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This study examines the interactions between the scientific communities of the Lawrence Berkeley Laboratory (LBL) and the Stanford Linear Accelerator Center (SLAC) in the discovery of the tau lepton by physicist Martin Perl between 1973-1977. Perl became interested in searching for heavy leptons through positron-electron collision experiments using the newly constructed Mark I detector at SLAC, and his search resulted in the discovery of the tau lepton. Although the experiments responsible for the discovery of this new particle were part of a collaborative effort between SLAC and LBL, Perl became known for his individual role in interpreting the data and was awarded the Nobel Prize in physics for his work in 1995.

Drawing upon personal and professional papers from the SLAC Archives and History Office, the LBL Archives and Records Office, and personal communications from the physicists involved in the discovery, I argue that the discovery of the tau lepton challenges many of the common generalizations regarding the practice of “Big Science.” Big Science has often been associated with a transformation in the life of the experimenter as individual autonomy was subsumed by a ‘factory’ work style typified by teamwork on a massive scale. However, an examination of the discovery of the tau

lepton reveals that physicists at SLAC worked in small research groups, enjoyed great scientific freedom, and maintained a direct and interactive role in shaping research. This study also illustrates how scientific ambition motivates decisions underlying priority and discovery, which is highlighted by Perl’s rush to publicize his findings in order to establish priority.

The Role of Competition, Community, and Priority in the Discovery of the Tau Lepton

by
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Major Professor, representing History of Science

Chair of the Department of History

Dean of the Graduate School

I understand that my thesis will become part of the permanent collection of Oregon State University libraries. My signature below authorizes release of my thesis to any reader upon request.

Leandra Swanner, Author

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Chapter One: Introduction

Section 1.1 Historical Significance of the Tau Lepton Discovery

The desire to comprehend the subatomic world has long been a driving force for philosophers, theorists and experimentalists seeking answers to the seemingly unknowable. Concepts of the fundamental building blocks of nature can be traced back to 460 B.C.E. with the Greek notion of the atom as uncuttable, literally meaning "that which cannot be divided." Centuries later, the enigmatic atom continued to invite speculation and investigation by noted theorists and experimentalists, ranging from J.J. Thomson's 'plum-pudding' model to Bohr's 'mini solar system' to the more accurate quantum mechanical model of the atom first mathematically described by Werner Heisenberg.

Twentieth-century physicists struggled to apply the tools of modern scientific inquiry to the exploration of the fundamental structure of matter. With a technology the Greeks could never have envisioned, physicists employed particle accelerators to 'peer' within the atom and discovered it had an unexpectedly complicated internal structure. In the age of "Big Science" spawned by large-scale government funding in World War II and huge research machinery, high-energy physicists at last came closer to establishing a coherent picture of the subatomic world through the formulation of a theory known as the Standard Model of physics.

The Standard Model of physics emerged gradually, and as new fundamental particles were found within the atom, the model was repeatedly modified to encompass new particles and the patterns governing the interactions between them. In its most simplified form, the model at the beginning of the twenty-first century contains six particles called quarks that are found within the protons and neutrons of the nucleus; six leptons, including the electron; and the electromagnetic, weak, and strong force carrier particles. The Standard Model has come to represent our modern understanding of matter, remarkably containing only sixteen particles from which hundreds of particles can be made. All of the known matter particles can now be successfully explained as composites of quarks and leptons, which interact by exchanging force carrier particles, forming everything from molecules to galaxies. These particles form three 'families,' or generations of matter, but until the mid-1970s, this extraordinarily coherent picture of the microcosmos consisted of only two generations of matter.[1]

J.J. Thomson's discovery of the electron in 1897, the first detection of the muon in the late 1930s, and the collaborative discovery of the charm quark at MIT and the Stanford Linear Accelerator Center (SLAC) in 1974 established the major features of the Standard Model by the 1970s. Physicists were satisfied with the model's

[1] For a brief overview of the major discoveries in particle physics in the late 19th and 20th centuries leading to the development of the Standard Model, see Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden, "The Rise of the Standard Model: 1964-1979," in *The Rise of the Standard Model: Particle Physics in the 1960s and Beyond*, ed. Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden (New York: Cambridge University Press, 1997), 3-35; S.S. Schweber, "From 'Elementary' to 'Fundamental' Particles," in *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Overseas Publishers Association, 1997), 599-616.

symmetric form because it contained two families of matter, each composed of two leptons and two quarks.[2]

However, a surprising new particle discovery in the mid-1970s tested the prevalent assumption that matter was orderly arranged into two categories. Between 1973 and 1977, nearly forty years after the second generation of matter had been firmly established with the muon, an unexpected new addition to the Standard Model was discovered by SLAC physicist Martin Perl using the results of a collaboration between the Lawrence Berkeley Laboratory (LBL) and SLAC. This particle was eventually named the tau lepton, a heavy and unstable 'cousin' of the electron which revealed the existence of a third generation of matter. The tau lepton came as a complete surprise to the physics community because it violated the existing symmetry of the Standard Model. Therefore, verifying the tau's existence proved to be a challenge that required Perl to confront ideological, social, and professional obstacles in addition to overcoming widespread scientific skepticism.

Section 1.2 Existing Literature on the Discovery of the Tau Lepton

The discovery of the tau lepton has drawn the attention of scientists and historians of science, who have addressed different elements of this story through widely varying perspectives. Brief references to the discovery of the tau lepton have

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[2] Prior to 1975, there was no experimental evidence for the generation of a third tier of particles. For an explanation of the scientific skepticism over the existence of a third generation of matter, see Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," in *Physics in Perspective*, vol. 6, ed. John S. Rigden and Roger H. Stuewer (Boston: Berkhäuser Verlag, 2004), 401-427; 420.

appeared in several major scholarly works examining the history of modern physics, but Perl's role has received relatively superficial treatment within the broader context of particle discoveries.[3] The discovery of the tau lepton was the focus of a 1987 Stanford University History of Science dissertation by Jonathan Treitel entitled "A Structural Analysis of the History of Science: the Discovery of the Tau Lepton." In fact, the dissertation devotes little analysis to the tau lepton discovery because it is largely concerned with establishing a new model in the philosophy of science. Treitel uses the discovery as a case study in order to present a classification scheme of laboratory structure that seeks to distinguish the research environments and scientific practices of SLAC and LBL according to simple dichotomous entities described as "exclusivists" and "inclusivists."[4]

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[3] The discovery of the tau lepton is mentioned in Laurie M. Brown et al., "The Rise of the Standard Model," 22; Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), 30; George Greenstein, *Portraits of Discovery: Profiles in Scientific Genius* (John Wiley & Sons, Inc.), chapter 7; W.K.H. Panofsky, "SLAC and Big Science: Stanford University," in *Big Science: The Growth of Large-Scale Research*, ed. Peter Galison and Bruce Hevly (Stanford: Stanford University Press, 1992), 129-146; 142; Michael Riordan, *The Hunting of the Quark: A True Story of Modern Physics* (New York: Simon & Schuster, 1987).

[4] See Jonathan Treitel, "A Structural Analysis of the History of Science: the Discovery of the Tau Lepton," (Ph.D. dissertation, Stanford University, 1987), chapter 1. Drawing upon Peter Galison's Image/Logic distinction, Treitel categorizes inclusivists and exclusivists as two separate traditons that vary according to their "confirmology," or their method of confirmation. Treitel defines inclusivists as physicists who obtain a picture of every event detected by an apparatus with the goal of later selecting only the pictures of interest, while the exclusivists rely on triggering devices that permit only certain events to be selected. Within this framework, Treitel describes LBL physicists as inclusivists and SLAC physicists as exclusivists. Extending Galison's assertion that Image and Logic traditions hybridized for the first time through the SLAC-LBL collaboration, Treitel explains that a fusion of inclusivist and exclusivist traditions occurred in the collaboration, which accounted for its success.

The tau lepton has also been the subject of several retrospective essays written by scientists who were directly involved, including Martin Perl and Gary Feldman of SLAC. Both Perl and Feldman have addressed the history of the discovery of the tau lepton from the perspective of a participant, and their scholarship thus reflects an emphasis on their respective roles. Feldman has authored a paper that examines the technical details of the discovery by discussing three of the earliest major publications on the tau lepton.[5] Perl has written extensively on his discovery both in scholarly publications, SLAC in-house publications, and several essays in a volume entitled *Reflections on Experimental Science.*[6]

Nevertheless, the existing literature on the discovery of the tau lepton overlooks or oversimplifies the sociological dimensions of the interactions between the scientific communities of SLAC and LBL. It is important to understand how the research environment at SLAC contributed to Perl's success in discovering the tau lepton, but the nature of scientific practice at SLAC has been insufficiently explored by other authors to address this critical issue. In addition, the initial controversy at SLAC and LBL over Perl's decision to prematurely publicize his experimental findings has been entirely ignored in previous accounts of the tau lepton's discovery.

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[5] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," in SLAC-R-412, *The Third Family and the Physics of Flavor: Proceedings of the 20th Annual SLAC Summer Institute on Particle Physics (SSI 92), 13-14 Jul 1992*, ed. Lilian Vassilian, SLAC, 636-646.

[6] Martin L. Perl, *Reflections on Experimental Science, World Scientific Series in 20th Century Physics, vol. 14* (River Edge: World Scientific Publishing Co., 1996); Perl,"The Discovery of the Tau Lepton," 401-427; Perl, "Reflections on the Discovery of the Tau Lepton," Nobel Lecture, 08 1995.

Section 1.3 Exploring New Dimensions of the Discovery

The discovery of the tau lepton is not simply a component history of the Standard Model of physics; it is an informative episode in the history of modern physics that provides new insights into how Big Science operated in the mid-1970s within the scientific communities of SLAC and LBL. By examining the nature of collaboration and competition, this thesis will explore the fundamental differences in laboratory culture between SLAC and LBL that both aided and hindered scientific collaboration between the two laboratories. Through a study of the discovery of the tau lepton, I examine the difficulty of validating a scientific discovery in high-energy particle physics and the potential social and professional consequences of attempting to establish priority. Furthermore, I argue that an examination of the research environment of SLAC reveals that elements of the standard conception of Big Science did not apply to SLAC during the late 1960s and early 1970s. I will demonstrate that that unlike LBL, SLAC did not truly function during that historical period as a multinational Big Science facility with large teams of scientists and little interface between the experimenter and the experiment.

Big Science typically has been characterized as large-scale, federally funded research that often requires enormous multidisciplinary and multinational teams of scientists working in collaboration. Peter Galison, Bruce Hevly, and others have argued that in high-energy particle physics—a prototypical Big Science field—the advent of Big Science practices caused physicists to become increasingly concerned with losing control of their research, and thus the character of research was fundamentally altered. According to this view, the life of the experimenter was radically transformed as individual autonomy was subsumed by a ‘factory’ work style typified by teamwork on a massive scale.[7] However, an examination of the discovery of the tau lepton suggests that physicists operated within a research environment at SLAC that challenges some of these common generalizations regarding the practice of Big Science. As I will demonstrate, SLAC physicists then enjoyed great scientific freedom, and the relationship between scientist and experiment was closely maintained throughout the period of SLAC’s enormously fruitful research during the 1970s. Due to the unique research environment at SLAC, Perl was afforded the resources to pursue his personal research interests despite considerable skepticism from his colleagues, and I will show that this atmosphere of scientific freedom ultimately enabled Perl to discover the tau lepton.

Section 1.4 Outline of Chapters

This study will begin with an examination of the distinct scientific communities of LBL and SLAC, including a brief history of each laboratory and a discussion of the organization of these research facilities. As part of this analysis, I will detail the dominant research performed in each laboratory. Galison has argued that LBL and SLAC were distinguished by an ‘image’/’logic’ division, and I will

[7] For a further account of the perceived distance between experimental physicists and their instruments due to new electronic technology, see Paolo Brenni, “Physics Instruments in the Twentieth Century,” in *Science in the Twentieth Century*, 754-755. The argument that experimentalists experienced a distressing loss of control over their research due to Big Science is found in Peter Galison, Bruce Hevly, and Rebecca Lowen, “Controlling the Monster: Stanford and the Growth of Physics Research, 1935-1962,” in *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992), 46-77; Galison, *Image and Logic*, 306-307.

analyze this classification scheme to determine its strengths and limitations.[8] I will conclude this chapter by assessing how well LBL and SLAC conformed to the standard conception of Big Science during the mid-1970s, a theme that will be revisited throughout this study.

Chapter 2 will trace the origins and dynamics of the collaboration between LBL and SLAC and provide an account of the events immediately preceding the discovery of the tau lepton. I will begin with an identification of the key historical actors involved in the collaboration, with particular emphasis on Martin Perl. Specifically, I will account for how the biographical, educational, and experimental background of Martin Perl shaped his scientific style prior to joining SLAC in 1963, including Perl's obsessive interest in solving the 'electron-muon' puzzle. I will then discuss the original SLAC-LBL collaboration on the SLAC-LBL Solenoidal Magnetic Detector, later known as the Mark I detector, at SLAC. In this chapter, I will explain how Perl's personal research interests were integrated into the Mark I proposal, thus revealing that Big Science practices did not alienate the researcher from the experimental apparatus at SLAC. Similarly, the subsequent collaboration on the Mark I design and construction, as well as the collaboration on the Mark I experiments, will demonstrate how Big Science remained small in character during this period. The analysis of the SLAC-LBL collaborations also highlights the nature of competition and cooperation, and I will conclude this chapter with an evaluation of the working relationship between the two laboratories.

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[8] For a complete description of the image/logic divide outlined by Galison, see Galison, *Image and Logic*, especially 517-552; see also Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model*, 308-337.

Chapter 3 will detail the discovery, publication, and subsequent verification of the tau lepton discovery. I will first attempt to account for Perl's decision to rush to publication without consulting his colleagues after interpreting his initial experimental results. After detailing the professional and social consequences of Perl's actions at SLAC and LBL, I will then trace the history of skepticism and the early failure to validate the discovery of the tau lepton through confirmation by an outside laboratory. Finally, I will explore the difficulty of establishing scientific discovery in general as I examine how and why the discovery eventually gained widespread acceptance, culminating with Perl's receipt of the Nobel Prize in Physics in 1995.

Chapter 4 will conclude this study with a discussion of the tau lepton's place within the Standard Model of physics and suggestions for further study.

Section 1.5 The Scientific Communities of SLAC and LBL

Section 1.5.1 The Origins of the Particle Physics Community

Although nuclear science predated World War II, particle physics did not emerge as a distinct field until the second half of the twentieth century.[9] As Helge Kragh observes, elementary particle physics was not yet considered a discipline in 1948.[10] The status of high-energy particle physics changed dramatically, however, as Big Science funding spawned by the wartime interest in nuclear physics led to the

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[9] For the advent of nuclear science and early accelerator experiments in the 1930s, see Mary Jo Nye, *Before Big Science: The Pursuit of Modern Chemistry and Physics, 1800-1940* (New York: Twane Publishers, 1996), 211-224; on the dominance of nuclear physics from the mid-1930s to the mid-1950s, see Helge Krage, *Quantum Generations: A History of Physics in the Twentieth Century* (Princeton: Princeton University Press, 1999), 326-328.
[10] Krage, *Quantum Generations,* 312.

development of more powerful accelerators and detectors in the 1950s. The success of this new generation of accelerators finally established high-energy particle physics as a recognized field, and the knowledge of elementary particles derived from these accelerators created a specialized scientific community of high-energy particle physicists composed of a multinational association of institutions that spoke a common technical language and communicated through conferences and journals. As the field entered its technological adolescence in the 1960s and 1970s, several major particle accelerators were operating successfully, and the laboratory culture varied significantly at each facility.

