matrix representing the adjacency pairing of these two authors.

Table 5.1. Partial matrix of co-authorship counts (n=75) in Singularity Theory (1974-2000).

	ARNOLD	AROCA	ARTALBARTOLO	BIERSTONE	BRASSELET	BRIANCON	BRIESKORN	BRUCE	BRYLINSKI	CAMPILLO	CHILLINGWORTH	DAMON	DIMCA	DUPLESSIS	EBELING	FUKUDA	FUKUI	GABRIELOV	GAFFNEY	GALLIGO	GIBLIN
ARNOLD	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
AROCA	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
ARTALBARTOLO	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0
BIERSTONE	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BRASSELET	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BRIANCON	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
BRIESKORN	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BRUCE	0	0	0	0	0	0	0	0	0	0	0	0	0	4	0	0	0	0	2	0	32
BRYLINSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
CAMPILLO	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
CHILLINGWORTH	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DAMON	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	3	0
DIMCA	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DUPLESSIS	0	0	0	0	0	0	0	4	0	0	0	0	0	0	0	0	0	0	3	0	0
EBELING	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
FUKUDA	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
FUKUI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GABRIELOV	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GAFFNEY	0	0	0	0	0	0	0	2	0	0	0	1	0	3	0	0	0	0	0	0	0
GALLIGO	0	0	0	0	0	1	0	0	0	0	0	3	0	0	0	0	0	0	0	0	0
GIBLIN	0	0	0	0	0	0	0	32	0	0	0	0	0	0	0	0	0	0	0	0	0

Once the co-authorship matrix was complete, it was then imported into the *UCINET* social network analysis program developed by Borgatti et al. (1999) and converted into a .kp file. The .kp file was used specifically as input to the *Krackplot* social network software program to generate an image of all the co-authorship ties (Krackhardt, 1995). A copy of the .kp file, including author co-ordinates and cell values, is provided in Appendix F. Figure 5.1 shows the resulting social network graph of co-authors comprised of connector lines with no arrows (a co-authorship tie is non-directional). Connector lines have been superimposed upon the ACA map of author co-ordinates for visual comparison: Do the Singularity Theorists who share the same intellectual space also produce co-authored works? Line thickness indicates which authors have the strongest or weakest co-authorship ties within the whole network structure (see Figure 5.1).

5.2 Network density

A Network density measure was calculated for the co-authorship matrix to determine the "proportion of possible lines that are actually present in the graph," or more specifically "the ratio of the number of lines present, *L*, to the maximum possible" (Wasserman, 1994, p.101). This measure can range from a value of 0, indicating no lines present, to 1, which is a complete graph with all possible lines present. With a binary matrix (only 0 or 1 values), the calculation is based on the total number of ties present divided by the number of possible ties (p. 101):

$$\Delta = \frac{L}{g\,(g\text{-}1)} = \frac{L}{g\,(g\text{-}1)} = \frac{\textit{number of lines}}{\textit{number of nodes}\,(\textit{number of nodes} - 1)}$$

When using a valued network, the density calculation differs in that it includes the sum of all the *values* attached to the ties present divided by the maximum number of possible ties (p. 143):

$$\Delta = \frac{\sum v}{g\,(g\text{-}1)} = \frac{\textit{Sum of all tie values}}{\textit{number of nodes}\,(\textit{number of nodes} - 1)}$$

Given that there are 75 author nodes in the full Singularity Theory co-author data set and a total of 562 *tie values*, the density measure for this study was calculated as follows:

$$\Delta = \frac{\sum v_k}{g(g-1)} = \frac{562}{75\,(75-1)} = \frac{562}{75(74)} = \frac{562}{5550} = 0.10$$

The resulting measure of .10 indicates that the valued graph has not reached its maximum completeness potential of 1: not all actors in this network are co-authoring with all other actors, but among certain actors, strong ties are present.

5.3 QAP Correlation

To test the degree of association or similarity between the ACA map (the observed network) and the co-authorship map (the expected network) a QAP correlation analysis was carried out using *UCINET*. The QAP procedure in *UCINET* "computes the correlation between entries of two square matrices, and assesses the frequency of the random correlations as large as actually observed" (Borgatti et al., 1999). There are two steps to the algorithm: 1) the computation of the Pearson's correlation coefficient between corresponding cells of the two data matrices and 2) a random permutation of the rows and columns of the observed network and subsequent re-computation of the correlation (Borgatti et al., 1999). The co-authorship matrix, in comparison to the observed ACA matrix, is termed the "expected" network because authors perceived as being most intellectually similar by co-citers are "expected" to be associated with one another also as co-authors. Table 5.2, presents the QAP matrix correlation output of the bivariate statistics in *UCINET*.

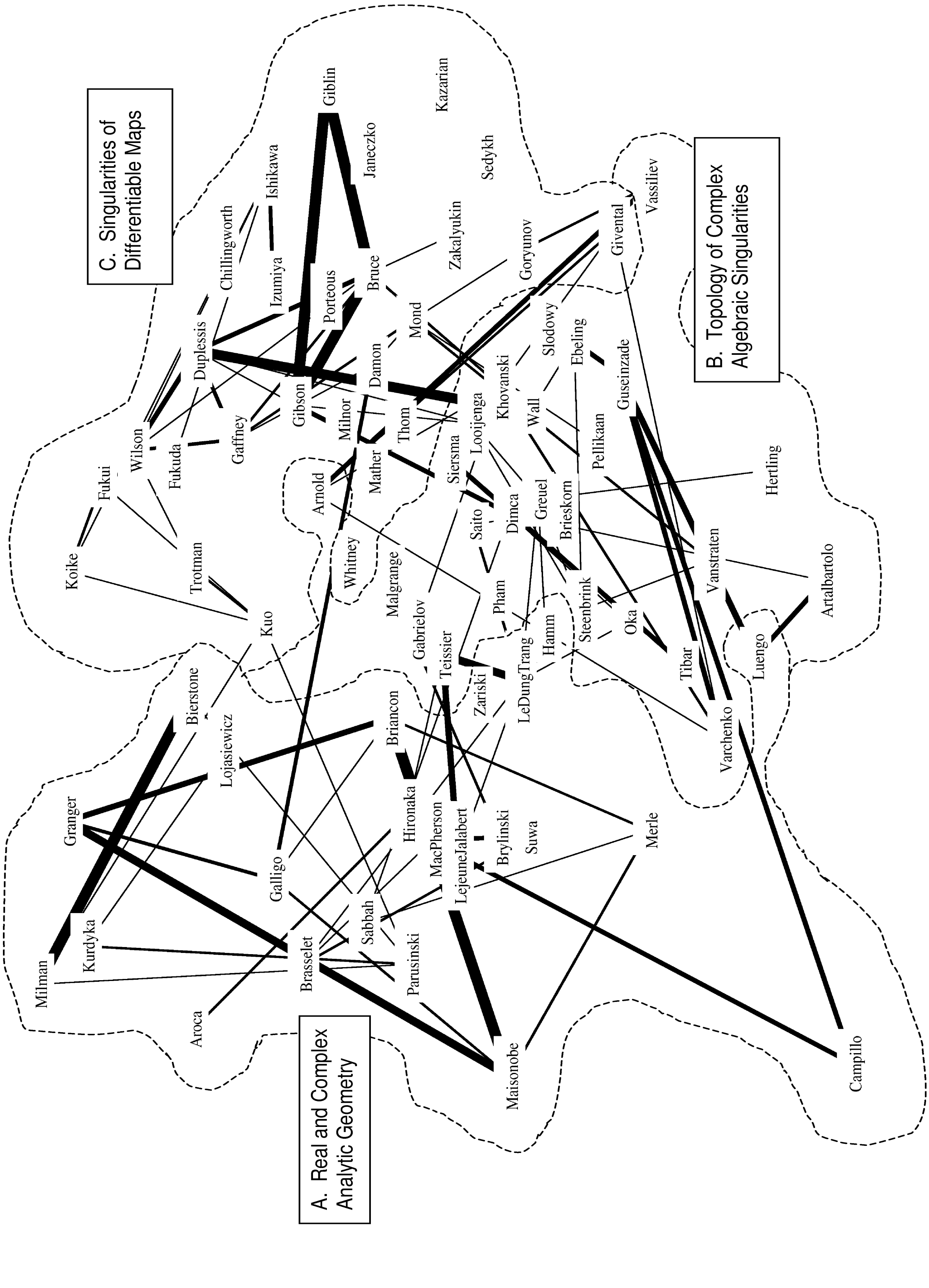

Figure 5.1. Coauthor map of 75 Singularity Theorists (1974-2000).

Table 5.2. QAP correlation of the Singularity Theorists' co-citation and co-authorship matrices.

Observed matrix:	Thesis-Cocite-EXP
Structure matrix:	Thesis-Coauthors-EXP
# of Permutations:	2500
Random seed:	977

Bivariate Statistics

		1 Value	2 Signif	3 Avg	4 SD	5 P(Large)	6 P(Small)	7 NPerm
1	Pearson Correlation:	0.060	0.016	-0.000	0.019	0.016	0.984	2500.000
2	Simple Matching:	0.367	0.000	0.359	0.008	0.000	0.999	2500.000
3	Jaccard Coefficient:	0.049	0.000	0.033	0.003	0.000	1.000	2500.000
4	Goodman-Kruskal Gamma:	0.765	0.000	0.005	0.116	0.000	1.000	2500.000

In Table 5.2, the "value" column at the first row indicates that the observed Pearson correlation between the two matrices was 0.060. The average random correlation was zero with a standard error of 0.019 and the percentage of random correlations as large as .060 was 1.6%. Since this is a low proportion (0.016 < 0.05) there is a strong relationship between the matrices that is unlikely to have occurred by chance. The Singularity Theorists are co-authoring papers with colleagues that they also recognize (from a co-citation perspective) as being "intellectually similar." This finding was expected and provides further evidence to support the *sociocognitive network* hypothesis postulated by White, Wellman and Nazer (2004): "sociocognitive ties, such as those between collaborators, blend interests and affections in positive feedback duets. Shared interests in a set of problems may lead scholars to become collaborators" (p. 112). In their own research based on the "Globenet" interdisciplinary research community, White, Wellman and Nazer (2004) found that "being a collaborator, and to a lesser extent, reading the other person's work, correlates significantly with both articles and book intercitation" (p. 119).

5.4 Hierarchical cluster analysis

Given the strong relationship between the ACA and Co-authorship matrices, a hierarchical cluster analysis was carried out in order to examine the actors' positions as co-authors more intently and compare these overlapping positions with their ACA positions of "intellectual" similarity. A cluster analysis is "ideally suited for partitioning actors … into subsets" and makes it easier for the social network analyst to observe which "entities within a subset are relatively similar to each other" (Wasserman, 1994, p. 381).

The clustering routine used for the co-authorship matrix was essentially the same as the routine used in the ACA (i.e., complete link method with a focus on similarities); however, *UCINET* was the preferred software this time, instead of *SPSS*. Table 5.3 presents the *UCINET* bar graph output, which points to where the established author clusters are within the Singularity Theory network (see Table 5.3). The results shown in this bar graph have been interpreted and re-organized for placement in an alternative figure, Figure 5.2, for the purpose of illustrating further where the co-authorship clusters lie in relation to the original ACA clusters. Boxes have been used to highlight the previously labeled ACA clusters, forming the major sub-topics in Singularity Theory, and ovals placed inside the boxes to represent the overlapping co-authorships. Although the majority of the co-author clusters represent pairs, there are at least three instances of a tie triad or three-way co-authorship: 1) GRANGER, MAISONOBE and BRIANCON, 2) BRUCE, GIBLIN and GIBSON, and 3) VARCHENKO, KHOVANSKII and GIVENTAL (see Figure 5.2).

A point of interest with respect to Table 5.3 is that there has been a tendency for the Singularity Theorists (with members of their specialty network only) to publish either single-authored or two-authored papers. Mathematicians, in comparison to other types of scientists, have a long history of being more individualistic in their publication patterns (e.g., Grossman & Ion, 1995).