The European Organization for Nuclear Research (CERN) was created in 1954 as a collaborative venture between twelve European nations dedicated to "pure" science with "no concern with work for military requirements."[11] Headquartered in Geneva, CERN's first large accelerators were constructed in the late 1950s with the goal of reaching an energy of 25 GeV. The 25 GeV proton synchrotron's energy was deliberately chosen to establish a more powerful machine at CERN than at any accelerator planned in the United States, which was viewed as the primary competition by CERN member states.[12] The founders of CERN conceived of an organization that would attract scientists of great integrity who engaged in research for idealistic reasons rather than personal gain. For this reason, permanent contracts and high

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[11] Armin Hermann, John Krige, Ulrike Mersits, and Dominique Pestre, *History of CERN* (New York: Elsevier Science Pub., 1987), 228. Also see 246-249 for a discussion of the Convention signed by the governments of the members of CERN.
[12] Dominique Pestre, "The Decision-Making Processes for the Main Particle Accelerators Built Throughout the World from the 1930s to the 1970s," in *Choosing Big Technologies*, ed. John Krige (Philadelphia: Harwood Academic Publishers, 1993), 163-174; 167.

salaries were to be discouraged, and CERN was not supposed to compete with research institutes in its member nations. These ideals were not achieved in practice, since CERN scientists were no less self-sacrificing or more committed to "pure" science than their counterparts in other high-energy physics facilities.[13]

The German electron synchotron, Deutsches Elektronen-Synchotron, or DESY, was constructed in the early 1960s in Hamburg and the electron-positron storage ring DORIS there was built over a five-year period beginning in 1969. In contrast to the multinational CERN, DESY was founded with the goal of becoming an internationally competitive national research center that focused on both basic and applied research in elementary particle physics.[14]

In Italy, the Frascati National Laboratories (LNF) were founded in 1955, and a synchotron of energy 1.1 GeV was in operation by 1959. A prototype colliding beam accelerator was built in 1961, followed by the larger ADONE in 1969, which was capable of reaching energy 3 GeV.[15] The success of Italy's colliding beam accelerator worried high-energy particle physicists in the United States, who feared that Europe would soon dominate the field because no electron-positron storage rings had been built in the United States by the late 1960s.[16]

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[13] Armin Hermann, John Krige, et. al., *History of CERN,* 229-230.
[14] See Elizabeth Paris, "Ringing in the New Physics: The Politics and Technology of Electron Colliders in the United States, 1956 to 1972," (Ph.D. dissertation, University of Pittsburgh, 1999); Paris, "Lords of the Ring: The Fight to Build the First U.S. Electron-Positron Collider," *Historical Studies in the Physical and Biological Sciences 31* (2001): 355-380.
[15] Paris, "Lords of the Ring," 371.
[16] Paris, "Lords of the Ring," 371.

The rapid growth of the European accelerator community and the success of the first colliding beams in Italy was particularly distressing for American physicists who felt they had formerly been pioneers in the field but were no longer leading the world in accelerator development. The history of high-energy particle physics in the United States Lawrence dated back to the founding of the Lawrence Berkeley Laboratory (LBL) in Berkeley, California in 1931, which is discussed at greater length in Section 1.5.3. Brookhaven National Laboratory (BNL) on Long Island, New York was founded in 1947 for peacetime research, run by a group of East Coast universities called the Associated Universities Incorporated (AUI).[17] In the mid-1950s, the proton synchotrons called the Bevatron at LBL and the Cosmotron at BNL were the highest energy accelerators in the United States, capable of reaching energies of between 1 and 2 GeV.[18] Fermilab, located in Illinois, was another major particle physics center, commissioned by the U.S. Atomic Energy Commission as the National Accelerator Laboratory in late 1967. Within the high-energy physics community, the dominant prewar facility, LBL, was criticized for excluding visiting scientists, and the National Accelerator Laboratory was created as a response to this problem.[19] Finally, the Stanford Linear Accelerator Center (SLAC) was founded in 1962, which is examined further in Section 1.5.4.

As we have seen, SLAC and LBL were not the only facilities engaging in high-energy particle physics during the 1960s and 1970s, but I have focused my discussion

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[17] Laurie M. Brown, Max Dresden, and Lillian Hoddeson, *Pions to Quarks: Particle Physics in the 1950s* (New York: Cambridge University Press, 1989), 201-212.
[18] Paris, "Lords of the Ring," 371.
[19] Laurie M. Brown et. al., *Pions to Quarks,* 17.

of high-energy physics laboratories on SLAC and LBL in this thesis principally because it was the collaboration between these two laboratories that was responsible for the tau lepton discovery. Each laboratory belonged to and competed with the larger high-energy physics community of the 1960s and 1970s, and it is within this context that I examine the particular scientific communities of these two laboratories.

Section 1.5.2 Defining the Image and Logic Traditions

In order to evaluate the distinct scientific communities of SLAC and LBL in the late 1960s and early 1970s, it is critical to understand how the unique circumstances surrounding the founding and growth of these two laboratories affected the major research trends and the character of the scientific workplace at each facility. Furthermore, the strengths and weaknesses of Galison's image/logic dichotomy pertaining to SLAC and LBL must be carefully considered.

Galison defines image and logic as "two competing traditions of instrument making," each drawing upon different skills and epistemic methods.[20] The image tradition is concerned with visual representations of knowledge and employs instruments such as bubble chambers, cloud chambers, and nuclear emulsions to produce images. By contrast, the logic tradition is based on statistical evidence, relying on machines that produce counts, such as spark chambers, wire chambers, and counters.[21]

According to Galison, "the tension between analog technical knowledge and digital technical knowledge is a deep one" because physicists historically favored

[20] Galison, *Image and Logic*, 19.
[21] Ibid.

either the image or logic tradition according to pedagogical, technical, and epistemic factors.[22] Each factor is said to have contributed to the "constantly reinforced continuity" of either tradition. Pedagogical continuity is described as passing techniques related to an image or logic device from instructor to student, resulting in the student's continued work in the tradition of the instructor. Technical continuity is defined as using a particular set of laboratory skills when working with either an image or logic device. Persistently using the same skill set reinforced the distinct nature of the two traditions because "…skills did not transfer easily across the image-logic divide."[23] Galison refers to demonstrative or epistemic continuity as continuity in the form of demonstration, which varied according to tradition. For image physicists, a single persuasive picture, or "golden event," was the preferred form of demonstration, while logic physicists favored statistical demonstrations.[24] In fact, Galison finds that the image and logic traditions were not only dedicated to their respective forms of demonstration, but that they were highly critical of the evidence used by the opposing tradition, since "each found its own form of argumentation persuasive and judged the competition to be faulty in certain respects…"[25] The incompatibility of image and logic traditions due to these factors meant that spark chamber physicists were determined to eradicate visual systems and had a "social

[22] Galison, *Image and Logic*, 41.
[23] Ibid, 21.
[24] Ibid, 22-23.
[25] Ibid, 24.

desire to recapture control over their workplace and avoid the hierarchical teams of the 'bubblers.'"[26] Galison claims that for physicists following the image tradition:

> there was always something suspect about the logic tradition's highly selective 'cuts' of the data before recording ever took place…by contrast, the logic experimenters professed horror at their competitors' passivity. As the logic physicists saw it, the image physicists had given up being experimenters when they removed themselves from the real-time manipulation of the apparatus.[27]

For Galison, LBL embodied the image tradition while SLAC adhered to the logic tradition. In the following discussion of the origin and growth of LBL and SLAC, I will evaluate this social and technical construction in detail with the goal of identifying how well these two scientific communities conformed to Galison's representation.

Section 1.5.3 Lawrence, Alvarez, and Bubble Chambers at LBL

LBL was founded in 1931 by Ernest Orlando Lawrence, who managed to firmly establish the Radiation Laboratory at the University of California at Berkeley even amid the Great Depression.[28] Lawrence was a charismatic figure who personally oversaw fundraising efforts and rapidly transformed the early Radiation Laboratory into a world-renowned nuclear science facility in the prewar period. Lawrence was responsible for developing the 27-inch cyclotron, an ambitious project that required an

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[26] Galison, *Image and Logic*, 40.
[27] Ibid, 25.
[28] J.L. Heilbron and Robert W. Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory: Volume I* (Berkeley: University of California Press, 1990), 26-28.

enormous budget that was largely met through private patronage. Robert Seidel notes that Lawrence's remarkable ability to secure funding for the cyclotron through state and private funds was aided greatly by the precedent set by the construction of other large research laboratories and astronomical observatories that had previously been funded by the University of California and the California Institute of Technology.[29] Lawrence took full advantage of the favorable attitude towards large-scale scientific enterprise that had already been established through the university system, operating as "a businessman of science, an entrepreneur culturally and socially well integrated in the San Francisco banking and industrial milieux."[30]

Following his success with the 27-inch cyclotron, Lawrence managed to secure funding for the construction of larger cyclotrons that functioned at increasingly higher energy ranges. Driven by the desire to build progressively more technologically sophisticated machinery on a grand scale, Lawrence was not concerned with personally making theoretical and experimental discoveries. Instead, he concentrated his efforts on designing cyclotrons primarily to challenge the limits of existing technology, and the laboratory experienced considerable growth as a result.

Between 1938 and 1940, the Radiation Laboratory expanded to include a staff of sixty members, which necessitated the formation of well-defined research groups.[31]

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[29] Robert Seidel, "The Origins of the Lawrence Berkeley Laboratory," in *Big Science: The Growth of Large-Scale Research* (Stanford: Stanford University Press, 1992), 21-45; 26.
[30] Dominique Pestre, "The Decision-Making Processes for the Main Particle Accelerators Built Throughout the World from the 1930s to the 1970s," in *Choosing Big Technologies*, ed. John Krige (Philadelphia: Harwood Academic Publishers, 1993), 163-174; 164.
[31] Robert Seidel, "The Lawrence Berkeley Laboratory," *Big Science*, 29.

Even as it became necessary to organize the laboratory into a hierarchical network of engineers, crew chiefs, assistant directors, and specialized committees, Lawrence maintained a prominent leadership position. As atomic physicist R.H. Fowler of the University of Cambridge observed in 1941, "a great toolmaker is a high estate, but we should do Lawrence scant justice if we did not hail him as a great team leader too."[32] Under Lawrence's directorship, the Radiation Laboratory became a highly structured working environment, and Seidel notes that although staff members disliked the rigid new controls of the workplace, they also found social and professional satisfaction in the group structure.[33]

Perhaps one of the most unique features of the early organization of the Radiation Laboratory at Berkeley was the multidisciplinary nature of the lab's research groups. Working with radioactive substances, neutron beams, and high-voltage X rays appealed to a wide variety of scientists, especially biologists, physicians, and chemists interested in treating disease. Thus, nuclear science at Berkeley came to encompass many different disciplines, and the multifaceted nature of research drew patronage from medical interest groups. Furthermore, it is important to note that physicists, biologists, chemists, and physicians efficiently worked together on many joint enterprises, achieving a truly interdisciplinary work environment that endures today.[34]

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[32] R.H. Fowler, "Professor Lawrence and the Development of the Cyclotron," *Science* 93 (1941), 76.
[33] Robert Seidel, "The Lawrence Berkeley Laboratory," *Big Science*, 30.
[34] Ibid, 36.

The Radiation Laboratory continued to experience substantial growth during World War II as additional branches were added to its existing internal organization. A new Operations division coordinated the activities of 398 staff members, and another new branch responsible for payroll and accounting employed 274 individuals. Most of the laboratory's other divisions employed a minimum of 100 individuals, including 166 physicists and technicians. The laboratory employed approximately one thousand staff members during the war, forming a work force that overshadowed the prewar number of employees by a factor of twenty.[35]

In the postwar years, high-energy physics at Berkeley continued to expand in scale when Luis Alvarez began his decade-long tenure as Associate Director in 1949. Alvarez, who had been a physicist at LBL since 1936, was determined to create a large, factory-like laboratory by dramatically enlarging the magnitude of bubble chamber research.[36] The first bubble chambers were constructed in 1952 by Donald Glaser at the University of Michigan, and Alvarez immediately began to refine the new machine for use in large-scale research.[37] Drawing upon his wartime experience in the Manhattan and Radar Projects, Alvarez assumed a prominent managerial role and organized bubble chamber physicists and staff according to a strict military-like hierarchical structure.[38] Through Alvarez's efforts, the laboratory boasted a massive

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[35] Robert Seidel, "The Lawrence Berkeley Laboratory," *Big Science*, 40.
[36] Galison, *Image and Logic*, 36.
[37] Andrew Pickering, *The Mangle of Practice* (Chicago: University of Chicago Press, 1995), 40.
[38] Ibid, 350.

72-inch cloud chamber by 1959, and the funding of such costly machines instigated a new rivalry between different research groups over budgetary issues.[39]

Berkeley unquestionably dominated bubble chamber work during this period due to Alvarez's influence, but in the late 1950s and early 1960s, the field began to change as new technology was integrated into bubble chamber physics that facilitated the processing of images by computers. Alvarez was firmly committed to the enduring utility of the image tradition throughout the 1960s, and even publicly defended the superiority of pictorial data in a debate staged with physicist Wolfgang Panofsky.[40] However, using computers to aid in the scanning of images was unavoidable in order to efficiently analyze the great influx of photographic data from the bubble chambers. Alvarez recognized the necessity of incorporating computers into bubble chamber data processing, but computers represented a serious threat to his experimental practices because the new "reading machines" could potentially remove the human element entirely. In response to this perceived tension between technology and the scientist, Alvarez developed an approach to pictorial data analysis that retained the tradition of human intervention while simultaneously integrating new technological innovations. His most effective strategy was to establish a factory-like workplace at LBL in which

[39] Andrew Pickering, *The Mangle of Practice* (Chicago: University of Chicago Press, 1995), 365-367.

[40] Personal communication from Pief Panofsky, SLAC, 29 Mar 06 and 27 Jul 2006. Edwin McMillan, who succeeded Lawrence at LBL, requested the debate between Panofsky and Alvarez. Alvarez believed bubble chamber methods should be used to the exclusion of other methods and the purpose of the debate was to challenge and defend the notion that pictorial data was superior. Panofsky provided many counterexamples to this claim, which Alvarez dismissed as exceptions.

physicists worked in rotating shifts twenty-four hours a day, seven days a week.[41] By the late 1960s, Alvarez's 'factory' had become so highly regulated that he found the work to be overly monotonous, and he lost interest in experimental physics altogether.[42]

From Lawrence's vision of continually evolving machinery to Alvarez's determination to build an industrial workplace, LBL quickly became a Big Science facility that enjoyed both the advantages and the pitfalls of large-scale scientific enterprise. By the 1960s, LBL was a well-respected laboratory with a proven track record that was viewed by the worldwide physics community not only as a center of bubble chamber physics, but of multidisciplinary nuclear science. This privileged status came at a heavy price for some physicists, however, since the type of research carried out at LBL was only made possible through rigidly defined research groups operating in a factory-style environment.

Section 1.5.4. Panofsky and the 'Monster'

As LBL enjoyed the continued security earned by a history spanning nearly three decades, plans for the Stanford Linear Accelerator Center (SLAC) were underway roughly 50 miles away. SLAC was the end result of years of painstaking administrative and political lobbying by particle physicist Wolfgang "Pief" Panofsky. Panofsky originally held a position at Berkeley's Radiation Lab beginning in 1945 and worked as a professor at the university between 1946 and 1951, but he left Berkeley to

[41] Galison, *Image and Logic*, 407-408.
[42] Ibid, 422.

accept a post at Stanford in 1951 in order to escape the politically restrictive environment at Berkeley brought about by the loyalty oaths.[43] After arriving at Stanford, Panofsky became interested in expanding the scope of particle accelerators at the university, a movement first initiated by Robert Hofstadter.[44] Discussions with other Stanford physicists and engineers who shared Panofsky's goal led to a series of evening meetings held at Panofsky's home, and detailed plans for the facility that would become SLAC began to emerge.[45]

The origins of electron linear accelerators at Stanford dated back to the work of Assistant Professor William Hansen, a member of the physics faculty who invented the rhumbatron in 1936 to make high energy possible at low power. Following the war, Hansen built a short linear accelerator of 6 MeV called the Mark I accelerator. With the hope of surpassing the experimental limitations of existing accelerators, Panofsky and his colleagues Edward Ginzton, Robert Hofstadter, and Leonard Schiff drafted a formal proposal to the Department of Defense, the National Science Foundation, and Atomic Energy Commission (AEC) in 1954.[46] The particle accelerators of the 1950s at BNL and LBL operated by accelerating particles into fixed targets, a process in which some of the energy for particle formation was lost. The advantage of a colliding beam accelerator was that little energy was lost when particles accelerated in opposite directions collided, and the Atomic Energy

[43] Peter Galison, Bruce Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and the Growth of Physics Research, 1935-1962," *Big Science: The Growth of Large-Scale Research,* 62-63.
[44] Personal communication from Pief Panofsky, 27 Jul 2006.
[45] David Perlman, "At 40, accelerator center full of energy: physicists pursued huge lab despite huge obstacles," *San Francisco Chronicle*, 30 Sep 2002.
[46] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.

Commission became convinced of the great experimental benefits of such a machine.[47]

Originally, the multibillion-electron-volt linear accelerator was known as project M for monster, since the two-mile long accelerator was the largest and most expensive high-energy physics machine ever proposed. Helge Kragh observes that the proposal would most likely have been rejected had not the humiliating display of Sputnik in 1957 threatened the image of the United States as a leading scientific power. For that reason, Eisenhower was motivated to endorse Project M as an impressive show of national scientific strength, and he requested $100 million from Congress to fund the construction of the new accelerator.[48]

The AEC was concerned that the new accelerator would dominate the physics department at Stanford and declared that funding was contingent upon establishing the accelerator as a "national laboratory."[49] The designation of national laboratory was meant to ensure that physicists from other universities and institutions would have access to the accelerator through the authority of a committee of outside physicists.[50] The national laboratory title was of great concern to the greater high-energy physics community due to the perception that the policy of accelerator use at LBL and BNL

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[47] See Elizabeth Paris, "Lords of the Ring," 355-380.
[48] Helge Kragh, *Quantum Generations: A History of Physics in the Twentieth Century* (Princeton: Princeton University Press, 1999), 304.
[49] For a discussion of the development and purpose of national laboratories in the United States, see Alvin M. Weinberg, *Reflections on Big Science* (Cambridge: The M.I.T. Press, 1967), 126-144.
[50] W.K.H. Panofsky, "SLAC and Big Science," in *Big Science*, 135-136.

unfairly excluded visiting scientists.[51] In fact, the movement for "a truly national facility" in the 1950s and 1960s became known by the acronym TNL, meant to reference BNL.[52] Ginzton, Panofsky, Hofstadter, and Schiff, however, had serious reservations about establishing the organizational structure of the accelerator as a national laboratory. Panofsky was opposed to the national laboratory status because he wanted to avoid the limitations imposed by the AEC-regulated restrictions he had witnessed at Berkeley.[53] Instead of a national laboratory designation, Panofsky proposed that the new accelerator should be a "national facility." According to Panofsky, a national facility was "not quite a national laboratory," since Stanford remained in control of managing the facility and had its own faculty.[54] The Stanford high-energy physicists would have preferred to run the accelerator as a 'university-style' laboratory, affording them privileged and unrestricted beam time. The project could not proceed without the AEC's funding, however, so the physicists were forced to compromise. After a prolonged battle, the AEC granted a budget that was increased to $114 million by 1961, and the new particle accelerator was eventually given the less contentious name SLAC: the Stanford Linear Accelerator Center.[55]

Although SLAC was originally conceived by Hofstadter and Ginzton, it was Panofsky who assumed administrative control once the massive accelerator project was initiated. Describing how he became the first director of SLAC, Panofsky recalls:

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[51] Personal communication from Pief Panofsky, 27 Jul 2006; Panofsky recalled that the pressure to establish SLAC as a national laboratory was generated by the scientific community, particularly Leon Lederman.
[52] Laurie M. Brown, et. al., *Pions to Quarks,* 17.
[53] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[54] W.K.H. Panofsky, "SLAC and Big Science," in *Big Science*, 135-136.
[55] Dominique Pestre, "The Decision-Making Processes," in *Big Science,*168.

> Ginzton and I were the leaders, but he was a microwave engineer, not a particle physicist, and he was torn between the industrial world in Silicon Valley and the academic world here at SLAC, and so I was then left as director of SLAC. There is no record of my appointment as director because the bureaucracy at that time was "highly messed up."[56]

Early in the planning stage of SLAC, Panofsky had the benefit of being intimately acquainted with LBL's history as well as its inner working environment, and he used this knowledge to great advantage. Panofsky's choices in designing SLAC and his development of a personal directorial style were directly based on his perceptions of LBL's failures. In order to avoid what he considered to be the repressive elements of LBL's administrative and scientific operations, Panofsky consciously chose to diverge from many of the standard practices common to establishing and managing high-energy physics laboratories. Instead, Panofsky modeled SLAC after his vision of a laboratory characterized by great scientific freedom and a relaxed working environment.

Well before the linear accelerator was completed in 1967, Panofsky decided to recruit researchers to SLAC in order to develop strong research programs that would complement the new technology built into the accelerator. Panofsky recalled, "I knew that SLAC had to be quite different from other labs, particularly Berkeley, for technical reasons rather than policy reasons."[57] In other labs, such as LBL and BNL, usually the machine was built first, then experiments were devised to fit the machines. Panofsky wanted to try a novel strategy, explaining:

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[56] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[57] Ibid.

> From the very beginning of SLAC, we were setting up very strong experimental teams… I took the strong position that the build-up of experiments and talent had to parallel the building of machines, vs. Berkeley, where Lawrence would build the machines and then decide what do to with them. Before the first shovelful of dirt, I knew we needed a strong group of physicists.[58]

Panofsky takes credit for the original approach of assembling teams as the equipment was built, but noted it was the physicists he chose as group leaders who designed the initial research orientations at SLAC. Panofsky gave his group leaders the freedom to decide which research fields would be most suitable for the laboratory, enabling them to play an active role in making decisions related to SLAC's early technical needs.

Discussing how group leaders were chosen, Panofsky noted, "the thing which is quite remarkable in retrospect: there was really very little systematic procedure, and also in some respect very little democracy in the fact that there weren't any rules as to who had to approve what appointment."[59] Panofsky explained, "we were also basically tailoring our structure to the technological necessities of getting work done. It was a mixture of building the right machines at the right time but also recognizing that the kind of approach used at Berkeley and at other labs simply wouldn't work here."[60] The kind of approach Panofsky wanted to avoid was the short-term strategy of assembling a group of scientists for the purpose of working together on a specific experiment for a limited period of time. Instead, Panofsky grouped together physicists who were interested in pursuing the same broad category of research problems. Joseph Ballam,

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[58] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[59] W.K.H. Panofsky, AIP Oral History Transcript, 02 May 1997. Interviewed by Harvey Lynch at Stanford Linear Accelerator, SLAC Archives and History Office, 26.
[60] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.

one of the original group leaders, attested to the distinctiveness of Panofsky's approach to establishing research groups at SLAC, observing that group structure at SLAC

> ...was different than many other laboratories in many respects. In other laboratories, groups were formed around an experiment. When the experiment was over, the groups would sort of break apart. And pieces of that group would form another group...it certainly happened that way a lot at Brookhaven and perhaps at Berkeley.[61]

By contrast, SLAC groups were created according to research interests, not experiments, and the groups endured over a long period of time. Moreover, SLAC research groups were fairly small, composed of a few physicists and support staff who worked closely with one another over the years. From Ballam's point of view, the strong unity of groups at SLAC was a major advantage since he believed, "it's very important to have a few technicians and one or two engineers associated with the group who over a long term; they establish a good rapport with the physicists..."[62] Similarly, Perl observed, "it was a support structure that built morale, and it was more fluid than it appears on paper. Groups' responsibilities were intermixed. There was a lot of communication between groups."[63]

Panofsky's philosophy as director was to resolve conflicts by permitting individuals to "blow off steam," a management style based upon his desire to be a

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[61] Joseph Ballam, Oral History Transcript, 1987. Interviewed by Natalie Roe and Bill Kirk at Stanford Linear Accelerator. Date: Session Two: 13 Nov 1987.
[62] Ibid.
[63] Personal communication from Martin Perl, SLAC, 28 Mar 2006.

director that facilitated an atmosphere of open communication at SLAC.[64] Panofsky also believed in permitting his physicists to carry out research programs of their own choosing. His view that physicists should have hands-on involvement with the design and maintenance of experimental apparatus, while maintaining a high degree of scientific autonomy, was evident in a 1961 letter to Herbert DeStaebler. Describing the five research groups that would be established in anticipation of the completion of the accelerator, Panofsky mentioned that

> at present the Research Division in general and individual members of the research groups have continuing responsibilities for the Beam Switchyard and some phases of accelerator engineering...the Laboratory will retain some unallocated research funds which may be used for programs initiated by individual members of the research staff but not sponsored by the groups.[65]

Referring to Panofsky's administrative style, Perl confirmed that as long as one stayed "within the bounds of the resources, and if the things you were proposing to do were not dumb, he let you do them."[66] As associate director of the department, Ballam described Panofsky's management of research programs in much the same way, recalling in a 1987 interview:

> first, there was a bit of money around so there was always the feeling that if you thought of something that was halfway intelligent and reasonable and that if you had a chance to carry it out, then that makes a lot of difference when you're trying to do something for a lab...you feel like you've accomplished something, that you carry through some general ideas and have them

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[64] W.K.H. Panofsky, OHI, 02 May 1997.
[65] W.K.H. Panofsky, Letter to Dr. Herbert DeStaebler, 15 Feb 1964, Series IV, Stanford Linear Accelerator Center, Subseries C, Box 16, folder 5, Personnel Planning and Recruitment for Research Division, 1961-64.
[66] Personal communication from Martin Perl, SLAC, 28 Mar 2006.

> result in a situation in which good physicists can do work.[67]

Panofsky also viewed himself as a director that had the ability to be actively involved in every stage of administrative and scientific practice, explaining,

> As a director, you have to be double-hatted—this was a change from Berkeley, where Lawrence was very authoritarian, and I specifically didn't want to do that. I participated in all levels with a clear understanding and without always having to play the director—I had the ability to operate as a technician."[68]

Panofsky has emphasized that SLAC physicists functioned as scientists first, while administration was regarded as a necessary but secondary responsibility. Clearly, Panofsky deliberately chose to make a distinction between SLAC and LBL with respect to leadership style and the focus of the laboratory itself.

In fact, one of the most significant organizational strategies implemented by Panofsky was his commitment to making SLAC a "single-function laboratory" rather than a multi-purpose, multidisciplinary laboratory like LBL. The choice to confine the research at SLAC to subfields of high-energy particle physics was a practical one, since Panofsky was also determined to remain a "double-hatted" director with a firm grasp of the technical skills required to understand all facets of research at his laboratory. Panofsky justified his somewhat restrictive approach to orienting research at SLAC by explaining:

> Being a single-function laboratory at SLAC made it easier to balance things. Berkeley always had many different disciplines, and clearly, a director can't be on

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67 Joseph Ballam, OHI, 05 Nov 1987.
68 Personal communication from Pief Panofsky, SLAC, 29 Mar 06.

> top of all of that. Therefore, the director becomes unavoidably more like an 'industrial chief,' and we struggled very hard against that here.[69]

As we have seen, throughout the planning stage and the early years of SLAC, Panofsky attempted to distance SLAC from LBL, but the same problems encountered by LBL over its much longer history could not always be avoided. In particular, Panofsky did not anticipate an antagonistic relationship to develop between SLAC and the physics department at Stanford similar to the uncomfortable tension between the physics department at Berkeley and the LBL. However, as it turned out, Stanford physicists had reservations about budgetary allocations shifting disproportionately to high-energy physics early in the planning stages of SLAC. Describing the relationship between the Stanford physics department and SLAC, associate director Joseph Ballam noted that the physics department

> ...did not want to be dominated by a bunch of people at SLAC and they always used the analogy of the Berkeley laboratory dominating the physics department at Berkeley...and I think the Berkeley analogy was not so bad because in the 50s and early 60s, I believe that the development of the Berkeley physics department was somewhat hampered by the dominance of the Berkeley lab...so they tried to maintain this dichotomy of structure between SLAC and the physics department.[70]

SLAC generated its first beam in 1966, nearly a decade after the proposal was first submitted.[71] By the 1970s, SLAC had evolved into a well-established high-energy physics laboratory that was independent from, yet connected to, the scientific

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[69] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[70] Joseph Ballam, OHI, 13 Nov 1987, 11.
[71] W.K.H. Panofsky, "SLAC and Big Science," in *Big Science*, 133.

community of LBL, its neighbor across the bay. The high-energy physics laboratories of LBL and SLAC were two distinct scientific communities, but were they communities characterized by a sharp divide between image and logic traditions, as Galison has suggested? If this model does not effectively describe the main differences between the two laboratories, what criteria can be used to distinguish the scientific communities of SLAC and LBL? Because it is important to determine how these communities functioned independently in order to understand the collaboration that led to the discovery of the tau lepton, I will compare SLAC and LBL in the following section using these questions as a framework.