Table 5.3. *UCINET* hierarchical clustering output. Singularity Theory co-authors (1975-2000).

```
Input dataset:                C:\Program Files\Ucinet 5\Alesia-Data\thesis-coauthors-EXP
Method:                       COMPLETE_LINK
Type of Data:                 Similarities

HIERARCHICAL CLUSTERING

                                                                                                                    L
          C                                                                                                         E
          H A                                                                                                       J
          I R                                                                                                       E
          L T                                             L L   G     M                     P                   S     U                 V   K
          L A                 B                           E O L U   B A     M                 A P                 T B   N                 A   H V   V D         Z
          I L     G         G R         I I J             D J O S   R C   B A M   B         R E   P             E R   E                 N G O A   A U         A
        C N B     A   G     O I   H   H S Z A K       U   U A O E   Y P G R I A   I       K U L   O         S E E A   J T   S     T   S I V R   S P   W G   K Z
        A G A   F B   A   G G R E G E   I H U N A       N S I I L L H R I S L M E M M   U S L   R   S     L B N S   A E   I     R   T V A C   S L   H A W A A
        M W R D U R D L B I I Y S R R   R I M E Z F     G I J N U I E A A O G A R I I   R I I   T M A   S O E B S   L I   E     O   R E N H A I E   I F I L R
        P O T I K I A L R B B U K E T A O K I C A U K H T E E Z E N R N N N R T S L L   D N K P E E B S E D L R E S A S   R T   T M A N S E R L S W T F L Y I
        I R O M U E M I U L S N O U L R N A Y Z R K O A R W N A N S S G C O A H T M N   Y S A H O R B A D O I I L U B S T S I K M O T T K N N I S A N N S U S
        L T L C D L O G C I O O R E I O A W A K I U I M A I G D G K O E O B N E O A O O K K A A U L A I Y W N N E W E I H M B U A N E A I K O E I L E E O K K
        L H O A A O N O E N N V N L N C K A   O A I K M N C A E O I N R N E G R N N R K A I N M S E H T K Y G K T A R E O A A O N D N L I O L V S L Y Y N I I
        O         V                 G A A   S   N   E   G Z                 E   E     A               O H           T R M   R               D             N

        1 1   1 1 1 1 2   2 2 2   2 2   3 3 3 3 3 1 3 2 3 4 4 2 4   4 2   4 4 4   4 5 5 3 5 5 5 5 4 5 5 5 6 1 6   6 4 6 6 6 6 3 6 5 6 2 3 6   7 1 7 7 1 7 7 7
Level   0 1 3 3 6 8 2 0 8 1 2 4 7 6 9 2 0 1 2 3 4 7 6 8 9 1 2 7 3 9 4 5 6 5 6 7 4 9 0 2 8 3 4 5 6 8 7 8 9 1 5 2 5 3 0 4 5 0 6 7 7 1 8 3 5 9 1 0 4 1 2 9 3 4 5
-----   - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - - -
   32   . . . . . . . . XXX . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . .
   17   . . . . . . . . XXX . . . . . . . . . . . . . . . . . . . . . . . . . . XXX . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . .
   11   . . . . . . . . XXXXX . . . . . . . . . . . . . . . . . . . . . XXX . . XXX . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . .
   10   . . . . . . . . XXXXX . . . . . . . . . . . . . . . . . . . . . XXX . . XXX . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . XXX . . . . .
    9   . . . . . . . . XXXXX . . . . . . . . . . . . XXX . . . . . . . XXX . . XXX . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . XXX . . . . .
    5   . . . . . . . . XXXXX . . . . . . . . . . . . XXX . . XXX . . XXXXX . . XXX . . . . . . . . . . . . . . . . XXX . . . . . . . . . . . . XXX . . . . .
    4   . . . . . . . . XXXXX . . . . . . . . . . . . XXX . . XXX XXX XXXXX . . XXX . . . . . . . . . . . . . . . . XXX . XXX . . . . . . . XXX XXX . . . . .
    3   . . . . . . XXX XXXXX . . . . . . XXX . . . . XXX . . XXX XXX XXXXX . . XXX . . . . . . . . . . . . . . . . XXX . XXX . . . . . . . XXX XXX . XXX . .
    2   . . . . . . XXX XXXXX . . . . XXX XXX . . XXX XXX . . XXX XXX XXXXX . . XXX . . XXX . . . . . . . . . . XXX XXX . XXX XXX XXX . XXX XXX XXX . XXX . .
    1   . . XXX . . XXX XXXXX . XXX . XXX XXX . . XXX XXX . . XXX XXX XXXXX . . XXX . . XXX . . . XXX . . . XXX XXX XXX . XXX XXX XXX XXXXX XXX XXX . XXX . .
    0   XXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXXX
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According to Fisher (1973) "the first relevant aspect of the training of mathematicians is a learned feeling of independence" (p. 1098). At no time has this level of independence been exemplified so dramatically than when Andrew Wiles isolated himself to work on his solution to Fermat's last Theorem (Singh, 1997). On the other hand, the "professionalization" of mathematics in the last 30 to 40 years, marked by an increase in mobility and the establishment of institutes, research centers and conference venues for information sharing and collaborative work, has had an interesting effect on the traditional image of the loner mathematician. One only needs to visit The Isaac Newton Institute in Cambridge (U.K) or the Fields Institute in Toronto (Canada) during the afternoon tea of a major workshop to see that mathematicians can be intensely communicative (and perhaps also competitive) with one another about their research. The informal behaviour associated with their collaborative work deserves further consideration and more attention to this topic will be given in the final section of this chapter.

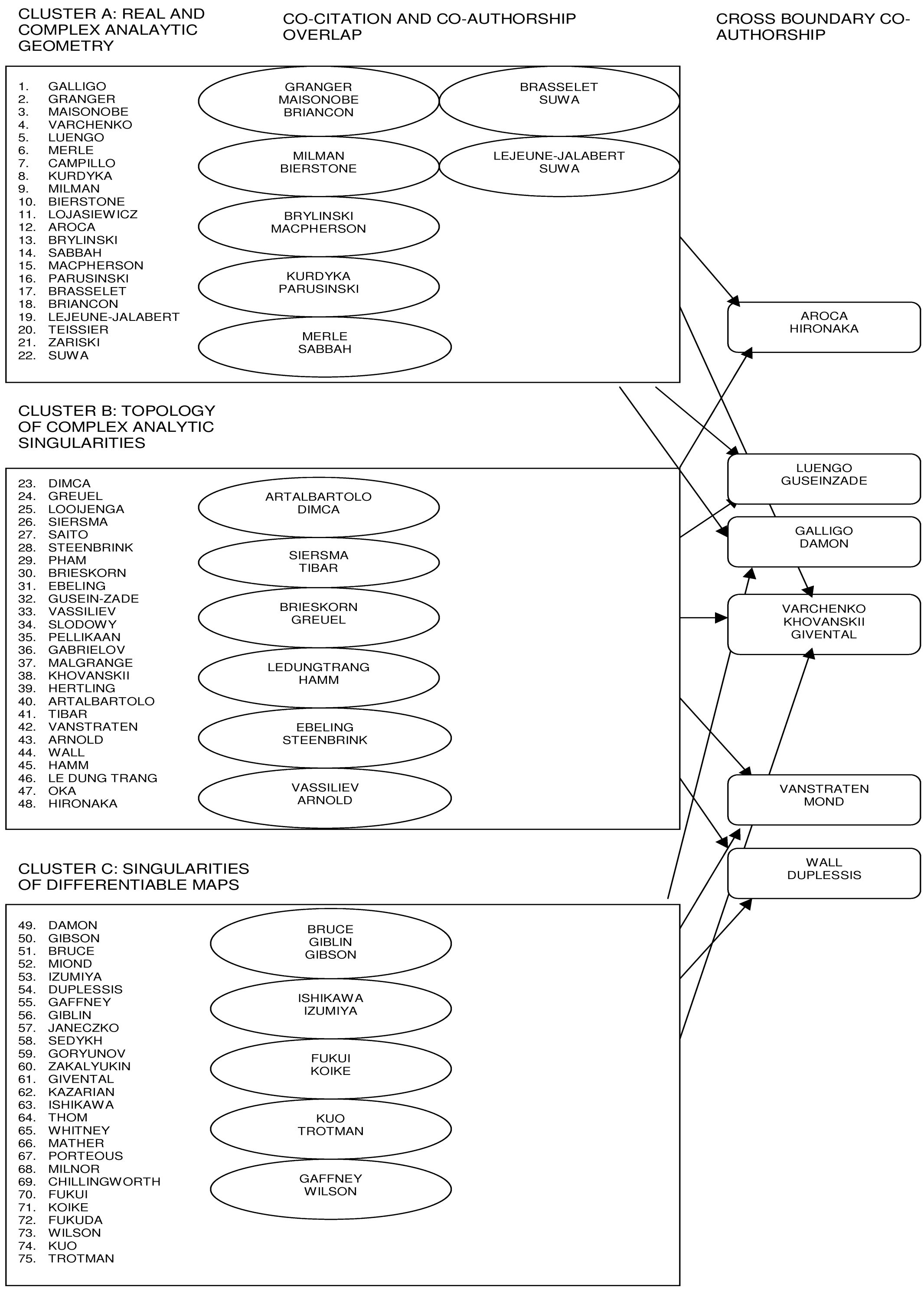

Figure 5.2. ACA cluster and co-authorship overlap. Singularity Theory (1974-2000).

5.5 *Interpretation of the ACA-Co-authorship map and clustering routine*

Based on a visual examination of the ACA-Co-authorship map (Figure 5.1) and the hierarchical clustering comparisons in Figure 5.2, we can see that many of the Singularity authors who have been co-cited together have further connections to one another as co-authors. Interviews with the Singularity Theorists helped to illuminate a few stories behind these co-authorship ties and provide a basis for recognizing three prominent themes. In the next paragraphs, the three themes are examined separately; yet it is important to recognize that they are not necessarily mutually exclusive.

The first thematic observation is that even though there is a level of congruence between the Singularity Theorists' perceived intellectual similarity (co-citedness) and co-authorship patterns, this congruence is not absolute. A background investigation concerning the research history of some of the authors reveals that proximity was perhaps another important factor in the co-authorship situation.[15] BIERSTONE and MILMAN, for instance, demonstrate a strong co-authorship tie in Cluster A, labeled Real and Complex Analytic Geometry. In Cluster A again, strong co-authorship ties also exist between BRIANCON and GRANGER, BRIANCON and MAISONOBE, and GRANGER and MAISONOBE (including a three-way tie). On the right hand side of the map, in Cluster C – Singularities of Differentiable Maps, additional strong ties can be seen between the GIBLIN and GIBSON, BRUCE and GIBSON, and BRUCE and GIBLIN (also including a three-way tie). All of these co-authorships are mentioned because the common element, besides an overlap in intellectual similarity and tie strength, is that the authors currently are or have been colleagues at the same university. BIERSTONE and MILMAN are both faculty members of the Mathematics department at the University of Toronto, Canada. GIBLIN, GIBSON and BRUCE are colleagues at the Mathematics Department at the University of Liverpool, England. MAISONOBE and BRIANCON work together at the Université de Nice-Sophia Antipolis in France (MAISONBE was

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[15] The underlying assumption concerning proximity is, of course, that authors would not choose to work together unless there was a certain degree of appreciation in terms of research ideas, work style and personality.

in fact BRIANCON's student) and the tie that both authors have to GRANGER is associated with a period in the early 1980s when he too was at the same university in Nice. Currently GRANGER is affiliated with the Département de Mathématiques at the Université d'Angers in France.

While many of the co-authorship ties on the social network map fall within the intellectual cluster boundaries, some actually cross the boundaries. This observation relates to a second theme, which emphasizes the role of travel in the development of co-authorships. DAMON and GALLIGO, for instance, are not frequently co-cited and belong to different intellectual clusters (DAMON in Cluster C – Singularities of Differentiable Maps and GALLIGO in Cluster A – Real and Complex Analytic Geometry), yet a time during their respective careers inspired them to collaborate. A fairly strong tie between them on the network verifies this. When I asked DAMON to tell me about his travel experiences and the opportunities he has had as a visitor to other research institutes or university departments, he explained the following:

> I've been at other universities, sort of as…you know, invited to visit for a period of time and I would have no teaching duties and I would usually be giving a few lectures. So that often would mean that it would function effectively like an institute, um, in that you have many of the same benefits of time, where..you know, time to yourself. You don't have to prepare lectures or do other things. You have time to think about questions and get together with the people, I guess, who invited you principally, because they're interested in the same things as you and want to talk about ideas…

During the same interview, I learned also that DAMON had spent time as a visitor in Nice, France and that this was where he had worked with GALLIGO:

> R. I've gone to places where I've stayed for several months, a number of times.
>
> I. *In general, what parts of the world?*
>
> R. What parts of the world? … Um… let me see… To Western Europe a lot… to England for several years on different occasions… the University of Liverpool and Warwick. Um… and in France, in Nice especially, I've worked with André Galligo several times, for several monthly periods at a time.

DUPLESSIS and WALL have a strong tie to one another, and similar to DAMON and GALLIGO, it is a tie that also spans two clusters. The story behind their tie is different however, in that it was not travel that brought them together, but a relationship that extended from an earlier period as supervisor and student. DUPLESSIS agreed to participate in an interview with me and spoke at length about his connection to WALL.

I. *Where did you do your Ph.D. and who was your advisor?*

R. I did my Ph.D. at Liverpool and Terry Wall was my advisor.

I. *Oh, he was your advisor! So you've maintained this close contact with your advisor over time?*

[Note: Du Plessis is currently located at the University of Aarhus in Denmark]

R. Yes.

I. *I guess in general, is it fair to say you've been very influenced by his way of thinking and his mathematics?*

R. It works very nicely.

I. *Is he perhaps your key influence, or are there other people in the community?*

R. Oh, I think he's the key.

I. *So would you consider the work that you do with Professor Wall…um… is there sort of an intellectual similarity with respect to the problems that you focus on, or do you both add opposite perspectives that seem to work together in the kind of mathematics that you do?*

R. I think we seem to be able to be interested in all of those ideas, problems and have predictable points of view as to how we do our research. Sometimes it's curious about the way our points of view complement one another, so it works really well.

I. *So depending on the moment, you will decide between the two of you when you're collaborating, which methods you'll use and which…*

R. Well, it's not as formal as that, it just happens, you know. You have a problem, Terry thinks about it his way, and I think about it my way, and sometimes progress comes from his way of thinking about things and sometimes from mine.

I. *OK. What are the ingredients then that you think makes this successful,… I mean I suppose you've mentioned some of them in the sense that you do like to work together … you have a common bond…*

R. I think that Terry's real power is that he has enormous experience and is very, very widely read, so in a sense he knows what kind of arguments could be available ……. It seems as if sometimes I am able to contribute new ideas, but basically I feel that I benefit from working with Terry...

Here we have the story of a thesis supervisor and student – two mathematicians involved in many hours of discussion – who now have a trusted and continued work relationship despite the fact that they are no longer working at the same university. The third theme highlighted by this data is that strong co-authorship ties can sometimes grow out of mentoring relationships; relationships that mature over time in terms of mutual influence and the output of joint research.

5.6 First or sole authored publications versus co-authored publications

One of the major problems with ACA is that all-author counts can be difficult, if not tedious to extract from the Dialog™ *SciSearch* science citation index; therefore, first-author counts are normally used and accepted as part of a standard approach to visualizing the intellectual structure of a research field. Rousseau and Zuccala (2003) examine this problem at length in *A Classification of Author Co-citations* and attribute it partially to the fact that the term "author co-citation" needs to be defined more clearly with reference to a classification scheme. Bibliometricians, they argue, should be noting more precisely vis-à-vis this new classification what is being counted when collecting author co-citation data. Rousseau and Zuccala also indicate that if one is to analyze the true level of contribution that a group of individual authors make to a research field, then special Dialog™ search procedures need to be worked out so that all-author counts may be used rather than the standard first-author counts.

In this case study of Singularity Theory it was previously stated that the standard first-author approach to ACA was used. However, because this research superimposes a co-authorship network on to the ACA mapping, it is important to acknowledge that there is a certain degree of incompatibility between the two structures. The ACA map is basically an approximation of each individual author's true level of intellectual contribution, whereas the co-authorship network is accurate and complete. Consequently, statistics based on the number of first/sole author counts and secondary author counts for each mathematician were gathered to determine exactly how much of the

Singularity Theory ACA was based on first/sole author data. Appendix G provides a table, which shows that the average percentage of first/sole author publications was almost 80 (see Appendix G).

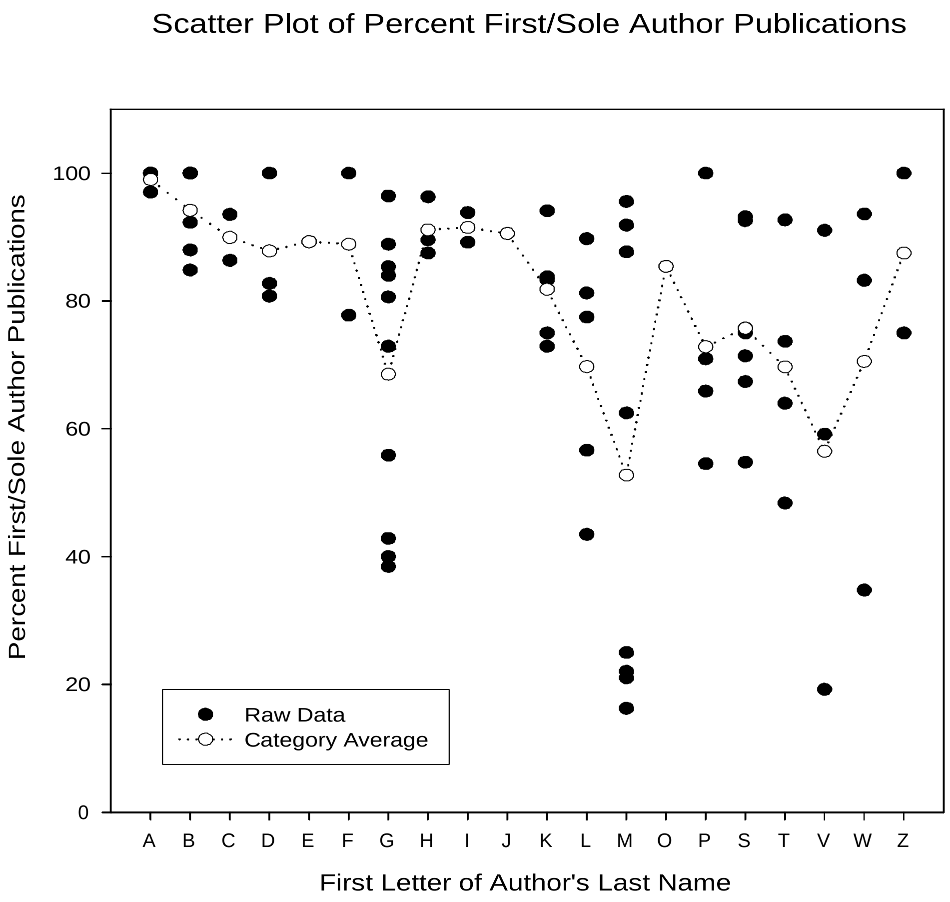

Figure 5.3. Co-authorship ranking as a function of surname.

Using the same authorship statistics, a further analysis was incorporated into the study to determine how much of the Singularity Theory author's intellectual positions might have been distorted as a function of their last name. In other words, are the mathematicians with last names beginning with letters at the end of the alphabet placed in secondary author positions more often than their colleagues who have surnames beginning with A, B, C, or D? Note from Figure 5.3 above, the bar graph of the category averages for the percent of first/sole author publications fails to demonstrate an obvious bias towards a negative trend. Certainly a few authors with surnames beginning with the letters G, M, V and W have been listed more frequently as secondary authors, but for the most part, the co-variability is unsystematic. MAISONOBE, for example, is perhaps not represented as accurately as an author node on the ACA map as someone like BRIESKORN, but his approximate representation is still reasonable.

5.7 Collaborative behaviour

In an article entitled *Collaboration and reward. What do we measure by co-authorships?* Laudel (2002) points to two major assumptions that researchers make when they are using co-authorship data to measure collaboration. The first assumption is that "all people who appear as a paper's co-authors actually took part in the research collaboration" and the second is that "all scientists who collaborate become co-authors" (p. 3). Not only are these assumptions examined in relation to previous studies[16], they are also systematically analyzed with the idea that the "co-authorship" indicator need not be "invalidated," but incorporated instead into "a micro-theory of that indicator" (p. 4). Six types of research collaborations form the basis of Laudel's micro-theory: 1) *Collaboration involving a division of labour, 2) Service collaboration, 3) Transmission of know-how, 4) Provision or access to research equipment, 5) Trusted assessorship, 6) Mutual stimulation*. All were derived from the bibliometric and qualitative analyses that she carried out with 57 different German research groups in science.

Laudel's (2002) study is noted in the introductory paragraph to this section because it brings into focus the problem of understanding collaborative work in science, specifically at the informal, behavioral level *before* a publication is produced. To fully understand collaboration in Singularity Theory, an extensive investigation needs to be carried out of the many communicative situations, events and acts of the mathematicians during their everyday work. This type of qualitative research

[16] For example, Katz & Martin (1997) and Biagioli (1999) observed that some co-authorships are not based on collaborative contributions, most notably in the case of honorary co-authors, which are prevalent in the field of biomedicine.

can be difficult to undertake, particularly because of the international nature of the Singularity Theory community. For this reason, the Isaac Newton Institute was considered a valuable place to observe the international mathematicians; many of them could be seen together in the one environment. A brief three-month ethnography of communication was included in this study to learn more about how the Singularity Theorists' share information and discuss ideas with one another. Mathematicians are social actors and within an invisible college it is understood that a strict count and interpretation of their co-authored publications will not reveal all aspects of what it means to collaborate.

During my visit to the Isaac Newton Institute, I spent time carrying out one-to-one interviews as well as observing the daily interactions of the visiting Singularity Theorists (Note: Appendix H includes a copy of the semi-structured interview schedule). The "tea-time" area on the first floor and also the mezzanine level or lounge area provided ample opportunities for unobtrusive observation or eavesdropping. Most of these observation periods were challenging because I could not understand the mathematical content of the conversations. I was aware of my limitations as an outsider before going out into the field, but decided that many aspects of the Singularity Theorists' communication patterns could still be observed. Even if I was a trained mathematician I might not necessarily have had an interpretive advantage. I expressed this concern to a mathematician friend, and he had reassured me that he probably could not understand a lot of Singularity Theory research as well.[17]

One afternoon at the Institute I was observing a conversation that was taking place between "John" and "Ben" in the mezzanine level of the Institute.[18] I was in the process of writing up field notes and feeling apprehensive about my ability to understand the message content of their communicative situation. This was during the early stage of my fieldwork and I was new to the

[17] This friend proved to be valuable another time when I asked him to check the blackboard in the men's room at the Newton Institute and tell me if anyone had written any mathematics. The Newton Institute is famous for having blackboards posted almost everywhere, including unusual places (e.g., the elevator) to assist the mathematicians with moments of inspiration. He agreed to this request and when he came back to me to give his report, he said that the boards had been wiped clear. He could not determine when they had been cleaned but could see evidence of erased markings.

[18] John and Ben are Singularity Theorists, but pseudonyms are used here to protect their real identities.

research experience. The tone of the event was relaxed and informal: both John and Ben were wearing casual summer clothing (e.g., short sleeved shirts, shorts, running shoes) and were working at a round coffee table situated next to a blackboard. There were only two blackboards in this lounge area and in the corner of the room, between the blackboards, a self-service food area was set up with boxes of tea and coffee, as well as chips, chocolate, yogurt, biscuits etc. for purchase. The setting was comfortable and designed with choice in mind, meaning that the visitors could do whatever they wanted in that space, including eat, drink, socialize, read quietly, or work with others in front of a blackboard. It was an 'open-concept' type of architecture in that all parts of the building were visually accessible to the mathematicians. The mathematicians on the second level could retreat to work in semi-private enclosed offices, but they could also come out of their offices and look over the balcony at any time to see who was working or relaxing on the mezzanine level below. Essentially, they could seek potential collaborators without having to make too much of an effort to search for them.

The general purpose of the meeting between John and Ben was to discuss mathematics, but I could not determine if the work was relevant to any of the lectures held at the Institute or if their discussion was about a joint project. I was also not sure if there was any intent towards establishing a new formal collaboration (i.e., something leading to a publication). Perhaps one of the mathematicians had asked the other for help, but this again was not clear to me because I came to the situation about 5 or 10 minutes after it had begun. A portion of my field notes appear as follows:

> *(Thursday, September 14, 2000, 12:30 pm).*
>
> *John is getting up from his chair to erase the blackboard. He has a notebook in his hand and is writing or copying equations (?) from the notepad onto the blackboard. Ben is sitting in the chair and watching intently. He follows what John is presenting and from time to time both mathematicians interject to make comments or ask questions.*
>
> [At this point I am lost and do not understand the math speak, and find it difficult to record the back and forth type of questions and comments]
>
> *"Is this not what you are saying?"*
>
> *"Ok, Ok, fair enough, I see what you're saying."*

“I don’t fully understand why, but let me just accept that fact.”

“The way I think of that is…. “

“My question for you before you go on…”

“This is so similar to what I was struggling with …”

“Let me absorb this for a second …”

Ben picks up his own notepad to write. He is sitting down and taking notes at the coffee table and as he is speaking to John and it seems that he is conveying his understanding of someone else’s work (?). Ben’s comment is “I agree with what you’ve established here… but if you go back…” He also says something about a “deformation argument.” John leaves the blackboard and now both of the mathematicians are sitting at the coffee table. Ben seems to be conveying his overall understanding of everything. John is silent and doing most of the listening, but as he is ‘thinking’ he stands up again to write on the blackboard. The amount of space on the black board is limited. There are many written numbers and symbols everywhere, but John does not erase anything. Ben approaches the blackboard too and he is now writing. The two mathematicians are standing side by side in front of the blackboard …

At the time that I was writing my notes another group of mathematicians from a different research program had gathered together in the common area.[19] They were standing around one person in particular who started to write on the second blackboard and due to the increasing noise level it became too difficult for me to eavesdrop.

Following my observation of John and Ben, I paused to finish my note taking and reflect on the situation. Their meeting had lasted for about an hour and from what I could see, there was evidence of mutual respect between the two mathematicians in terms of understanding one another’s ideas and points of view. There was no apparent struggle for them to communicate; both spoke the same language (English) and both seemed at ease with the other’s mathematical experience. The collaborative work between them was not a secret and they seemed unconcerned about whether or not any other mathematician at the Institute was watching them or listening to them, including me.

[19] The Singularity Theory program of 2000 was operating in parallel to another program in mathematics that year devoted to the study of Geometry and Topology of Fluid Flows.

The two men were approximately the same age, perhaps middle to late 50s, and came from different university departments in the United States. Both knew each other fairly well and had met each other before (note: this was confirmed to me in an interview that I held with each of them later). Perhaps the phrase that best captures the intent of their interaction is "*mutual stimulation*." Again, Laudel (2002) uses the phrase "*mutual stimulation*" in her own research to describe a situation where two or more scientists communicate in a way that stimulates them to think about unsolved problems in their field, about possible new research projects, or about the interpretation of data.

I did not have an opportunity to follow-up the observation period by asking John or Ben about what they were hoping to achieve through their meeting. My sense at the time was that it was important not interrupt their discussion and there were many issues of awkwardness for me, particularly in the beginning, with respect to approaching the Singularity Theory visitors. I later saw Ben and another mathematician, whom I will refer to as "Simon," engaging in a research discussion. I asked the two mathematicians if they would not mind having me join their discussion as a participant observer. With a more interactive approach, I was hoping that I might witness more of what takes place during the collaborative discussion and ask questions for clarification. As it turned out, Ben politely refused. He explained that the two of them had waited for a very long time to talk with one another in person and would prefer to just continue with their work alone and uninterrupted. I understood Ben and respected the situation.

On another occasion at the Institute, I had an opportunity to observe John again, this time while he was talking with "Peter" (a pseudonym). Both mathematicians were located in the mezzanine in front of one of the blackboards. After having had a little more experience at being the unobtrusive observer, I decided to approach John immediately after the conversation had ended and ask him if he would explain to me what it was about. In this situation, he told me that he needed to relate some ideas concerning a mathematical proof to Peter, whom he regarded as an expert on the topic in question. He wanted Peter to verify a particular technique/approach to the proof: a kind of "*transmission of know-how*" (Laudel, 2002). I asked John if he had received the verification that he needed, and his reply was “yes.” My follow-up conversation with was John was very brief, since he seemed eager to return to his office and continue with his work.

Efforts were made at a variety of other times to observe the mathematicians’ informal communications, including different periods of the day (i.e., mornings, afternoons and evenings) and periods also when I could not understand the language spoken. For example, I tried to eavesdrop on a meeting between “Igor” and “Oleg” (pseudonyms) while they were sitting together at one of the Institute mezzanine coffee tables, conversing in Russian and leaning over a paper with red ink markings. Both men appeared relaxed and were drinking tea or coffee. Body language in this situation conveyed a high level of familiarity and collegiality, but my inability to understand Russian made it impossible to recognize the purpose or intent of the conversation. I knew that if I wanted to ask Igor or Oleg questions about their work that they could speak English, yet for some reason I felt very uncomfortable about approaching them. One certainly gravitates towards people who come from similar cultures and speak the same language. If I felt this way in my own experience of research, I could only speculate that they too might think this way sometimes and gravitate towards more ‘familiar’ people. Kretchmer (1997) confirms this viewpoint in her article concerning the “birds of a feather” phenomenon in research communities: the *psychosocial approach* to collaboration. Perhaps the psychosocial approach explains to some degree why Singularity Theorists with Russian surnames tend to co-author papers more frequently, as well as those with French and English surnames. Mathematicians who share the same language and come from similar cultural backgrounds and/or similar academic systems can take for granted certain ways of communicating, which can contribute to a sense of ease in a collaborative situation.

While it was clear during my Institute visit that certain mathematicians from the same “school,” or cultural background were seen having discussions with one another, it was certainly also the case that they did not limit their interactions in this way. The Singularity Theory program was organized to foster international connections and the mathematicians’ efforts in establishing such connections occurred on a number of occasions as well. More details concerning Newton Institute’s role in supporting mathematics research will be discussed in the next chapter, but in terms of connecting and collaborating, some of the data that I collected during my interviews are worth highlighting. For instance, Ricardo Uribe-Vargas (a student of ARNOLD) explained that he was both interested and successful in meeting with one of the Japanese visitors to the Institute:

I. I'm interested in the fact that you came back to the Newton Institute and that you're here to do some collaborative work with certain persons?

R. Yes, I want to discuss some of my texts with Prof. Kazarian and Prof. Zakalyukin …. to discuss some things with Izumiya

I. Are you working on a new problem? Is it related to your thesis?

R. Yes some problems are related to my thesis. I was writing my thesis and I found some new theorems on things and I've discussed these things with them [Kazarian, Zakalyukin] and also Izumiya showed me some papers that he is writing.

I. Oh yes… when did he show you this paper?

R. Yesterday or two days ago.. I told him of some paper that I am writing and he told me that it is related to his work …. and he told me about his work …

I. So there are some common things between your work and his?

R. Yes.. some related things… not the same things, but they are very related…. Izumiya will give a talk and he told me what's the subject of the talk and I discuss with him also some thesis related work … it is related to some things, which I think of two years ago …

I. So do you think that you might publish a paper together as a result of this discussion?

R. I don't know… maybe for the moment we will interchange ideas and I will show him some things that maybe are interesting for him and then …

I. So you are mostly working with ideas right now.

R. I will work with Izumiya and he told me … and I put some questions to him that I don't understand… and he told me about some special curves … some curves which have some characteristics… that may be defined by these characteristics … which are called Bertram curves?

I. Bertram curves?

R. Yes…the name is Bertram curves…because it is French… Izumiya told me about Bertram curves, but I look in many times in old books. I see about Bertram; he is an old mathematician from the last century. I never looked at this part… but then yesterday Izumiya told me that he is doing something about these Bertram curves and I asked him to explain to

> me what is Bertram curves … but then even if I don't know what it is I can ask to him to explain it…

Note from the transcription above that Uribe-Vargas indicated a specific preference for sharing ideas with people familiar to him, Professors ZAKALYUKIN and KAZARIAN – mathematicians who were former students of ARNOLD and/or members of the Moscow seminar group. Earlier in the same interview he had stated: "I hope to meet many times more with Zakalyukin and Kazarian. Sometimes they come to Paris. If not I hope to go next year to Moscow one more time." On the other hand, Uribe-Vargas' collaborative experiences were not limited because the Institute program had given him the opportunity to discuss mathematics also with Prof. IZUMIYA from Japan. His social network seemed to be expanding and he was allowing himself learn about parts of mathematics that he had not thought about before: Bertram curves.

The act of questioning ideas or parts of a proof is as important feature of collaborative behaviour in mathematics, as well as face-to-face communication. Uribe-Vargas' comment concerning the act of questioning was that

> sometimes when [a mathematician] is working on some subject… even if he knows the subject very well, he may find some problem. There are some questions related that he would never think about. This is sometimes for me. I give sometimes my work to some friends and they put questions to me and some of these questions are interesting to me… for me to answer, because uh.., I never thought or I think more or less, but not so explicitly… then they try to understand that and they put some questions and some of it was very useful to me.

With respect to meeting face to face, BOGAEVSKY, another former student of ARNOLD, asserted a distinct preference. Mathematicians today can choose to collaborate with one another via communication technologies such as e-mail, fax or telephone, but BOGAEVSKY made it clear: "I don't like email." I asked him to comment further on why he did not like this specific mode of communication:

> You know… e-mail requires more time. Sometimes you have to spend on hour to write one e-mail, instead of just telling someone. [When speaking face-to face], if you don't understand something, you can just interrupt. When you write an e-mail, you have to write exactly what you mean, but when you talk to a person you can ask something that you don't understand…

Also, one of the clearest confirmations of the importance of face-to-face interaction in scientific research comes from an article written by Stephen Braham (1995), director of the Center for Experimental and Constructive Mathematics (CECM). Braham explains that:

> …it is easy to collaborate with somebody who can walk into your office, and work through a problem on a blackboard with you, or who can sit down at a table and circle important points on a research publication. Now put your colleague on the other end of a phone, or e-mail discussion. Any of us who have tried the latter know that things suddenly become very difficult. It is very hard to visualize the fine details of any mathematical science in your head, especially if those details are coming from somebody else's head. Even if one is lucky, and has a copy of a common research paper (maybe sent via electronic mail), it takes a long time to see exactly what point a researcher is looking at (p. 1)

In Singularity Theory research, there are times when face-to-face communication benefits the mathematician, especially if he wants to understand something "imaginatively," rather than by a written formula. Uribe-Vargas elaborated on the geometric nature of his work concerning curves and spoke at length about his desire to learn more about Bertram curves during a face-to-face discussion with IZUMIYA:

> R. There are many points to explain in mathematics or to write in mathematics or to think about in mathematics …. an object…a mathematics object … sometimes you can describe it by mathematical formulas …
>
> *I. By formulas…*
>
> R. But sometimes you can give a description in more geometric terms. In terms of some properties that are easy to capture by imagination
>
> *I. Oh I see…*
>
> R. Yes…they are easier to capture if you make a design…
>
> *I. So sometimes you can describe mathematics by making designs?*
>
> R. Yes… and maybe it is easier to understand and to think about. Mathematical formulas I don't like…
>
> *I. It's more difficult*?
>
> R. Sometimes yes. You must have both things…but uh… formulas must express the geometric object, but you must be able to… I think that it is better to be able to understand also without formulas. It depends on the deductions from the concepts….. I show to [IZUMIYA] some descriptions of the curves in three-dimensional space and some special points of these curves… then there are some points of these curves that are related to Bertram

curves.. but I want to understand what is the geometrical meaning of these points…

I. You want to understand the geometrical meaning… and he is more of an expert in explaining this?

R. No, I ask to him…. after I discuss with him [in person]… I realized that these points are related to Bertram curves

I. You realized that these are related and he [IZUMIYA] understands why?

R. Yes, he understands why… I told to him about this after he told me about Bertram curves…

I realized that these Bertram curves are related to these points.

I. Ah OK, so something that he told you reminded you of something you observed and you went back to him to tell him [in person] about how you saw the link between Bertram curves and what you had observed ….

R. Yes, yes yes yes…. there is a relation to Bertram curves. [IZUMIYA] understands and of course I understand, but I would like to have more of a geometrical description [from IZUMIYA] in order to understand what these points mean for a curve…

I. You want to have more of a geometrical description… and an equation?

R. I want to eliminate the equation…

Further insight into collaborative behaviour in Singularity Theory came about during an interview with Professor SIERSMA. When I had an opportunity to talk with SIERSMA, I discovered that not all mathematicians who collaborate become co-authors:

I. Is there a situation in the past that you could describe… uhm… where you broke through with something important and you were discussing with someone… ?

R. Oh yes…

I. At a meeting? During a conference?

R. I have a good memory of a train trip when I was discussing with Steenbrink. We came back from a meeting in Nancy and uh… and we took the train to [?] and so we had some discussions. I had several things in mind at that moment and we were discussing that… and so after that train trip, I had to put just immediately the next steps onto a page due to the discussion that we had… so I had already the problem, I had already a strategy in mind to solve the problem, it was just the discussion worked in such a way.

I. So did you and Prof. Steenbrink publish a joint paper?

R. No…

I. But you just… uhm… it was a beneficial discussion…?

R. Yes yes… and in fact in mathematics it's not so common to write joint papers, so even if you have the feeling that the other person brings 30% of the ideas … it's uh… you will not … you will write the paper alone and it's well understood then. This is in contrast sort of with an area like physics you find a paper with six authors…

I. Yes. yes… because they work together in the same laboratory…

R. Yes.

During this same interview, I learned that SIERSMA was working on a joint paper with TIBAR and I asked him to explain what he looks for in a person with whom he chooses to collaborate:

R. Well… so the reason is just that you start talking about a subject and you found that you could contribute something together on that subject…

I. So it's just purely mathematical?

R. Yes.,. yes…

I. Is there also a level of relationship that is important to you?

R. I think that with certain people you would never write.. you wouldn't like to cooperate … you get into troubles…

I. Naturally…

R. But sometimes you see also papers … two persons … where exactly each of them wrote half of the paper ..

I. They each wrote half…?

R. Half of the paper yes. So the paper will exist perhaps of two ideas, one is the concept and the other is the application and so in a certain sense they have not to meet…

I. Oh I see …

R. So they divide the job into two pieces ..

I. Is this what you and Tibar are doing?

R. No no no … although we sort of stand… [pause]…We have slightly different tasks, otherwise if you are exactly the same, you don't benefit

I. No?

R. No. So I am more of the kind… I look for examples and for ideas and he is very good

> in the technical things…and he makes the ideas deeper then…and so it's a kind of a "ping pong" mode…

SIERSMA's "ping pong" metaphor refers to the back and forth conversational exchanges that occur between mathematicians when they are formalizing ideas and techniques together in a written paper. He did not elaborate on his experience of the "ping pong" mode; however, I think that the main point he was trying to get across was that some collaborative-type discussions play a role in enhancing individual work, while others lead specifically to jointly published work. I did not have an opportunity to interview TIBAR at the Newton Institute; therefore, the perspective that SIERSMA has of their collaborative situation may be the same or may in fact differ from TIBAR's view.

There are alternative approaches that mathematicians may take when collaborating, for example, a division of labor approach. Collaborative situations also depend on the co-authors research styles, their preference for more or less interaction, their proximity to one another and the subject nature of the research. One of the Singularity Theorists, namely LOOIJENGA, even mentioned a slight preference for avoiding jointly published research. When I asked why, he said: "I've wondered myself. I don't know… One of the things certainly is that I like to be somehow in control of what I write, and if you are collaborating with someone then you give away a bit of that control."

LOOIJENGA's comment about maintaining a sense of control in research raises another issue related to collaborative behaviour -- the issue of competition. When a mathematician feels competitive he/she may be inclined to work alone. This does not mean that the individual will cease to communicate or discuss research with colleagues; he or she will just be more likely to use information (even third-party information) for the purpose of acquiring a competitive 'edge.' A research strategy of this nature is generally acceptable. If it is done fairly, it may lead to a feeling of great satisfaction and reward. We saw this need for control specifically in Andrew Wiles when he adopted a reclusive research style while working on his solution to Fermat's Last Theorem. If he succeeded at providing the world with a complete and flawless proof, then the glory associated with winning the most coveted prize in mathematics would be “his and his alone” (Singh, 1997, p. 4). But even Wiles could not avoid collaborative behaviour completely. When he feared that the flaw in his original solution would inspire “questions and demands for clarification from would-be gap fixers” he knew that it was necessary to take his colleague Richard Taylor into confidence for a period of intense communication (Singh, 1997, p. 267).

Andrew Wiles was, in a sense, one of the more fortunate mathematicians because his competitive strategy worked to his benefit. Fisher (1973), for instance, describes how competition over the solution of a famous Poincaré conjecture resulted in more disheartening interactional consequences for some mathematicians. Over the years this “conjecture [gave] rise to intense competitive feelings among [certain] men ..... sometimes openly displayed as subtle remarks in research papers, but more often evident in the men’s pattern of association, in the gossip of the community, and from the men’s own reports” (p. 1114). The older generation of mathematicians “[tended] to be secretive” and became “very cautious in their conversations with others.” Other men resorted to “belittling the work of others. While having great respect for their competitors as mathematicians, they would often say that what the others [were] doing [was] somewhat off the point or misguided” (p. 1115).

When competition becomes a major aspect of a collaborative situation, research discussions are likely to take on a different tone or energy. Babai (1990), for example, documents an interesting example of e-mail use in mathematics, highlighting the "unexpected power of [e-mail] interaction.” The mathematicians in his article -- members of the computing theorists’ invisible college – were the recipients of a group e-mail message, which presented a challenging problem and prompted a serious race to find an answer. Babai details the communication patterns associated with the e-mail group and considers the impact the electronic space had on their individual and collective egos. What he found was that “e-mail is capable of creating an ultra-competitive atmosphere on a much greater scale [in mathematics] than any medium before” (p. 40). He also discovered that the receivers of privileged information in mathematics (i.e., those who were part of the e-mail interaction) tended to be part of an “extremely powerful elite group” (p. 41).

Overall, competition can be a very positive aspect of mathematics research. Through its healthy manifestation, it can bring a significant amount of vitality to a subject and promote rapid progress in problem solving. My impression of the Singularity Theory community was that the informal "collaborative" discussions were oriented mainly towards joint problem solving. Competition was not obvious to me at the Newton Institute, although I would not rule out that it was not present. During most of my interview situations it was an awkward topic: few if any of the Singularity Theorists were comfortable in admitting competitive feelings towards colleagues and certainly nobody wanted to talk about information seeking for competitive purposes.

The qualitative investigation that I have carried out at the Isaac Newton Institute is by no means a full-scale analysis of collaborative work in mathematics, but what it does show is that the bibliometric map of co-authorship in Singularity Theory can only tell part of the collaboration story. Much of what appears to be collaborative work in this subject does not necessarily lead to a joint publication, and behind the co-authorship structure there is perhaps more going on in Singularity Theory research than meets the eye. There is, however, no specified standard as to how often international members of a cohesive subject specialty must collaborate – i.e., demonstrate collaboration through co-authorship – before the specialty can actually be recognized as an invisible college network. In the absence of a specific standard, I can only suggest that the Singularity Theorists are, as social actors, discussing mathematics and sharing ideas with one another at a significant level, even if the actual co-authorship counts are low.

In the next chapter, my layered approach to investigating the multi-dimensional “invisible college” phenomenon will continue. The international mathematics institute is one of the main topics in focus, given that it has the unique role of making the invisible college of Singularity Theorists more visible.

CHAPTER 6: COLLEGIALITY IN SINGULARITY THEORY AND THE INTERNATIONAL MATHEMATICS RESEARCH INSTITUTE'S ROLE IN FOSTERING INVISIBLE COLLEGE ACTIVITY

The international mathematics institute is a fairly new space for research. Many of the institutes operating today, including the Isaac Newton Institute in Cambridge, England, the Fields Institute in Toronto, Canada, and the Mathematical Sciences Research Institute in Berkeley, California have only just been established in the 1980s and early 1990s. Since their development, mathematicians from around the world have had more significant opportunities for collegial interaction. The institutes have in effect transformed the loner "gentlemen's tradition" of doing mathematics into a highly "professionalized" discipline. With this idea of "professionalization" there comes a certain degree of accountability: each of the institutes mentioned above is required to produce an Annual Report outlining their research activities. This is largely related to the funding that they receive, mainly from government sources. Their Annual Reports are published to demonstrate, for the most part, that their money has been well spent and that their programs are contributing effectively to the advancement of mathematics. One of the most positive contributions that they can make, at least from the outset, is to help foster invisible college activity. Mathematicians are invited by the institute's scientific steering committees to propose and organize programs around a specific subject, usually a well-established, mature subject so that work may be done to re-vitalize the research area. Every program proposal is expected to explain how a series of seminars, workshops affiliated with the program might further the subjects' goals, for instance, by inspiring colleagues to focus on new applications and/or develop interactions with other research areas. Usually the activities corresponding to the goals are somewhat formal (e.g., organized workshops), but they are designed overall to get the mathematicians talking, thinking, discussing and collaborating informally. Since the mathematicians cannot stay at the Institute environment forever the hope is that after they visit they will take new ideas away with them and work on intellectual construction or "bridge building" beyond the programmed situation.

The Fields Institute asserts that its “mission is to enhance mathematical activity in Canada by bringing together mathematicians from Canada and abroad, and by promoting contact and collaboration between professional mathematicians and the increasing numbers of users of mathematics.” (The Fields Institute for Research in Mathematical Sciences, 2003). Likewise, The Mathematical Sciences Research Institute (MSRI) “exists to further mathematical research through broadly based programs in the mathematical sciences and closely related activities” (The Mathematical Sciences Research Institute, 2003). The Isaac Newton Institute also shares a similar mission as “an international visitor research institute” and “runs research programs on selected themes in mathematics …with applications over a wide range of science and technology” (The Isaac Newton Institute for Research in Mathematical Sciences, 2003). In this study, the international mathematics institute is viewed primarily as a special “Information Use Environment.” Again, the IUE may be any type of space “designed to affect the flow and use of information messages;” however, its relationship to the invisible college is that it fortifies invisible college activity with the provision of human, physical and technological resources (Taylor, 1986 p. 3; see Figure 1.2). The Newton Institute is the IUE that will receive the most attention here, since it facilitated the Singularity Theory program of 2000 and was the place in which I carried out my fieldwork.

The purpose of this chapter is to examine the extent to which international meetings or conferences have influenced collegial contact in Singularity Theory in past years. Collegiality is defined in terms of the number of times each of the 75 Singularity Theorists named in this study have participated in the same conference activities from 1970 to 2000. It is based on the social network data that I collected from both published versions of conference proceedings and proceedings posted on the World Wide Web. The collegial structure of Singularity Theory is introduced first and then compared statistically to its other structural measures: What is the structure of collegiality in Singularity Theory research and how does this structure relate to this specialty’s intellectual structure and co-authorship structure? To complement the collegiality analysis, I will then related some of my own observations of the Isaac Newton Institute as an Information Use Environment, the Singularity

Theory program activities that took place in this IUE and the experiences related to me by the mathematician visitors. Both the observations and interviews will be used to highlight invisible college activity, but they will also show how the Institute affects this activity in a situation based on dual interaction. And finally, the third intent of this chapter is to examine one of the most important issues concerning international mathematics institutes today - their continued financial support and the need to find a way to measure their impact on mathematical developments.

6.1 Conference attendance and the collegial network

In social network studies of scientific communication, researchers often use questionnaires or other methods of investigation (e.g., interviews) to elicit information from scientists' about who they talk to about their research, how often and when. Such studies are designed to learn more about person to person communication or "friendship" ties within a research community and may be used as part of a comparative analysis with the scientists formal publication and/or citation patterns (e.g., Crane, 1972; White, Wellman & Nazer, 2004).

The unique aspect of this research is that "collegiality" is the construct under investigation. Collegiality is not the same as a "friendship" or communication tie, since these other constructs preclude at least some form of personal interaction. Collegiality simply means that a relationship or tie exists, with some measure of strength, or does not exist between two mathematicians depending on their act of participating in the same conference. A collegial tie is non-directional. Also, it is important to note that no assumptions can be made about specific behaviours, for example, that colleague A actually met and discussed research with colleague B during the conference, although it is certainly possible.

The importance of examining the collegial structure of Singularity Theory is that it relates to the original definition of the invisible college proposed by Price in the late 1960s. Price (1986) commented on the fact that "people [within invisible college's] claim to be reasonably in touch with everyone else" and have the power to confer power and prestige on one another. With respect to how the members stay in touch, he specified that they "meet in select conferences (usually held in rather pleasant places), they commute between one center and another, they circulate preprints and reprints to each other, and they collaborate in research" (p. 119). Of interest in this study then is the extent to which the Singularity Theorists have been "reasonably in touch" with each other and their subject area through regular attendance at "select conferences." Conference collegiality is recognized as a major part of how the invisible college functions.

Much of the difficulty in collecting collegiality data in Singularity Theory relates to the fact that more information is available for conferences in recent years, than the 1970s and 1980s. One explanation is that advances in communication technology, mainly the World Wide Web, have made it easier now to trace conference information. Also, Singularity Theory has had a long history of being associated with other major subjects in mathematics, for example, Algebraic Geometry; therefore mathematicians from earlier periods may have been participating in conferences identified by other names. Prior to the use of the World Wide Web most of the information pertaining to Singularities conferences could only be located in published proceedings, researched through the use of *MathSciNet* and library catalogues. In this study, I often discovered that copies were either not available in the University of Toronto Library system or that the full participant lists from these earlier proceedings were not included. If a certain publication could not be found in the library, I was sometimes able to acquire a copy of the participants' list via e-mail or fax from the conference organizers or editors. Overall, it was difficult to trace the precise history of collegial activity in Singularity Theory; hence the information collected for this portion of the study is incomplete. The analysis however is still reasonable, because there is enough data to provide a thematic picture. In the future, as it becomes easier to collect conference collegiality data on the World Wide Web, more accurate studies of this nature may be implemented.

Raw counts for the collegiality structure, collected from proceedings' participant lists, were aggregated into a third adjacency matrix, identical in construction to both the co-citation matrix and co-authorship matrix. Each count was binary, in that two mathematicians who had participated in the same conference were given a count of “1” in the cell between them, plus additional counts of “1” for any other shared meetings. If two mathematicians had not participated in the same conference, a count of “0” was added to a cell. The cells along the matrix diagonal were also given a count of “0.” Below, a small portion of the full collegiality matrix is presented in Table 6.1. Table 6.2 presents the list of international conferences (also referred to as workshops and symposiums) that were included in the development of the matrix and network illustration.

Table 6.1. Partial matrix of collegiality counts. Singularity Theory (1970-2000).

	ARNOLD	AROCA	ARTALBARTOLO	BIERSTONE	BRASSELET	BRIANCON	BRIESKORN	BRUCE	BRYLINSKI	CAMPILLO	CHILLINGWORTH	DAMON	DIMCA	DUPLESSIS	EBELING	FUKUDA	FUKUI	GABRIELOV	GAFFNEY	GALLIGO	GIBLIN
ARNOLD	0	0	0	1	1	0	1	0	0	1	0	0	1	1	1	0	0	0	0	0	0
AROCA	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
ARTALBARTOLO	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BIERSTONE	1	0	0	0	1	0	1	1	0	1	0	1	0	1	1	0	0	0	1	1	1
BRASSELET	1	1	0	1	0	0	1	6	0	2	1	5	2	4	4	1	0	0	6	0	4
BRIANCON	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
BRIESKORN	1	0	0	1	1	0	0	1	0	2	0	2	1	2	2	0	0	0	1	1	2
BRUCE	0	0	0	1	6	0	1	0	0	3	3	7	3	6	5	0	1	0	6	1	7
BRYLINSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
CAMPILLO	1	0	0	1	2	0	2	3	0	0	1	4	1	4	4	0	0	0	3	2	3
CHILLINGWORTH	0	0	0	0	1	0	0	3	0	1	0	2	1	2	1	0	1	0	1	0	3
DAMON	0	0	0	1	5	0	2	7	0	4	2	0	2	4	5	0	2	0	5	1	5
DIMCA	1	0	0	0	2	0	1	3	0	1	1	2	0	2	2	0	1	0	1	0	2
DUPLESSIS	1	0	0	1	4	0	2	6	0	4	2	4	2	0	3	0	2	0	5	1	4
EBELING	1	0	0	1	4	0	2	5	0	4	1	5	2	3	0	0	1	0	4	1	4
FUKUDA	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0	1	0	0
FUKUI	0	0	0	0	0	0	0	1	0	0	1	2	1	2	1	1	0	0	3	0	3
GABRIELOV	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
GAFFNEY	0	0	0	1	6	0	1	6	0	3	1	5	1	5	4	1	3	0	0	1	5
GALLIGO	0	0	0	1	0	0	1	1	0	2	0	1	0	1	1	0	0	0	1	0	1
GIBLIN	0	0	0	1	4	0	2	7	0	3	3	5	2	4	4	0	3	0	5	1	0

Once the data were assembled in the collegiality matrix, it was then imported into UCINET (Borgatti et al., 1999) and converted into a special .kp file for input to Krackplot (Krackhardt, 1995). Figure 6.1 shows the resulting Krackplot sociogram of all collegial ties within Singularity Theory 1970-2000 (see Figure 6.1). Note that similar to the co-authorship network, this mapping is superimposed on the original co-citation map. The image however is quite dense, unlike the co-authorship network, and does not allow for interpretation through direct visual examination. Consequently, most of what can be recognized and understood from this network is derived from the density calculation, QAP analysis and hierarchical cluster analysis, carried out in the next section.

Table 6.2. List of Singularity Theory conferences (1970-2000).

1. Singularities Symposium (Liverpool, England): September 1969-August 1970
2. Journées Singulieres de Dijon (Dijon, France); June 12-16, 1978
3.Singularities Summer Research Institute at Humboldt State University (Arcata, California): July 20-August 7, 1981
4. Proceedings of the IMA Participating Institutions Conference (Iowa City, U.S.A.): July 28-August 1, 1986
5. Stratifications and Singularities (Luminy, France): May 21-25, 1990
6. Singularities (Honolulu, Hawaii): August 6-10, 1990
7. Workshop on Singularity Theory (Leuven, Beligium): November 19-20, 1993
8. Third International Workshop on Real and Complex Singularities (Sao Carlos, Brazil): August 1-5, 1994
9. Brieskorn's 60th Birthday Conference (Oberwolfach, Germany): July 14-20, 1996
10. Fourth International Workshop on Real and Complex Singularities (Sao Carlos, Brazil): July 22-26, 1996
11. Wall's 60th Birthday Conference (Liverpool, England): August 19-24, 1996
12. 70th Birthday of S. Lojasiewicz - Symposium on Singularities (Krakow, Poland): September 24-29, 1996
13. Topology of Real Algebraic Varieties (Toronto, Canada): January 6-7, 1997
14. Geometry and Complexity (Toronto, Canada): May 5-11, 1997
15. Arnol'dfest (Toronto, Canada): June 15-21, 1997
16. Symplectic Geometry (Toronto, Canada): June 23-27, 1997
17. Resolution of Singularities (Obergurgl, Austria - Tribute to Oscar Zariski): September 7-14, 1997
18. Singularity Theory Workshop (Leuven, Belgium): November 20-22, 1997
19. 5th International Workshop on Real and Complex Singularities (Sao Carlos, Brazil): July 27-31, 1998
20. Singularity Theory and Differential Equations (Kyoto, Japan): February 1-4, 1999
21. Journées Singularites (Lille, France): March 25-27, 1999
22. Workshop on Singularity Theory (Warwick, England): June 21-25, 1999
23. Aspects of Singularities (Lille, France): May 18-19, 2000
24. The 6th Workshop on Real and Complex Singularities (Sao Carlos, Brazil): July 17-21, 2000
25. Nato Advanced Study Institute (Cambridge, England): July 31- Aug 11, 2000
26. Workshop on Real Singularity Theory (Yokahoma, Japan - In honour of T. Fukuda and S. Izumi): November 8-11, 2000
27. Applications of Singularity Theory to Geometry (Liverpool, England) December 16-20, 2000

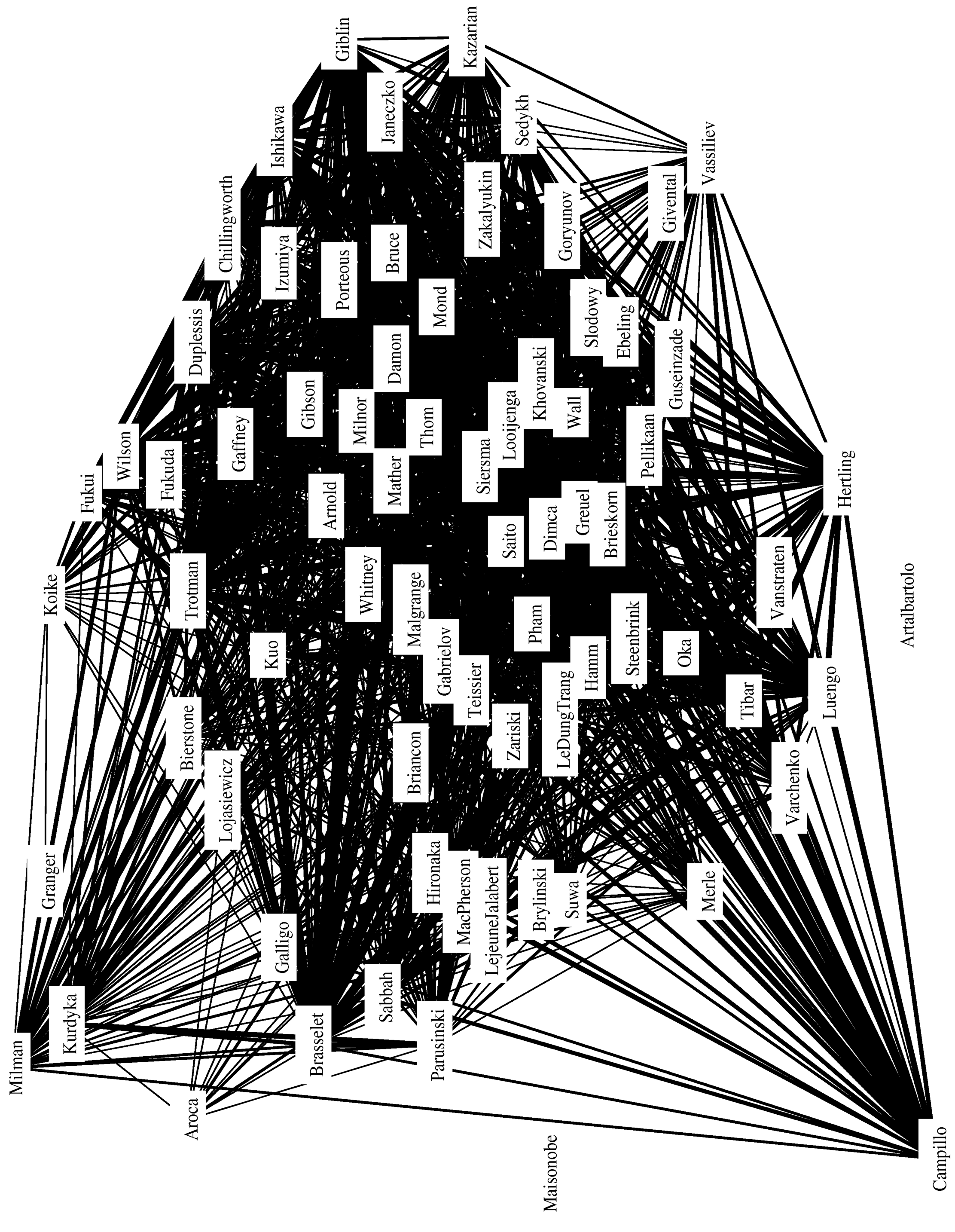

Figure 6.1. Collegialty map of 75 Singularity Theory authors (1970-2000).

6.2 Network Density, QAP Correlation and Hierarchical Cluster Analysis

To calculate the network density for the collegiality map or sociogram, the following formula was used:

$$\Delta = \frac{\sum v}{g\,(g\text{-}1)} = \frac{\textit{number of tie values}}{\textit{number of nodes}\,(\textit{number of nodes} - 1)}$$

$$\Delta = \frac{\sum v}{g\,(g\text{-}1)} = \frac{5668}{75\,(75-1)} = \frac{5668}{75(74)} = \frac{5668}{5550} = 1.0213$$

The resulting measure of 1.0213 indicates that the density is at a relatively high level because some of the lines in the network are greatly valued. A line with multiplicity 3, for example, is being counted as the equivalent of three lines. If we disregard the values of the lines and treat the graph as a simple undirected binary graph, the density measure is 0.5150:

$$\Delta = \frac{L}{g\,(g\text{-}1)} = \frac{L}{g\,(g\text{-}1)} = \frac{\textit{number of lines}}{\textit{number of nodes}\,(\textit{number of nodes} - 1)}$$

$$\Delta = \frac{L}{g\,(g\text{-}1)} = \frac{2858}{75(75\text{-}1)} = \frac{2858}{5550} = .5150$$

Based on this second calculation we see that the network is half way to reaching its maximum completeness potential (density =1). Many of the Singularity Theorists named in the collegiality sociogram have been "reasonably in touch" with one another at some major conferences. Within an invisible college network, this level of social activity is expected; therefore, the next step is to determine if there is a significant relationship between a) the Singularity Theory co-citation network and the collegial network, and b) the Singularity Theory co-authorship network and the collegial network. To investigate these relationships two QAP correlation analyses were carried out for the matrix pairs. The results are presented in Table 6.3 and Table 6.4.

Table 6.3. QAP correlation of the Singularity Theorists' co-citation and collegiality matrices.

```
Observed matrix:              Thesis-Cocite-EXP
Structure matrix:             Thesis-Collegiality-EXP
# of Permutations:            2500
Random seed:                  46


Bivariate Statistics

                                    1         2         3         4         5         6         7
                                Value    Signif       Avg        SD  P(Large)  P(Small)     NPerm
                            --------- --------- --------- --------- --------- --------- ---------
  1   Pearson Correlation:      0.002     0.463     0.001     0.054     0.463     0.536  2500.000
  2       Simple Matching:      0.231     0.283     0.224     0.014     0.283     0.725  2500.000
  3   Jaccard Coefficient:      0.410     0.264     0.396     0.022     0.264     0.744  2500.000
  4 Goodman-Kruskal Gamma:      0.069     0.264     0.000     0.105     0.264     0.744  2500.000
```

Table 6.4. QAP correlation of the Singularity Theorists' co-authorship and collegiality matrices.

```
Observed matrix:              Thesis-Coauthors-EXP
Structure matrix:             Thesis-Collegiality-EXP
# of Permutations:            2500
Random seed:                  518


Bivariate Statistics

                                    1         2         3         4         5         6         7
                                Value    Signif       Avg        SD  P(Large)  P(Small)     NPerm
                            --------- --------- --------- --------- --------- --------- ---------
  1   Pearson Correlation:      0.133     0.000    -0.001     0.020     0.000     1.000  2500.000
  2       Simple Matching:      0.485     0.000     0.474     0.010     0.000     1.000  2500.000
  3   Jaccard Coefficient:      0.051     0.000     0.032     0.004     0.000     1.000  2500.000
  4 Goodman-Kruskal Gamma:      0.585     0.000    -0.000     0.110     0.000     1.000  2500.000
```

Note from Table 6.3, in the "value" column, that the observed Pearson correlation between the co-citation matrix and the collegiality matrix was 0.002. The average random correlation was .001 with a standard error of 0.054. The percentage of random correlations as large as .002 was 46.3%. Since this is a high proportion (0.46 > 0.05) the relationship between the matrices is non-significant, with some similarities based on chance. This means that there are two distinct patterns of behaviour among the community members: one pattern in Singularity Theory associated with the authors' select conference attendance and another separate pattern reflecting how the authors' perceive one another (i.e., co-cite) in terms of intellectual similarity. The lack of congruence between the two matrices clearly reflects White's (2001) understanding that social ties are not necessary for citation: authors will cite other authors without knowing them. Also, social interaction is not sufficient for citation in the sense that simply knowing authors is not reason enough to cite them.

In Table 6.4 the "value" column indicates that the observed Pearson correlation between the co-authorship matrix and the collegiality matrix was 0.133. The average random correlation was zero with a standard error of 0.020. The percentage of random correlations as large as 0.133 was 0%. This is a significantly low proportion (0.00 < 0.05); therefore a strong relationship exists between the matrices, which is not likely to have occurred by chance. Among the Singularity Theorists who have co-authored papers, several have been together at the same conferences. No assumption may be made however that the joint papers were the result of the co-authors meeting and actually working together at a conference. What the data seems to be showing then is that there is a significant connection between the two types of social activities in mathematics: keeping "reasonably in touch" with international colleagues and their current work in the subject, and knowing each other well enough to form collaborative relationships.

A hierarchical clustering routine, performed on the collegiality data, illustrates more clearly who among the Singularity Theorists have attended the same conferences. The output of this analysis, using *UCINET*, is shown in Table 6.5. Several of the international mathematicians have been quite mobile and some have been more involved in conference activities than others. The data is historical in nature; therefore senior mathematicians who have spent a longer portion of their career in Singularity Theory (e.g., WALL) will obviously show higher rates of attendance than others. All of the past conferences have been held in "rather pleasant places," as Price (1986) suggests, and financial support has been offered in some cases (e.g., "Arnoldfest" – Fields Institute, Toronto) to enable long distance travel.

Table 6.5. *UCINET* hierarchical clustering output. Collegiality in Singularity Theory (1970-2000).

```
Input dataset:          C:\Program Files\Ucinet 5\Alesia-Data\thesis-collegiality-EXP
Method:                 COMPLETE_LINK
Type of Data:           Similarities

HIERARCHICAL CLUSTERING

                          L
                          E                                                                                                                     C
      A                   J                                                                                                                     H
      R                   E                                                                                                                     I
      T                   U L       P                 M         L       K                                   V G           S                     L
      A   B           M   N O       A           L   B A       P E       H         V V           D           A U           T       B             L       Z
      L B R G G       A   E J M     R           O   R C     P E D       O B       A A       I I U           N S           E     G R             I J     A
      B R Y A I G     I   J A A   K U       G H O   I P     O L U       V I     S R S   W   S Z P G T     T S E E     C S E H   O A             N A   K K Z
      A I L B V R F   S   A S L   U S     S A I I   E H M   R L N       A E M M L C S   H   H U L A R W G E T I B G L A I N E   R S       G     G N   A A A
      R A I R E A U   O   L I G   R I D   A L R J M S E A   T I G     A N R I I O H I   I F I M E F O I I I R N E R U M E B R   Y S   D   I B   W E S Z L R
      T N N I N N K K N A A E R   D N I   B L O E E K R T P E K T S S R S S L L D E L   T U K I S F T L B S A Z L E E P R R T T U E H A M B R W O C E A Y I
      O C S E T G U O O R B W A K Y S M   B I N N R O S H H O A R A U N K T M N O N I T N K A Y S N M S S S T A I U N I S I L I N L A M O L U A R Z D R U S
      L O K L A E D I B O E I N U K K C O A G A G L R O E A U A A I W O I O A O W K E H E U W A I E A O O I E D N E G L M N I B O E M O N I C L T K Y I K K
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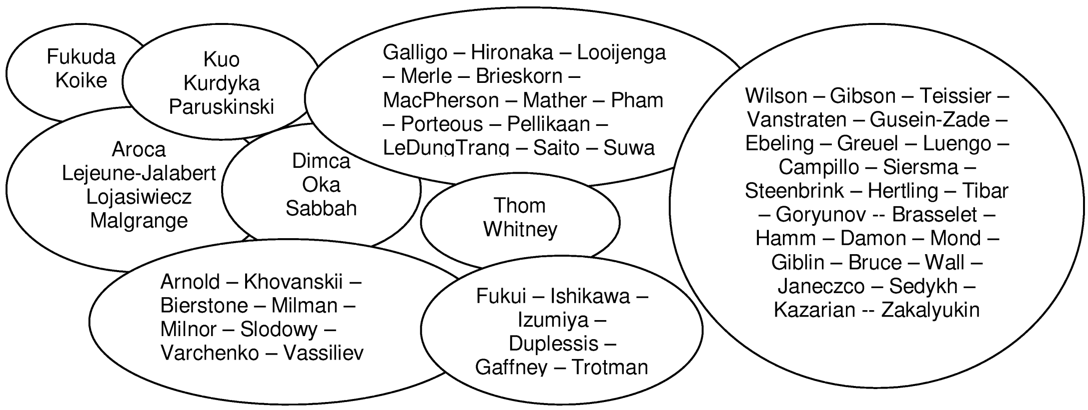

As for each of the individual clusters, it is difficult to know what sort of underlying factors have played a significant role in grouping the names. For instance, consider the large cluster, which includes the names of WILSON, GIBSON, TEISSIER, VANSTRATEN etc. Were all of these mathematicians highly interested in and invested in the topics covered by the conferences they attended? Can they be considered the more "social" mathematicians of their international colleagues or are they simply among the senior "elite" with the most access to funding for travel? Do these mathematicians live in geographic locations that make it easier for them to travel? All of these questions clearly lead towards a deeper investigation into the actual motivations and behaviours of the individual mathematicians themselves. From a retrospective standpoint, this information could be surveyed among selected authors; however, the mathematicians would have to be relied upon to remember and think clearly about what was important to them at the time. Collegiality data can therefore be expressed neatly as a structural pattern, but as an "experience" the best way to understand why the Singularity Theorists participate in conferences is to study how they view and make use of the environments that presently host their collegial activities.

6.3 The Isaac Newton Institute as an Information Use Environment

In order to do research scientists – as social actors -- are dependent on space. A research "space" can be a laboratory or office, or it can be an internet-based "arena" designed to facilitate observations and experiments, the flow of ideas and facts, and the need for discussion. Whenever or wherever a certain amount of space has been constructed for science, information and its artifacts (e.g., data; publications) are likely to be found within. In which case, a scientific workspace can also be referred to as an information environment or more precisely, an "Information Use Environment." The Isaac Newton Institute is a special type of Information Use Environment, because it brings international mathematicians together in one physical space so that they can effectively share information with one another and use this information to advance their research.

In past years, very few studies concerning Information Use Environments in Science have been carried out in the field of Information Studies (e.g., Kinsella, 1998) even though there has been ample discussion as to what actually constitutes an "Information Use Environment" (i.e., Rosenbaum, 1993; 1996; Taylor, 1986; 1991). Taylor's (1986) value-added theory asserts that the Information Use Environment (IUE) is a space where information related behaviours occur and that such behaviours constitute “the sum of activities through which information becomes useful” (p. 221). The IUE may be defined as “the set of those elements that a) affect the flow and use of information messages into, within, and out of any definable entity; and b) determine the criteria by which the value of information messages will be judged” (Taylor, 1986, pp. 3-4). To fully understand the IUE, or any IUE, Taylor (1991) proposes that it is important to collect data concerning the following elements: sets of people, the structure of their problems, typical settings and the resolution of problems.

Rosenbaum’s (1996) critique of Taylor’s IUE is that it is “fundamentally ambiguous” and that much of what is emphasized in his theory creates a tension between “perspectives based on structural assumptions and those based on action-oriented assumptions” (1996, p. 71). Consequently, Rosenbaum argues for a structurational approach to the IUE, which combines both Taylor’s value-added theory with Gidden’s (1984) theory of structuration. With this new structurational approach, the *duality of structure* is given precedence so that the two main constructs of Taylor’s original dichotomy – the IUE and information-related behaviours – form an interrelationship. The IUE is “instantiated in action” and “routinely produced and reproduced through the social practices or information behaviour of users” (Rosenbaum, 1996, p. 112). Similarly, information behaviours can “certainly be constrained, shaped, and enabled by the IUE” (p. 112). Essentially “the presence of each other makes the other possible; neither has meaning without the other” (1996, p. 112).

More often than not, research concerning the scientists’ IUE has occurred in the field of sociology. Among sociologists of science, physics and biology laboratories have been the main choices for ethnographic fieldwork and the social constructivist view science, a predominant research theme (e.g., Knorr-Cetina, 1981; 1983; Latour & Woolgar, 1979; Merz, 1998; Traweek, 1988). Social constructivists believe that science is mainly a contextualized activity, or more precisely that “the products of science are contextually specific constructions [and] bear the mark of the situational contingency and interest structure of the process by which they are generated” (Knorr-Cetina, 1981, p. 5). Social constructivism was, in part, a reaction against Merton’s (1957; 1967) normative/functionalist model of science and the Mertonian tenet that social factors influence scientists and how they interact, but are irrelevant to knowledge produced by science.

In this study, Rosenbaum's (1996) structurational perspective of the Information Use Environment is recognized and accepted as the predominant theme. The Isaac Newton Institute is the chosen IUE, with an understanding that mathematicians as social actors have developed a need for this type of research space. By virtue of their social and collegial activities over the years, mathematicians have "instantiated in action" the existence of the Institute environment. Likewise, the Isaac Newton Institute, as an equal participant in a bi-directional structurational relationship, has the power to shape and influence the mathematicians' experience of sharing and doing research.

6.4 Ethnography of Communication at the Isaac Newton Institute

To my knowledge there has never been an ethnographic study carried out in a Mathematics Research Institute. This is the first of its kind, although it is perhaps not as extensive as some other ethnographic studies that have taken place in scientific research environments. Traweek's (1988) study of the high-energy physics community, for example, in *Beamtimes and Lifetimes*, was carried out for over a one-year period. By contrast, this study at the Isaac Newton Institute occurred for three months and may be classified as a "mini-ethnography of communication." The benefit of this "mini-ethnography" is that it places the construct of collegiality in Singularity Theory into a more dynamic context for analysis. It also allows me as the researcher to focus on answering the following questions: How does the international mathematics research institute function as a specialized information use environment? What types of resources are available to the mathematicians who visit this environment? How do the visiting mathematicians communicate information to each other in the institute environment, and what are their personal collegial experiences?

6.4.1 Gaining entry to the field

The process of 'gaining entry to the field' began in 1999 when I first discovered that the Isaac Newton Institute was preparing to host a Singularity Theory program in the autumn of 2000. I was at the stage of writing my Ph.D. thesis proposal and found the announcement to this program on the Newton Institute's Web site. My proposal described a visit to the Newton Institute for qualitative data collection purposes; thus in keeping with this plan, I wrote a letter to the Director concerning my thesis, outlining its general purpose and my need to carry out ethnographic fieldwork. I explained my interest in the Singularity Theory community as an invisible college and also commented on my background experience with this research community as the former Program Coordinator of the Fields Institute in Toronto. The Newton Institute's Director received my letter and was amenable to having me come to Cambridge as a special research visitor. Nevertheless, my request was somewhat unusual to him, so he sent an e-mail message first to the Deputy Director of the Fields Institute to find out more about me, clarify my connection to the Fields Institute and also confirm the legitimacy of my study. All communications with the field "gatekeeper" were eventually successful, and once I received a letter of acceptance concerning my visit, I proceeded to make the necessary travel arrangements.

I arrived in Cambridge, England during the first week of September 2000 and spent the first two days settling in to my place of accommodation. On September 8, I took a bicycle tour through Cambridge and with a city map, I eventually found my way to the Isaac Newton Institute building at 20 Clarkson Road. The Clarkson Road area of the Cambridge campus was several kilometers west of the city center. I was living on the East End of the city, so it took me approximately 30 minutes to get there. Once I arrived, I was delighted to see that the Institute building was situated in a rural setting. The lawns surrounding the building were freshly mown and green, and there was a neighborhood of quaint English homes on the opposite side of the street. Just to the left of the Institute was the University's newly built Centre for Mathematical Sciences. To the right of the Institute was the

Wolfson Court housing unit of Girton College, and directly behind the Institute building, a construction site was in the process of developing a new physical sciences library, now the Betty and Gordon Moore Library. The Singularity Theory program term was already in progress and as I walked through the main entrance to the Institute I noticed that the building was populated with a few visitors.

My first task on my day of entry was to introduce myself to the staff, particularly the Secretary to the Director, since she was the person with whom I had the most communication before my arrival. The Director, Professor K. Moffatt, was out of town at the time, and I was told that I would not have an opportunity to meet him until a week or so when he returned. Ms. Abbott, the Secretary, provided me with a key card to the Institute and gave me instructions on how to use it to enter into to the building. With this key card, I could access my research environment any time that I wished. The secretary also gave me a copy of the Singularity Theory program package – a folder containing general information about the Institute and announcements regarding all upcoming workshops – and introduced me to some other staff members, namely the receptionist, the program coordinators and the computer systems managers. After making myself known to the staff and the purpose of my visit, I was then left on my own to proceed with my work.

My first observations were focused on the physical environment of the Institute. I was not prepared to initiate any observations or interviews until I had actually met some of the program visitors. I also wanted to take some time to ease into my role as ethnographer and let the mathematicians become more comfortable with my presence. Contrary to what I expected, the mathematicians were not all that uncomfortable. They were not particularly concerned about my presence and I suspect that this was because the Institute, as a temporary space for research, was as new to them as a visitor, as it was to me. I was not intruding upon a well-established, permanent community of research scientists; therefore my position was such that I could have been easily been mistaken for another visiting mathematician.

Soon after my arrival date, the Singularity Theory program officer approached me and suggested that I might want to post an announcement to all visitors concerning my research. She offered to help prepare the announcement and post it on the bulletin boards located in both the main entrance and the mezzanine level. I was grateful to her for her suggestion, and when we met in her office I allowed her to take a Polariod camera photograph of me to copy on to an 8 x 11½-inch piece of paper. On the same paper, we included photocopy of my University of Toronto business card and added a brief description of my research role, which read as follows:

> Alesia Zuccala is here at the Institute to carry out research for her thesis on the bibliometric structure of Singularity Theory research (as an “Invisible College” network) and the social processes of communication among mathematicians in the Institute environment. As an essential part of her research, she would like to speak to those working in Singularity Theory. If you would like to help, please speak to Alesia or contact reception.

For a start, the announcement was valuable because it generated a certain amount of curiosity among the visitors. Some of the visitors who saw the posts approached me and asked me a few questions about the study. The announcement also marked an important aspect of my entry to the field: the Institute’s acceptance of my ethnographic role and the willingness of the staff to welcome me and introduce me to the scientific community.

6.4.2 Physical layout of the Institute, staffing and services

When I first entered the main doors to the Newton Institute, I was impressed by how similar the interior architecture was to the Fields Institute in Toronto. I felt inspired to be there and at the same time comfortable, because I had just gained entry to what I considered familiar territory. The Newton Institute was built in 1992, and close to the time it was under construction a group of mathematicians and architects from Canada had traveled specifically to Cambridge to see it. The Fields Institute was later modeled after this Cambridge prototype and constructed at the University of Toronto campus in 1995. Today both the Fields Institute and the Newton institute are recognized world wide as first rate research environments. The Newton Institute in particular received the

Queen's Anniversary prize in 1998 for Higher and Further Education. It also presents itself as a very pleasant environment decorated with personalized artifacts and art pieces (e.g., The Queens Anniversary Prize Medal, a portrait of Isaac Newton and a bust of Sir Michael Atiyah, the Institute's first Director). A photograph of the Newton Institute building from the outside is shown in Figure 6.2. Figures 6.3 and 6.4 present hand-drawn diagrams of the first floor level, mezzanine level and second level of the Institute's interior floor plan (see Figures 6.2, 6.3 and 6.4).

Photo Credit: A. Zuccala

Figure 6.2. The Isaac Newton Institute For Research in Mathematical Sciences.

Note from the diagram of the first floor level that the reception area was located just inside the main entrance. The receptionist was the first person to greet incoming visitors to the Institute and it was she who provided general assistance and information to them upon their arrival. At the reception desk, visitors received their building entrance key cards and program packages. All details pertaining to the Cambridge University campus, the Institute building, upcoming talks and workshops, and services provided by the staff were included in the program package. If the visitors were interested in acquiring information about upcoming events, they could also refer to the daily and weekly postings on the bulletin board in the main reception area or the Newton Institute's home Web site.