Section 1.5.5. Analyzing the Image/Logic Divide

Galison’s depiction of the image and logic traditions followed by LBL and SLAC does provide an accurate description of the dominant research trends in each laboratory—LBL was involved predominantly in bubble chamber work and SLAC was concerned with electronic detectors. Yet several of his claims warrant further examination. First, Galison argues that image and logic stood in constant tension due to the experimenters’ *conscious aversion* to the opposing tradition. According to Galison, pedagogical, technical, and epistemic factors played a deciding role in determining a physicist's loyalty to a particular tradition. In other words, logic physicists were pitted against image physicists in a dogmatic debate. The supposedly wide gulf between image and logic at LBL and SLAC, however, was not as well-defined as Galison’s depiction implies.

According to Galison, image and logic traditions only merged in the construction of the Mark I detector at SLAC in 1971 when bubble chamber physicists from Berkeley collaborated with SLAC physicists to construct a 'hybrid' apparatus that bridged the image/logic divide.[72] In fact, image and logic technologies were not mutually exclusive even before the construction of the Mark I. Alvarez's determination to prove the superiority of bubble chambers does seem to support Galison's representation of physicists' steadfast commitment to a particular tradition, but Alvarez also recognized the necessity of using electronic technology in order to facilitate the scanning of photographic data. Thus, electronic data-analysis systems were used in conjunction with bubble chamber images. While Galison provides a well-documented discussion of the competing computer programs used for data analysis, he does not emphasize that the use of these electronic systems blurred the line between image and logic.

Indeed, physicists did not restrict themselves to an image/logic boundary in either laboratory. In fact, when the 82-inch bubble chamber was transferred to SLAC from LBL, SLAC became "the world's most prolific producer of bubble-chamber film for a wide outside community."[73] As Panofsky has pointed out:

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[72] See Galison, *Image and Logic,* Chapter 3. Treitel accepts Galison's contention that the first fusion of image and logic traditions occurred during the Mark I collaboration; see Treitel, "A Structural Analysis of the History of Science," Chapter 1; also see the following chapter in this thesis for my discussion of the construction of the Mark I detector.
[73] W.K.H. Panofsky, "SLAC and Big Science," in *Big Science,* 137. For a discussion of the sentimental value of the 82-inch bubble chamber at SLAC, see Sharon Traweek, Sharon, *Beamtimes and Lifetimes: The World of High Energy Physicists* (Cambridge: Harvard University Press, 1988), 53-56.

> We followed the pictorial tradition on a few things and at Berkeley, it wasn't that simple, either. The antiproton work they did there was done in the electronic tradition—in both labs, both traditions were practiced, although it's true that at SLAC, the pictorial tradition was relatively short-lived. The image and logic idea is a real oversimplification of a very complicated situation because these things tend to be driven by the opportunities. People are flexible enough to adjust to the technology, and the work was a function of the technology. Each lab followed the best strategy for getting the information out.[74]

For Panofsky, then, the image/logic division described by Galison does not accurately reflect the true motivations for incorporating a particular technology into experimental practice because the choice to use image or logic technology was based on *pragmatic* criteria. In a review of *Image and Logic*, Panofsky complains, "Galison does not mention the hybrid bubble chamber in which an electronic adjunct decides when a picture shall be taken or not."[75] In the same review, Panofsky also faults Galison for "drawing broad conclusions from narrow case histories."[76]

Panofsky is not the only SLAC physicist to take issue with Galison's interpretation. When asked if he accepted Galison's image/logic distinction, Perl replied,

> I don't agree—we *did* scan bubble chamber pictures here, we had spark chamber photos and as soon as we could with the collider (which was all an electronic detector), we got the software to produce pictures.

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[74] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[75] W.K.H. Panofsky, Review, "Image and Logic: A Material Culture of Microphysics," *Physics Today* (1997), 65.
[76] Ibid.

> Pictures were *very* important; you still had to look at them to see what was going on.[77]

Furthermore, Perl has noted that while most of the groups at SLAC were focused in one particular area of high-energy physics, physicists "still did bubble chamber experiments at Berkeley in Group E."[78]

It should now be evident that neither laboratory completely fit the model outlined by Galison, since image and logic traditions were intermingled at each laboratory. In addition, most physicists were not bound to a particular tradition by idealistic loyalty and would not have identified themselves as strictly adhering to either the image or logic tradition. I argue that the most important distinguishing feature between LBL and SLAC was not a division between image and logic, then, but the fundamental difference in the organization and size of research groups as well as the administrative philosophy of the directors. Both laboratories practiced Big Science, but the differences in the management style and the size of research groups at SLAC and LBL created dramatically dissimilar working environments for physicists at each laboratory.

Section 1.5.6 Big Science at LBL and SLAC

It is commonly asserted that Big Science practices radically altered the life of the experimenter, and this claim must be reevaluated in the context of the scientific

[77] Personal communication from Martin Perl, SLAC, 28 Mar 2006.
[78] Ibid.

communities of LBL and SLAC.[79] Galison claims that physicists were greatly concerned about losing control of their research due to Big Science developments that changed the laboratory environment, but this claim is only partially supported by the LBL/SLAC analysis. It appears that physicists at Berkeley were indeed disturbed by the increasing scale of particle physics research and the accompanying loss of scientific freedom. Galison notes that Alvarez eventually abandoned the field of particle physics in 1967 because he felt it had become too restrictive. Alvarez then turned his attention to cosmic ray research, a field that enabled him to engage in "small science" work.

Alvarez was not the only physicist to become disenchanted with the hierarchically ordered, teamwork-oriented nature of research at LBL promoted by the advent of Big Science. Glaser, the Nobel Prize-winning inventor of the bubble chamber, joined LBL as a group leader in 1960. Only a few years later, Glaser decided to leave the field of particle physics upon discovering that his desire for creative opportunity and scientific freedom could be better fulfilled by research in microbiology.[80] Galison's contention that physicists were alarmed about losing scientific autonomy certainly applies to LBL, but did SLAC physicists experience similar anxiety? From the testimonials of Ballam and Perl, it appears that on the

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[79] The theme that Big Science caused a shift in the role of the physicist leading to a disassociation with hands-on experimental research and the decline of individual research has been emphasized by several historians. See Peter Galison, Bruce Hevly, and Rebecca Lowen, "Controlling the Monster: Stanford and the Growth of Physics Research, 1935-1962" in *Big Science,* 46-77; Helge Kragh in *Quantum Generations*, 307-308; Paolo Brenni, "Physics Instruments in the Twentieth Century," *Science in the Twentieth Century*, 754-755.
[80] Galison, *Image and Logic*, 420.

contrary, Panofksy's managerial style afforded an atmosphere of great scientific freedom at SLAC.

Galison also refers to the "fragmentation of the experiment/experimenter" that resulted from Big Science practices.[81] He explains that a "painful mutation in practice" occurred in the postwar period in which the physicist became completely dependent upon large collaborations.[82] Unless physicists participated in such cooperative ventures, they were barred from any direct interface with experimental apparatus, and Galison further claims that "the removal of the physicist from the apparatus, the specialization of tasks, the increased role of computation, and the establishment of hierarchical collaborations have become hallmarks of high energy physics experiments."[83]

This characterization of the changes in the life of the experimenter due to the advent of Big Science is a fitting description of the scientific workplace at LBL, but Galison's analysis does not fully accommodate the unusual research environment at SLAC. SLAC was undoubtedly a Big Science laboratory, yet SLAC physicists did not immediately experience the alienation from hands-on experimental work and the loss of control over research that characterized Big Science research at LBL and other high-energy physics laboratories during this period. Throughout the 1970s, SLAC managed to practice Big Science while retaining elements of the "small science" dynamic common to earlier generations.

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[81] Galison, *Image and Logic*, 307.
[82] Ibid, 318.
[83] Ibid.

Small science is distinguished from Big Science by the organization and practice of experimental work. Summarized by Andrew Pickering:

> small science was the traditional work style of experimental physics—an individualistic form of practice, requiring only a low level of funding obtainable from local sources and little in the way of collaboration, and promising quick returns on personal initiatives.[84]

SLAC occupied a unique position in the world of high-energy particle physics, since Big Science was practiced with the rewards of small science. Unlike their LBL counterparts, SLAC physicists enjoyed a significant degree of scientific freedom in controlling their research, and maintained an intimate relationship with experimental apparatus because they were granted the authority to design and construct new machinery.[85]

To summarize, LBL and SLAC were high-energy physics laboratories marked by different styles of organization and scientific practice. At LBL, Lawrence and Alvarez built a hierarchically arranged, bureaucratized laboratory in which physicists belonged to large interdisciplinary research groups and often felt isolated from their experimental work. Therefore, LBL embodied the new standards of Big Science research. By contrast, SLAC was organized by Panofsky as a semi-democratic facility in which physicists could directly assume control of their research while working in small groups. In Chapter 2, I will provide further insight into the cultural dynamic of scientific practice at these two communities by analyzing the working relationship

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[84] Pickering, *Mangle of Practice,* 43.
[85] This theme will be further explored in the following chapter through an analysis of how Perl and his colleagues at SLAC and LBL directly contributed to the design, construction, and testing of the Mark I detector.

between the two laboratories and addressing the role of competition in the collaboration that led to the discovery of the tau lepton.

Chapter Two: The Sociology of the Laboratory: Collaboration and Competition between SLAC and LBL

Section 2.1 Overview

This chapter is concerned with describing the nature of collaboration and competition between LBL and SLAC leading to the discovery of the tau lepton using the Mark I detector at SLAC. In this discussion, I return to my argument that the spirit of small science was preserved in Big Science practices at SLAC during the 1970s in the SLAC-LBL collaboration. Specifically, I discuss how the scientists' involvement in the proposal and construction of the Mark I as well as the subsequent collaboration on the Mark I experiments demonstrate that both scientific freedom and a close interaction between the scientist and the experimental apparatus were maintained at SLAC during this period. In order to effectively analyze the dynamic of the working relationship between these two scientific communities, it is useful to first identify the lead scientists who participated in the collaboration. In this chapter, therefore, I will first introduce Martin Perl's personal and professional history prior to joining SLAC. I will then turn my attention to the SLAC-LBL collaboration and the key historical actors involved in the discovery of the tau lepton at both laboratories.

The SLAC-LBL collaboration has not been analyzed by many historians, and there are relatively few letters documenting the personal and professional relationship between Perl and members of the collaboration in open archives.[1] Because of the

[1] Perl has not yet made his personal and professional correspondence available to the SLAC Archives and History Office or any other archive. There is also a paucity of

dearth of contemporaneous documents, I have drawn heavily from Perl's own recollections as reported in oral history interviews, personal communications, and his later Nobel Laureate address to reconstruct the sociological and scientific details of the collaboration. Relying on oral history can be analytically problematic for the historian, since such accounts reflect scientists' biased interpretations of past behavior and events. A broader range of sources from this period would have facilitated the task of analyzing the critical stages of Perl's life and career, but assessing this history from

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correspondence between Perl and members of the collaboration at the Lawrence Berkeley Archives and Records Office, since the majority of the collection is composed of laboratory notebooks from this period. The most directly relevant material is found in a laboratory notebook belonging to Gerson Goldhaber of LBL in a section labeled "Perl's Pearls," which discusses Perl's electron-muon events (SPEAR MARK I Controlled Notebooks, PEP 1975, folder 27, Ernest Orlando Lawrence Berkeley National Laboratory Records Transmittal Division: Physics, Department: Division Office, Filing Code ARO-3528). For further reading on the SLAC-LBL collaboration, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), 517-538; Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model: Particle Physics in the 1960s and Beyond*, ed. Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden (New York: Cambridge University Press, 1997), 308-337; Andrew Pickering, *Constructing Quarks* (Chicago: University of Chicago Press, 1984), 253-282; Michael Riordan, *The Hunting of the Quark: A True Story of Modern Physics* (New York: Simon & Schuster, 1987), 257-321. See also the *AIP Study of Multi-Institutional Collaboration, Phase 1: High-Energy Physics, Report No. 4: Historical Findings on Collaborations in High-Energy Physics* (New York: American Institute of Physics, 1992). The AIP Center for the History of Physics conducted a ten-year study of large scientific collaborations, including collaborative research in high-energy particle physics. As part of the larger study, AIP project members examined the SLAC-LBL collaboration on the Mark I detector. The majority of this study is concerned with assessing the status of preserved archival material relating to collaborative research in physics and with providing recommendations for future preservation. AIP project members also examined changes in collaborations over time by conducting interviews with scientists about the process of collaboration, specifically focusing on experiments between 1973 and 1974, which included the J/psi and upsilon discoveries and the CLEO collaboration at the CESR facility.

Perl's perspective provides valuable insight.[2] By treating Perl's recollections as an interpretive tool of analysis, this chapter shows that Perl's formative experiences significantly shaped his scientific style and describes how Perl perceived the working environment at SLAC during the collaboration.

Section 2.2 Biographical, Educational, and Experimental Background of Martin Perl

Martin Perl's philosophy as an experimental physicist is best summarized in his own words:

> I like the feeling of being part of a small band of explorers starting out on a very uncertain search, the feeling of being free to set our own pace and make our own mistakes…this is not rationality; it is personality and personal scientific taste…I know that many researchers share my taste for small and lonely bands of scientific explorers. After all, the popular image of the scientist is Pasteur proposing his germ theory of disease while his colleagues laughed at him, the popular image of the scientist is Einstein working in the Patent Office. These images sent many of us into science.[3]

Despite a career characterized by participation in large collaborations at a Big Science institution, Perl nostalgically sees himself as a member of a small group of individuals

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[2] As the oral historian Alessandro Portelli has observed, oral histories can yield significant revelations when examined critically for the meaning of subjectivity in historical memory. See Portelli, *The Death of Luigi Trastulli and Other Stories: Form and Meaning in Oral History* (Albany: State University of New York Press, 1991). For a discussion of analyzing oral interviews in the history of science, see David H. DeVorkin, "Interviewing Physicists and Astronomers: Methods of Oral History," in John Roche, ed., *Physicists Look Back: Studies in the History of Physics* (London: Adam Hilger, 1990), 44-65.

[3] Martin L. Perl, "Reflections on Experimental Science," in *Reflections on Experimental Science*, World Scientific Series in 20th Century Physics, Vol. 14 (River Edge, NJ: World Scientific, 1996), 528-529.

who practice science in the style of a bygone era. Perl distinguishes himself from the majority of his profession through this romantic vision of his life as an experimental physicist, and consequently, his career reflects the investigation of research problems according to his own "personal scientific taste." Perl's somewhat maverick approach to experimental science is not surprising when viewed in the context of his undergraduate and graduate training as well as his post-doctoral work as a young researcher. Because of these experiences, Perl's scientific style was firmly established prior to joining SLAC in 1963. In fact, Perl's attitude towards scientific practice was shaped at an early age, and thus his actions can best be understood through a consideration of his formative childhood experiences fostered by his Jewish upbringing and the pedagogical influences on his scientific style as an experimental physicist.