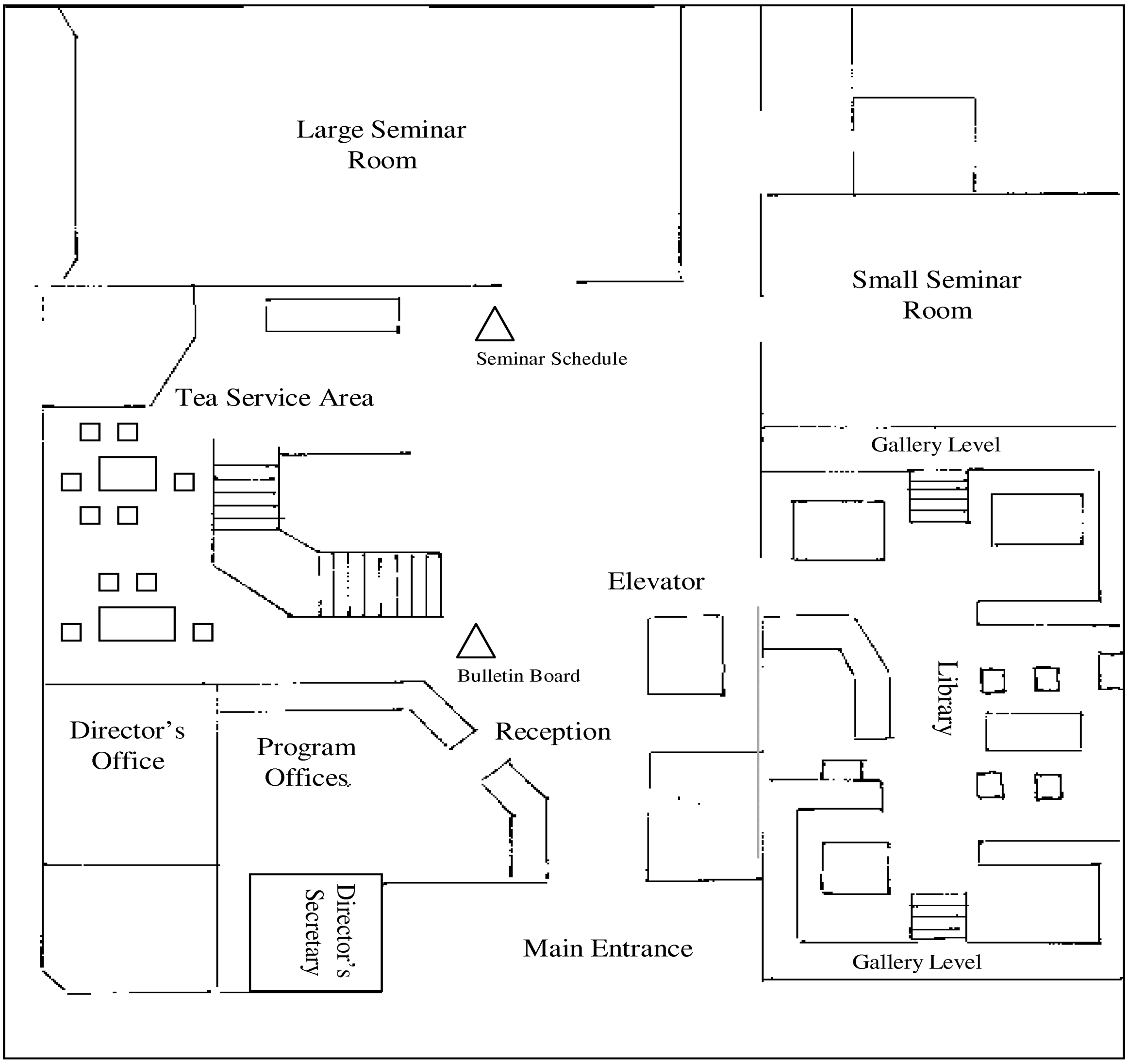

Figure 6.3. Isaac Newton Institute interior plan. First floor level.

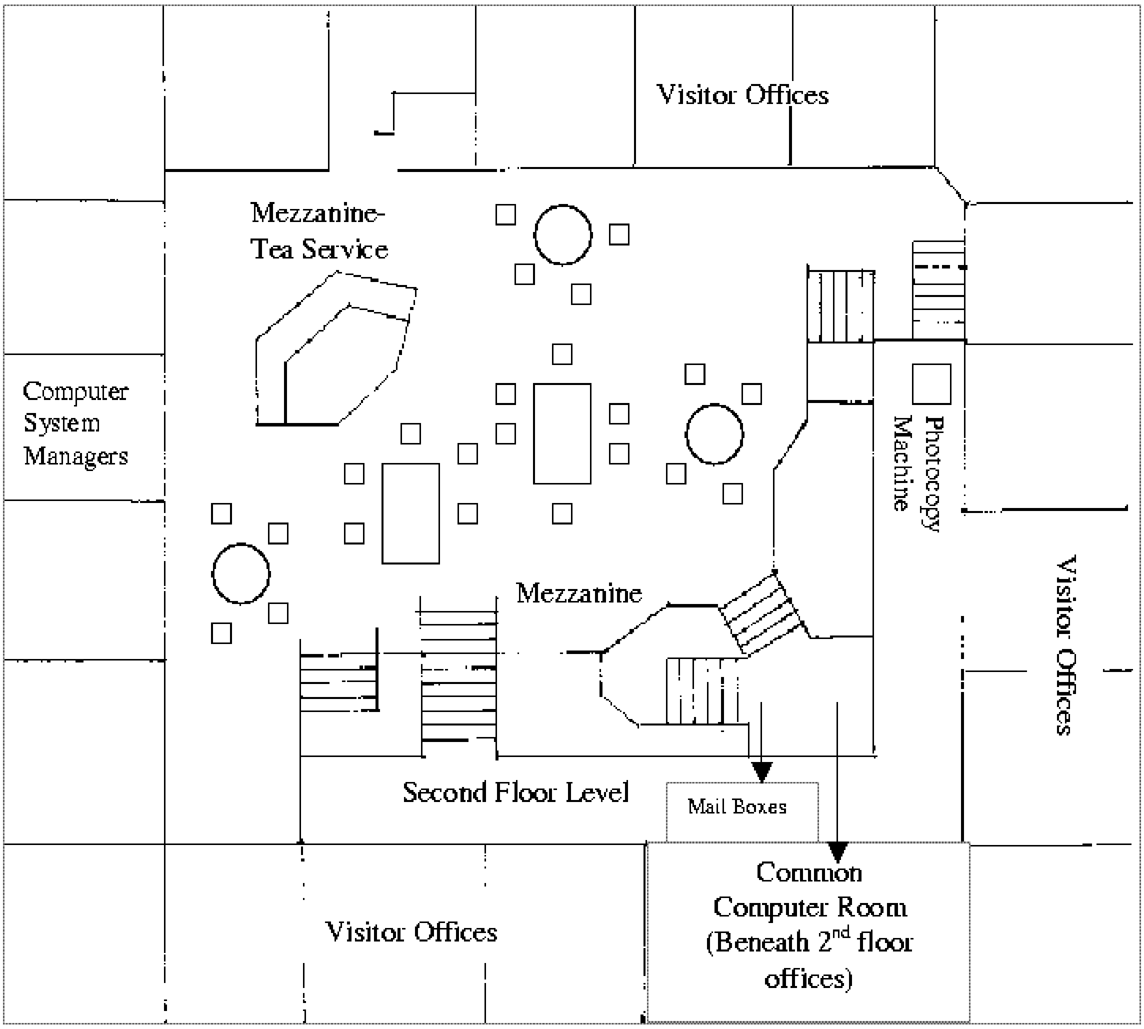

Figure 6.4. Isaac Newton Institute interior plan. Second floor and mezzanine levels.

Behind the reception desk, a windowed partition and door separated the open offices of the two program secretaries. The duties that they performed at the Institute were the most familiar to me. I once held the position of Program Coordinator at the Fields Institute; therefore I recognized that the most significant part of their job was to maintain regular contact with the scientific organizing committees. The Singularity Theory program was operating in parallel to the Geometry and Topology of Fluid Flows program; hence the secretary assigned to work with the Singularity Theory organizers was expected to ensure that all activities associated with this specific program were running efficiently. She was responsible for a variety of details, including mailing out invitation letters, receiving talk abstracts from speakers and developing talk schedules.

Two additional offices, inside the open program office, were reserved for the Secretary to the Director and the Director of the Institute. The Secretary's role was to allocate seminar rooms, assign offices to visitors, manage the use of all office equipment and provide general secretarial services to both the directorate and visitors to the program. The Director's role, as head of the Institute, was to chair the Institute's Management Committee. This Committee meets once a term to consider matters related to finance, staffing etc. and it is the Director who issues recommendations concerning all past and present scientific programs. The Director also chairs another meeting twice a year that concerns all members of the Scientific Steering Committee: a representative group of international mathematicians who offer advice pertaining to future programs. I met with the Director only once during my visit, and although he was quite welcoming, we had only a 15-minute conversation.

The Institute's library was located to the right of the main entrance, and this was where I spent the majority of my first few days. Also, because I did not have a private office within the building, the library became a makeshift office for me when I needed space to carry out one-to-one interviews. It was a reasonably large area, designed with a first floor level and mezzanine gallery. Windowed partitions facing the inside of the Newton Institute building and a series of large windows facing the outside filled the room with a lot of natural light. The first floor level housed all the mathematical texts and monographs; while the mezzanine gallery was reserved for the storage of archived mathematics journals. All texts were classified according to the Library of Congress Classification Scheme and representative of a variety of mathematics subjects. The archived journals were mainly acquired by donation and had been organized on the gallery shelves alphabetically. Large wooden tables with chairs were positioned near the shelving units at both ends of the Library's first floor, so that the visitors could sit quietly to work or read. In a special reading area, newspapers from the Cambridge and London areas were laid out on a coffee table. Visitors who chose to relax in this reading area were provided with comfortable lounge chairs next to a bay window. A narrow shelf, also located to the left of the bay window, was reserved for a new book display for previewing current publications related to the Singularity Theory Program theme. Among these publications were the Isaac Newton Institute's own series of monographs, published through a joint agreement with the Cambridge University Press. The library enquiry desk, near the entrance, was organized with a presentation of pamphlets and maps pertaining to the city of Cambridge. Visitors could browse through the pamphlets in order to learn more about current events in the city (e.g., theatre), and also find information about local restaurants and cafes. Next to the library's enquiry desk there was a photocopy machine and two bookshelves. One of the bookshelves housed a series of binders containing papers (preprints) produced by program participants, annual report data and photographs and press cuttings concerning major events held at the Institute (e.g., Wiles' solution to Fermat's Theorem). If the mathematicians needed to search the electronic catalogues of the full Cambridge library system, computer terminals were available for them near the enquiry desk. The small library of the Institute, however, was not very sophisticated and it seemed in to be set up mainly for browsing purposes. The circulation system for borrowing books was basic (i.e., not automated), and all visitors who wanted to take a book out of the library were requested to sign a book loan card and place it in an index box. General instructions were given to the visitors to keep all library materials in their offices and not remove them from the Institute premises. I was in contact with the librarian on a number of occasions during my visit, and similar to other staff members, I found her to be quite helpful. Her role as the chief information officer was to run the library, provide assistance to the visitors with their use of other library facilities in Cambridge and maintain a record of publications resulting from work done in the Institute.

A housing service was available to the mathematician visitors, and was staffed by one person at the Institute located in a small office on the second floor. I did not communicate with the housing officer regarding my own housing needs in Cambridge; however, I knew that her role was to assist both the long-stay mathematician visitors and short-stay visitors to the Institute with the most convenient forms of accommodation. The mathematicians could apply for accommodation prior to their visit online through the Institute's Web page. Depending on how long they were planning to participate in the program, the visitors had an option of either staying in a fully furnished town house unit in a neighborhood near the Institute or a guesthouse.

One of the most regularly used areas of the Institute building was the small computer room located near the mezzanine. The Secretary to the Director had introduced me to the computer systems managers on my first day and when I discovered that I was entitled to have access to this system as a special visitor, I completed a request form for an Institute e-mail address. Office space was reserved specifically for the mathematicians; yet, because of my special status, I was allowed to make use of the general computer workstations. If I needed any computing assistance, I could, like all other visitors, knock on the office door of the two systems managers (second floor mezzanine level) and ask for help.

The computer system within the Institute was based on a mix of Unix workstations, including Sun SPARC workstations, Hewlett-Packard 7000 series workstations and Silicon Graphics 02 workstations. Most of the software programs available on these workstations (e.g., Maple V; Mathematica; LaTeX 2e) were not suitable for my work, but it was clear that they were useful to the mathematicians. Throughout my visit, the second floor computer room tended to house a lot of visitors especially those who wanted to quickly log on to e-mail or use some of the software for short-term work. All of the long-stay mathematicians had computers in their semi-private offices (i.e., two people assigned to an office); however, anyone who came to the Institute for about a week or two was relegated to a shared workstation. At times the common computer room was so busy, especially during the workshop periods, I had to wait awhile before a workstation was available.

The most important communication areas of the Institute were the two seminar rooms located on the first floor level, the tea service area behind the staircase, and the mezzanine level on the second floor. Once I became more comfortable with my ethnographic research role, I spent most of my time on the mezzanine level because this seemed to be the prime location for informal communication and conversation eavesdropping. In Chapter 5, I describe the interior layout of the mezzanine and some of the collaborative situations that I observed there. Opportunities for eavesdropping were not as plentiful in the tea service area as they were in the mezzanine, primarily because the visitors tended to only congregate in the tea service area either at 10:00 am or 3:00 p.m., after the talks. The tea service area, however, was not used only for the Institute's regular "tea-time." At the start of a workshop or after a special evening talk, wine was often served to the visitors. Sometimes I offered to help out the service staff during these evening receptions, because I found that it was a good way to make myself more visible. The "tea-time" or reception area was not very spacious, and when fifty or so of the visitors crowded together to socialize, there was standing room only. Often the mathematicians stood and conversed together in groups of three or four persons, and when an interesting mathematics discussion took place, some of them made use of the nearby blackboard attached to the back of the Institute's main staircase.

The two seminar rooms in the Institute were spacious, but reserved mainly for lectures. In the larger of the two seminar rooms, rows of tables and chairs were set up to face the front lecture stage. An overhead projector and screen were available, as well as a podium. Perhaps the most distinguishing features of the lecture stage – a feature one would expect in a mathematical environment – was the overhead set of vertical sliding blackboards. A speaker could write out parts of his/her lecture on these blackboards, and instead of erasing the work, pull down another board to create more writing space. When I spent time observing the seminar periods in the lecture halls, it was mainly to listen to the Singularity Theorists present lectures and to learn more about the mathematicians' formal ways of speaking or communicating.

6.4.3 The Singularity Theory program

The Singularity Theory program term at the Isaac Newton Institute was planned by a scientific organizing committee chaired by Professor V. ARNOLD, Professor J. W. BRUCE, Professor D. SIERSMA and Professor V. GORYUNOV. Due to a serious bicycling accident ARNOLD "was not able to attend the program" (Newton Institute Annual Report, 2001, p. 26). Consequently, I did not have an opportunity to meet with him or request an interview during my ethnographic visit. In fact, I did not have an opportunity to talk at length with any of the program organizers, accept for SIERSMA. SIERSMA, along with GORYUNOV was responsible for "the day-to-day" activities at the Institute, while Professors ZAKALYUKIN, WALL, GIBLIN and NIKULIN also played significant roles during the program as organizers of specific workshops.

The first organized event associated with the 2000 Singularity Theory term was the NATO Summer School on *New Developments in Singularity Theory* (July 31–August 11). Work-related obligations in the city of Toronto prevented me from traveling to Cambridge in July; therefore, my fieldwork did not include this period. However, following my arrival on September 8th, I participated in the autumn Workshop on *Applications to Wave Propagation Theory and Dynamical Systems* (September 25-29). I also participated in the Workshop on *Applications to Quantum Field Theory* (October 23-27), as well as the informal discussion meeting on *Different Aspects of Singularity Theory* (November, 24-25). A Satellite Workshop on *Applications of Singularity Theory to Geometry* was held at the University of Liverpool in December (December 16-21), but because my research plan was focused on the Newton Institute environment, I did not make the effort pursue further research outside the Clarkson Rd. building. I spent on average, three to four days a week at the Institute, generally from 10:00 a.m. to 6:00 p.m. in the evening. By December 16th, when all the mathematician visitors had left for Liverpool, I had collected a significant amount of data and decided that it was an appropriate time for me to conclude my fieldwork.

The workshop activities brought in a large number of short-term visitors to the Institute, many of whom traveled to England from other parts of Europe, North America and Russia, and others who came in locally from the Cambridge, Oxford and London areas. [20] During the registration period of the first autumn workshop (i.e., *Applications to Quantum Field Theory),* I recall being impressed by the multi-lingual atmosphere. I wandered about the main entrance to the Institute, and as I passed some of the small groups of mathematicians engaged in informal conversations, I recognized a mix of languages and accents, including French, Russian, English, Spanish etc. There was a comfortable feeling of "collegiality" among the visitors and it seemed as though they were re-acquainting themselves with old friends.

The workshops, as I soon discovered, were not exclusively based on Singularities research, even though this subject area occupies a fairly central position in mathematics. Since it is a mature subject much of the foundational work has already been done. It is now at a stage where the theories are applicable to other research areas; hence part of the Newton Institute's role throughout the program term was to facilitate the advancement of these applications:

> Singularity Theory lies at the crossroads of the paths connecting the most important areas of mathematics … For example, it connects the investigation of optical caustics with simple Lie algebras and regular polyhedra theory, while also relating hyperbolic PDE wave fronts to knot theory and the theory of the shape of solids to commutative algebra (Isaac Newton Institute Annual Report, 2001, p. 24).

While short-term visitors were travelling to and from the Institute for the weeklong workshop periods, approximately 50 of the mathematicians remained on site for the full program term. Professors TROTMAN, BOGAEVSKY, DAMON, SEDYKH and GAFFNEY were among the long-stay visitors; however, I did not necessarily see them at the Institute every day. Sometimes they remained at home in their guesthouses, traveled for short periods, and/or took part in tourist activities. I did not follow or observe the mathematicians when they were tourists, but this was expected in a

[20] The Isaac Newton Institute's Annual Report notes that there were 87 short-term visitors in total.

beautiful city like Cambridge, and comments regarding such activities came up in friendly conversation.

When the long-stay visitors were seen at the Institute, I noticed that they would come in at different hours of the day – some arriving in the morning, others later in the afternoon – to carry out private or collaborative work, or participate in the daily seminars. Usually they worked in their office, with their doors left slightly open, signifying a certain level of availability or openness to having other colleagues 'drop in' for conversation. When they emerged from their offices, it was mainly to have a tea/coffee break, check their mailboxes on the second floor, pick up papers from the printer, chat with other visitors on the mezzanine level or leave the building. By contrast, the short-term visitors (up to one-week stay) were generally seen in the seminar rooms and if they were not attending lectures, they usually sat in the tea service and mezzanine areas to relax, eat lunch or discuss research.

I am not a trained mathematician, and have very little knowledge of Singularities research; therefore one of the most important events for me at the Institute was the special staff lecture given by Professor SIERSMA. The Singularity Theory program coordinator informed me of this lecture on the same day that it had occurred (September 20th) and invited me to attend in the small seminar room. Elementary talks to the staff concerning the program themes were apparently a regular occurrence at the Institute. Their purpose was to give all of the staff members an opportunity to learn more about the scientific activities they were supporting. Since I was present for my fieldwork, I had an opportunity to learn something as well. I brought a notebook with me to the lecture and made an effort to copy some of SIERMA's chalk-drawn figures. Despite my attentiveness, I found some of the mathematical concepts difficult to understand. When the lecture was finished, I remained in the seminar room with the program coordinator to ask a few questions. This staff member seemed to understand Singularities research a lot better than I did, and unfortunately, both my questions and hers did not enlighten me too much. Professor SIERSMA was keen to explain the mathematics, but much of the knowledge that I have acquired to this day is still probably best summed up by a general definition of a Singularity: “the point on a curve that is different from all others.”

With respect to scholarly communication within the Institute – i.e., the mathematicians’ social habits of communicating and sharing information – I was perhaps in a much better position to observe and take notes. Communication in the scholarly tradition of mathematics is not very different from communication in other scholarly areas (e.g., Information Studies) since there are both formal and informal aspects. The formal aspects in mathematics include the writing of papers for peer review and publication, and the oral presentation of papers at academic conferences. Informal communication occurs when mathematicians discuss their research, for instance, by sharing ideas in conversation with one another, exchanging preprints in person or through e-mail, or working together front of a blackboard to write out and question the meaning of a mathematical object or proof.

At the Isaac Newton Institute, my observations of the mathematicians’ formal communication habits occurred during the Singularity Theory program seminars, and also during some of the Institute’s general, non-program talks. Throughout the weeklong workshops, the seminar periods occurred up to 5 times daily, but during the regular program period, there were usually two seminars per day. It was the norm for all visitors to the Institute environment to view a posting of a workshop or program term talk schedule at least one week before it occurred. On the bulletin board located just outside the large seminar room, the mathematicians could preview the names of upcoming speakers, talk titles and a short abstract of the talks in order to have an idea of what to expect. When a regular program seminar took place, approximately 20 to 30 of the mathematicians attended. More visitors (approximately 40 to 50) participated in the workshop seminars; however the seminar room was not often filled to capacity.

There was only one time when the seminar room was completely filled, and this was when Professor H. M. S. Coxeter arrived at the Newton Institute to give a “special seminar.” Special seminars or “distinguished lectures” as they are called at the Fields Institute, are common in mathematics and are usually established to honour the work of well-known researchers who have made significant contributions to a subject over the course of their career. I was not surprised to see that Professor Coxeter's name was attributed to this type of seminar at the Isaac Newton Institute. Due to my program work at the Fields Institute in Toronto, I was aware of his status as a geometer at the University of Toronto and I also knew that the Fields had named its own special Coxeter Lecture Series after him (Fields Institute for Mathematical Sciences, 2003).

Professor Coxeter's special seminar was held on the evening of September 18th. A variety of people came to hear him speak, including many of Singularity Theory visitors. There was a feeling of reverence in the seminar room and the audience members were generally quiet. I suspect that the large turnout was due not only to his status as a mathematician, but also to his age: he was 94 years old at the time. [21] A question and answer period was reserved for after the talk; therefore, nobody motioned to interrupt Professor Coxeter. Also, none of the audience members spoke to one another; they seemed intent on giving him their undivided attention. I would not classify this behaviour as the norm for all other types of seminars or lectures in mathematics. I think that because it was a very special lecture it generated a more obvious tone of respect.

When I sat in the seminar room to observe the Singularity Theory seminars, the tone was still formal, but generally more relaxed than it was during Professor Coxeter's talk. At a regular talk, it was not unusual for mathematicians in the audience to interrupt a speaker to ask a question. Questioning behaviour seemed to be a function of the audience members' stimulated interest and/or perceived relevance to their own work. Unless the speaker or seminar chair instructed people in the audience to reserve questions for later, the speaker would oblige a colleague's interruption with a comment or explanation. Sometimes, depending on the nature of the question, a conversation might occur between the speaker and audience member, but normally such sideline conversations were not carried out too long. The seminar periods were limited to 45-minutes; thus the speaker generally had to focus on economizing his/her time. On workshop days when up to four or five talks were

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[21] Professor Coxeter died at the age of 96 in March, 2003.

scheduled, the tea breaks were especially valuable to the mathematicians. If they did not have an opportunity to ask a question during the seminar, or clarify certain parts of the talk, they had an opportunity to do so with the speaker at the Institute tea.

Often before a mathematician gave a seminar at the Institute, he or she would comment on its overall purpose. If the speaker intended to present new mathematical results or if he or she simply designed an elementary talk, accessible to a wider group of mathematicians, the audience members were told beforehand. For example, in the large seminar room of the Institute, I heard one speaker say: "Because the audience is made up of Singularity Theory people, I'll assume that you don't know anything about mechanics. This is an elementary talk…." I also witnessed a speaker directly ask his audience for their mathematical input: "I'm not an expert on Singularity Theory. I see some members of the audience who are, and maybe they can help. Maybe we can work together to solve the problem."

Compared to talks given by conference speakers in the social sciences (e.g., Information Studies) mathematicians give the impression of being more spontaneous. They do not always deliver information with a full set of overhead slides, and even when speakers did use overhead transparencies, they rarely consisted of paragraphed notes and were often left blank for writing purposes or to present hand-drawn figures. It was typical for the speaker to highlight parts of a proof or a figure with blue, red or green marker on a transparency for emphasis. More often than not, the mathematicians would bring pages of personal notes into the seminar room, place the notes on the podium, and generate their talks dynamically from their minds. Occasionally they would refer to their notes when they proceeded to write parts of a proof or a figure on the blackboard. Use of the overhead blackboards in the Newton Institute seminar room meant that speakers sometimes needed to turn their backs to the audience; however they would usually turn around again to address their audience directly, and use a pointer or simply gesture to what they had written.

Throughout my Institute visit, there were only so many talks that I could attend without reaching a point of feeling bored. I admit to this experience, because it was something that I eventually identified as being a mild source of anxiety. If I was becoming bored with my observations of the seminar periods, did this mean that I was not a good ethnographic researcher? Did this mean that I would not succeed at interpreting the mathematicians' communication patterns? Chatman (1984) assures all researchers in my position that a certain amount of stress and anxiety in the field is normal. Once I grew to accept this, it became a little easier for me to tolerate and reflect on the nature of my boredom, and work towards overcoming my difficulties.

Mathematics research to me is a world of incomprehensible ideas, problems, and proofs. Each time I participated in the seminar periods and each time I struggled to understand their meaning it became clear that the prescribed rituals, acts, norms, and behaviours associated with these communicative events could only convey a small part of the collegiality story. Communication during the Institute seminars may start with a seminar ritual, but it does not end there. A lot of what happens within this type of communicative situation and beyond occurs also in the mind of the receiver. For this reason, I decided to interject my observation periods with periods devoted to one-to-one interviews with the Singularity Theorists. With my interviews, I could focus more on the thoughts and experiences of the mathematicians themselves and learn more about what it was like for them to be an Institute visitor.

All of the serious aspects of my ethnographic fieldwork occurred during the daily program-related events at the Institute. Fortunately, I had the opportunity to participate in "less serious" periods of fieldwork outside the Institute environment. The Cambridge University Press wine and cheese party was one such activity, as well as an evening dinner hosted by one of the Singularity Theory mathematicians and his wife. The dinner in particular allowed me to observe some of the friendships among members of this community – details of which I am not at liberty to write about in this study. It was an event that brought me into the community as an "insider" for a brief time. The select persons at this dinner party knew who I was and were amiable towards their international colleagues and me. Many of them had seen me at the Institute and some had even participated in an interview. We conversed about a variety of topics, including politics, travel, and family life.

Needless to say, I grew to appreciate the social personalities of the mathematicians; I was pleased that I had the chance to know them both as researchers and as people.

6.4.4 The mathematicians' collegial experiences

Every year an Annual Report is published by the Isaac Newton Institute to elaborate on the success of its programs and to confirm the research achievements of its visitors. One and a half years after my ethnographic fieldwork at the Institute, I contacted the Institute's librarian by e-mail to ask if I could have a copy of the 2001 Report highlighting the activities associated with the Singularity Theory program. She sent this Report to me by regular mail and after I read the information, I decided that it would be useful to reflect on some of the noted program outcomes vis-a-vis my interview data.

The 2001 Annual Report indicates, for example, that "there was a very strong participation in the conferences, workshops and other events during the [Singularity Theory] program" and that "the majority of experts in the field attended" (p. 25). This statement is true; however, my conversations with some of the mathematicians confirmed that in terms of bringing together international experts, the program had suffered to a degree because ARNOLD was absent. ARNOLD is a very dynamic figure in this international research community and VASSILIEV, his former student, had suggested in our interview (see discussion section, chapter 4) that there was an energy crisis without him. As one of the main experts in Singularity Theory, ARNOLD would have been a valuable leader in terms of asking questions and promoting ideas to "establish good relations" among the visitors. This does not mean that such relations did not occur, but they may have taken a different quality if ARNOLD had been there.

The Annual Report indicates that:

> there was a certain amount of interaction with the participants of the parallel programme on *Geometry and Topology of Fluid Flows*. Contacts with Pelz Ricca, Michor, Khesin, Shnirelman and others helped the [Singularity Theorists] to realize current needs of this branch of applied mathematics (p. 28, original emphasis).

Also in terms of specific collaborative achievements of the Singularity Theorists, the Report states that

> Ebeling and Gusein-Zade made considerable progress in their work on an algebraic formula for the index of a differential form at an isolated complete intersection singularity.
>
> Damon, Gaffney and Mond worked on a conjecture claiming that the image of a map-germ from n-space to (n+1)-space is a free start divisor in a sense of Damon. This would yield a formula for the image Milnor number (p. 29).

Clearly important mathematical outcomes resulted from the program, but somewhere in between the program's initiation and the Annual Report's publication, the mathematicians themselves were present and "collegially" involved with one another in Institute's Information Use Environment. My aim as a special visitor to this environment was to acquire as much of an in-depth view as possible of their personal experiences (see Appendices H and I respectively for interview schedule and interviewee consent form).

As mentioned previously, SIERSMA was one of the scientific organizers. When we had an opportunity to sit down together for an interview, I asked if he would tell me, from his own perspective, how the program term began and how he thought it was progressing:

> *I. What were some of the events leading up to the Singularity Theory program here at the Newton Institute and how did you become one of the organizers?*
>
> R. Ok.. well.. that is easy. So the first organizer was Arnold and he brought up the idea [of having this program] in relation to the Newton institute. He was in contact with the Director and the idea came up that he would be the organizer of the Singularity Theory program here and find some co-organizers. And, when I met him… I think three years ago, at Oberwolfach [Germany]…. he asked me "Dirk will you write together with me a proposal"and then we talked a little bit and we also saw Bill Bruce around and together we wrote the proposal.
>
> *I. So from what I understand, one of the policies of the Newton Institute is to foster a kind of' interdisciplinarity' between one area of mathematics and its applications to another area. In your proposal did you put forth this idea?*
>
> R. Oh yes…. If you look to the proposal, there are lots of ideas, which give insight into different mathematical streams and also some complex applications.
>
> *I. To areas such as physics?*
>
> R. Physics… and things… everything related to wave propagation theory …

I. So now a whole month has passed since the program has started. What is your impression of the process of the program… for instance, how it has been going.. uhm…what are some of the interesting developments ..?

R. So… lots of short-term kind of processes are occurring as you see, especially during the meetings… so people who come in for a short time can interact with the other people, listen to talks and also talk with each another. It is a fairly fast exchange; nevertheless, it is an exchange of ideas… and so you see it also now if you look around …[tape inaudible]… And then you have the long-term people, so the influence of some people like Vassiliev and other people…[inaudible]. and Dubrovin.

[*Note: Dubrovin gave a series of nine lectures during his long-term on Frobenius manifolds*]

R. [The long-stay visitors] have more time to talk a little bit more about new directions. And then of course, one of the optimal things is that people have more time to think. At the Institute there are so many others here, so they influence one another. They then get new ideas and time to explore those ideas …. and part of that they could also have done it just alone… but …. I think it is very important that there are a lot of experts next door, then you can ask something about which you are not really a specialist in and you know who to ask.

I. I know that you have been really busy with your scientific organizing role, but have you found it helpful to be here with respect to your own research?

R. I have been still quite busy with things… in organizing the first three weeks, the first meeting…but then after that I have been profiting especially by meeting also Tibar here… and we are still working on joint research. But still being an organizer is not the best thing…

SIERSMA's position as a program organizer was not ideal because he had less time than many of the other visiting mathematicians to benefit from the Institute's collegial environment. He was, however, still very involved in the seminars and did manage to set aside time to focus on his own research interests. When he was not preoccupied with his program role, I saw him engaged in informal meetings with TIBAR to continue their work on a joint paper: one that they had started prior to the program term. It was the case then that not all of research at the Institute came from communicative situations derived from specific program activities. Sometimes the mathematicians were simply making use of the Information Use Environment to focus on projects (collaborative or otherwise) that were already in progress.

Long-term visitors, in particular, appreciated their stay at the Institute because it offered them a reprieve from the teaching and administrative duties of their home universities. Yet during this reprieve, it was important for them to maintain a purposeful research balance. When I asked DAMON to relate his impression of the Institute, he said:

> Um… wow!. It's sort of great! I really love being here! It's one of these things where.. I mean.. I even said this at a meeting with some visiting committee that there seems to be an overabundance of riches in as far as just the sheer amount of activity, you know.. with conferences and talks and everything… I think the hardest part is forcing yourself to divide up your time, so that you have time to do your own things and not just be totally absorbed in all the lectures that are going on… That's something for me that takes self-control, because it is very easy… I mean there are so many thing interesting things being covered by these conferences and seminars that it would be very easy for me to go to everything.. and never do anything yourself… uhm the main thing is realizing 'look, I only have so much time' but I'm going to limit what I go to…

Since DAMON was visiting for an extended period, the need for balance may have been more critical for him than for others who had arrived for only a one or two-week visit. In light of this, I asked him what his experience might be if he had come in for just a short term to attend a workshop:

> I think probably I would go to the majority of lectures at a workshop…. and also try to fit in time to talk to people.. to ask questions ..

As a long-stay visitor, DAMON had been assigned an office, so I was curious to know if he was spending a lot of his time there when he was not at the lectures:

> I spend, I guess about three days a week, roughly… some weeks maybe four.. and I'm not even there the whole day. Sometimes I don't get there until later in the morning. Some days I stay back … you know…where I'm living, just so that I have time to think about stuff. … I mean it's great to talk with people… you know, to kind of throw ideas around and hear what people are thinking…and that has long-term benefits, but in terms of the immediate thing you are trying to accomplish.. you know.. you want to get something written up.. or completed… you need time alone to think.

Mathematics research involves maintaining a balance between work that is social and work that is private. Mathematicians develop their technical skills, for the most part, as subject specialists. When they are learning new techniques or evaluating the significance of a problem, interaction is valued with other similar specialists to make judgements about, or acquire new information. On the other hand, when ideas must be developed for publication, private time is needed for uninterrupted thinking and writing. The Institute's Information Use Environment, which is a rich space for social interaction, makes it even more essential to preserve this need. Publications legitimize the mathematicians' research experiences; therefore, an appropriate amount of space must be reserved to ensure that work is produced for formal recognition.

Merz (1998) relates a similar finding with the theoretical physicists she observed as visitors to CERN (the European Organization for Nuclear Research). Research among the physicists alternated between "talking phases" and phases of doing physics. Face-to-face contact occurred when two or more physicists involved themselves in a research discussion based on setting up a problem or talking about physics while writing computations on a blackboard. As one visitor explained: "you are sitting at a coffee table, say, and you meet some colleague and you say 'oh so what are you doing now' and so on" (p. 317). At other times the physicists retreated to their offices alone as a way of "claiming space" for themselves. This was when they preferred "not to be continuously 'bothered;' to follow [their] own tempo when doing a computation or solve a technical problem without having somebody 'look over their shoulder' (p. 324). Merz refers to this back and forth shift from solitude to social interaction in physics as the "balance between the preference for independence and the wish not to struggle alone" (p. 324).

At the Newton Institute, not all of the mathematicians arrived with the intention of staying for a long period; therefore, part of my interview time was devoted to learning more about short-stay visitors. My interview with Dr. Romero-Fuster is of particular interest:

> *I. What is your general impression of the Newton Institute?*
>
> R. I think that it is a pleasant place… a quite and nice place to spend some time doing research. What I find is that the library is not very complete. I don't know whether this is because there is perhaps some other library in Mathematics on the campus somewhere else… or why… probably I would find that if I had to be here for awhile, I would need some more…..[long pause].
>
> *I. Have you been here for very long*?
>
> R. I have only been here for two days… but I took a quick look at the library and found that it was not very complete …
>
> *I. Have you already experienced a need to use the library?*
>
> R. No… I just went there to have a look…

I. Just browsing?

R. Yeah, just browsing. But anyway [laughter] I do not have any time to read… because I am at the lectures. I think that one of the main reasons for being in an Institute like this is to make contact with people and do something… kind of you know… make a collaboration on research.

I. Were there any particular information-related reasons that brought you to the workshop? [Workshop on Applications to Wave Propagation Theory]?

R. No.. no…

I. A problem that you are working on…. or ideas that you have…

R. Yes yes yes yes…the people and to know what they are doing… Actually the people at this conference, I already knew… so it is always nice to know what they are doing at the moment and to tell them what you are doing .. Sometimes you are attending to a lecture and the speaker says something that is connected with something you thought about… and then maybe the speaker is presenting another viewpoint… or using another technique, which you think might be very useful. If you joined them, the things that you already have with that persons.. it could be very useful. You would like to learn more about the technique…

I. So the lectures are useful?

R. The general point about the lectures is that you are not expected to understand everything that they say… but it's something… you know… which makes you get more interested. It may be the case that the lecture has nothing to do with what you have done. But if it has something to do with what you have done and it is put in another viewpoint, then you get interested more and more…

When Dr. Romero-Fuster reflected on her primary reasons for coming to the Institute, the most notable aspect of her response was her reference to "the people," rather than the subject area. Her motivations were obviously social in nature: " I think that one of the main reasons for being in an Institute like this [i.e., for a short-stay] is to make contact with people." She had been invited to give a talk on *Contact directional fields and Legendrian singularities in the global geometry of submanifolds*, but when she participated in the one week *Applications to Propagation Theory* workshop, her expectation was that she may or may not understand all the information presented. Still, she was curious to find out what other mathematicians were doing within the Singularities community – a community in which she obviously felt she belonged. Romero-Fuster's sense of belonging is significant, considering that her name was not included on the final ACA map of Singularity Theory. From an historical co-citation perspective she was not a major research figure.

In fact, she was peripheral, but her current involvement in the broad subject area was evident and I had realized this when she commented on the number of other Singularity Theory conferences she had been to in the past. She had, for instance, attended the conference in honor of Vladimir ARNOLD, the “Arnoldfest” held at the Fields Institute (June 15-27, 1997) and even mentioned ARNOLD as one of her main mathematical influences. With SEDYKH, one of ARNOLD’s former students, she had also published a collaborative paper *On the Number of Singularities, Zero Curvature Points and Vertices of a Simple Convex Space Curve* (Romero-Fuster & Sedykh, 1995).

In contrast to Dr. Romero-Fuster, LOOIJENGA was presented as one of the more highly co-cited mathematicians in the Singularities literature. When I spoke with him about his visit to the Institute, I came across yet another perspective. He seemed to be less invested in the subject, although his long history of being socially connected to the community was evident. LOOIJENGA was once a student of mathematics in the Netherlands, at the same time as SIERSMA. Both of them had the same thesis supervisor, Professor N. Kuiper and LOOIJENGA spoke briefly with me about the period of his career when he followed Professor Kuiper to the IHES in France, where Kuiper was working with THOM. My understanding was that LOOIJENGA’s short-term visit was due to the connection between Singularity Theory research and other closely related areas in mathematics, namely Algebraic Geometry. I had asked him to comment both on the problems he was working on, including his interest in *Applications to Quantum Field Theory*:

> R. I’ve sort of left Singularity Theory. I am no longer really active in the Singularities area… On the other hand Singularity Theory is part of mathematics where other parts of mathematics come together … If you think of mathematics as a spectrum… well.. there’s algebra, there’s geometry, there’s analysis… and so on…And so these are names that cover only parts of mathematics… and Singularity Theory is touching… or is a part of where many of these things get together.. And since I work in adjacent areas, I occasionally run into Singularity Theory.
>
> *I. So at the moment are you working in an adjacent area?*
>
> R. Yes, I would think so. I gave a talk here so…that’s about automorphic forms.. and, well it is about research. that for me goes back to the particular time when I was working on a specific problem in Singularity Theory…

I. Oh it does. OK. So there is a connection to some of the research that you've done in the past…Are there people here at the Institute with whom you have some intellectual connections?

R. Yes… I think so. There was another talk this afternoon by Professor Nikulin and that talk was in the same general area. I mentioned his work and then later, in his talk he mentioned my work.

I. Ok.. so you have been referring to his work and following his papers in the past year or two years… and he as well?

R. Well, I don't know… he gave his talk after mine and he was referring to my talk. I thought … well maybe that's not quite the same thing (as referring to past papers).. but mainly I am answering the question of whether or not we are aware of what each other is doing … I think that what I'm doing fits in, but it is not Singularity Theory per se…

I. I spoke with Professor Nikulin earlier and he said that he could more or less be classified as an Algebraic Geometer. Is this the same for you then?

R. Yes…and I'm not the only one here…

As LOOIENGA suggests, the Newton Institute environment was populated with a broad spectrum of mathematical visitors. Each had some connection to or interest in Singularity Theory, but not necessarily in a pure sense. This spectrum ranged from persons newly involved in the research area (i.e., Romero-Fuster), to persons well-recognized (co-cited) for their long-term connection (i.e., LOOIJENGA), to other visitors like Dr. Morrison, who had been invited in as an 'outsider' to help build a closer relationship to with the theoretical physics community. Morrison was another short-stay visitor who participated in an interview and when we conversed, I discovered that he was a unique type of outsider in that he was a former mathematician-turned-physicist. The following transcript reveals some of the significant aspects of his experience:

I. I'm interested in your position with respect to Singularity Theory research. I know that you are here giving a lecture at this conference, well actually, you are giving three and from what I understand you are not really a Singularity Theorist. Were you officially invited to the Newton Institute? Can you tell me some of the events leading up to your arrival?

R. Nikulin, one of the workshop organizers, called me up and told me about this workshop and program term… and asked whether or not I could come for some part of it…. or just the workshop. Since I'm teaching, to come for more than a week was not really possible, so I said that I could just come for this workshop. Uhm.. in the early planning phases of this workshop, as I understood it, they were really trying to bring in a lot of people who are both mathematicians and physicists – people who are working at the interface of the field.

I. What is your impression of the research environment here at the Newton Institute?

R. For workshops like this, I think the main thing that makes it a valuable workshop is which colleagues show up. If there are people to talk to and they are giving interesting seminars then the actual physical setting of the workshop is a little irrelevant except for the fact that it is a pleasant place, or the staff are really good a helping people out… I haven't had a chance to come for a longer stay, so I don't really have an opinion about how the place functions for long term visits. The reputation is good and because of the reputation I would come and try it out for a longer stay.

I. Of course if you were here for a longer stay, then you would be here to see a variety of different people come and go over that period and this would increase the chance of meeting with the colleagues that interest you.

R. Sure…

I. I guess the mathematics community is fairly mobile.

R. I am one of the more mobile persons.. .. I love travelling

I. So have you visited other Institutes like the Isaac Newton Institute?

Morrison related details associated with his visit to the Institute for Theoretical Physics in Santa Barbara, as well as his visit to the Mathematical Sciences Research Institute (MSRI) at Berkeley.

I. Were you at the MSRI for a Singularities Conference?

R. No it was a program in Algebraic Geometry in 1993 or 1994. So Algebraic Geometry is… people who call themselves Singularity Theorists would probably see themselves as taken with Algebraic Geometry and other similar parts of mathematics – it's one of the basic underpinnings…

I. So you are quite mobile then and would you say then that you are mobile as a physicist or as a mathematician?

R. Well…during that period [at the Institute for Theoritical Physics, Santa Barbara] I had a kind of a conversion from a mathematician to a physicist. I was really a straight, pure mathematician, until 1991, and uh… in 1991 I started working with some physicists for what I thought was a part-time basis and then it sort of took over everything until I got to where I am now doing a lot of work fairly directly in physics, although I still bring a mathematician's perspective….. In fact, I would have to say that that year and a half at the Institute was an important time for this transformation…So that was fall of 92, spring of 93 and fall of 93… so I had been kind of trying to do this work myself for a year…and I went into an environment where I was talking to both mathematicians and physicists at the same time and having both of those groups there at the Institute was really important I think…I developed strengths in both areas…

I. Maybe you can tell me what problems you are working on as a mathematician or a physicist that are relevant to Singularity Theory….

R. So uh… let me just preface this by saying that this workshop has been interesting because there are a number of different ways in which Singularity Theory has had some intersection with String Theory… the particular area that I am working in …um and the organizers I think have tried to tap into several different areas… but really the workshop has kind of taken a life of its own and a lot of the talks do not have much to do directly with Singularity Theory…

I. Oh really?

R. Yes… [the workshop] has to do with these topics that one might say were an outgrowth of the intersection between Singularity Theory, Algebraic Geometry and String Theory. I don't want to characterize the seminars that I've heard so far… but uhm… certainly some of them have been really interesting talks, but it would be hard to say that these talks were really about Singularity Theory. I am going to give three talks… and in the first talk I am going to describe a point of contact between Singularity Theory and physics. It's actually going to be an update of a talk that I gave four years ago… My second lecture is really about something I've been thinking about recently and it is about another theme between Algebraic Geometry and String Theory… and there are points of contact between that and the Singularity Theory stuff.. so in lecture three, I am going to try to bring it all together… but I have to admit that that's a work in progress and I'm going to keep working on it until Friday and on Friday I'll know better exactly what I am going to say…

I. That's cutting it close!

R. That's cutting close.. that's cutting it close ..… but I have things I could say no matter what… but I'd like to be able to talk about something new…

I. So you are presenting new results then…

R. At the third lecture….

The role of the Isaac Newton Institute as a special research environment was to shape or re-shape the mathematicians' experience of Singularity Theory research by advancing its connection or "bridge" to other subjects. Dr. Morrison's role, based on his dual strengths as a physicist and mathematician, was to function as a "conduit" to this type of bridge building. Although Morrison explains that the important aspect of a workshop is "which colleagues show up" and not the physical environment, Rosenbaum's (1993; 1996) structurational theory of the IUE may be used to explain an alternative view. The Isaac Newton Institute provides a physical space for mathematics research, yet it has no meaning without the mathematicians' interactivity as social actors. The social actors "instantiate in action" the Institute's existence with "routinely produced and reproduced social practices" (Rosenbaum, 1996, p. 112). On the other hand, when the social actors enter the Institute environment, it too imposes structure to their communicative interactions; their information-related behaviours become “constrained, shaped and enabled by the IUE” (Rosenbaum, 1996, p113). For instance, lecture periods constrain or shape research discussions to a certain period, allowing only a few people to interact at a time (e.g., the speaker and one audience member). Alternatively, the tea service periods provide a more informal context for interaction, allowing more persons to become involved in a research discussion for a longer period. With these environmental elements in mind, Morrison’s other statement holds more interest: “the workshop has kind of taken a life of its own.” The *Applications to Quantum Field Theory* workshop had created a new “life” in mathematics because something unique had occurred when the Institute’s IUE intersected with the particular group of social actors who agreed to share their research. According to the Newton Institute’s Annual Report (2001) “it was one of the most inspiring events in the entire program bringing a large number of new problems into the area of Singularity Theory” (p. 27).

So far the experiences that I have highlighted in this section have been those of persons holding senior research/teaching positions in mathematics. Senior mathematicians generally have research money available for travel and are usually invited on a regular basis to participate in major conferences, however, the Singularities program did also attract a small number of student and postdoctoral visitors to the Isaac Newton Institute. Two of TROTMAN’s doctoral students traveled from Marseille, France for separate visits to attend both the Summer School on *New Developments in Singularity Theory* and a week-long portion of the autumn program term. Some of Dubrovin’s students and postdoctoral fellows were present for a short-term visit as well. M. SAITO and I spoke briefly in an interview about his students in Kyoto, Japan and I had asked him if there was a plan for them to travel to Cambridge. When he said that they were not involved, I asked if it was due to a lack of financial support. SAITO explained: “Yeah, it was something like that. I don’t want to talk much about this, but there was some confusion between me and the organizing committee… but anyway this confusion made some of these young people expect that they would attend.”

One of TROTMAN’s students stated in an interview that “there were many different subjects” highlighted at the *New Developments in Singularity Theory* Summer School, but she also said: “half I understood… and the other half I did not.” I then asked her if she was able to take away anything useful from the half that she did understand, and her response was “yes.” Her English language skills were not fluent therefore she could not elaborate. This student had been following a thesis program on Real Case Singularities, so relative to her Ph.D. research, I was curious about whether or not she would feel comfortable about speaking to or asking questions of any of the senior visitors at the Institute. She was in fact confident that “yes… you can ask anybody.” There was no reason to feel intimidated by the academic hierarchy – senior and junior researchers alike could approach one another at a conference for question-asking or discussion purposes.

A visiting postdoctoral fellow, associated with Dubrovin, confirmed a similar impression:

> I think it is really nice here… It is sort of comfortable. I’m working in a place right now in Italy, which doesn’t have the same atmosphere, but here there are always some people around here that you can speak to. You just sort of bump into people who want to ask a question of you or you want to ask a question of them. There are spontaneous meetings like that. I am working in a department now in Italy and there are perhaps two people with whom I can speak to quite often about what I do. To come here and be surrounded by people who spend all of their days thinking about the same stuff, it makes you feel part of a community you know… like you are not stuck in the middle of nowhere doing something that nobody is interested in. Just having the idea that there people who are interested in what you do and in related areas… it sort of takes you out of isolation.

This postdoctoral researcher mentioned that he had had some positive mathematics discussions with both M. SAITO and LOOIJENGA. Yet, as a young mathematician, he was clear about his core research value of

> trying to work things out by sitting and thinking about it and having a look at books first. You can do what you want, but at the end of the day, you want to obtain your own understanding of what is happening in an area. If this understanding comes purely from asking someone else who will explain, you will actually not understand it well unless it is something that you thought through yourself.

Another postdoctoral visitor to the Institute, also female, spoke at length about what it was like to be a young woman in mathematics. She was confident about the work she was doing and had colleagues with whom she could collaborate in her immediate social circle, namely her former research supervisor. Despite her successes she was aware of a few social barriers. This woman preferred not to be named in this study, and although she was participating in the Singularities program, she did not refer to herself as a Singularity Theorist. As with all my one-to-one interviews, I had asked "Julia" (a pseudonym) to comment on her general impression of the Institute, and this was when she spoke candidly:

> R. I don't know most of the people in this program, and I find this environment scientifically interesting. It doesn't overlap with my field, so this is why I don't know the people.
>
> *I. So scientifically it is interesting, and do you think there are pieces of information that you are absorbing from the talks that would be valuable to your research?*
>
> R. Yes, I think so…
>
> *I. What is the problem area that you are working on?*
>
> R. I am working on a specific problem, which is related to Singularity Theory, but quite far from it …. so that's why I came here. So for me it is quite hard to follow the talks because it is not really my field. I don't have most of the instruments…
>
> *I. Are there any specific people here that you want to connect with?*
>
> R. Yes… the two Saitos… and I think I am quite interested in the work of Barannikov. There is a talk tomorrow morning of Marco's that I am interested in….
>
> *I. So you are interested in the work of certain people…*
>
> R. There was somebody who I thought was going to be here… but I think he was here last week.
>
> *I. Are you comfortable with speaking to whom ever you wish. Do you feel that there are any social barriers?*
>
> R. No.. I don't think that there are in general. I mean I didn't experience this. Uhm… I don't know if you are interested in this… especially in this conference and these people… up to now I didn't find any barriers uh… probably it would be better if we talk again on Friday about this specific group of people.
>
> *I. Have you had some good informal discussions…*
>
> R. Yes… You know, I think that the one difficult thing about this conference is that there is one huge group of people who are speaking Russian… so this makes it difficult from a social point of view… right… if you are just walking around, you can't just add yourself to a conversation and this is specific to this conference and it happens so many times because the Russians can be so strong. But uh… in this special conference I would say that the stronger barrier that I would have is with people who are speaking another language with one another to each other. You can't just speak with people who don't speak your language.
>
> *I. I guess maybe part of learning what you want to learn, might involve eavesdropping on people who are speaking English.*

R. Yes…

I. It helps because then if you hear them talking English… you can sit down and ask questions… or just listen in to what to they are saying. Even if you don't speak with them you can still listen and maybe something of what they say is of interest.

R. Yes…

I. So there is a language barrier… and you have also told me that you are the only woman…

R. Well… I mean… you know this is more general. This is common in mathematics. It is always like that. I happen to be the only woman around several times. And what I find difficult … you know it depends. In general if I go to a conference there are some people who tend to not take me seriously…

I. And you think that this is directly related to the fact that you are a woman?

R. I think so…. In this specific conference I did not have any problem and I think it is because [supervisor's name] was here… and he advertises that I am good.. and so essentially people just take me seriously because somebody else told them to take her seriously.

I. In some sense does that bother you? Are you happy that he does this?

R. No…. well,… I am happy he does this. I know that he needs to do this. The fact that he does is positive.. it is very supportive. I mean, we can't live in an ideal world. We know that the situation is like this, so I like him to react….

I. Do you think that there is a difference between how someone might react to you as a senior researcher as opposed to a junior researcher -- someone closer to you in age?

R. I think also age is important, but the combination of the two things is more difficult …. because it has happened to me also with people who are much younger.

An interesting aspect of the Singularity Theory research community, one that I have not commented on until this point, is that it is predominantly male. In my ACA mapping of the subject, only one female mathematician yielded high co-citation counts for inclusion and that was LEJEUNE-JALABERT. I did not have an opportunity to meet or interview LEJEUNE-JALABERT, so I was not able to acquire any insight into her collegial experiences. With respect to "Julia's" experience however, I understood it to be just one and not necessarily representative of all women in mathematics. Nevertheless, as I listened to what "Julia" had to say, I began to recognize how an individual's research experiences, particularly in terms of "social capital," could be attributed to their gender. A Cambridge University Press publication entitled *Athena Unbound: The Advancement of*

Women in Science and Technology recently gave explicit attention to this issue. The authors highlight specifically the value of "social capital" in science – defined as "the web of contacts and relationships that provide information, validation, and encouragement." Social capital "provides an approach for analyzing differences in the success of men and women in a social context in which productivity is based on managing interdependence with others" (Etzkowitz, Kemelgor & Uzzi, 2000, pp. 117-118). In this study, gender-related issues are not central to the research theme; therefore I can only appreciate the willingness of someone like "Julia" to share an honest perspective. Her experience as a short-term visitor to the Newton Institute adds to all the other experiences described to me by other visitors. It also demonstrates the complexity of trying to uncover and understand fully the nuances of collegiality in mathematics, let alone in one community like Singularity Theory.

6.5 Measuring the Newton Institute's impact on mathematical advancements

One of the main challenges associated with studying an invisible college is that it is for the most part "invisible" – a characteristic based on the fact that scholar members tend to be separated from one another by geography. International Research Institutes, like the Isaac Newton Institute bring mathematicians together in a way that can make the invisible college or parts of the invisible college more visible. In the physical space of the Institute, mathematicians have the opportunity to meet face to face to discuss problems and share ideas with colleagues they might otherwise not meet with regularly.

A significant amount of funding is required to operate research environments like the Newton Institute. Consequently, there is an increasing need to find a reliable way to measure the success of their programs. The most difficult programs to measure are those designed to influence the cross-fertilization of ideas and research among scholars from related subjects. I became aware of this problem during an interview with the Isaac Newton Institute's Deputy Director, Dr. Robert Hunt:

I. Do you think that when workshops or programs occur with a focus on "interdisciplinarity" that this is affecting the course of mathematics? Are the visitors actually sharing ideas with each another and publishing collaborative papers?

R. This is a question that we get asked again and again by people at EPSRC [Engineering and Physical Sciences Research Council] who want us to provide some sort of evidence that the money they have given us has been used successfully. So what we do is… It's a very difficult thing to measure obviously, that is the first thing to say because a large amount of what we're about is getting a lot of people from different fields together, getting them chatting and getting them to realize that there is work that can be done and setting up a collaboration. It may well be that they are only here for three months or something and they don't get time to actually sit down and write a paper while they're here. When they leave the go to their home universities and they stay in touch and they work on a problem together and publish a paper or several papers a year down the line or something like this. Of course it is very difficult for us to keep track of that. Nevertheless we do try to. We keep records of all papers that are published which arose from some kind of collaboration that started at the institute. We ask participants to provide us with that information after they have left.

A few days after my interview with the Deputy Director, I obtained a draft copy of a survey that the Newton Institute was developing to evaluate the success of their programs. This survey, based on a one-page letter and questionnaire, was produced as a result of a 1999 EPSRC Panel review and the Institute Scientific Steering Committee's agreement to adopt a pilot evaluation scheme concerning the *Mathematics of Atmosphere and Ocean Dynamics* (July to December 1996). In the questionnaire, the mathematicians were asked to comment on whether their participation in the named program had had any significant effect in opening up new research directions. Another question required them to list any new work, collaborative or otherwise that had been initiated within the Institute environment. Past visitors were also asked to comment, in retrospect, on how they feel the *Atmosphere and Ocean Dynamics* program might have been better organized to maximize the possibility of creative and collaborative research. The survey was brief and without commenting on its reliability or validity as a research instrument, it was clear that the mathematicians' experiences were regarded as critical to the evaluation process. As I read through the questions, I began to reflect on my own research and it was then that I realized that the techniques I was using (i.e., ACA, Social Network Analysis) for my own study might have something significant to add to the evaluation.

The combined techniques of ACA and Social Network Analysis have been used specifically in this research to examine the intellectual and social process of Singularity Theory research in light of the "invisible college" hypothesis. This bibliometric approach need not be limited however to the invisible college problem. Another application would be to trace the history of a select group of authors involved in a Mathematics Research Institute program or workshop. With an overlapping ACA-Social Network Analysis, the intellectual and collaborative connections among the visitors' can be measured up to and including the present (i.e., 2000) and then re-evaluated in future (i.e., 2005 or later) to determine the program or workshop's impact on their research. The following example provides further clarification.

To understand the role of the Isaac Newton Institute in fostering mathematical advancements, I have created another "birds-eye view" or ACA map of the authors who attended the workshop on *Singularity Theory and Its Applications to Quantum Field Theory*. I have chosen this workshop because the Institute's 2001 Annual Report states that it had brought in "a large number of new problems into the area of Singularity Theory" and because it was described as an event which inspired a significant amount of mathematical growth (p. 27).

Raw co-cited author data (first/sole author counts only) as well as co-authorship data, extracted from Dialog™ *SciSearch* and *MathSciNet* respectively, were collected for each of the workshop participants and used as input to the *SPSS*, *UCINET* (Borgatti et al., 1999) and *Krackplot* (Krackhardt et al., 1995) data analysis programs. Table 6.6 below provides a full list of the workshop participants' names (n=48) and in Table 6.7, all summary statistics of the overlapping ACA-Social Network analyses are presented. Both the ACA and Social Network maps, shown in Figure 6.5 and Figure 6.6, illustrate the intellectual similarities between the authors and their co-authorship links to one another. Note specifically on the ACA map, that three cluster groups have been identified as key topic areas (see complete ACA iteration history and hierarchical cluster routine in Appendix J). The ACA has also detected and grouped a small number of subject newcomers – authors who have zero rates of co-citation with all the other authors and have not built up a research identity in association with any of the research topics (see Figure 6.5).

Table 6.6. List of workshop participants. *Singularity Theory and Its Applications to Quantum Field Theory* (1974-2000).

ANISOV, S. S.	HERTLING, C.	SIERSMA, D.
BARANNIKOV, S. A.	HITCHIN, N. J.	SLODOWY, P.
BERTOLA, M.	IVANOV, R. I.	SPIELBERG, H.
BOALCH, P.	IZUMIYA, S.	TAKAHASHI, A.
CHEKANOV, Yu. V?	KAZARIAN, M.	TIBAR, M.
DAMON, J. N.	JANECZKO, S.	TROTMAN, D. J. A.
DE GREGORIO, I.	KOBAYASHI, M.	VAN STRATEN, D.
DIJKGRAAF, R. H.	LOOIJENGA, E.	VANENCKEVORT, C. J.
DOLGACHEV, I. V.	MAZZOCCO, M.	VASSILIEV, V. A.
DUBROVIN, B. A.	MORRISON, D. R.	VIRO, O.
DU PLESSIS, A. A.	NATANZON, S. M.	WALL, C. T. C.
EKHOLM, T.	NIKULIN, V. V.	WULFF, C.
EVANS, D. E.	POLYAK, M.	WILSON, P. M. H.
GAFFNEY, T.	REES, E. G.	ZAKALYUKIN, V.
GODDARD, P.	SAITO, K.	
GORYUNOV, V. V.	SAITO, M.-H.	
GRITSENKO, V. A.	SANGUINETTI, G.	
GROSS, M.	SHAPIRO, M. Z.	