Perl was born on 24 June 1927 to parents who immigrated to the United States from the Polish area of Russia in 1900 and eventually settled in Brooklyn, New York. He grew up in a Jewish middle class neighborhood after his father founded a successful printing and advertising company that enabled his family to escape the widespread poverty of the Great Depression.

Perl received a high-quality education in the Brooklyn school system, and he believes the attitude of his parents towards school was particularly instrumental in his early preparation for work as an experimental scientist. Perl's parents thought very highly of the teaching profession and expected him to do well academically, but they never visited his school or met with teachers. Because he was essentially on his own as a student, Perl recalled, "I learned early to deal with an outside and sometimes hard world. Good training for research work!"[4] Perl saw another correlation between his parents' high expectations of academic success and the development of his scientific style, noting

> Whatever the course, whether the course was boring or interesting to me, whether I was talented in mathematics or not talented in languages, my parents expected A's. This was good training for research, because large parts of experimental work are sometimes boring or involve the use of skills in which one is not particularly gifted.[5]

After graduating from high school at the age of sixteen, Perl enrolled in the Polytechnic Institute of Brooklyn and studied chemical engineering, a promising field in the late 1930's and early 1940's. Although Perl won the physics medal upon graduating from high school, read avidly in physics and mathematics texts, and described himself as "very mechanical as a child," pursuing a career in physics was not an option for him at the time.[6] Explaining his decision to major in chemical engineering rather than physics, Perl commented

> I never thought of becoming a scientist. That was because as the children of immigrants, my sister and I were taught that we must use our education to "earn a good living." In fact, we didn't have to be taught that. It was obvious to us...a good living in the Jewish middle class meant that a girl should become a teacher or nurse;

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[4] Martin Perl, *Les Prix Nobel: The Nobel Prizes 1995*, Editor Tore Frängsmyr, Nobel Foundation, Stockholm, 1996, available at http://nobelprize.org/nobel_prizes/physics/laureates/1995/perl-autobio.html, last accessed 22 Jun 2006.

[5] Ibid.

[6] Martin Perl, Oral History Interview, 1988. Interviewed by Natalie Roe and Bill Kirk at Stanford Linear Accelerator. Session One, 02 Feb 1988, SLAC Archives and History Office, 1.

> a boy should become a doctor, dentist, lawyer, or accountant.[7]

Perl also observed that before World War II, "physics was sort of obscure, and that was the big day of chemistry. Better things for better living through chemistry...chemical engineering seemed like the right compromise."[8]
Perl studied chemical engineering at the Polytechnic Institute until the advent of World War II, which prompted him to join the United States Merchant Marine. Leaving school, Perl served as an engineering cadet at the Kings Point Merchant Marine Academy in New York during the war.[9] He left the Merchant Marine when the war ended, but was drafted into the United States Army immediately, and served in the Army for a year before returning to college and receiving a summa cum laude bachelor's degree in Chemical Engineering in 1948.[10]

While Perl has acknowledged that the laboratory techniques and mathematics knowledge acquired at the Polytechnic Institute were critical to his later experimental work, the pivotal moment in his academic career occurred after his graduation. Perl immediately joined General Electric as chemical engineer, where he was employed to troubleshoot production problems in television vacuum tubes. He soon found that he needed to learn more about vacuum tubes in order to perform his job duties, so he began taking courses in atomic physics and advanced calculus at Union College in Schenectady, New York. According to Perl, General Electric's on the job training

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[7] Martin Perl, *Les Prix Nobel: The Nobel Prizes 1995*.
[8] Perl, OHI, 02 Feb 1988, 2; Personal communication from Martin Perl, 30 Jul 2006. For Perl, the DuPont Company's 1930s slogan "Better Things for Better Living Through Chemistry" symbolized the superior power of chemistry compared to physics.
[9] Personal communication from Martin Perl, 30 Jul 2006.
[10] Perl, OHI, 02 Feb 1988, 2.

"was quite famous" at that time because the engineers were trained to use more sophisticated mathematics.[11] It was at Union College that Perl found his true calling as a scientist, for he recalled, "I got to know a wonderful physics professor, Vladimir Rojansky. One day he said to me 'Martin, what you are interested in is called physics, not chemistry!' At the age of 23, I finally decided to begin the study of physics."[12]

Following this interaction, Perl decided to go back to school, and he entered the Ph.D program in physics at Columbia University in the fall of 1950. Despite having taken only two elementary courses in physics as an undergraduate, Perl recalled, "I had very good marks...I had no trouble getting into Columbia."[13] He soon realized that his lack of physics training compared to his fellow graduate students placed him at an extreme disadvantage, and he considered abandoning the program. However, the expectations of his parents once again influenced his academic performance. As Perl recalled, "I had explained the return to school to my astonished parents by telling them that physics was what Einstein did. They thought if Einstein, why not Martin; I could not quit."[14]

Perl's doctoral research at Columbia under the supervision of his advisor, Isidor Rabi, played perhaps the most crucial role in developing his scientific philosophy as an experimentalist.[15] Perl's doctoral work focused on using the atomic

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[11] Perl, OHI, 02 Feb 1988, 2.
[12] Martin Perl, *Les Prix Nobel: The Nobel Prizes 1995.*
[13] Perl, OHI, 02 Feb 1988, 2.
[14] Martin Perl, *Les Prix Nobel: The Nobel Prizes 1995.*
[15] For a detailed account of Rabi's career and influence, see John Rigden, *Rabi: Scientist and Citizen* (New York: Basic Books, Inc., 1987); Daniel J. Kevles, *The Physicists: the History of a Scientific Community in Modern America* (New York: Knopf, 1977); for an overview of Rabi's views on the relationship between science

beam resonance method to measure the quadrupole moment of the sodium nucleus, which involved a measurement in an excited atomic state. Rabi was an authority on this type of work, since he had pioneered the atomic beam resonance method, for which he received a Nobel Prize in 1944. According to Perl, however, Rabi "...didn't do experiments himself."[16] Since Rabi never used tools or directly worked with the apparatus, Perl was forced to learn experimental techniques from older graduate students or from Rabi's colleague, Polykarp Kusch. For Perl, asking Kusch for assistance "…was always an unpleasant experience. He had a loud voice which he deliberately made louder so that the entire floor of students could hear about the stupid question asked by a graduate student."[17]

Consequently, Perl became a largely self-taught experimentalist, which forced him to learn quickly because he had to find his own answers when measurements were inaccurate or equipment failed. He found that his engineering background served him well in visualizing how to solve the problems he encountered during the course of his dissertation research, explaining

> I developed much of my style in experimental science in the course of this thesis experiment. When designing the experiment and when thinking about the physics, the mechanical view is always dominant in my mind. More important, my thinking about elementary particles is physical and mechanical.[18]

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and politics, religion, and education, see Michael A. Day, "I.I. Rabi: The Two Cultures and the Universal Culture of Science," in *Physics in Perspective*, vol. 6, ed. John S. Rigden and Roger H. Stuewer (Boston: Berkhäuser Verlag, 2004), 428-476.

[16] Perl, OHI, 02 Feb 1988, 3.

[17] Ibid.

[18] Perl, "Reflections on the Discovery of the Tau Lepton," Nobel Lecture, 08 Dec 1995, 1.

Building upon his earlier childhood experiences and educational background, Perl cultivated his personal scientific style through his doctoral research, but his scientific philosophy was also shaped by Rabi's influence. Nearing the end of his studies at Columbia, Perl's patience was tested by Rabi's unwavering conviction that it was critical to get the right answer and check it completely. When Perl finished his doctoral work on the measurement of the quadrupole moment, he remembers being "…eager to publish and to get on with earning a living."[19] Rabi, however, was insistent upon taking the time to confirm reports of similar results by a French laboratory before he would permit Perl to publish the results of his work. After waiting six to eight weeks, Rabi and Perl received the anticipated confirmation by mail, and Rabi finally allowed Perl to publish his findings. For Perl, the message was clear: "It is far better to be delayed, it is better to be second in publishing a result, than to publish first with the wrong answer."[20] The frustration Perl experienced over waiting so long to publish his results seems to have played an instrumental role in shaping his ideas about scientific discovery and priority, since he ignored this strategy years later when he chose to prematurely publish his results from the tau lepton experiments.

Although he may have exercised less caution than Rabi would have advocated in his rush to publicize the tau lepton results in 1975, Perl fully embraced most of the advice Rabi offered to him. Significantly, Perl learned that it was important to choose his own research problems. Perl believes the best thing he learned from Rabi was not to do things that other people were already doing because Rabi stressed the importance

[19] Martin Perl, *Les Prix Nobel: The Nobel Prizes 1995.*
[20] Ibid.

of working on a fundamental problem in an "uncrowded" area of physics. This advice effectively summarizes the search for heavy leptons Perl would undertake years later at SLAC.[21] Rabi advised Perl to work in particle physics rather than atomic physics, and after earning his Ph.D in 1955 and receiving multiple job offers, Perl clearly based his employment decision on Rabi's suggestion. In this way, Rabi directly influenced Perl's career path and his eventual experimental success.

After finishing his dissertation at Columbia, Perl had offers of employment from Yale, the University of Illinois, and the University of Michigan, where Donald Glaser had recently invented the bubble chamber. For high-energy physics, Perl needed to go to Michigan, Yale, or Illinois.[22] Turning down offers from Yale and the University of Illinois—institutions with superior reputations in elementary particle physics at the time—Perl went to the University of Michigan instead. Explaining this choice, Perl admits

> I followed a two-part theorem that I always pass on to my graduate students and post-doctoral research associates. Part 1: Don't choose the most powerful

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[21] Perl, OHI, 02 Feb 1988, 4.

[22] Ibid. For a brief overview of the research interests in high-energy physics at the University of Michigan during the 1950s, see Donald W. Kerst, "Accelerators and the Midwestern Universities Research Association in the 1950s," in *Pions to Quarks: Particle Physics in the 1950s* (New York: Cambridge University Press, 1989), 201-212. Just prior to Perl's arrival at the University of Michigan, the Midwestern Universities Research Association (MURA) was established to construct a large accelerator in the midwest. By 1955, when Perl began working in the physics department, a collaboration composed of members from the University of Michigan, the University of Minnesota, Iowa State University, the University of Iowa, the University of Chicago, Indiana University, the University of Wisconsin, and the University of Illinois had begun constructing the Mark IB accelerator, which operated at 500 keV. Throughout Perl's tenure at the University of Michigan, it was a hub for high-energy particle physics, attracting physicists interested in accelerator work worldwide.

> experimental group or department—choose the group or department where you will have the most freedom. Part 2: There is an advantage in working in a small or new group—then you will get the credit for what you accomplish.[23]

Perl's "two-part theorem" is strikingly reminiscent of Rabi's advice to him, and further reveals that Perl was already focused on issues of priority even as he was beginning his scientific career. He spent eight years at the University of Michigan, learning experimental techniques while engaging in new research. Initially, Perl worked in bubble chamber research with Donald Glaser, the Nobel Prize-winning inventor of the bubble chamber. According to Perl, however, the future of the bubble chamber lay in hydrogen or magnetic field work, and "Glazer [sic] didn't want to do either…so Glazer didn't want to go that way and clearly Alvarez already…the labs were moving in. You needed a lab."[24]

Recognizing that Alvarez was already beginning to dominate bubble chamber work, Perl decided to shift his experimental focus. After learning that Russian physicists had invented a device called the luminescent chamber, in which charged particle trails were made visible using a high intensity image intensifier, Perl began to do luminescent chamber work with his University of Michigan colleague Lawrence W. Jones.[25] Working on a pion scattering experiment at the Berkeley Bevatron near the end of the 1950s, however, Perl and the other members of his group "...saw the spark chambers. They had been invented and re-invented by Japanese physicists."[26]

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[23] Martin Perl, *Les Prix Nobel: The Nobel Prizes 1995.*
[24] Perl, OHI, 02 Feb 1988, 5. See Galison, Peter, *How Experiments End* (Chicago: University of Chicago Press, 1987) for a discussion of Glaser's work and practice.
[25] Personal communication from Martin Perl, 30 Jul 2006.
[26] Perl, OHI, 02 Feb 1988, 6.

The Jones-Perl research group then immediately abandoned their luminescent chamber work and began building spark chambers at Berkeley. The spark chambers were constructed by Perl and his colleagues at the University of Michigan, then loaded onto a truck and brought to Berkeley, where the equipment was set up and experimented on for about a month at a time before being dismantled.[27] Collaborators included physicists from the University of Washington, but there was no in-house Berkeley group that worked with them on the experiment. Instead, Berkeley provided the electronics.[28]

Due to Perl's gradual transition from bubble chambers to spark chambers, Galison has asserted that Perl was a rare breed of physicist who had the ability to bridge the image/logic division between LBL and SLAC.[29] As I have shown in Chapter 1, the image logic boundaries at LBL and SLAC were less distinct than Galison has acknowledged. Therefore, while Perl's focused training in bubble chambers may have set him apart from many of the other physicists at SLAC, his colleagues at both laboratories shared his ability to move between the image and logic traditions when necessary.

Perl first became affiliated with Stanford physics when he met Pief Panofsky in 1961 while on a research trip to California. Panofsky had learned of Perl's work from Joe Ballam, who worked with Panofsky at Stanford. Although Ballam and Perl had never worked together, Ballam was familiar with Perl's work because he had done

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[27] Perl, OHI, 02 Feb 1988, 12.
[28] Ibid, 7-8.
[29] Peter Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model: Particle Physics in the 1960s and 1970s,* 313.

research with bubble chambers at Michigan.[30] Perl's initial impression of Stanford was that communication among physicists was "…very limited. We knew our own Michigan people, I knew the Bevatron people, I knew people at Brookhaven, because I'd been there, I knew the bubble chamber people. Everything was just very departmental."[31] The following year, Perl returned to California after accepting a summer position as a SLAC associate professor. This time, Perl found that SLAC appealed to him because Michigan was very formal in dress and attitude, since it was an "an old-fashioned physics department," and the atmosphere at SLAC was more casual.[32]

Section 2.3 The Electron-Muon Problem and Early Heavy Lepton Searches

Following his positive experience during the summer of 1962, Perl decided to leave Michigan in 1963 to accept a full-time position as an associate professor and group leader at SLAC, which had not yet been constructed. SLAC was attractive to Perl not only because it would provide a more relaxed working environment, but because it would soon offer the kind of research opportunities he was looking for. Indeed, in the early days of SLAC, Perl was too focused on potential research opportunities in weak interaction physics to take notice of the building of the

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[30] Personal communication from Martin Perl, 30 Jul 2006. Ballam, a former faculty member of the Michigan State University Physics Department in East Lansing, Michigan, corresponded with Perl at the University of Michigan, roughly 60 miles away. Since Perl and Ballam were both involved in bubble chamber research, they communicated frequently and exchanged physics information on a regular basis.
[31] Perl, OHI, 02 Feb 1988, 9.
[32] Ibid, 10.

'monster.' When asked about his memory of the construction of the accelerator, Perl recalled, "it's hard to believe, I'm so unaware of these things. I mean I was a very hungry, very difficult person and really saw very little, except what I wanted and what was in my way. I really paid little attention to really building the machine."[33] He describes himself as independent and self-important in the 1960s, but defends his arrogance by declaring "…almost everybody was and you could be then, because experiments were much smaller."[34] Evidently, Perl was more concerned with establishing his scientific reputation than with the challenges of building the accelerator. According to Perl, his self-interest and arrogant attitude created early friction with Panofsky, who was occupied with the completion of SLAC. In an early experiment, Perl built an apparatus at Stanford that was then taken to Berkeley. Although the experiment was successful, yielding a great volume of data, Perl was annoyed by what he perceived to be unacceptable engineering mistakes. He called Panofsky to complain that the experimental apparatus was flawed, but Panofsky was in the process of overseeing the construction of SLAC and had little sympathy for Perl. According to Perl, "...Pief actually had me in his office and said either shut up or get out."[35]

Eager to find his place at SLAC, Perl began working on muons after a chance meeting with Rod Cool from Brookhaven, who was performing muon experiments there. According to Perl, Cool remarked that if he was working at SLAC, he would be doing muon experiments. Recalling Rabi's advice once again, Perl had been looking

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[33] Perl, OHI, 02 Feb 1988, 11.
[34] Ibid, 4.
[35] Ibid, 12.

for a project that he could adopt as his own work at SLAC, remarking, "I was already trying to see what to do here, that would make room for me..."[36] The relationship between the electron and the muon, both charged leptons, had two puzzles that suggested a promising new avenue of research to Perl. First of all, the properties of particle interactions are the same for the electron and muon, since both particles participate in the electromagnetic and weak interactions but not the strong interaction, yet surprisingly, the muon is 206.8 times heavier than the electron. Secondly, the muon is unstable, and its decay process was expected to produce a photon plus an electron or positron. Physicists expected such a reaction because the photon would carry away the difference between the muon mass and the electron mass. Instead, however, the muon decays to an electron by a more complicated process, which greatly puzzled physicists.

Interested in a new approach to solving this problem, Perl proposed that high-energy experiments with charged leptons "…might clarify the nature of the lepton or explain the electron-muon problem."[37] Describing why the idea to search for new leptons became so attractive, Perl responded, "well, I never liked looking at complicated things...I really liked the idea of just looking at a couple of particles coming out, and we knew that the heavy lepton signal would be a few particles coming out."[38]

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[36] Perl, OHI, 02 Feb 1988, 11.
[37] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers" in Perl, *Reflections on Experimental Science*, 4.
[38] Perl, OHI, 02 Feb 1988, 16.

Perl's experimental interest in heavy leptons was also influenced by several papers that appeared in the early 1960s. Based on the 1961 paper of Nicola Cabibbo and Raoul Gatto, "Electron-Positron Colliding Beam Experiments," Perl began to think about using an electron-positron collider to search for new charged leptons. The 1964 Stanford Positron-Electron Asymmetric Ring (SPEAR) proposal at SLAC would enable Perl to run electron-positron experiments to test his ideas, but he had to wait five years for the construction of SPEAR to be funded. In the meantime, the concept of the existence of charged leptons in addition to the electron and muon was more fully developed through the papers of Ya Zel'dovich in 1963 and E.M. Lipmanov in 1964. Perl also read a 1968 paper by K.W. Rothe and A.M. Wolsky on the mass and decay of a heavy lepton, which helped to further shape his thinking on heavy leptons.[39]

While SLAC was still under construction, Perl's group planned experiments to identify new differences between electrons and muons. Shortly after SLAC began operation in 1966, Perl's group began looking for charged leptons in earnest through photoproduction searches. One of the experimental constraints of this type of approach was that a particle would have to have a long lifetime in order to be detected.[40]

[39] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 5.
[40] The mass of the tau lepton was first estimated in 1975 in the first paper as 1.6-2 GeV/c; see Perl, et al., "Evidence for Anomalous Lepton Production in e^+e^- Annihilation," *Physical Review Letters* 35 (1975). The measurement of the tau's mass was improved in 1994 using the Beijing Spectrometer (BES) at the Beijing Electron-Positron Collider (BEPC) and was reported to be 1776.96 MeV/c^2; see Eric Soderstrom, "Final Result on the Mass of the Tau Lepton From the BES Collaboration," SLAC-R-484, *Proceedings of the 1994 SLAC Summer Institute on Particle Physics: Particle Physics, Astrophysics, and Cosmology.*

Unfortunately, the group did not recognize that the more massive particles they were looking for would have short lifetimes, and thus this experiment failed to detect any new particles.[41] After the photoproduction searches proved unsuccessful in providing any new insights into the electron-muon problem, Perl's group turned to muon-proton inelastic scattering experiments beginning in the late 1960s. Measuring the differential cross sections for inelastic scattering of muons on protons, Perl and his colleagues compared these cross-sections to corresponding electron-proton cross sections. The goal was to find a magnitude difference that could be explained as the result of a new interaction between muons and hadrons, but the differences between the cross sections were not statistically significant, so this search method had to be abandoned.[42]

In recognition of his earlier failed attempts to detect new particles, Perl has remarked that "my colleagues and I cast a wide experimental net in our studies of leptons," and fortunately, the "third cast" of the net met with great success.[43] Perl now turned his attention to electron-positron colliding beam searches for new heavy

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[41] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 7. Measurements of the tau lifetime were not possible until higher energies were achieved with the Positron-Electron Tandem Ring Accelerator (PETRA) at the Deutsches Elektronen Synchotron (DESY) in Hamburg and the Positron Electron Project (PEP) at SLAC. The average value of the tau lifetime is 2.95 to 3.04 x10 seconds; see Perl, "The Discovery of the Tau Lepton," in *The Rise of the Standard Model*, 79-100; 97.

[42] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 8.

[43] Ibid, 5.

leptons, and this time, his efforts to resolve the electron-muon problem would lead to the discovery of the tau lepton.[44]

Section 2.4 Assembling the Team: Collaboration on the Mark I

By the early 1970s, Perl's thoughts on heavy leptons had become more formalized through his development of the "sequential lepton model," which played a critical role in his thinking about leptons. Discussions with physicists Paul Tsai of SLAC and Gary Feldman of LBL helped to shape Perl's model for new leptons, but the 1965 paper by Tsai and SLAC physicist Anthony C. Hearn as well as the 1971 paper by Tsai were particularly influential in guiding Perl's approach to colliding beam searches using the lepton model. The model was essentially a search method that paired electrons, muons, and other heavy leptons with their corresponding neutrinos and assigned each pair a unique lepton number. The goal was then to run experiments that produced detected particles such as electrons, positrons, muons, etc., along with their associated neutrinos, which would carry off energy. Because electrons and muons were the easiest particles to identify, electron-muon events with missing energy would stand out, and it would be possible to search for lepton pairs by their electron-

[44] Electron-positron colliding beam searches for heavy leptons were already underway in the late 1960s at the ADONE electron-positron storage ring in Frascati Italy by two groups of experimenters, but neither group found new heavy leptons. V. Alles-Borelli and his collaborators published results in 1970, and M. Bernardini of the same group published new results in 1973. These experimenters searched for heavy leptons up to a mass between 1-1.4 GeV. The second group at ADONE was led by Shuji Orito and Marcello Conversi, and also limited the search to masses of 1 GeV. These early heavy lepton searches were unsuccessful because no heavy leptons exist in these energy ranges; see Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," in *Physics in Perspective*, 415.

muon decay modes. The model simplified the search method tremendously, and Perl acknowledges that "the 1971 paper of Tsai was the bible for my work on sequential heavy leptons," since he incorporated Tsai's theoretical work on the sequential heavy lepton model in a new detector proposal at SPEAR.[45]

Perl's recollection of his role in drafting the 1971 Mark I detector proposal—originally known as the SLAC-LBL Solenoidal Magnetic Detector—reveals that he was indeed still preoccupied with the unresolved electron-muon problem.[46] Perl recalls that

> I was thinking a lot about muons and electrons, and what we might do, and then we started writing the SPEAR proposal and I was already trying to find a place where Burt [Richter] wasn't, which is never easy, and I'd gotten to this looking for heavy leptons. He didn't like it too much, because I wrote a very big section [on searching for new leptons]; it was as big as the rest of the original Mark I physics proposals; in fact, it's in the appendix mostly.[47]

Clearly, Perl was concerned with carving out his own experimental niche at SLAC, and he wanted the opportunity to conduct research separate from Burton Richter, another group leader at SLAC. As Perl mentions, his determination to distinguish himself manifested itself in the SPEAR proposal's appendix, which he authored, on searching for new leptons. The proposal became not merely a vehicle for Perl to

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[45] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 11.
[46] The detector was renamed the Mark I when the Mark II detector was built, but because most of the collaboration members refer to the detector by its later name, I will refer to the detector as the Mark I for the sake of clarity.
[47] Perl, OHI, 02 Feb 1988, 16.

further his lepton research; it marked the first significant SLAC-LBL joint venture and the beginning of a fruitful new working relationship between the two laboratories.

Section 2.4.1 Drafting the Mark I Proposal

In 1971, Perl's Group E and SLAC Professor Burton Richter's group C joined with LBL Professor Willy Chinowsky's group and an LBL group led by Professors George Trilling and Gerson Goldhaber to collaborate on the proposal for the Mark I detector at the SPEAR electron-positron colliding beam storage ring at SLAC. The four teams, composed of two SLAC groups and two LBL groups, formed a dynamic association of distinguished experimental physicists. As the SLAC physicist who assembled the collaboration, Richter recruited team members from both laboratories according to their particular strengths and maintained a prominent leadership role throughout all stages of the collaboration.[48] A group leader who had worked in electron physics for years, Richter explained the reasoning behind his choice of physicists:

> I was leading one of the bigger groups, C, so I had to bring on people who could build, design, and do some physics with other facilities. I realized relatively early that C was too small to do the machine and all of the physics—I needed to expand…I went after people at Berkeley because we needed them.[49]

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[48] Richter's leadership in the SLAC-LBL collaboration and his participation in designing research and technical programs at SLAC is reported in the *AIP Study of Multi-Institutional Collaboration, Phase 1: High-Energy Physics, Report No. 4: Historical Findings on Collaborations in High-Energy Physics, Part B: Design and Planning for SPEAR (1961-1970).*
[49] Personal communication from Burton Richter, SLAC, 30 Mar 06.

In other words, Richter selected the LBL group members for the collaboration because he recognized that their bubble chamber experience would be useful for the design, construction, and testing of the Mark I. George Trilling had accompanied Donald Glaser when he left Michigan to begin working at Berkeley on bubble chambers, and Gerson Goldhaber had previously worked with Trilling to head a bubble chamber group. Chinowsky came to LBL through Goldhaber's efforts, and also worked with bubble chambers, although he had used counters in his doctoral work and was thus proficient in the electronic tradition. Each of the LBL physicists in the collaboration was proficient in programming and identifying tracks, and the analytic skills they had cultivated through bubble chamber work would be needed in the design and operation of the Mark I detector at SLAC.[50] Goldhaber remembers that the consensus at LBL was that "we felt the bubble chamber was on its last legs," and therefore the SLAC collaboration offered a new means of implementing their specialized skills while participating in a promising new line of research.[51]

The members of the collaboration worked together on the wording of the proposal, which declared that "a large magnetic detector facility is being constructed at SLAC to be used with the positron electron storage ring SPEAR."[52] The detector would be the first of its kind: a large, solid-angle, general-purpose detector built for colliding beams. Among the specific features to be studied, the subject of "heavy

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[50] Peter Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model,* 311-312.
[51] Personal communication from Gerson Goldhaber, LBL, 02 Mar 2006.
[52] A.M. Boyarski, J. Dakin, G. Feldman et al., "Proposal for a Magnetic Detector for SPEAR," 28 Dec 1971 (SP-1), Series IV: Stanford Linear Accelerator Center, Subseries O, Box 100, folder 7, SLAC Proposal Summaries.

leptons" was listed last, following boson form factors, baryon form factors, the total hadronic cross section, and inclusive spectra.[53] The heavy lepton section was saved for last and was afforded only three pages because, according to Perl, "to most others it seemed a remote dream."[54] Feldman confirms that the heavy lepton search was not taken seriously when the proposal was first drafted, recalling:

> most physicists considered the first three topics the "real proposal," and this last topic "a joke." I distinctly remember that as we were putting the proposal together in its final form, one senior member of the collaboration quipped, *Ha, heavy leptons! If Martin discovers that, we will let him publish it by himself.* Four years later, that quip had been long forgotten, and almost everyone signed the paper.[55]

Referring to the potential existence of heavy leptons, the proposal concluded with the statement, "if such particles exist, it is hard to see how they can be missed."[56] It is important to note that since heavy leptons were assigned a low priority at the time, the original Mark I design did not include an external muon detector. Perl insisted that the muon detector should be incorporated into the Mark I proposal, and he credits Feldman's influence with campaigning for the detector, since "it was Gary who sort of

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[53] A.M. Boyarski et al., "An Experimental Survey of Positron-Electron Annihilation into Multiparticle Final States in the Center-of-Mass Energy Range 2 GeV to 5 GeV," 7 Jan 1972 (SP-2), Series IV: Stanford Linear Accelerator Center, Subseries O, Box 100, folder 7, SLAC Proposal Summaries.
[54] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 12.
[55] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," in SLAC-R-412, *The Third Family and the Physics of Flavor: Proceedings of the 20th Annual SLAC Summer Institute on Particle Physics (SSI 92), 13-14 Jul 1992*, ed. Lilian Vassilian, SLAC, 636-646; 634.
[56] Ibid.

fought at the last minute to put muon detection in the Mark...just tremendous luck."[57] It was an important victory for Perl, who was already disappointed in the meager three-page section on heavy leptons. While his colleagues felt that the proposal would appear unbalanced if the heavy lepton section were expanded, Perl strongly believed that it was necessary to include more information on the electron-muon problem and heavy leptons in the proposal. Eventually, the collaboration reached a compromise by permitting Perl to write a ten-page appendix to the proposal on searching for heavy leptons.[58] The supplemental section posed the following questions: (1) Are there charged leptons with masses greater than the muon? (2) Are there anomalous interactions between the charged leptons and the hadrons? It was then explained that the detector would enable researchers to address the first question directly and that it would be possible to gather information on the second question that could lead to further experiments.[59]

The compromise on the ten-page appendix and the addition of the external muon detector in the Mark I proposal reveals that the practice of Big Science did not always distance the experimenter from the experiment. These concessions also illustrate an important instance of a physicist successfully defending his research agenda rather than experiencing a loss of control over both the experimental apparatus and the direction of the research itself. As it turned out, Perl's personal research interests were not only integrated into the Mark I proposal. The proposal was accepted

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[57] Perl, OHI, 02 Feb 1988, 17.
[58] Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," in *Physics in Perspective*, 414.
[59] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 12.

and the collaboration received funding for the new detector, so Perl's research interests were represented in the construction of the Mark I detector, and the discovery of the tau lepton would not have been possible without these additions.