Table 6.7. Summary Statistics of ACA-Social Network Analysis. *Singularity Theory and Its Applications to Quantum Field Theory* (1974-2000)

Number of unique pairs over 48 authors	48(48-1)/2 = 1128
Mean co-citation rate (over 48 authors)	3
Range of raw co-citation counts	0 – 185 (Dijkgraaf, R. H. and Goddard, P.)
Range of mean co-citation rates	0 – 12 (Boalch=0, de Gregorio=0, Ekholm=0, Ivanov=0, Ivanov=0, Spielberg=0, Vanenckevort=0; Looijenga=12)
Number of unique author:	
pairings never made or equal to "0"	828 (connectivity ratio = 37%)
pairings made only once or equal to "1"	92
Co-author network density:	0.031 (completeness = 1.0)

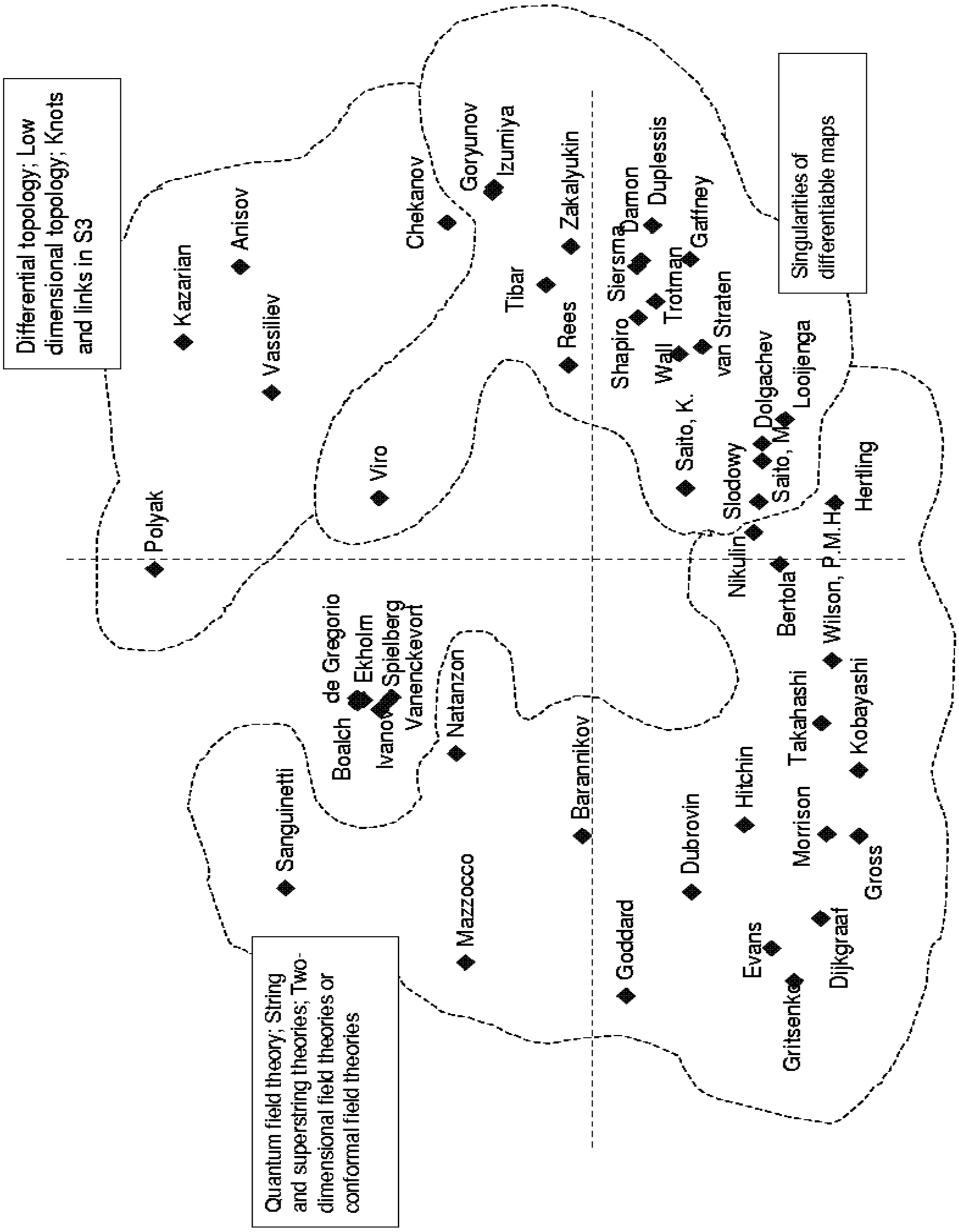


Figure 6.5. ACA map of the workshop on *Singularity Theory and Its Applications to Quantum Field Theory* (1974-2000).

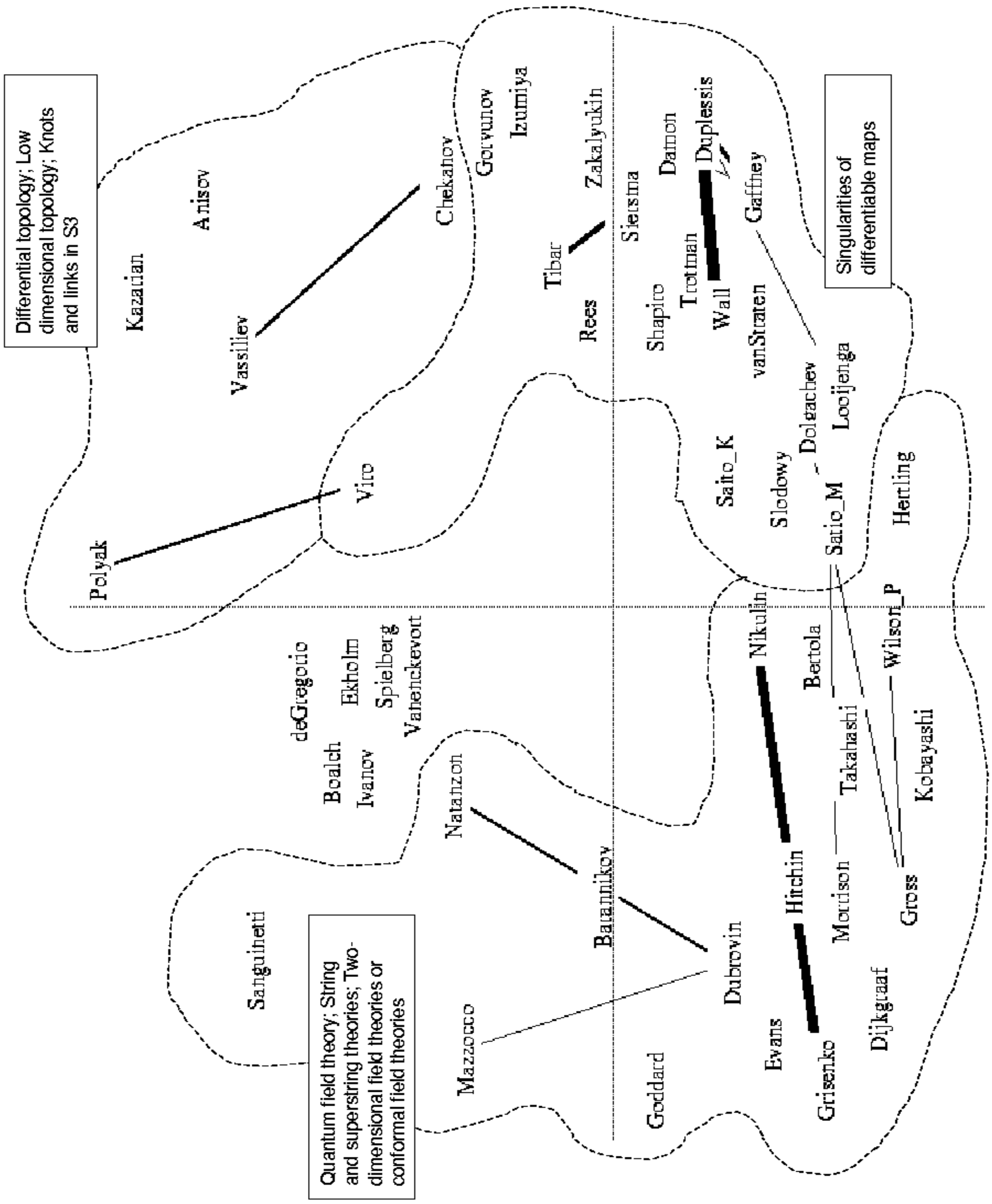


Figure 6.6. ACA-Social Network map of the workshop on *Singularity Theory and Its Applications to Quantum Field Theory* (1974-2000).

After a period of 5 years or more, the ACA and co-author network maps can be reconstructed to include post-2000 co-cited author data and co-author data on the same author group. The new map may be re-examined for evidence of structural change. An expected outcome is that a change will be detectable; that is, the author nodes will shift slightly to a new position and additional co-author links will appear. A follow-up questionnaire similar to the Newton Institute's survey for the *Atmosphere and Ocean Dynamics* program may then be sent to the authors including copies of the 2000 and post-2000 ACA maps. With this questionnaire, data concerning collaborative behaviour could be collected, for the purpose of adding an extra "layer" to the bibliometric map, for instance: With whom did you choose to stay in touch after attending the workshop? How many times in the past few years did you contact this colleague to discuss mathematics? The mathematicians may also be asked whether or not the workshop activities inspired them to incorporate new ideas or techniques into their research. If new co-authorship ties (formal publications) are present on the updated version of the ACA-Social Network map, the follow-up inquiry might require the mathematicians to state whether or not their collaborative relationships had been initiated during their workshop visit. Of interest also would be to uncover the reasons as to why the mathematicians chose to collaborate and how they managed to continue their work after leaving the Institute.

CHAPTER 7: CONCLUSION

An "invisible college" is defined in this study as a set of interacting scholars who share similar research interests concerning a subject specialty, who often produce publications relevant to this subject specialty and who communicate both formally and informally with one another to work towards important goals in the subject, even though they may belong to geographically distant research affiliates.

To understand how an invisible college functions, a systematic approach was taken to analyze the interrelationship between a group of scientists' intellectual identities (i.e., their subject specialty), their collaborative (co-authorship) research habits as social actors and their collegial connections to one another as international conference participants. This systematic approach, based on a structurationally informed conceptual framework (see chapter 1), has given special attention to Singularity Theory research in mathematics.

Singularity Theory research possesses the initial component of an invisible college because it is a mature, well-defined subject. Although a subject specialty is not necessarily an invisible college or as Hagstrom (1970) suggests, a tightly knit network of communication, the subject is still an important component because it provides a critical context for interaction. Among the 75 authors selected for this study, high co-citation rates produced a cohesive intellectual structure based on three key topic areas: REAL AND COMPLEX ANALYTIC GEOMETRY, SINGULARITIES OF DIFFERENTIABLE MAPS and THE TOPOLOGY OF COMPLEX ALGEBRAIC SINGULARITIES. The level of cohesiveness, or connectivity ratio for the 75(75-1)/2=2,775 unique author pairs was 63%. At the core of this structure a group of influential research 'stars' (e.g., THOM, ARNOLD, WHITNEY, and MILNOR) were identified and recognized for having contributed to the foundational aspects of this subject.

One important question addressed by this research was the following: Can the formal cognitive aspects of an author co-citation analysis help to uncover significant information about underlying social relationships? The qualitative portion of the study confirmed that much of what is presented on an ACA mapping is in fact based on a significant amount of underlying social activity. The nature of this activity is not evidenced by the map alone, but becomes clear when the map is used as a navigation tool – a tool for investigating personal histories – within the research "territory" or "invisible college." When presented with the Singularity Theory map during interviews, many authors confirmed that it made sense to them: underlying personal and intellectual connections to other authors were both recognized and confirmed as being true (e.g., LE DUNG TRANG). Some of the mathematician informants also related stories of how they met colleagues in the past and how certain authors, specifically those mapped in close proximity, had influenced the development of their careers (e.g., SLODOWY).

The Singularity Theory research community has not only become a cohesive intellectual structure, but also a well-established network of social actors. The mathematicians involved in this subject know one another quite well and have been active as collaborators and co-authors. With the overlapping ACA-Co-authorship network analysis (Chapter 5), we see a significant relationship between the intellectual similarities of the mathematicians and their co-authorship patterns (e.g., BIERSTONE and MILMAN). Authors are primarily working on joint projects with the colleagues who are part of the same knowledge cluster. Although the co-authorship counts are quite low (i.e., network density = 0.10), some of the ties on the social network map represent very strong work relationships (e.g., DUPLESSIS and WALL; GIBSON, GIBLIN and BRUCE).

Co-authorship counts were recognized as only a part of what it means to collaborate; therefore this study also examined 'up close' the day to day work habits of the Singularity Theorists at the Isaac Newton Institute. Observations and interviews carried out in this environment revealed that much of the collaborative activity associated with this research specialty tends to be informal. The mathematicians enjoy discussing problems with one another in a variety of settings, for instance, in front of a blackboard, in their private offices, at a coffee table in a lounge area or even on a train (e.g., SIERSMA and STEENBRINK). They are open about sharing ideas and research techniques, and believe it is important to challenge approaches to problem solving with thought-provoking questions (e.g., URIBE-VARGAS). The Singularity Theorists have been and still are involved in joint projects (e.g., SIERSMA and TIBAR) and find it productive to meet face to face for this purpose; however, their discussions are not always established to produce a co-authored publication. Informal research conversations, even if carried out for competitive reasons, help to confirm the importance of problems and problem-solving techniques; mathematician's value collaborative-type discussions because they strengthen individual research.

The Singularity Theorists' interest in setting new research goals for their subject has opened up opportunities in the past for them to connect with one another collegially at international conferences. This study traced the collegial activity of the Singularity Theorists back to an early meeting in 1969-1970 (i.e., Singularities Symposium in Liverpool England) to some of the more recent meetings of the 1990s and 2000 (e.g., The Arnoldfest at the Fields Institute and the Singularity Theory program at the Newton Institute). With the collegial network superimposed upon the intellectual (ACA) mapping of the specialty (chapter 6), we see that many of the Singularity Theorists have kept in touch with one another and kept up to date with the latest research (network density = .52). Although the mathematicians' conference attendance patterns differ considerably from their co-citation patterns, most of the co-authoring Singularity Theorists have been attendees at the same international meetings. A co-authorship connection is mathematically deeper than a collegial connection, but both social activities seem to work 'hand-in-hand' in the sense that colleagues who travel often and become familiar with the work of international colleagues are more likely to collaborate (e.g., DAMON and GALLIGO).

An in-depth examination of one specific conference, the 2000 Singularity Theory program at the Isaac Newton Institute, demonstrates that even though the mathematicians have been meeting regularly (i.e., once or twice per year), their collegial network is not exclusive. The ACA mapping of the *Applications to Quantum Field Theory* workshop (see chapter 6) shows that several mathematicians from outside, but closely related research areas were participants. Some of these participants (e.g., Dubrovin; Morrison) were in fact deliberately invited by the workshop organizers to encourage cross-disciplinary interaction. The Information Use Environment of the Isaac Newton Institute provided a venue for all the talks, seminars, receptions and teas that took place during the workshop. As a physical space, the Institute gave the Quantum Field Theorists and Singularity Theorists an opportunity to meet personally to share ideas with on another, discover common research goals and focus on mathematics requiring complementary expertise. The end result was that "a large number of new problems" were opened up (Isaac Newton Institute Annual Report, p. 27). Although the Institute was successful at fostering invisible college activity, the degree to which the related research areas converge depends on how the mathematicians continue to work together in the upcoming years. The ACA mapping of the *Applications to Quantum Field Theory* workshop may be used again (i.e., perhaps five years from now) to trace the publication output of the participants and make further sociometric and qualitative inquiries into their collaborative activities.

As an invisible college network, Singularity Theory has reached a critical subject development and social phase where the mathematicians' have become highly responsive to a "bridge building" process in mathematics. Singh (1997) explains that "the value of mathematical bridges is enormous. They enable communities of mathematicians who have been living on separate islands…to explore each other's creations" (p. 191). Clearly the term "bridge building" refers to knowledge growth in science, but there are actually two different views concerning this idea: the Kuhnian model and the branching model.

Kuhn's (1996) theory is that paradigm shifts are responsible for knowledge growth in science. His understanding of the term "paradigm" is that it relates closely to the structure and activity of "normal science." A scientific community engages in normal science when it accepts a theory and set of procedures and uses them as a foundation for its research. Singularity theory is one accepted theory, but there are others in science, for instance, Einstein's theory of relativity. The community's guiding theoretical framework, or paradigm gains its status because it is more successful than any other "in solving a few problems that the group of practitioners has come to realize as being acute" (p. 23). Although a paradigm is "rarely an object for replication," anomalies arise in science and often the ensuing crisis demonstrates that a paradigm is not longer useful for the construction of new problems to solve. Kuhn's main point then, is that when a new paradigm has the power to substitute an established paradigm, there is a structural revolution in the research field.

Mathematics research is an interesting discipline to reflect on with respect to paradigms, simply because mathematicians are preoccupied with the discovery of truths. Once a mathematician has established a truth by means of a proof, it is not usually subject to controversy. A flaw in the proof may be found, but if it is corrected and the proof is established as an indisputable truth, it is true forever. This truth then becomes a foundational aspect of mathematics and in turn makes it all the more possible for a branching model of scientific growth to take place. For this reason, Kuhn's view of a paradigm shift is probably not applicable, and perhaps the branching theory is more relevant to what is happening, or about to happen in Singularity Theory.

Scholars who oppose Kuhn's view are convinced that paradigm-based revolutions in science occur rarely and only in special cases (eg., Mulkay, Gilbert & Woolgar, 1975; Mulkay, 1976). The central tenet of the branching model is that new fields and specialties in science grow because scientists are attracted to problems outside, but related to their own specialty areas, for instance, Singularity Theory and its Applications to Quantum Field Theory. Migrating scientists realize that the techniques available to them, techniques that have successfully been used in the solution of other problems, can easily be transferred and applied to a new set of problems. With the transference of techniques, a new cycle of exploration begins among the problem area researchers, including follow-up periods of unification and decline or displacement. Eventually the emergent specialty (both the problem area and its member network) experiences an increased level of acceptability within the wider scientific community.

Before Singularity Theory became an invisible college, it was once in its earlier phase part of an outgrowth of another subject in mathematics (e.g., Algebraic Geometry). I came to this research specialty with a mapping technique – Author Co-citation Analysis (ACA) – to capture "a snapshot at a distinct point in time of what is actually a changing and evolving structure of knowledge" (Small, 1993, p. 5). The complementary quantitative and qualitative analyses carried out in this study demonstrate how it is

> possible to follow this evolution either at the micro-level, where we deal with the histories of individual scientific ideas … or at the macro-level, where change occurs in entire bodies of knowledge or their interactions with one another (Small, 1993, p. 5).

Essentially the work of a co-citationist does not have to begin and end simply with a map for interpretation. ACA maps can certainly be interpreted, but they possess a more potent function when taken out into an actual research "territory" to study the social histories of the scholars/scientists involved. Within the territory of an invisible college, the ACA map is particularly meaningful if it is used in conjunction with a set of research themes – the subject specialty, the scientist as a social actor, and the Information Use Environment (IUE). Each theme opens up an opportunity to superimpose sociometric data on bibliometric data, and make complementary data analyses leading to a "peeling away" of invisible college layers. The initial map or structure of the subject specialty serves as a starting point for the researcher as he/she embarks on a journey to gather information about the elements that corroborate the invisible college's temporal dimensions (i.e., co-authorship, collegiality, mentorship, etc.).

In the future, researchers may wish to consider re-using the invisible college model illustrated in Figure 1.2 (Chapter 1). With additional studies based on this model there would be a better opportunity to make more appropriate invisible college comparisons. Do all invisible college networks function like the Singularity Theory community? If not, what are some of the unique or differing attributes in terms of co-authorship, collegiality or research environments used to support information sharing? Why do some invisible colleges thrive and others languish and what can we learn for policy development purposes from their comparative strengths and weaknesses?

Information Use Environments are obviously critical for making invisible colleges more visible; hence future research may also consider the selection of different IUEs for analysis. In this study, the IUE was grounded by a physical space (The Isaac Newton Institute); however, it does not necessarily have to be. The IUE is above all "the set of elements that affect the flow and use of information messages into, within, and out of any definable entity" (Taylor, 1986, p. 3). By definition, "any entity" could be an e-mail environment or perhaps a special electronic 'collaboratory' designed for use via the World Wide Web. E-mail has become important to scholars in recent years because it complements face-to-face interaction: "it is used most by scholars who are collaborators or friends" (Koku, Nazer & Wellman, 2001, p. 1752). In time, changes are anticipated with respect to Internet use and there is a possibility that such changes will occur with the development of Virtual Institute Networks (VINs) (e.g., Braham, 1995). VINs are more interactive than e-mail because they are designed to allow scientists and scholars to communicate with one another as though they were meeting face to face.[22] If the VIN becomes commonplace, then research will be needed to understand its social and scientific impact. Will scientists make use of VINs like they use current international research institutes? What effect will the VIN have on research output, peer review or the collective egos of scientists? Will scientists spend less time traveling and more time writing for publication? Will they become more interested in sharing ideas, more competitive or less competitive, or, more trusting or less trusting of one another? What effect will the VIN have on scientists coming from less technologically advanced societies? Brunn and Lear (1999) suggest that certain social consequences are likely to deepen "between … members of the 'electronic invisible college' versus those who cannot and choose not to participate. Exclusion may be based on the inability or unwillingness on the part of the individual or the state to invest in those technologies…" (p. 299).

[22] Braham's (1995) version of the VIN interface talks to a Virtual Internet Server (a modified IRC server with new protocols) and is set up to appear as a sophisticated blackboard. A VIN user may load images and everybody tuned into the network will see the image. Scientists may write on the image, type text on the blackboard, erase the blackboard, change the colours of lines and texts, move between channels, create new channels, find out who else is on the channel, browse the World Wide Web and use a chat function to engage in discussion.

Finally, it is important to note that invisible colleges, despite their overall significance to the advancement of scholarly research, are very difficult to study. The approach that I have taken to examine Singularity Theory as an invisible college is just one. Many other approaches have been taken, and may still be developed, but my aim has been to present one that is systematized so that it is not only replicable but also clear to other researchers what the critical elements of an invisible college are. As an information scientist, I chose Author Co-citation Analysis or ACA as the basis for my research and hopefully what it shows is that ACA is not just a bibliometrician's research tool but also a tool for the "sociologist-of-science-as-explorer." With ACA we have an opportunity to develop a map, and with a map we can examine where scholars have been in the past, where they are today, and where they may be going in the future.

APPENDIX A. Pilot ACA of Singularity Theory (1974-2000).

Table A.1. Raw Co-citation Matrix (n=24).

	ARNOLD	BIERSTONE	BRIESKORN	BRUCE	DAMON	GORYUNOV	GUSEINZADE	HIRONAKA	LOJASIEWICZ	MALGRANGE	MATHER	MILNOR	MOND	SAITO	SIERSMA	TEISSIER	THOM	TROTMAN	VARCHENKO	VASSILIEV	WALL	WHITNEY	WILSON	ZARISKI
ARNOLD	116	35	189	161	92	56	43	44	26	126	329	469	43	115	89	66	321	12	14	55	170	166	9	45
BIERSTONE	35	22	3	8	15	1	0	95	72	55	45	27	3	7	7	17	21	3	0	0	28	49	4	19
BRIESKORN	189	3	40	14	12	12	10	53	5	62	27	195	1	83	23	46	24	1	12	10	54	13	1	80
BRUCE	161	8	14	30	52	12	5	8	2	7	73	44	29	37	24	23	42	9	0	0	100	29	8	7
DAMON	92	15	12	52	26	16	5	14	5	17	95	31	30	26	22	31	36	5	0	1	66	19	9	12
GORYUNOV	56	1	12	12	16	9	6	0	0	1	12	11	17	7	11	5	7	1	0	11	12	4	1	2
GUSEINZADE	43	0	10	5	5	6	6	2	0	4	3	16	1	5	10	6	3	2	1	0	6	4	0	3
HIRONAKA	44	95	53	8	14	0	2	50	129	76	79	136	1	25	11	97	82	17	3	1	26	94	4	174
LOJASIEWICZ	26	72	5	2	5	0	0	129	27	65	47	74	1	4	1	23	48	10	2	1	18	67	1	21
MALGRANGE	126	55	62	7	17	1	4	76	65	36	56	86	4	50	12	29	39	5	14	0	23	75	8	27
MATHER	329	45	27	73	95	12	3	79	47	56	71	140	38	50	37	75	197	26	2	1	134	137	28	31
MILNOR	469	27	195	44	31	11	16	136	74	86	140	117	16	74	47	99	302	9	16	25	584	211	3	147
MOND	43	3	1	29	30	17	1	1	1	4	38	16	13	12	19	17	5	2	7	2	42	9	1	4
SAITO	115	7	83	37	26	7	5	25	4	50	50	74	12	29	22	39	17	3	8	0	28	16	15	41
SIERSMA	89	7	23	24	22	11	10	11	1	12	37	47	19	22	20	28	32	5	1	1	31	12	1	11
TEISSIER	66	17	46	23	31	5	6	97	23	29	75	99	17	39	28	39	50	16	40	1	36	52	5	106
THOM	321	21	24	42	36	7	3	82	48	39	197	302	5	17	32	50	68	29	4	2	132	152	15	28
TROTMAN	12	3	1	9	5	1	2	17	10	5	26	9	2	3	5	16	29	9	1	0	20	25	6	4
VARCHENKO	14	0	12	0	0	0	1	3	2	14	2	16	7	8	1	40	4	1	6	1	0	0	2	3
VASSILIEV	55	0	10	0	1	11	0	1	1	0	1	25	2	0	1	1	2	0	1	5	4	3	0	2
WALL	170	28	54	100	66	12	6	26	18	23	134	584	42	28	31	36	132	20	0	4	69	75	20	27
WHITNEY	166	49	13	29	19	4	4	94	67	75	137	211	9	16	12	52	152	25	0	3	75	54	11	50
WILSON	9	4	1	8	9	1	0	4	1	8	28	3	1	15	1	5	15	6	2	0	20	11	7	1
ZARISKI	45	19	80	7	12	2	3	174	21	27	31	147	4	41	11	106	28	4	3	2	27	50	1	36

Table A.2. Pearsons r correlation matrix (n=24).

	ARNOLD	BIERSTONE	BRIESKORN	BRUCE	DAMON	GORYUNOV	GUSEINZADE	HIRONAKA	LOJASIEWICZ	MALGRANGE	MATHER	MILNOR	MOND	SAITO	SIERSMA	TEISSIER	THOM	TROTMAN	VARCHENKO	VASSILIEV	WALL	WHITNEY	WILSON	ZARISKI
ARNOLD	0	0.14	0.5	0.4	0.51	0.19	0.27	0.37	0.33	0.42	0.47	0.38	0.35	0.5	0.55	0.51	0.66	0.51	0.16	0.28	0.82	0.75	0.49	0.4
BIERSTONE	0.14	0	0.21	0.13	0.18	-0.1	0.04	0.5	0.81	0.63	0.33	0.3	0.01	0.16	0.1	0.5	0.4	0.52	0.02	0.06	0.11	0.53	0.23	0.54
BRIESKORN	0.5	0.21	0	0.59	0.44	0.56	0.78	0.39	0.3	0.69	0.64	0.48	0.36	0.8	0.77	0.65	0.78	0.09	0.44	0.79	0.72	0.71	0.09	0.56
BRUCE	0.4	0.13	0.59	0	0.88	0.85	0.77	-0	0.02	0.51	0.84	0.77	0.85	0.64	0.9	0.3	0.76	0.43	0.12	0.71	0.38	0.57	0.57	0.08
DAMON	0.51	0.18	0.44	0.88	0	0.68	0.56	0.05	0.09	0.41	0.68	0.63	0.88	0.59	0.82	0.33	0.7	0.55	0.13	0.49	0.36	0.59	0.73	0.08
GORYUNOV	0.19	-0.1	0.56	0.85	0.68	0	0.88	-0.2	-0.2	0.43	0.71	0.51	0.68	0.67	0.86	0.13	0.6	0.05	0.16	0.85	0.29	0.36	0.15	-0
GUSEINZADE	0.27	0.04	0.78	0.77	0.56	0.88	0	0.03	0	0.61	0.75	0.53	0.54	0.8	0.88	0.31	0.72	0.06	0.32	0.94	0.45	0.5	0.04	0.23
HIRONAKA	0.37	0.5	0.39	-0	0.05	-0.2	0.03	0	0.54	0.54	0.24	0.21	-0.1	0.3	0.14	0.65	0.37	0.34	0.3	0.09	0.39	0.57	0.05	0.46
LOJASIEWICZ	0.33	0.81	0.3	0.02	0.09	-0.2	0	0.54	0	0.62	0.32	0.2	-0.1	0.17	0.11	0.59	0.43	0.5	0.09	0.06	0.33	0.62	0.18	0.7
MALGRANGE	0.42	0.63	0.69	0.51	0.41	0.43	0.61	0.54	0.62	0	0.7	0.51	0.19	0.68	0.62	0.64	0.77	0.39	0.27	0.64	0.49	0.73	0.22	0.61
MATHER	0.47	0.33	0.64	0.84	0.68	0.71	0.75	0.24	0.32	0.7	0	0.76	0.56	0.62	0.86	0.43	0.8	0.58	0.21	0.73	0.49	0.75	0.42	0.29
MILNOR	0.38	0.3	0.48	0.77	0.63	0.51	0.53	0.21	0.2	0.51	0.76	0	0.54	0.51	0.65	0.4	0.61	0.57	0.1	0.5	0.24	0.55	0.5	0.23
MOND	0.35	0.01	0.36	0.85	0.88	0.68	0.54	-0.1	-0.1	0.19	0.56	0.54	0	0.46	0.72	0.17	0.58	0.34	0.06	0.44	0.3	0.35	0.59	-0
SAITO	0.5	0.16	0.8	0.64	0.59	0.67	0.8	0.3	0.17	0.68	0.62	0.51	0.46	0	0.82	0.57	0.7	0.12	0.48	0.75	0.53	0.58	0.19	0.46
SIERSMA	0.55	0.1	0.77	0.9	0.82	0.86	0.88	0.14	0.11	0.62	0.86	0.65	0.72	0.82	0	0.45	0.82	0.35	0.37	0.82	0.59	0.7	0.38	0.3
TEISSIER	0.51	0.5	0.65	0.3	0.33	0.13	0.31	0.65	0.59	0.64	0.43	0.4	0.17	0.57	0.45	0	0.61	0.42	0.22	0.32	0.53	0.68	0.22	0.72
THOM	0.66	0.4	0.78	0.76	0.7	0.6	0.72	0.37	0.43	0.77	0.8	0.61	0.58	0.7	0.82	0.61	0	0.51	0.22	0.75	0.77	0.86	0.41	0.46
TROTMAN	0.51	0.52	0.09	0.43	0.55	0.05	0.06	0.34	0.5	0.39	0.58	0.57	0.34	0.12	0.35	0.42	0.51	0	0.06	0.02	0.24	0.62	0.71	0.31
VARCHENKO	0.16	0.02	0.44	0.12	0.13	0.16	0.32	0.3	0.09	0.27	0.21	0.1	0.06	0.48	0.37	0.22	0.22	0.06	0	0.27	0.25	0.26	-0.1	0.51
VASSILIEV	0.28	0.06	0.79	0.71	0.49	0.85	0.94	0.09	0.06	0.64	0.73	0.5	0.44	0.75	0.82	0.32	0.75	0.02	0.27	0	0.52	0.56	-0	0.25
WALL	0.82	0.11	0.72	0.38	0.36	0.29	0.45	0.39	0.33	0.49	0.49	0.24	0.3	0.53	0.59	0.53	0.77	0.24	0.25	0.52	0	0.77	0.14	0.5
WHITNEY	0.75	0.53	0.71	0.57	0.59	0.36	0.5	0.57	0.62	0.73	0.75	0.55	0.35	0.58	0.7	0.68	0.86	0.62	0.26	0.56	0.77	0	0.42	0.54
WILSON	0.49	0.23	0.09	0.57	0.73	0.15	0.04	0.05	0.18	0.22	0.42	0.5	0.59	0.19	0.38	0.22	0.41	0.71	-0.1	-0	0.14	0.42	0	-0
ZARISKI	0.4	0.54	0.56	0.08	0.08	-0	0.23	0.46	0.7	0.61	0.29	0.23	-0	0.46	0.3	0.72	0.46	0.31	0.51	0.25	0.5	0.54	-0	0

Table A.3. Iteration history for the 2 dimensional solution (in squared distances)

```
Young's S-stress formula 1 is used.
Iteration    S-stress    Improvement

1            .24154
2            .17694      .06460
3            .17049      .00645
4            .16959      .00091

Iterations stopped because
S-stress improvement is less than  .001000


Stress and squared correlation (RSQ) in distances

RSQ values are the proportion of variance of the scaled data
(disparities)in the partition (row, matrix, or entire data) which
is accounted for by their corresponding distances.

Stress values are Kruskal's stress formula 1.

Stress  =  .14175    RSQ =  .88629
```

Table A.4. Stimulus Coordinates.

```
Configuration derived in 2 dimensions

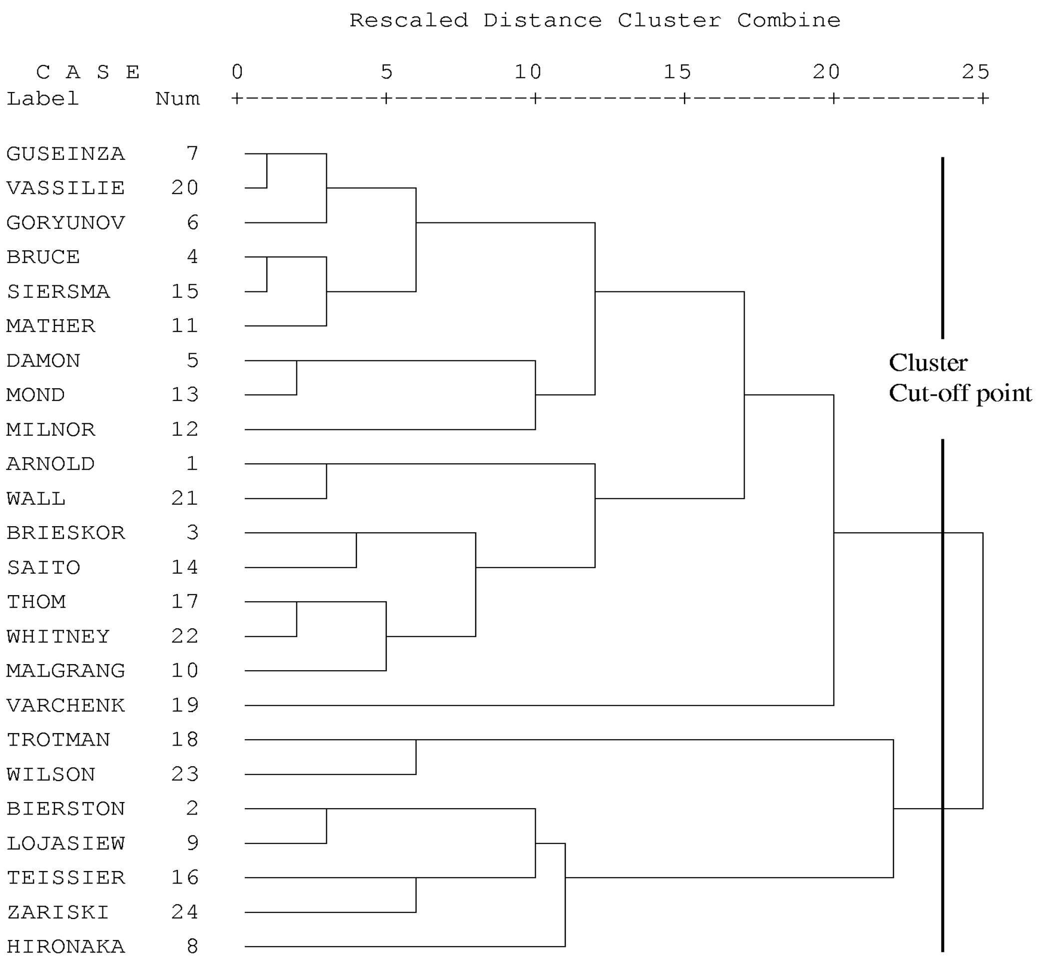
                Dimension

Stimulus  Stimulus    1      2
 Number    Name

   1    ARNOLD    -.2781   .8173
   2    BIERSTON  -1.8439   .8599
   3    BRIESKOR    .1492  -.8938
   4    BRUCE    1.1788   .4526
   5    DAMON    1.0052   .7806
   6    GORYUNOV  1.8645  -.3266
   7    GUSEINZA  1.2017  -.8612
   8    HIRONAKA  -1.9997  -.5546
   9    LOJASIEW  -2.0654   .3008
  10    MALGRANG  -.4690  -.3164
  11    MATHER    .4270   .2296
  12    MILNOR    .5666   .7620
```

13	MOND	1.6757	.7661
14	SAITO	.4889	-.7354
15	SIERSMA	.8265	-.0686
16	TEISSIER	-1.0307	-.1262
17	THOM	.1081	.1150
18	TROTMAN	-.8099	1.5369
19	VARCHENK	-.2960	-2.0667
20	VASSILIE	1.0402	-.9671
21	WALL	-.1590	-.9161
22	WHITNEY	-.4037	.0908
23	WILSON	.2247	2.0013
24	ZARISKI	-1.4019	-.8801

Figure A.1. Pilot cluster analysis dendogram using complete linkage.



APPENDIX B. Sample author co-citation search strategy using Dialog™ *SciSearch*.

```
SYSTEM:OS  - DIALOG OneSearch
  File  34:SciSearch(R) Cited Ref Sci  1990-2000/Dec W4
      (c) 2000 Inst for Sci Info
  File 434:SciSearch(R) Cited Ref Sci  1974-1989/Dec
      (c) 1998 Inst for Sci Info

Set  Items  Description
   ---  -----  -----------

?s (CA=KAZARIAN M? or CA=KAZARYAN M?)
          52  CA=KAZARIAN M?
          97  CA=KAZARYAN M?
   S1    143  (CA=KAZARIAN M? OR CA=KAZARYAN M?)

?s S1 AND CA=ARNOLD V?
         143  S1
       12274  CA=ARNOLD V?
   S2     18  S1 AND CA=ARNOLD V?

?s S1 AND CA=ARTALBARTOLO E?
         143  S1
          23  CA=ARTALBARTOLO E?
   S3      0  S1 AND CA=ARTALBARTOLO E?

?s S1 AND CA=BIERSTONE E?
         143  S1
         313  CA=BIERSTONE E?
   S4      1  S1 AND CA=BIERSTONE E?

?s S1 AND CA=BRASSELET JP?
         143  S1
          65  CA=BRASSELET JP?
   S5      0  S1 AND CA=BRASSELET JP?

?s S1 AND CA=BRIANCON J?
         143  S1
         288  CA=BRIANCON J?
   S6      0  S1 AND CA=BRIANCON J?

?s S1 AND CA=BRIESKORN E?
         143  S1
         905  CA=BRIESKORN E?
   S7      0  S1 AND CA=BRIESKORN E?
```

```
?s S1 AND CA=BRUCE J?
        143  S1
       4850  CA=BRUCE J?
   S8     1  S1 AND CA=BRUCE J?

?s S1 AND CA=BRYLINSKI J?
        143  S1
        629  CA=BRYLINSKI J?
   S9     0  S1 AND CA=BRYLINSKI J?


……[SETS 10 to 71 OMITTED]……….


?s S1 AND CA=VASSILIEV V?
        143  S1
        320  CA=VASSILIEV V?
   S72     6  S1 AND CA=VASSILIEV V?
?
?s S1 AND CA=WALL C?
        143  S1
       2905  CA=WALL C?
   S73     0  S1 AND CA=WALL C?
?
?s S1 AND CA=WHITNEY H?
        143  S1
       2718  CA=WHITNEY H?
   S74     1  S1 AND CA=WHITNEY H?
?
?s S1 AND CA=WILSON L?
        143  S1
      10326  CA=WILSON L?
   S75     0  S1 AND CA=WILSON L?
?
?s S1 AND CA=ZAKALYUKIN V?
        143  S1
         82  CA=ZAKALYUKIN V?
   S76     3  S1 AND CA=ZAKALYUKIN V?
?
?s S1 AND CA=ZARISKI O?
        143  S1
       2144  CA=ZARISKI O?
   S77     0  S1 AND CA=ZARISKI O?
```

APPENDIX C. Two-dimension ACA results in *SPSS* (n=75). Singularity Theory (1974-2000).

Table C.1. Iteration history for the two-dimensional solution (in squared distances).

```
Young's S-stress formula 1 is used.

Iteration    S-stress    Improvement

1           .36450
2           .26046      .10404
3           .24522      .01524
4           .24159      .00363
5           .24067      .00092

Iterations stopped because
S-stress improvement is less than  .001000


Stress and squared correlation (RSQ) in distances

RSQ values are the proportion of variance of the scaled data
(disparities)in the partition (row, matrix, or entire data)
which is accounted for by their corresponding distances.

Stress values are Kruskal's stress formula 1.

For matrix
Stress  =  .18157    RSQ =  .84367
```

Table C.2. Configuration derived in two-dimensions.

```
Stimulus Coordinates

                Dimension

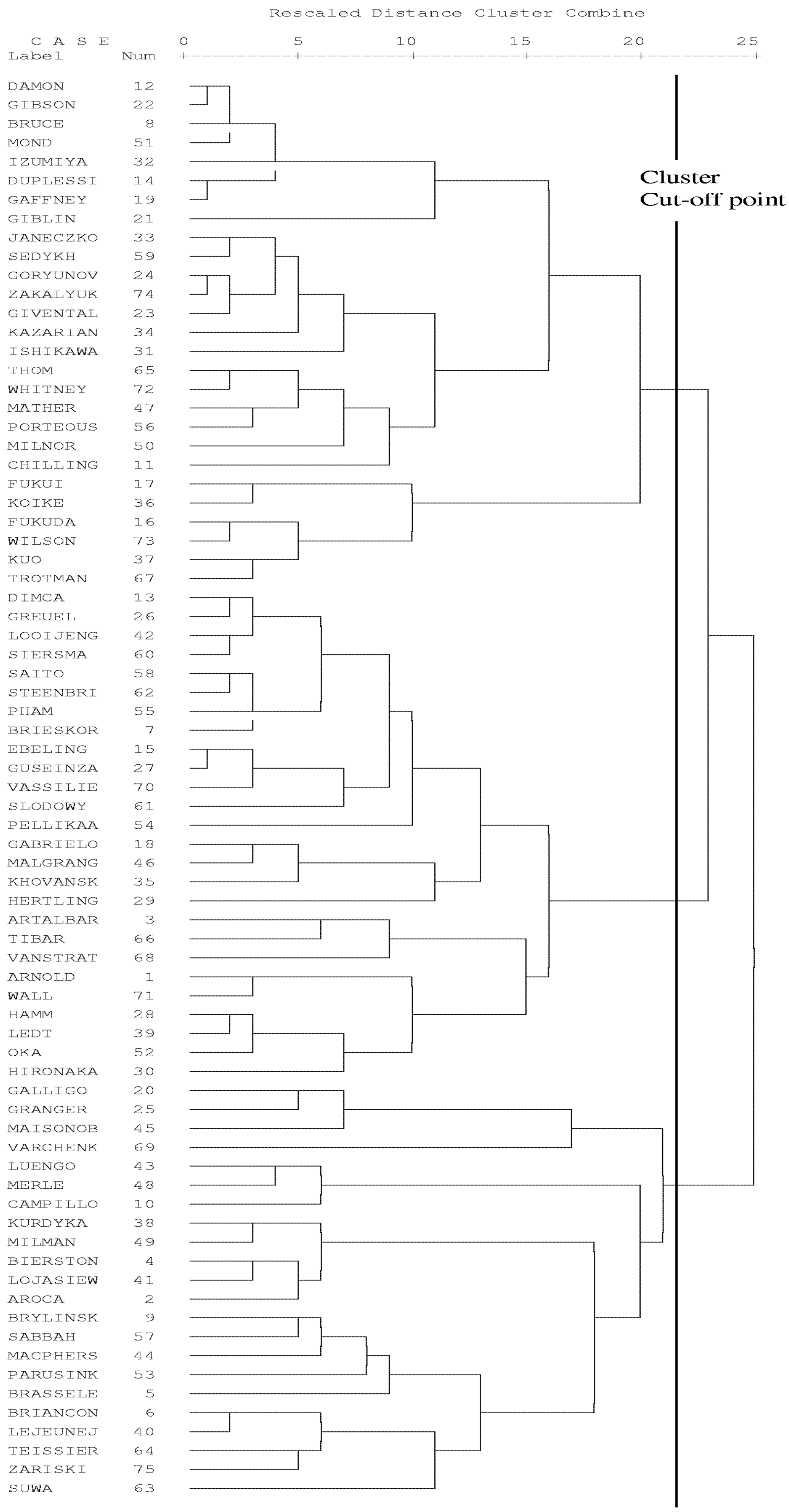
Stimulus  Stimulus    1      2
 Number    Name

   1    ARNOLD     .2949   .4636
   2    AROCA    -2.2033  1.0967
   3    ARTALBAR  -.1288  -1.8964
   4    BIERSTON  -.6615  1.1094
   5    BRASSELE -1.8234   .6748
   6    BRIANCON  -.9374   .0238
   7    BRIESKOR   .1986  -.6557
   8    BRUCE     1.1487   .1392
```

9	BRYLINSK	-1.2817	-.4082
10	CAMPILLO	-2.3645	-2.0368
11	CHILLING	1.4216	.9517
12	DAMON	.6371	.3090
13	DIMCA	.1957	-.4567
14	DUPLESSI	.8611	1.0467
15	EBELING	.8936	-.6505
16	FUKUDA	.4050	1.1895
17	FUKUI	.3379	1.4850
18	GABRIELO	-.3874	-.0050
19	GAFFNEY	.6039	.9244
20	GALLIGO	-1.5061	.7164
21	GIBLIN	2.1017	.3879
22	GIBSON	.6397	.5271
23	GIVENTAL	1.4484	-.8046
24	GORYUNOV	1.4071	-.4728
25	GRANGER	-1.3011	1.6718
26	GREUEL	.1741	-.5539
27	GUSEINZA	.9215	-.7452
28	HAMM	-.2527	-.4872
29	HERTLING	.3893	-1.6317
30	HIRONAKA	-.9859	.0106
31	ISHIKAWA	1.7676	.7364
32	IZUMIYA	1.2541	.7160
33	JANECZKO	1.8441	.2638
34	KAZARIAN	2.1682	-.0749
35	KHOVANSK	.5816	-.3076
36	KOIKE	-.0833	1.6327
37	KUO	-.3249	.7631
38	KURDYKA	-1.8834	1.6548
39	LEDT	-.4702	-.3809
40	LEJEUNEJ	-1.2171	-.1253
41	LOJASIEW	-.9135	.9475
42	LOOIJENG	.5373	-.2619
43	LUENGO	-.4779	-1.5758
44	MACPHERS	-1.4199	-.0296
45	MAISONOB	-2.4281	-.4091
46	MALGRANG	-.1536	.0826
47	MATHER	.5062	.2577
48	MERLE	-1.2816	-1.0657
49	MILMAN	-1.9943	1.8216
50	MILNOR	.5383	.2900
51	MOND	1.1061	.1199
52	OKA	-.3205	-.9629
53	PARUSINK	-1.7196	-.0443
54	PELLIKAA	.5068	-.7034
55	PHAM	-.1542	-.3673
56	PORTEOUS	1.1943	.4621
57	SABBAH	-1.5557	.1264

58	SAITO	.2397	-.3833
59	SEDYKH	1.8507	-.2267
60	SIERSMA	.4481	-.2354
61	SLODOWY	.9492	-.6141
62	STEENBRI	-.0334	-.6990
63	SUWA	-1.2842	-.4317
64	TEISSIER	-.4674	-.0758
65	THOM	.5318	.2046
66	TIBAR	-.5145	-1.2315
67	TROTMAN	-.0707	1.0918
68	VANSTRAT	-.0355	-1.3576
69	VARCHENK	-.8624	-1.4191
70	VASSILIE	1.5814	-.8850
71	WALL	.7110	-.5777
72	WHITNEY	.0624	.3051
73	WILSON	.4797	1.3745
74	ZAKALYUK	1.5164	-.1688
75	ZARISKI	-.9549	-.1594

Figure C.1. Cluster dendogram for two-dimensional solution using complete linkage.



APPENDIX D. Three-dimension ACA results in *SPSS* (n=75). Singularity Theory (1974-2000).

Table D.1. Iteration history for the three-dimensional solution (in squared distances).

```
Young's S-stress formula 1 is used.

Iteration    S-stress    Improvement

1           .32557
2           .20815      .11742
3           .19512      .01303
4           .19139      .00373
5           .19006      .00133
6           .18957      .00049

Iterations stopped because
S-stress improvement is less than  .001000


Stress and squared correlation (RSQ) in distances

RSQ values are the proportion of variance of the scaled data
(disparities)in the partition (row, matrix, or entire data)which
is accounted for by their corresponding distances.

Stress values are Kruskal's stress formula 1.

For  matrix
Stress  =  .13052    RSQ =  .89528
```

Table D.2. Configuration derived in three-dimensions.

```
Stimulus Coordinates
                   Dimension

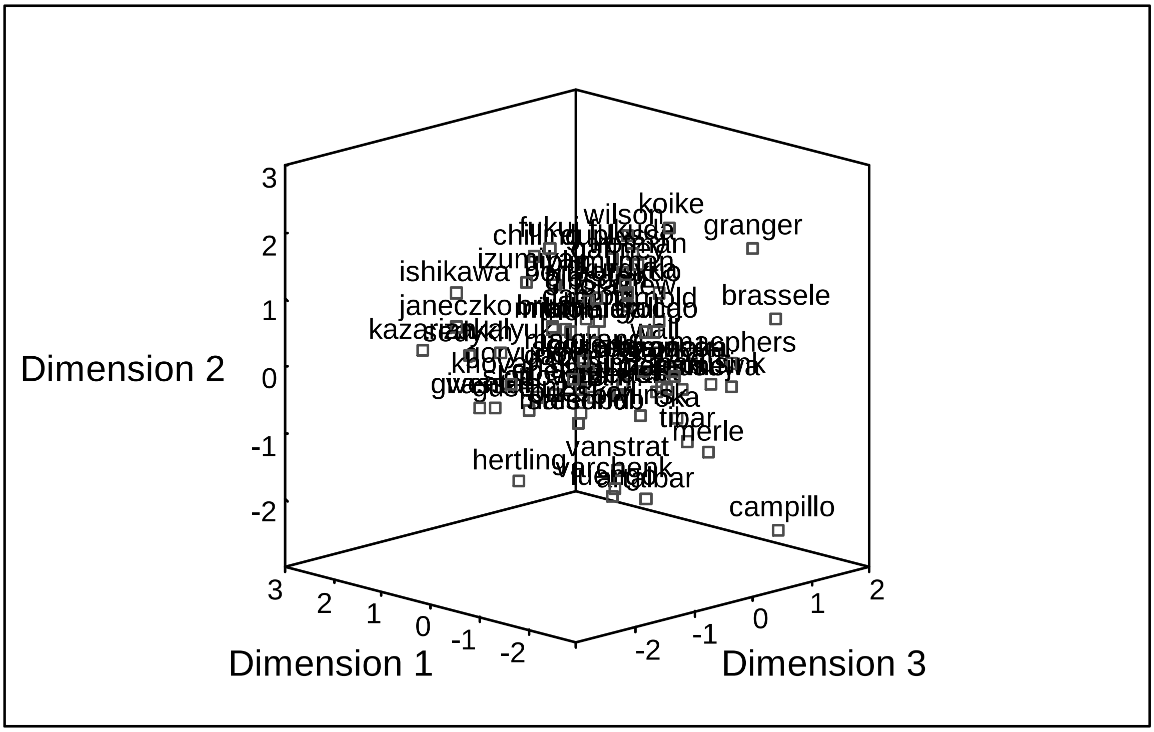
Stimulus  Stimulus    1      2      3
 Number    Name

   1    ARNOLD    .2216   .2734  1.0819
   2    AROCA    -2.3291  1.1246  -1.2594
   3    ARTALBAR  -.2441  -2.1834   .4817
   4    BIERSTON  -.7755  1.2176  -.8121
   5    BRASSELE  -1.8318   .6408  1.3743
   6    BRIANCON  -1.1688   .0374   .1079
```

7	BRIESKOR	.2523	-.8236	-.2213
8	BRUCE	1.3429	.1635	.2172
9	BRYLINSK	-1.4885	-.4324	-.6750
10	CAMPILLO	-2.6405	-2.2371	.7537
11	CHILLING	1.6428	1.1747	.1552
12	DAMON	.7995	.3799	.3124
13	DIMCA	.2213	-.5475	.4662
14	DUPLESSI	.9283	1.1147	.8729
15	EBELING	1.0904	-.7583	.0527
16	FUKUDA	.4029	1.3162	.7697
17	FUKUI	.3305	1.7220	-.6960
18	GABRIELO	-.3886	-.0212	-.8787
19	GAFFNEY	.6536	.9959	.8104
20	GALLIGO	-1.7267	.8599	-.6133
21	GIBLIN	2.2496	.4468	1.0079
22	GIBSON	.7626	.6260	.3147
23	GIVENTAL	1.6078	-.8337	-.8312
24	GORYUNOV	1.6416	-.5535	-.0084
25	GRANGER	-1.4674	1.6320	1.2735
26	GREUEL	.2051	-.6804	.3937
27	GUSEINZA	1.1036	-.8860	-.3844
28	HAMM	-.3200	-.5210	.6717
29	HERTLING	.4147	-1.6513	-1.1625
30	HIRONAKA	-1.2391	.0378	.0992
31	ISHIKAWA	1.9477	.8126	-.9252
32	IZUMIYA	1.5082	.8809	-.1160
33	JANECZKO	2.0213	.2689	-.8742
34	KAZARIAN	2.2679	-.0300	-1.2330
35	KHOVANSK	.5809	-.2522	-1.1363
36	KOIKE	-.1853	1.7885	.9431
37	KUO	-.4002	.9187	.5664
38	KURDYKA	-2.0160	1.6582	-1.3011
39	LEDT	-.6120	-.4746	.3266
40	LEJEUNEJ	-1.5013	-.1728	.0339
41	LOJASIEW	-1.1047	1.1169	-.5748
42	LOOIJENG	.6613	-.3183	.2450
43	LUENGO	-.5625	-1.8901	-.3541
44	MACPHERS	-1.6090	.0425	.8079
45	MAISONOB	-2.0327	-.0871	-2.1636
46	MALGRANG	-.1684	.1036	-.5383
47	MATHER	.6558	.3421	-.1617
48	MERLE	-1.5171	-1.2310	.4595
49	MILMAN	-2.1014	1.8300	-1.4517
50	MILNOR	.7107	.3598	-.3340
51	MOND	1.1534	.1522	.8241
52	OKA	-.4045	-1.0142	.8644
53	PARUSINK	-2.0324	-.0180	.0729
54	PELLIKAA	.5638	-.8122	.6364
55	PHAM	-.1751	-.4776	-.3360

56	PORTEOUS	1.3134	.5547	.6728
57	SABBAH	-1.8520	.1814	-.3605
58	SAITO	.3370	-.5094	-.1456
59	SEDYKH	2.1128	-.2126	-.5740
60	SIERSMA	.5993	-.3009	.0830
61	SLODOWY	1.1520	-.7539	-.2150
62	STEENBRI	-.0107	-.8556	-.4840
63	SUWA	-1.4395	-.3720	.9281
64	TEISSIER	-.6023	-.1021	.1359
65	THOM	.6776	.2753	-.0624
66	TIBAR	-.5666	-1.3300	.9142
67	TROTMAN	-.1181	1.3279	.4372
68	VANSTRAT	-.0666	-1.6874	.1397
69	VARCHENK	-1.0001	-1.6159	-.7006
70	VASSILIE	1.8319	-.9938	-.3755
71	WALL	.5555	-.3189	1.3200
72	WHITNEY	.0814	.4208	-.1445
73	WILSON	.5122	1.5559	.7893
74	ZAKALYUK	1.7866	-.1701	-.3276
75	ZARISKI	-1.2032	-.2238	.0151

Figure D.1. Three-dimensional ACA configuration (Euclidean distance model).



APPENDIX E. AMS classification codes relevant to Singularity Theory.

CODE	DATE	SUBJECT
14 -XX	(1940-now)	ALGEBRAIC GEOMETRY
14Bxx	(1973-now)	Local theory
14B05	(1973-now)	Singularities
14B07	(1980-now)	Deformations of singularities
14Dxx	(1973-now)	Families, fibrations
14D07	(1991-now)	Variation of Hodge structures
14Exx	(1973-now)	Birational geometry
14E15	(1973-now)	Global theory and resolution of singularities
14Hxx	(1973-now)	Curves
14H20	(1973-now)	Singularities, local rings
14Jxx	(1973-now)	Surfaces and higher dimensional varieties
14J17	(1980-now)	Singularities
14Pxx	(1991-now)	Real algebraic and real analytic geometry
14P10	(1991-now)	Semialgebraic sets and related spaces
14Qxx	(1991-now)	Computational aspects in algebraic geometry
14Q99	(1991-now)	None of the above but in this section
32 -XX	(1940-now)	SEVERAL COMPLEX VARIABLES AND ANALYTIC SPACES
32Bxx	(1973-now)	Local analytic geometry
32B20	(1973-now)	Semianalytic and Subanalytic sets
32B30	(1980-1990)	Local singularities
32Cxx	(1973-now)	General theory of analytic spaces
32C05	(1973-now)	Real-analytic manifolds, real-analytic spaces
32C10	(1973-1999)	Complex manifolds
32C38	(1985-now)	Sheaves of differential operators and their modules, D-modules
32C40	(1980-1990)	Singularities
32C42	(1980-1990)	Stratified sets, etc
32Dxx	(1973-now)	Analytic continuation
32D20	(1973-now)	Removable singularities
32Sxx	(1991-now)	Singularities
32S05	(1991-now)	Local singularities
32S10	(1991-now)	Invariants of local analytic rings
32S15	(1991-now)	Equisingularity (topological and analytic)
32S20	(1991-now)	Global theory of singularities; cohomological properties
32S22	(2000-now)	Relations with arrangements of hyperplanes
32S25	(1991-now)	Surface and hypersurface singularities
32S30	(1991-now)	Deformations of singularities, vanishing cycles
32S35	(1991-now)	Mixed Hodge theory of singular varieties
32S40	(1991-now)	Monodromy; relations with differential equations and D-modules
32S45	(1991-now)	Modifications; resolutions of singularities
32S50	(1991-now)	Topological aspects: Lefchetz theorems, topological classification, invariants
32S55	(1991-now)	Milnor fibration; relations with knot theory
32S60	(1991-now)	Stratifications; constructible sheaves; intersection cohomology
32S65	(1991-now)	Singularities of holomorphic vector fields and foliations
32S70	(1991-now)	Other operations on singularities

32S99	(2000-now)	None of the above, but in this section

34 -XX	(1940-now)	ORDINARY DIFFERENTIAL EQUATIONS
34Cxx	(1973-now)	Qualitative theory
34C08	(2000-now)	Connections with real algebraic geometry (fewnomials, desingularization, zeros of Abelian integrals, etc.
34Mxx	(2000-now)	Differential equations in the complex domain
34M35	(2000-now)	Singularities, monodromy, local behaviour of solutions, normal forms

35 -XX	(1940-now)	PARTIAL DIFFERENTIAL EQUATIONS
35Axx	(1973-now)	General theory
35A20	(1973-now)	Analytic methods, singularities
35A21	(2000-now)	Propogation of singularities
35Lxx	(1973-now)	Partial differential equations of hyperbolic type
35L67	(1980-now)	Shocks and singularities

37 -XX	(2000-now)	DYNAMICAL SYSTEMS AND ERGODIC THEORY
37Dxx	(2000-now)	Dynamical systems with hyperbolic behavior
37D50	(2000-now)	Hyperbolic systems with singularities

55 -XX	(1940-now)	ALGEBRAIC TOPOLOGY
55Rxx	(1980-now)	Fiber spaces and bundles
55R55	(1980-now)	Fiberings with singularities

57 -XX	(1959-now)	MANIFOLDS AND CELL COMPLEXES
57Rxx	(1980-now)	Differential topology
57R45	(1980-now)	Singularities of differentiable mappings
57Mxx	(1980-now)	Low dimensional topology
57M25	(1980-now)	Knots and links in S^3 (for higher dimensions, see 57Q45)
57Qxx	(1980-now)	PL-topology
57Q45	(1980-now)	Knots and links (in high dimensions) (for the low dimen. case, see 57M25)

58 -XX	(1973-now)	GLOBAL ANALYSIS, ANALYSIS ON MANIFOLDS
58Axx	(1973-now)	General theory of differential manifolds
58A35	(1980-now)	Stratified sets
58Cxx	(1973-now)	Calculus on manifolds; nonlinear operators
58C27	(1980-1999)	Singularities of differentiable maps
58C28	(1980-1999)	Catastrophes
58Fxx	(1973-1999)	Ordinary differential equations on manifolds; dynamical systems
58F05	(1973-1999)	Hamiltonian and Lagrangian systems; symplectic geometry
58F14	(1980-1999)	Bifurcation theory and singularities
58J47	(2000-now)	Propagation of singularities; initial value problems
58Kxx	(2000-now)	Theory of singularities and catastrophe theory
58K05	(2000-now)	Critical points of functions and mappings
58K15	(2000-now)	Topological properties of mappings
58K20	(2000-now)	Algebraic and analytic properties of mappings
58K25	(2000-now)	Stability
58K30	(2000-now)	Global theory
58K35	(2000-now)	Catastrophe theory
58K40	(2000-now)	Classification; finite determinacy of map germs
58K45	(2000-now)	Singularities of vector fields, topological aspects

58K50 (2000-now)	Normal forms
58K55 (2000-now)	Asymptotic behavior
58K60 (2000-now)	Deformation of singularities
58K65 (2000-now)	Topological invariants
58K70 (2000-now)	Symmetries, equivariance
60 -XX (1940-now)	PROBABILITY THEORY AND STOCHASTIC PROCESSES
60Gxx (1973-now)	Stochastic processes
60G30 (1973-now)	Continuity and singularity of induced measures
74 -XX (2000-now)	MECHANICS OF DEFORMABLE SOLIDS
74Gxx (2000-now)	Equilibrium (steady-state) problems
74G70 (2000-now)	Stress concentrations, singularities
83 -XX (1940-now)	RELATIVITY AND GRAVITATIONAL THEORY
83Cxx (1980-now)	General relativity
83C75 (1985-now)	Space-time singularities, cosmic censorship, etc.