Section 2.4.2. Building the Mark I

Just as the proposal was a collaborative effort between SLAC and LBL, the construction and testing of the Mark I detector was a joint venture involving the same group of physicists from both labs. When asked if there was a different role for different groups at SLAC when compared with groups at LBL, Perl responded, "no, everybody shared everything. We were pretty well mixed in."[60] Even a cursory examination of the individual tasks assigned to each member of the collaboration shows that as Perl indicated, the responsibility for building and testing various components of the detector among the group members according to their specialization did not result in segregation along laboratory divisions.

LBL members were put in charge of designing computer software for analysis and building an electronic shower counter for the detector, while SLAC physicists were responsible for the majority of the electronic equipment, yet both labs worked as a single cooperative unit in many areas.[61] Gerald Abrams developed an independent analysis system for the detector at Berkeley, and Adam Boyarski was responsible for another independent analysis system at SLAC. Using existing bubble chamber software, Chinowsky's group worked on producing visual track reconstruction

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[60] Perl, OHI, 02 Feb 1988, 17.
[61] Galison, Peter, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model,* 312.

displays of data at LBL, but Harvey Lynch and Roy Schwitters also participated in this type of work at SLAC.[62] The Goldhaber-Trilling group, with the help of Abrams and John Kadyk of SLAC, built liquid argon photon shower detectors at LBL, but later switched to analysis methods, so there was considerable overlap between the responsibilities of each member of the collaboration.[63]

In order to maintain maximum efficiency, this collective effort required physicists from each lab to communicate with one another on a frequent basis. Explaining how the collaboration worked on a functional level, Ballam stated:

> the analysis was done at home, a lot of it. Then people would come and present their data at collaboration meetings and so forth from time to time. That kind of sociology was already fairly well developed. Berkeley did a tremendous job because what Berkeley did was to develop the programs for analyzing the data and spreading that information around to a lot of people.[64]

Throughout the design, construction, and testing phases of the Mark I detector, Richter continued to act as the team coordinator. He described his hands-on role by explaining, “in designing the Mark I, I made the deliberate decision to limit the angular coverage and to use the cheapest possible gamma-ray detection system

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[62] Gerson Goldhaber, “From the Psi to Charmed Mesons: Three Years with the SLAC-LBL Detector at SPEAR,” in *The Rise of the Standard Model,* 57-58.
[63] Personal communication from Gerson Goldhaber, LBL, 02 Mar 2006.
[64] Joseph Ballam, Oral History Interview, 1987. Interviewed by Natalie Roe and Bill Kirk at Stanford Linear Accelerator. Date: Session One: 05 Nov 1987, 17.

because I viewed it as an exploratory instrument to be designed to do as much exploration as possible at minimum cost."[65]

Richter's involvement in planning and building the Mark I should not be interpreted as a result of his privileged status as the organizer of the collaboration, since the other members of the team also directly participated in every stage of the Mark I's development. Clearly, then, the Mark I collaboration represents a Big Science project that retained the character of a small science enterprise. Physicists did not farm out the building of parts to outside institutions; instead, the engineering and building of detector components was done by group members. Building the Mark I was a display of great ingenuity, for physicists in the collaboration frequently made innovations to existing technology, such as the modification of bubble chamber tracking software. As Richter has noted, it was "…a much different and more adventurous time when you could do great things with less money."[66]

In addition, the size of the collaboration remained relatively small—on the order of twenty to thirty physicists throughout the duration of the project—which facilitated open communication rather than isolation. It is important to note, however, that while the number of researchers involved may seem unusually small by the standards of contemporary Big Science collaborations consisting of hundreds or even thousands of individuals, the SLAC-LBL collaboration still qualified as a Big Science venture. During the early 1970s, as Roy Schwitters has observed, "those of us who

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[65] Burton Richter to Dr. William Wallenmeyer, 25 Jul 1976. Series IV: Stanford Linear Accelerator Center, Subseries I (Research Division, Groups A,B,C,D,E,F), Box 67, folder 7, Mark II Detector Development Correspondence, July 1976.

[66] Personal communication from Burton Richter, SLAC, 30 Mar 06.

were fortunate enough to be part of the Mark I collaboration thought that 35 authors was a pretty big list."[67] According to Richter, the twenty-seven principal researchers in the collaboration was "the biggest in physics at the time."[68] Nonetheless, even when a group of twenty to thirty physicists is labeled a "large collaboration," physicists in the SLAC-LBL collaboration were still afforded the scientific freedom, close interaction with experiments, and communication with colleagues that characterized the pre-Big Science generation. Commenting on the sociology of large collaborations, Schwitters confirms that

> the 'doing' of physics inside such a collaboration works very well…insiders know who is doing the work and making the innovations. The exciting physics opportunities and diversity of colleagues can make scientific life in a large collaboration most interesting and rewarding."[69]

Section 2.4.3. Mark I Experiments and the Question of Competition

The SLAC-LBL collaboration began running experiments using the Mark I detector in 1973, collecting a considerable amount of data from the particle collisions at SPEAR for over a year. The groups led by Richter, Perl, Chinowsky, Trilling, and Goldhaber each had a different experimental interest, but Goldhaber explained that instead of running multiple experiments, "there was *one* experiment. The experiment was to see what happens…first we found Ψ, then Ψ', other excited states…we were

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[67] Roy Schwitters, "Development of Large Detectors for Colliding-Beam Experiments," in *The Rise of the Standard Model*, 299-307; 306.
[68] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[69] Roy Schwitters, "Development of Large Detectors for Colliding-Beam Experiments," in *The Rise of the Standard Model,* 306.

looking for charmed mesons."[70] Richter has also described the Mark I experiment as "one experiment, one big data archive" from which "everyone looked at the same data and pulled out what was interesting to them."[71]

Curiously, although the untested new detector offered great experimental promise to these potentially rival laboratories, competition was not a major factor in the interactions between members of the collaboration. Goldhaber of LBL remembers, "not only were we lucky in that we were sitting on a "gold mine" at SPEAR, we also had a very congenial group of people. Since we had so much new data, a new discovery came up every few weeks, and there was very little infighting."[72] Richter also compared the data produced by the Mark I detector at SPEAR to a mine filled with precious gems, explaining "all you had to do was take your shovel and you could find your own jewels."[73] Perl expressed similar sentiments, although he has acknowledged that

> there was always a certain amount of disparaging. We used to complain that they didn't build photon detectors right and there was some sort of friction about that. The Berkeley people were amazing, though. It was always civil, although there was some friction.[74]

[70] Personal communication from Gerson Goldhaber, LBL, 02 Mar 2006. Goldhaber is referring to the "November Revolution" at SLAC in 1974, a name that was meant to be a witty allusion to the storming of the Winter Palace when the charm quark and its antiparticle were found as constituents of the J/psi meson.
[71] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[72] Gerson Goldhaber, "From the Psi to Charmed Mesons: Three Years with the SLAC-LBL Detector at SPEAR," in *The Rise of the Standard Model,* 74.
[73] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[74] Personal communication from Martin Perl, SLAC, 28 Mar 06.

Despite occasional "friction," in general, the collaboration operated as "one well-integrated team."[75] Competition truly did not play a significant role in the interactions between SLAC and LBL, and the working relationship between the two laboratories that had begun with the Mark I proposal in 1971 continued to function smoothly. Remarkably, the members of the collaboration were rewarded for their efforts with the detector's incredible productivity, resulting in multiple particle discoveries over a period of only a few years.

[75] Personal communication from Burton Richter, SLAC, 30 Mar 06.

Chapter Three: Discovery, Doubt, and Confirmation

Section 3.1 Overview

Martin Perl's dream of detecting new heavy leptons finally came to fruition with the discovery of the tau lepton using SLAC's newly constructed Mark I detector in the second half of the 1970s. Unlike the rapidly confirmed discovery of the charm quark the previous year, however, Perl's experimental findings were subjected to close scrutiny that soon led to great skepticism within the high-energy physics community. The triumph of discovering the tau lepton was extremely short-lived, as Perl anxiously awaited outside confirmation of the tau's existence by other laboratories.

In this chapter, I will show how the discovery of the tau lepton provides useful insight into two important dimensions of scientific practice: discovery and priority. When is it possible to declare a new 'discovery,' and how do scientists establish priority in large collaborations? By tracing the discovery from Perl's initial interpretation of the data to the eventual outside confirmation of the tau's existence, it becomes evident that, as in the case of many other discoveries, this discovery was not the result of a single experiment. Instead, the discovery of the tau lepton paralleled many earlier particle discoveries, since the discovery only became established through the ongoing process of building scientific credibility. In addition to validating the discovery itself, establishing priority was extremely important to Perl. An important aspect of the tau lepton discovery that has been entirely neglected in earlier historical accounts is Perl's decision to risk the disapproval of his colleagues at SLAC and LBL in order to ensure that he would receive credit for discovering the tau. Drawing upon unpublished materials and discussions with physicists at SLAC and LBL, I will show how Perl's professional ambition led to his somewhat controversial decision to publicize his experimental results without first consulting his colleagues, which was a violation of the collaboration's protocol for publication. Pief Panofsky and Burton Richter of SLAC were particularly upset by Perl's rush to publication because they had been careful to acknowledge LBL's role in the collaboration when new experimental findings from the Mark I were published. Furthermore, an earlier priority controversy between SLAC and MIT over the J/psi discovery had established in Panofsky's mind the necessity of exercising great caution when it came to taking credit for a new discovery. Within this context, Perl's behavior was initially deemed rash and insensitive by some of his colleagues at both SLAC and LBL, but he was more concerned with professional ambition than personal repercussions.

Section 3.2 Perl's 'Eureka' Moment

When the Mark I experiment began operation in 1973, it produced a great volume of data that Perl began to analyze beginning in 1974. Electrons and positrons collided and produced charged particles such as pions, electrons, and muons that immediately followed a curved path induced by the magnetic field. After muons reached the outer layers of the detectors, they were detected by muon wire chambers, and electrons were distinguished by the electromagnetic showers they produced in the detector's shower counters. Although Perl found the muon detection system to be "crude," the experiment obtained a total energy of 4.8 GeV, which proved to be more than sufficient.[1] A large mainframe computer recorded the data onto a magnetic tape and computers also processed the raw data, but it was Perl who noticed anomalous electron-muon events in the data. Perl explained, “I began to see some of these events, and the big question was are they background, low energies...and then I would classify events. I had a table, and I thought there were too many e-mu’s.”[2] Perl’s table of electron-muon events was the tool of analysis that ultimately enabled him to recognize the production of tau particles buried in the data, but he remained unsure of the meaning of the events until he consulted his colleague at SLAC, Jasper Kirkby. As Perl recalls:

> I once was talking to Jasper Kirkby, and he was sick, and he said he’ll take the table home. When he came back, he said, you know, if you just calculate the misidentification from all the other things you have—throw away all of the other events with photons so you just have e’s and mu’s...you cannot misidentify so much that you can get that many e-mu’s. It doesn’t matter what you assume. Very important point.[3]

Kirkby had suggested a new approach to resolving Perl’s uncertainty about his interpretation of the data by encouraging him to simply calculate the hadron misidentification probabilities from the rest of the data. By calculating the probabilities that the anomalous electron-muon events were actually electron-hadron,

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[1] Martin L. Perl, “The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years,” in *Physics in Perspective*, vol. 6, ed. John S. Rigden and Roger H. Stuewer (Boston: Berkhäuser Verlag, 2004), 414-416.
[2] Martin Perl, Oral History Interview 1988. Interviewed by Natalie Roe and Bill Kirk at Stanford Linear Accelerator. Session One, 02 Feb 1988, SLAC Archives and History Office, 18.
[3] Ibid.

muon-hadron, or hadron-hadron events, Perl immediately became convinced of his discovery: “I realized, that in fact we really had...the misidentification was 20%. It was a very big number...we had it.”[4]

Section 3.3 Going Public

Once Perl was convinced of the existence of the new heavy lepton, he authored a press release announcing the discovery of an unknown particle. Explaining why he chose to announce the preliminary findings in this way, Perl acknowledges, “I can remember just making the decision and going down to the public relations people to put it on the wire...”[5] Creating immediate controversy at SLAC and LBL, Perl described the press statement as “…the famous press release which they nearly threw me out of SLAC for, because by that time I could see that I thought we had it, and I really wanted the credit for myself. That’s never been forgiven or forgotten.”[6] Perl has also admitted “I didn’t want to share the credit with my colleagues because while they didn’t argue against me, they also didn’t support me, and it was my discovery.”[7]

As Perl recalls, the reaction from his colleagues was negative, since

> my name came first, and maybe a paragraph or two later a few more names came. It was just done that way. And the Berkeley people were furious. Burt [Richter] I don’t

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[4] Perl, OHI, 02 Feb 1988, 19.
[5] Ibid.
[6] Ibid. On the release of controversial press releases prior to publication of a scientific paper, see Bruce Lewenstein, “From Fax to Facts: Science Communication in the Cold Fusion Saga,” *Social Studies of Science* 25 (1995), 403-436; for an overview of the relationship between journalism and science, see Dorothy Nelkin, *Selling Science: How the Press Covers Science and Technology* (New York: W.H. Freeman, 1995).
[7] Personal communication from Martin Perl, SLAC, 28 Mar 2006.

> know. Pief [Panofsky], really bad, but I decided it was now or never. That's never been forgotten.[8]

Perl's desire to achieve recognition for the discovery took clearly precedence over the concerns of his colleagues. In a 1987 interview, Perl justified the press release further by revealing that at SLAC, there has been a "disagreement about press releases, every time it comes up, but I'm not sorry."[9]

Perl believes that since his name appeared first in the press release, the dispute over his early effort to publicize his results was related to his failure to adequately acknowledge the other members of the collaboration. Perl may certainly have offended his colleagues by neglecting to properly acknowledge them in the press release, but it is important to note that listing authors in this way was commonplace. When asked how decisions were made about the list of authors on a paper in collaborative efforts, Ballam explained that the list of names would generally include the principal investigator, followed by "various students, post-docs, faculty, and so forth...And then later on, if someone else joined and did some work, why then they would deserve their name to be on it as well. So that's how we decided."[10] Perl's decision to place his name as the lead author on the press release was thus not unusual, but his decision to act alone in publicizing the discovery was definitely frowned upon.