APPENDIX F. Coauthor network .kp file. Singularity Theory (1974-2000).

```
75 FILE = THESIS~1.kp
366 294 Arnold
 70 358 Aroca
318  27 Artalbartolo
254 360 Bierstone
106 300 Brasselet
242 255 Briancon
357 164 Brieskorn
488 265 Bruce
176 197 Brylinski
 51  12 Campillo
502 342 Chillingworth
437 264 Damon
355 192 Dimca
445 356 Duplessis
450 158 Ebeling
381 369 Fukuda
372 406 Fukui
291 241 Gabrielov
396 336 Gaffney
153 316 Galligo
591 288 Giblin
415 304 Gibson
512 137 Givental
501 185 Goryunov
185 424 Granger
363 177 Greuel
432 134 Guseinzade
288 173 Hamm
379  56 Hertling
189 246 Hironaka
544 318 Ishikawa
483 316 Izumiya
556 267 Janeczko
592 229 Kazarian
425 197 Khovanski
323 423 Koike
294 321 Kuo
116 414 Kurdyka
263 186 LeDungTrang
166 219 LejeuneJalabert
224 342 Lojasiewicz
405 211 Looijenga
276  63 Luengo
175 232 MacPherson
 44 192 Maisonobe
316 255 Malgrange
378 264 Mather
180 119 Merle
 95 439 Milman
407 280 Milnor
462 243 Mond
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295 130 Oka
106 244 Parusinski
395 147 Pellikaan
309 199 Pham
475 288 Porteous
129 268 Sabbah
349 211 Saito
552 205 Sedykh
385 223 Siersma
457 173 Slodowy
302 154 Steenbrink
176 182 Suwa
277 227 Teissier
404 249 Thom
272 101 Tibar
325 358 Trotman
329  87 Vanstraten
230  80 Varchenko
536 119 Vassiliev
411 181 Wall
328 277 Whitney
390 389 Wilson
509 222 Zakalyukin
255 209 Zariski
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APPENDIX G. First/sole author publications (n=75). Singularity Theory (1974-2000).

Author Name	First/Sole Author	Secondary Author	% of First Author Papers	Named AFTER the following First Author:
ARNOLD	231	7	97.06%	Agranovich, Anosov (3), Osipov, Sena, Vainberg
AROCA	14	0	100.00%	
ARTALBARTOLO	21	0	100.00%	
BIERSTONE	38	0	100.00%	
BRASSELET	44	6	88.00%	Barthel(5), Birbrair
BRIANCON	24	2	92.31%	Biosca, Skoda
BRIESKORN	26	0	100.00%	
BRUCE	96	0	100.00%	
BRYLINSKI	56	10	84.85%	Baum, Bell, Blanc, Borho (6) Bressler,
CAMPILLO	38	6	86.36%	Briales (3), Gusein-Zade (3)
CHILLINGWORTH	29	2	93.55%	Birman, King
DAMON	51	0	100.00%	
DIMCA	48	10	82.76%	Artal-Bartolo, Barthel, Bonnet, Cassou-Nouges, Choudary (5), Delign
DUPLESSIS	21	5	80.77%	Bruce (3), Gaffney, Gibson
EBELING	25	3	89.29%	Feistel, Kremp, Mikhailov
FUKUDA, Takuo	14	4	77.78%	Aoki, Nishimira, Masako-Fukuda (2)
FUKUI, Toshizumi	20	0	100.00%	
GABRIELOV	27	1	96.43%	Fuchs
GAFFNEY	25	6	80.65%	Banchoff (2), Bruce (2), Damon, du Plessis
GALLIGO	19	15	55.88%	Caniglia (3), Bayer, Briancon (2), Damon (3), Emiris (2), Fitchas (3), Hassold
GIBLIN	26	39	40.00%	Banchoff (2), Bruce (32), Chen, Cipolla, Fidal, Hallinan, Soares
GIBSON	15	24	38.46%	Bedford (3), Bruce (11), Cocke, Dimca (3), Donelan (4), Giblin, Johns
GIVENTAL	32	4	88.89%	Arnold (2), Eliashberg, Varchenko
GORYUNOV	35	6	85.37%	Arnold (2), Chmutov (4)
GRANGER	9	12	42.86%	Assi (3), Balquiere, Briancon (5), de Brucq, Galligo (2)
GREUEL	35	13	72.92%	Brieskorn, Brucker, Buchweitz (3), Decker (2), Drozd (4), Grassman (2)
GUSEINZADE	42	8	84.00%	Arnold (2), Campillo, Ebeling (4), Varchenko
HAMM	26	1	96.30%	Greuel
HERTLING	7	1	87.50%	Greuel
HIRONAKA	43	5	89.58%	Aroca (2), Babakhanian (2), Cossart
ISHIKAWA	33	4	89.19%	Brodersen, Fukuda, Hayakawa, Nishimori
IZUMIYA S	61	4	93.85%	Hayakawa, Ishikawa, Li, Nishimori
JANECZKO	48	5	90.57%	Cao Long Van (2), Domitrz (3),
KAZARIAN	16	1	94.12%	Habsieger
KHOVANSKII	35	13	72.92%	Arnold, Bernstein, Danilov, Gabrielov, Gelfond (2), Ilyashenko, Kantor, Kozlova, Makarov, Pukhlikov (2), Varchenko,
KOIKE	15	5	75.00%	Bekka (2), Fukui (2), Ishikawa,
KUO	31	6	83.78%	Bochnak (2), Fukui, Kobayashi (2), Koike
KURDYKA	20	4	83.33%	Coste (2), Denkowska, Gwozdiewicz
LEDUNGTRANG	62	18	77.50%	Brasselet, Cano, Cheniot (2), Greuel, Hamm (9), Ha Huy Vui, Henry, Iverson, Lejeune
LEJEUNEJALABERT	17	13	56.67%	Angeniol (2), Bermejo, Campillo (4), Gonzalez-Sprinberg (5), Hironaka
LOJASIEWICZ	52	12	81.25%	Andreotti (2), Bochnak (2), Bojarski, Denkowska (3), Fortuna (2), Golpolhkab, Kurdyka
LOOIJENGA	35	4	89.74%	Faber, Hain, Gibson, Greuel,
LUENGO	10	13	43.48%	Alonzo, Artal-Bartolo (5), Gomez-Mont, Gusein-Zade (5), Lopez
MACPHERSON	15	53	22.06%	Baum (3), Beilinson (2), Borho (5), Braden, Brasselet, Cheeger, Coveyou, De Concini, Deligne, Fulton (7), Gelfand (5), Goresky (18), Grinberg, Hain (3), Hanamura (2), Verdier
MAISONOBE	5	15	25.00%	Biosca, Briancon (10), Galligo (2), Granger (2)
MALGRANGE	102	9	91.89%	Borel, Brylinski (2), Garding (2), Koszul, Laurent, Levy, Lions
MATHER	65	3	95.59%	Bellissaard, Fathi, Fell
MERLE	4	15	21.05%	Briancon (2), Giusti (2), Henry (11)
MILMAN	7	36	16.28%	Melles, Bierstone (25), Bos (5), Grant, Kuo, Lambert, Michelli, Milman

Author Name	First/Sole Author	Secondary Author	% of First Author Papers	Named AFTER the following First Author:
MILNOR	107	15	87.70%	Barratt, Bass, Bott, Dawson, Friedland, Fox, Goldberg (2), Herstein, Hirsch, Kervaire (2), Lepowsky, Lusztig, Lyubich,
MOND	20	12	62.50%	Cooper, Damon, Gaffney (2), Goryunov, Holland (2), Marar (2), Morton, Castro-Jiminez (2)
OKA	41	7	85.42%	Jiang (3), Nakazawa, A'Campo, Le Van Thanh, Lê Dung Tráng
PARUSINSKI	22	9	70.97%	Bierstone, Gwozdziewicz, Kuo, Kurdyka (2), Marzantowicz, McCrory (3)
PELLIKAAN	18	15	54.55%	Beelen, Blokhuis, de Boer (3), Heijnen, Høholdt (4), Kirfel, Mond, Munuera (2), Porter
PHAM	29	15	65.91%	Bessis, Bros, Boutet de Monvel, Candelpergher (2), Dai, Delabaere (7), Duc, Fotiadi
PORTEOUS	18	0	100.00%	
SABBAH	25	8	75.76%	Dimca, Garcia-Lopez, Henry, Loeser (3), Nemethi, Nitsure
SAITO	41	3	93.18%	Brieskorn, Le Dung Trang (2)
SEDYKH	25	2	92.59%	Romero-Fuster (2)
SIERSMA	18	6	75.00%	Hazewinkel, Jiang (2), Khimshiashvili, Massey, Sotomayer
SLODOWY	20	8	71.43%	Bliss, Cassens, Daboul (2), Gutkin, Hirzebruch, Knorrer, Richardson
STEENBRINK	23	19	54.76%	de Jong (2), Ebeling, Greuel, Jeurissen, Kearton, Le Van Thanh, Looijenga, Nemethi (3), Namikawa, Oort, Peters, Pfister, Scherk, Schrauwen, van Doorn, van Straten
SUWA	31	15	67.39%	Bedford, Brasselet (2), Cerveau, Honda, Khanedani, Lehmann (4), Ohmoto, Seade (3),
TEISSIER	32	18	64.00%	Eisenbud, Garcia, Goldin, Hauser, Henry, Hironaka, Le Dung Trang (6), Lejeune-Jalabert (5), Lipman
THOM	127	10	92.70%	Banchoff, Connes, Dold (2), Kergosien, Peixoto (3), Sebastiani, Williams
TIBAR	14	5	73.68%	Dimca, Siersma (4),
TROTMAN	15	16	48.39%	Bekka (2), Brodersen, Kuo (2), Kambouchner, Kwiecinski, Murolo (3),Navarro, Noirel, Orro (3), Risler
VANSTRATEN	5	21	19.23%	Altmann, Batyrev (3), Birkenhake, de Jong (8), Fantechi, Izadi, Mond (2), Montaldi (2), Parameswaran, van Geemen
VARCHENKO	71	49	59.17%	Arnold (5), Barlet, Beulinson (2), Blok, Brylawski, Chmutov, Etingof (4), Feigin (3), Felder (11), Frenkel, Gusein-Zade, Markov, Mukhin (6), Reschetikhin, Schechtman (4), Tarasov (6)
VASSILIEV	51	5	91.07%	Arnold (5)
WALL	139	28	83.23%	Atiyah, Browder, Bruce (2), Duplessis (9), Ebeling, Edwards, Frohlich (4), Haefliger, Hsiang (2), James, Johnson, Madsen (2), Scott, Thomas
WHITNEY	44	3	93.62%	Dold, Gleason, Loomis,
WILSON	8	15	34.78%	Bleeker (2), Bruce, duPlessis (3), Ferrarotti (3), Gaffney (2), O'Shea (2), Sun, Trotman
ZAKALYUKIN	18	6	75.00%	Agrachev (2), Bruce, Davydov (2), Roberts
ZARISKI	78	0	100.00%	
Totals	2810	733		
Percent	79.31%	20.69%		
Grand Total	3543			

APPENDIX H. Questions for a semi-structured one-to-one interview schedule.

THE INSTITUTE AS AN INFORMATION USE ENVIRONMENT:

1. [Invited visitor] I'm interested in knowing why you were invited to the Newton Institute to participate in this year's Singularity Theory program?

2. [Non-invited visitor] What made you decide to visit the Newton Institute and participate in the Singularity Theory program, even though you were not formally invited?

3. What is your impression of the physical arrangement of facilities at the Newton Institute?

4. What is your impression of the Newton Institute scientific support staff? Have they been helpful to you so far?

5. What are some of the Newton Institute resources/facilities that you anticipate using or have been using?

6. Is there a particular area of the Institute in which you like to work? Why?

7. Is this your first visit to the Newton Institute?

8. What was the reason for your last visit?

9. How long did you stay? Why?

10. Describe your thoughts/opinions about the Newton Institute during the time of your first visit? Have they changed?

11. Can you give me some specific reasons as to why you might plan another visit to the Newton Institute again?

12. Have you participated in other program activities at a research institute similar to the Newton Institute?

13. How does the Newton Institute compare with the other research institutes that you have visited in the past?

THE INTELLECTUAL STRUCTURE OF THE INVISIBLE COLLEGE:

1. Describe the type of problems that you are currently working on in the Singularity Theory research field.

2. Do you think that there are a lot of important problems left to work on?

3. Are there any new areas of thought or research trends emerging in this field?

4. Describe some of the important rules and norms that govern the work in this field.

5. (Show informant the co-citation map). This is a map of the co-citation patterns of a selection of members of the Singularity Theory research field. What is your initial reaction to this map?

6. (Informant located on map). Do you agree with your position and do you see yourself as being similar to the other mathematicians with whom you are clustered on this map?

7. (Informant not on map). Where would you expect to be placed on this co-citation map?

SOCIAL ROLES AND INFORMAL COMMUNICATION PROCESSES:

1. Are you a senior professor, assistant (untenured) professor, graduate student or postdoctoral fellow in the Singularity Theory research field?

2. (Graduate student). Who is your Ph.D. thesis advisor?

3. Is your advisor also visiting the Institute during this program term?

4. Do you plan to discuss mathematics regularly with your advisor throughout the period of your visit? In person? By electronic mail, fax or telephone?

5. (Postdoctoral fellow). Who was your Ph.D. thesis advisor?

6. Is your former advisor also participating in this program?

7. Have you kept in contact with your former advisor since you graduated?

8. Do you plan to communicate with him or her while you are visiting the Newton Institute? In person? By electronic mail, fax or telephone?

9. (Senior professor / Assistant professor). How long have you been working in the Singularity Theory field?

10. Have you continued to work in the same research area as when you first graduated?

11. Are you currently an advisor of any graduate students in this field?

12. Did you encourage your graduate student to participate in this program and visit the Institute at the same time as your visit?

13. [All visitors] Do you feel comfortable in communicating with all the visiting members of the Newton Institute? Is there any reason why you would not talk with one of the visitors or approach one of the visitors for a person-to-person conversation?

14. Do you recognize a tendency for certain mathematicians to talk with certain others during a program visit? What would be the reason for this tendency?

SOCIAL ACTOR (COLLABORATIVE WORK):

1. Is there anyone who is participating in the current Singularity Theory Program that you would like to meet and talk to? Why?

2. Have you made arrangements prior to your visit to meet with a particular colleague for collaborative purposes?

3. Describe a collaborative project that you are currently working on.

4. Tell me about your role in this collaborative work.

5. What is the role of your colleague(s) in this collaborative work?

6. Tell me about the nature of some of the past collaborative projects that you initiated or worked on during an Institute program term.

APPENDIX I. Interview consent form.

EXPLANATION OF THE RESEARCH:

The purpose of this research is to examine the intellectual structure and social process of communication in Singularity Theory research. The objective is to understand the bibliometric structure of this subject, the collaborative and collegial network relationships of the mathematicians involved in the subject, and the role that the Isaac Newton Institute plays in supporting these relationships.

Alesia Zuccala (zuccala@fis.utoronbto.ca) is carrying out this research as a requirement for her Ph.D. thesis. Her supervisor is Prof. J. Dilevko of the Faculty of Information Studies, University of Toronto. The other members of her research committee include Prof. E. Bierstone (bierston@math.utoronto.ca) of the Department of Mathematics, University of Toronto, and Prof. B. Wellman (wellman@cgass.utoronto.ca), of the Department of Sociology, University of Toronto.

SUBJECT CONSENT:
As an interview informant for this study:

a) I understand that all the important ethical considerations concerning my rights, needs and values will be recognized;

b) I understand that if I participate in an interview that it will be tape-recorded, transcribed verbatim, and used for analysis;

c) I understand that there are no psychological or physical risks associated with my role as an interview informant;

d) I understand that I have the option of withdrawing from the study at any point in time, or refusing to answer specific questions without any negative consequences;

e) I understand that the researcher may need to communicate with me by e-mail for follow-up questions after the Newton Institute program term has ended, and that I have the option of continuing or not continuing my role as an informant;

f) I understand that the researcher would like to use my actual name in connection with the information I provide in an interview session in her written research report;

g) I understand that I have the voluntary option of allowing or not allowing my name to appear in the written research report;

h) I understand that if I choose NOT to allow my name to appear in the written research report, that the information I provide during the interview will be reported in connection with a pseudonym.

I the undersigned: agree / do not agree (circle one) to allow the researcher, Alesia Zuccala, to use my name in her written Ph.D. research report.

Informant's signature:

____

E-mail address:

____

APPENDIX J. ACA results in *SPSS* (n=48). *Singularity Theory and Its Applications to Quantum Field Theory* (1974-2000).

Table J.1. Partial raw co-citation matrix.

	ANISOV	BARANNIKOV	BERTOLA	BOALCH	CHEKANOV	DAMON	DE GREGORIO	DIJKGRAAF	DOLGACHEV	DUBROVIN	DUPLESSIS	EKHOLM	EVANS	GAFFNEY	GODDARD	GORYUNOV	GRITSENKO	GROSS	HERTLING	HITCHIN	IVANOV	IZUMIYA	KAZARIAN
ANISOV	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	1
BARANNIKOV	0	0	0	0	0	0	0	4	0	5	0	0	0	0	0	1	0	0	1	1	0	0	0
BERTOLA	0	0	0	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0
BOALCH	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
CHEKANOV	1	0	0	0	0	1	0	0	0	1	0	0	0	0	0	1	0	0	0	0	0	0	2
DAMON	0	0	0	0	1	7	0	0	5	0	29	0	0	45	0	18	0	0	0	0	0	16	0
DE GREGORIO	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
DIJKGRAAF	0	4	0	0	0	0	0	9	3	0	0	0	8	0	185	0	15	8	0	54	0	0	0
DOLGACHEV	0	0	0	0	0	5	0	3	4	2	2	0	0	3	1	2	5	12	0	11	0	0	0
DUBROVIN	0	5	2	0	1	0	0	0	2	4	0	0	1	0	21	1	0	1	1	74	0	0	0
DUPLESSIS .	0	0	0	0	0	29	0	0	2	0	3	0	0	31	0	2	0	0	0	0	0	8	0
EKHOLM	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
EVANS	0	0	0	0	0	0	0	8	0	1	0	0	1	8	5	0	0	1	0	0	0	0	0
GAFFNEY	0	0	0	0	0	45	0	0	3	0	31	0	8	5	0	2	0	2	0	0	0	5	0
GODDARD	0	0	0	0	0	0	0	185	1	21	0	0	5	0	6	0	4	2	0	31	0	0	0
GORYUNOV	1	1	0	0	1	18	0	0	2	1	2	0	0	2	0	3	0	0	0	0	0	5	3
GRITSENKO	0	0	0	0	0	0	0	15	5	0	0	0	0	0	4	0	1	6	0	0	0	0	0
GROSS	0	0	0	0	0	0	0	8	12	1	0	0	1	2	2	0	6	2	0	9	0	0	0
HERTLING	0	1	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0
HITCHIN	0	1	0	0	0	0	0	54	11	74	0	0	0	0	31	0	0	9	0	6	0	0	0
IVANOV	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
IZUMIYA	0	0	0	0	0	16	0	0	0	0	8	0	0	5	0	5	0	0	0	0	0	2	0
KAZARIAN	1	0	0	0	2	0	0	0	0	0	0	0	0	0	0	3	0	0	0	0	0	0	0

Table J.2. Partial correlation matrix (Pearson's *r*).

	ANISOV	BARANNIKOV	BERTOLA	BOALCH	CHEKANOV	DAMON	DE GREGORIO	DIJKGRAAF	DOLGACHEV	DUBROVIN	DUPLESSIS	EKHOLM	EVANS	GAFFNEY	GODDARD	GORYUNOV	GRITSENKO	GROSS	HERTLING	HITCHIN	IVANOV	IZUMIYA	KAZARIAN
ANISOV	0.000	0.032	0.017	0.000	0.640	0.151	0.000	-0.109	0.069	-0.064	-0.044	0.000	-0.129	-0.023	-0.090	0.386	-0.112	-0.141	-0.043	-0.114	0.000	0.141	0.520
BARANNIKOV	0.032	0.000	0.631	0.000	0.101	0.028	0.000	0.034	0.164	0.180	-0.077	0.000	0.345	-0.036	0.633	0.011	0.307	0.195	0.186	0.855	0.000	-0.099	-0.026
BERTOLA	0.017	0.631	0.000	0.000	0.198	0.190	0.000	0.021	0.227	0.194	-0.050	0.000	-0.034	0.060	0.020	0.183	-0.047	-0.056	0.555	0.475	0.000	-0.014	-0.063
BOALCH	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
CHEKANOV	0.640	0.101	0.198	0.000	0.000	0.474	0.000	-0.104	0.199	-0.068	0.310	0.000	-0.141	0.354	-0.088	0.701	-0.140	-0.124	0.073	0.039	0.000	0.592	0.289
DAMON	0.151	0.028	0.190	0.000	0.474	0.000	0.000	-0.067	0.470	-0.040	0.783	0.000	0.160	0.755	-0.094	0.574	-0.099	-0.009	0.157	0.002	0.000	0.664	0.062
DE GREGORIO	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
DIJKGRAAF	-0.109	0.034	0.021	0.000	-0.104	-0.067	0.000	0.000	0.134	0.476	-0.077	0.000	0.340	-0.058	0.073	-0.117	0.223	0.303	0.036	0.321	0.000	-0.095	-0.111
DOLGACHEV	0.069	0.164	0.227	0.000	0.199	0.470	0.000	0.134	0.000	0.208	0.330	0.000	-0.045	0.428	0.028	0.315	0.530	0.431	0.237	0.174	0.000	0.170	-0.036
DUBROVIN	-0.064	0.180	0.194	0.000	-0.068	-0.040	0.000	0.476	0.208	0.000	-0.069	0.000	0.004	-0.043	0.097	-0.038	0.006	0.283	0.075	0.116	0.000	-0.095	-0.059
DUPLESSIS	-0.044	-0.077	-0.050	0.000	0.310	0.783	0.000	-0.077	0.330	-0.069	0.000	0.000	0.221	0.842	-0.072	0.544	-0.101	0.018	-0.059	0.007	0.000	0.747	-0.075
EKHOLM	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
EVANS	-0.129	0.345	-0.034	0.000	-0.141	0.160	0.000	0.340	-0.045	0.004	0.221	0.000	0.000	-0.065	0.624	-0.136	0.387	0.096	-0.014	0.469	0.000	-0.009	-0.102
GAFFNEY	-0.023	-0.036	0.060	0.000	0.354	0.755	0.000	-0.058	0.428	-0.043	0.842	0.000	-0.065	0.000	-0.072	0.617	-0.090	0.005	0.035	0.028	0.000	0.782	-0.083
GODDARD	-0.090	0.633	0.020	0.000	-0.088	-0.094	0.000	0.073	0.028	0.097	-0.072	0.000	0.624	-0.072	0.000	-0.123	0.576	0.231	-0.027	0.602	0.000	-0.093	-0.091
GORYUNOV	0.386	0.011	0.183	0.000	0.701	0.574	0.000	-0.117	0.315	-0.038	0.544	0.000	-0.136	0.617	-0.123	0.000	-0.148	-0.123	0.132	-0.072	0.000	0.701	0.383
GRITSENKO	-0.112	0.307	-0.047	0.000	-0.140	-0.099	0.000	0.223	0.530	0.006	-0.101	0.000	0.387	-0.090	0.576	-0.148	0.000	0.441	-0.055	0.417	0.000	-0.144	-0.132
GROSS	-0.141	0.195	-0.056	0.000	-0.124	-0.009	0.000	0.303	0.431	0.283	0.018	0.000	0.096	0.005	0.231	-0.123	0.441	0.000	-0.039	0.304	0.000	-0.087	-0.143
HERTLING	-0.043	0.186	0.555	0.000	0.073	0.157	0.000	0.036	0.237	0.075	-0.059	0.000	-0.014	0.035	-0.027	0.132	-0.055	-0.039	0.000	0.097	0.000	-0.010	0.151
HITCHIN	-0.114	0.855	0.475	0.000	0.039	0.002	0.000	0.321	0.174	0.116	0.007	0.000	0.469	0.028	0.602	-0.072	0.417	0.304	0.097	0.000	0.000	-0.073	-0.140
IVANOV	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
IZUMIYA	0.141	-0.099	-0.014	0.000	0.592	0.664	0.000	-0.095	0.170	-0.095	0.747	0.000	-0.009	0.782	-0.093	0.701	-0.144	-0.087	-0.010	-0.073	0.000	0.000	0.129
KAZARIAN	0.520	-0.026	-0.063	0.000	0.289	0.062	0.000	-0.111	-0.036	-0.059	-0.075	0.000	-0.102	-0.083	-0.091	0.383	-0.132	-0.143	0.151	-0.140	0.000	0.129	0.000

Table J.3. Iteration history for the two-dimensional solution (in squared distances).

```
Young's S-stress formula 1 is used.

Iteration   S-stress    Improvement

1           .44530
2           .35656      .08874
3           .34504      .01152
4           .34336      .00169
5           .34294      .00042

Iterations stopped because S-stress improvement is less than
.001000

Stress and squared correlation (RSQ) in distances

RSQ values are the proportion of variance of the scaled data
(disparities) in the partition (row, matrix, or entire data)
which is accounted for by their corresponding distances.

Stress values are Kruskal's stress formula 1.

For  matrix
Stress  =  .27000    RSQ =  .62582
```

Table J.4. Configuration derived in two dimensions.

```
Stimulus Coordinates

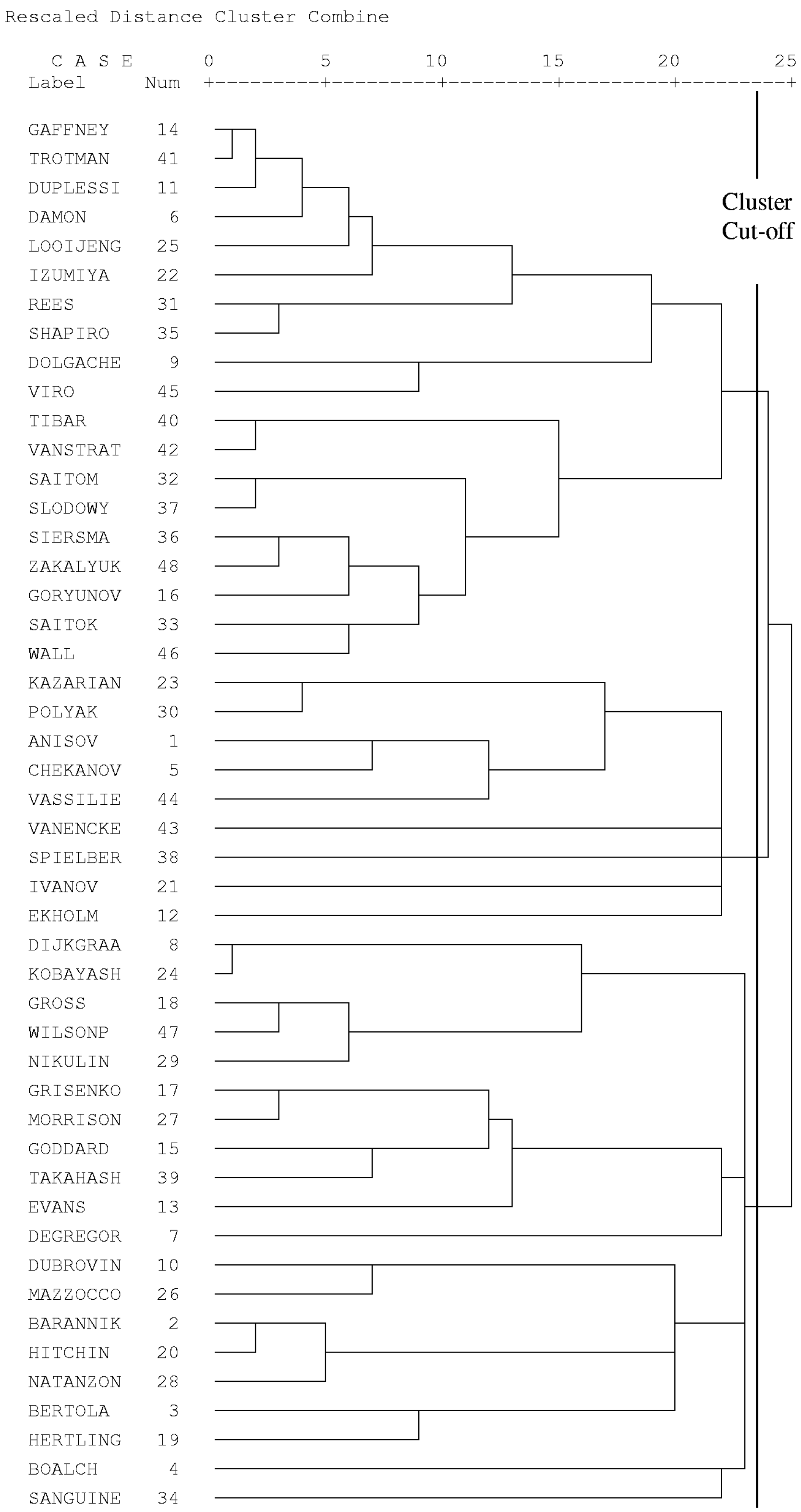
Dimension

Stimulus  Stimulus    1      2
 Number    Name

   1    ANISOV    1.2476  1.6778
   2    BARANNIK  -1.1763   .0493
   3    BERTOLA   -.0196  -.8911
   4    BOALCH    -.6119  1.1205
   5    CHEKANOV  1.4353   .6912
   6    DAMON    1.2746  -.2315
   7    DEGREGOR  -.5894  1.1223
   8    DIJKGRAA  -1.5278  -1.0854
   9    DOLGACHE   .4202  -.8085
  10    DUBROVIN  -1.4152  -.4713
  11    DUPLESSI  1.4238  -.2849
```

12	EKHOLM	-.5977	1.0916
13	EVANS	-1.6550	-.8537
14	GAFFNEY	1.2798	-.4655
15	GODDARD	-1.8580	-.1592
16	GORYUNOV	1.5656	.4780
17	GRISENKO	-1.7956	-.9595
18	GROSS	-1.1767	-1.2706
19	HERTLING	.2419	-1.1544
20	HITCHIN	-1.1317	-.7243
21	IVANOV	-.6394	1.0131
22	IZUMIYA	1.5852	.4695
23	KAZARIAN	.9265	1.9488
24	KOBAYASH	-.8968	-1.2700
25	LOOIJENG	.5971	-.9166
26	MAZZOCCO	-1.7159	.6040
27	MORRISON	-1.1686	-1.1167
28	NATANZON	-.8257	.6483
29	NIKULIN	.1168	-.7683
30	POLYAK	-.0402	2.0860
31	REES	.8271	.1137
32	SAITOM	.4233	-.8081
33	SAITOK	.3041	-.4432
34	SANGUINE	-1.3996	1.4618
35	SHAPIRO	1.0309	-.2185
36	SIERSMA	1.2477	-.2114
37	SLODOWY	.2470	-.7925
38	SPIELBER	-.6015	.9711
39	TAKAHASH	-.6970	-1.0909
40	TIBAR	1.1704	.2208
41	TROTMAN	1.1000	-.3025
42	VANSTRAT	.9061	-.5228
43	VANENCKE	-.5872	.9568
44	VASSILIE	.7120	1.5285
45	VIRO	.2627	1.0166
46	WALL	.8754	-.4110
47	WILSONP	-.4293	-1.1416
48	ZAKALYUK	1.3347	.1040

Figure J.1. Hierarchical cluster dendogram for two-dimensional solution using complete linkage.



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