Richter recalls that the anger over the press release was due to Perl's rush to publicize his results without consulting his colleagues or following the established

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[8] Perl, OHI, 02 Feb 1988, 20.
[9] Ibid.
[10] Joseph Ballam, Oral History Interview, 1987. Interviewed by Natalie Roe and Bill Kirk at Stanford Linear Accelerator. Session Two: 13 Nov 1987, 15.

procedures agreed upon by both laboratories. When he learned of the press release, Richter recalls being "pissed off because he [Perl] was establishing his priority but no one had checked his work, so was he right?"[11] For Richter, then, the controversy over the first press release was not an issue of priority. Richter was instead concerned that because Perl had not waited for his peers to review his findings, the publicity could potentially jeopardize the scientific credibility of the laboratories and the reputations of the physicists at SLAC and LBL.[12] It was a risky decision both professionally and personally, but Perl wanted to be sure that he received credit for what could turn out to be a major new discovery in particle physics. Accounting for Perl's decision to publicize before his results had been internally confirmed, Panofsky explained, "the credibility of an experiment grows in time—if you want to be conservative, you risk getting scooped. He decided to publish fairly early when the data was still dubious."[13]

When asked if he felt that he had violated an agreement when he chose to publicize his discovery, Perl defended himself by saying, "I sure knew what I was doing...well, there was no agreement, but then it was now or never on that one, that's what happens with these things."[14] Although Perl claimed there was no agreed-upon procedure for handling publication, he also admitted, "I knew I should have consulted them. Pief called me and he was pretty unhappy about it."[15] According to Richter, there was indeed an in-house protocol that collaborators were expected to follow. Richter explained that there was a "check system" for new experimental findings that

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[11] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[12] Ibid.
[13] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[14] Perl, OHI, 02 Feb 1988, 20.
[15] Personal communication from Martin Perl, SLAC, 28 Mar 2006.

was intended to regulate all forms of publication. Before publicizing new results, a research group was supposed to submit a first draft of a paper for a review committee to analyze. The committee's job was to return the paper to the authors with comments and questions, and when the committee was satisfied with the revisions, the paper was deemed ready for outside publication. Consequently, in Richter's view, Perl drew negative attention from his peers because he ignored the protocol for presenting results from the collaboration when he issued the first press release.[16]

Thus, while Perl and Richter both admit that the press release was controversial, they provide somewhat differing accounts of the reason for the controversy. Perl cites the order of the listing of authors on the press release as the major source of contention, while Richter claims the negative reaction was due to Perl's decision to act alone without prior review. It is more likely that his colleagues were angry with Perl for both reasons, especially since SLAC had recently weathered another controversy over a particle discovery that involved two laboratories.

Section 3.4 A Troublesome Discovery: the J/psi Controversy

Indeed, Panofsky's treatment of the recent discovery of the subatomic particle called the J/psi provides insight into his desire to treat other laboratories with sensitivity, since he was drawn into an unpleasant exchange with Samuel Ting of MIT over the J/psi discovery of 1974.[17] Ting happened to be visiting SLAC when the peak

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[16] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[17] For an overview of the J/psi discovery, see Andrew Pickering, *Constructing Quarks* (Chicago: University of Chicago Press, 1984), 253-279; Peter Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model: Particle*

was discovered that SLAC researchers identified as the ‘psi’ particle. Panofsky recalls that when Ting was informed of the findings, he “...visibly paled and he then told us about his experiments. We then decided to have a joint or parallel, I don’t remember which it was, seminar here where Ting discussed his stuff and Burt’s group discussed our stuff.”[18] Apparently, Ting had also discovered the same particle through an entirely different set of experiments. However, Ting’s group had been extremely cautious in publishing on what they called the ‘J’ particle because Ting was known to have “...very high standards in terms of really wanting to believe it was right.”[19] Panofsky recalls that after Ting learned of the SLAC peak:

> There were several really strange situations. The fact that both groups did the work independently is clear; the fact is that in one case credibility rose slowly and in the other case it rose rapidly...it was one of those wonderful days of physics but also clearly it hurt Ting to some extent because he had been laboring for a long time on this particular thing and then suddenly it jumped out in front of his face, being done here. So clearly, he was not a happy man.[20]

Ting was not simply disappointed; he felt that he had been robbed of the discovery and implied that the SLAC-LBL group had previously learned of the MIT-BNL work and

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Physics in the 1960s and Beyond, ed. Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden (New York: Cambridge University Press, 1997), 308-337; 327; Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997), 538-545; Michael Riordan, *The Hunting of the Quark: A True Story of Modern Physics* (New York: Simon & Schuster, 1987), 262-307.

[18] W.K.H. Panofsky, AIP Oral History Transcript, 02 May 1997. Interviewed by Harvey Lynch at Stanford Linear Accelerator, SLAC Archives and History Office, 14.

[19] Ibid.

[20] Ibid.

only began their work after hearing of its success. Explaining Ting's line of reasoning in 1997, Panofsky stated:

> ...a rumor spread in the East that in fact the tentative presence of this peak, which had not been publicly announced, had leaked here and that therefore the people here had set the energy at the peak and therefore scooped, if you wish, the MIT group. That rumor was so intense that some of the senior people at MIT actually believed it, including Victor Weisskopf and Martin Deutsch.[21]

In a letter to the editor of *Science*, Deutsch supported this rumor by recounting a chance encounter in Cambridge. Deutsch claimed that while Richter was in town to lecture at Harvard, he "did not seem particularly impressed by my stories told at a cocktail party at the end of October" in which Deutsch allegedly discussed the work of the Ting group.[22] Responding to this claim, Richter sarcastically noted, "...it must have been a cautious and obscure discussion indeed..."[23]

The public debate over priority continued when Ting wrote a letter to *Science* expressing his feelings about the J/psi discovery. Panofsky attempted to convince Ting to withdraw his letter before publication, but both Deutsch and Ting "...expressed surprise that people on the West Coast would consider the letter to contain innuendos

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[21] W.K.H. Panofsky, OHI, 02 May 1997, 15.
[22] Deutsch, letter to *Science* magazine, 05 August 1975, Series IV: Stanford Linear Accelerator Center, Subseries I (Research Division, Groups A,B,C,D,E,F), Box 67, folder 2: SPEAR, New Particle, Papers and Correspondence, 1974-75.
[23] Burton Richter, Letter to the editor, *Science* Magazine. 14 September 1975, 2, Series IV: Stanford Linear Accelerator Center, Subseries I (Research Division, Groups A,B,C,D,E,F), Box 67, folder 2: SPEAR, New Particle, Papers and Correspondence, 1974-75.

about the independence of the SLAC discoveries."[24] Furthermore, "Deutsch said he felt it was necessary for Ting to write such a letter to (a) set the record straight and (b) as a matter of psychological therapy for his mental health."[25] Panofsky then tried to stop publication on the issue of *Science* by calling the editor directly, but he was informed that the letter would be printed and Stanford could respond in a later issue.[26]

Desperate to end the feud, Panofsky urged Karl Strauch at Harvard to aid him in confirming that "the agonizing over the psi data here was independent," adding that "I find this whole exchange quite unpleasant."[27] The president of MIT, Jerry Wiesner, was concerned that Panofsky had overstepped his authority by responding to the letters because he worried that the tension could develop into "an institutional argument" given Panofsky's position as head of SLAC. After reassuring Weisner by phone that he intended only to diffuse the situation, Weisner "did not really seem to be much concerned," and Panofsky concluded by reporting that "I have the general feeling that the matter will happily subside at this point."[28]

The "unpleasant" priority dispute over the J/psi began in late 1974 and continued throughout 1975, so Panofsky was already grappling with one controversial

[24] Panofsky, Memorandum to the Files, "Sam Ting's Letter to the Editor of SCIENCE Magazine," 03 September 1975, Series IV: Stanford Linear Accelerator Center, Subseries I (Research Division, Groups A,B,C,D,E,F), Box 67, folder 2: SPEAR, New Particle, Papers and Correspondence, 1974-75.
[25] Panofsky, Memorandum to the Files, "Sam Ting's Letter to the Editor of SCIENCE Magazine," 03 September 1975.
[26] Ibid.
[27] Panofsky to Strauch, Karl, 05 September 1975, Series IV: Stanford Linear Accelerator Center, Subseries I (Research Division, Groups A,B,C,D,E,F), Box 67, folder 2: SPEAR, New Particle, Papers and Correspondence, 1974-75.
[28] Panofsky, Memorandum to the Files, "Sam Ting's Letter to the Editor of SCIENCE Magazine," 03 September 1975.

particle discovery when Perl made his unexpected announcement. Avoiding another potential "institutional argument" would have been foremost on his mind, especially since Panofsky had always been careful to assign proper credit to LBL once the collaboration was established. As his editorial comments reveal, Panofsky had previously taken great pains to ensure that the involvement of LBL was appropriately recognized in all publications.

In 1972, the senior editor of *Physics Today*, Gloria B. Lubkin, sent Panofsky a copy of an article on SPEAR and the Positron-Electron Project (PEP) that was to be printed in the September issue in the "Search and Discovery" section of the journal, requesting his help in correcting any "errors, omissions or distortions of fact."[29] In his response letter, Panofsky defended his revisions to the article on SPEAR and PEP sent to him for review by commenting, "the principal changes relate to the possible sensitivities of the collaborators in the joint study to make sure that the article correctly represents the collaboration."[30] Although the original article included several references to the joint collaboration between Berkeley and SLAC, Panofsky was clearly interested in fairly representing the involvement of both research institutions. Concerned about the original article's omission of Berkeley's role, Panofsky altered the text "...SLAC is looking ahead to a still more powerful colliding-beam device called 'PEP'" by inserting the text, "SLAC in collaboration with the Lawrence

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[29] Gloria B. Lubkin to Pief Panofsky, 25 July 1972. Series IV: Stanford Linear Accelerator Center, Subseries A (Director's Office, Administration Files), Box 10, folder 9: Miscellaneous News Media, Correspondence 1956-75, I.

[30] Panofsky to Gloria B. Lubkin, 27 July 1972, 1. Series IV: Stanford Linear Accelerator Center, Subseries A (Director's Office, Administration Files), Box 10, folder 9: Miscellaneous News Media, Correspondence 1956-75, I.

Berkeley Laboratory (LBL) is looking ahead to a still more powerful colliding-beam device called 'PEP.'"[31] He also changed the text "SLAC envisions submitting a proposal..." to "the collaborating laboratories—LBL and SLAC—envision submitting a proposal..."[32]

The article sent by Lubkin to Panofsky concerning the J/psi discovery was also sent to Richter, and Richter's alterations similarly involved emphasizing Berkeley's participation. A hand-written note on his copy of the article read, "LBL should be informed of this before it goes back to G. Lubkin."[33] Richter also wanted to change the discussion of PEP to read, "PEP, the joint SLAC-LBL project."[34] This correspondence shows that Richter and Panofsky were both concerned about maintaining a cordial relationship with LBL, and thus they may have been upset with Perl for introducing tension to the collaboration.

When asked about the decision to go public concerning the J/psi discovery, Panofsky replied pointedly, "in this particular case, there was zero problem, the fact that there was a peak was obvious in one day; people began writing their paper during the data taking process. There wasn't any decision to be made as far as publishing is concerned."[35] By specifically mentioning that there was "zero problem" with publishing the results of the J/psi discovery, Panofsky appears to be distinguishing this discovery from other cases in which the data were less sound. Even though he

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[31] Panofsky to Gloria B. Lubkin, 27 July 1972, 1.
[32] Ibid, 4.
[33] Burton Richter, Annotated copy of *Physics Today* article, 25 July 1972. Series IV: Stanford Linear Accelerator Center, Subseries A (Director's Office, Administration Files), Box 10, folder 9: Miscellaneous News Media, Correspondence 1956-75, I.
[34] Ibid.
[35] Panofsky, OHI, 02 May 1997, 14.

approved of the J/psi publication, however, Panofsky himself was extremely cautious in interpreting the new data. A few months after the announcement was made, Panofsky stated, “the honest answer is that there is no one interpretation which is even remotely convincing.”[36] Panofsky chose to avoid drawing hasty conclusions, instead summarizing the possible meaning of the experimental findings in the J/psi experiment. In early accounts, Panofsky explained that the new particles could be the intermediate vector boson carrying the weak interaction. Another possibility Panofsky suggested was that the new particles were the bound states of the charmed quark and its anti-particle, although the anti-particle itself was still only a conjecture.[37] Panofsky also mentioned that the new particles could be quark anti-quark bound states, a scheme that would require the involvement of quarks having different colors. In Panofsky’s opinion, “...the ‘charmed’ quark interpretation is simplest and perhaps least defective...” However, he conservatively concluded that “it is likely that the correct answer will be the unstated item 5: ‘something else’.”[38]

Section 3.5 Risk and Reward

The controversy over Perl’s initial press release was limited to the disapproval of some of his immediate peers at SLAC and LBL, and does not qualify as a true scientific controversy by standard definitions.[39] Nevertheless, it is instructive to

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[36] Panofsky, “Retiring President's Address to the American Physical Society,” 30 January 1975, 18.
[37] Ibid.
[38] Ibid, 19.
[39] See Ernan McMullin, “Scientific Controversy and Its Termination,” in H. Tristram

examine Perl's motivations for taking an action that would upset his colleagues because his behavior represents a defining feature of scientific practice: the pressure to publish.

John K. Merton and Augustine Brannigan provide differing explanations for the scientist's urgency to publish new findings. Brannigan argues that for scientists, discoveries are "singletons," or singular events, which accounts for the scientist's anxiety over priority and the consequent rush to publish new findings. Since scientists regard discoveries as singular in nature, they must be the first to claim priority through publication. Brannigan links the scientist's view of singular scientific discovery not to originality, however, but to the desire to establish a novel achievement.[40] Merton, by contrast, asserts that for scientists, "all scientific discoveries are in principle multiples."[41] Merton argues that scientists recognize that due to simultaneous independent discoveries, even singleton discoveries are truly multiples. Thus, "since the culture of science puts a premium not only on originality but on chronological

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Engelhardt, Jr. and Arthur L. Caplan, eds. *Case Studies in the Resolution and Closure of Disputes in Science and Technology* (Cambridge: Cambridge University Press, 1987). McMullin defines controversy in science as a publicly conducted, persistently maintained dispute over a matter of belief considered significant by a number of practicing scientists, and it is formed by epistemic or non-epistemic factors. The negative reaction over Perl's rush to publication does not meet these criteria and therefore should not be classified as a scientific controversy, but as an internal institutional dispute.

[40] Augustine Brannigan, *The Social Basis of Scientific Discoveries* (London: Cambridge University Press, 1981), 59-60.

[41] Robert K. Merton, *On Social Structure and Science* (Chicago: The University of Chicago Press, 1996), 307.

firsts in discovery, this awareness of multiples understandably activates a rush to ensure priority."[42]

Whether the rush to publication results from the scientist's view of discovery as single or multiple in nature, it is clear that scientists are eager to establish priority in order to achieve the personal and professional rise in status associated with scientific discovery. Indeed, Perl's rush to publication and his stated lack of regret reflect a strong desire to reap the benefits of the 'reward system of science'. According to Merton, the reward system of science includes eponymy, the Nobel Prize and other medals, honorary membership in academic sciences, and being memorialized in the history of science. Merton explains:

> the reward system of science reinforces and perpetuates the institutional emphasis upon originality. It is in this specific sense that originality can be said to be a major institutional goal of modern science, at times the paramount one, and recognition for originality a derived, but often as heavily emphasized, goal. In the organized competitions to contribute to scientific knowledge, the race *is* to the swift, to they who get there first with their contributions in hand.[43]

Wary of losing the race, then, Perl's efforts to publish first are not surprising, and his unapologetic attitude can be better understood as his practical evaluation of the necessity of establishing a priority claim in scientific work. Merton asserts that when a scientist is forced to confront the conflicting values of humility and originality, originality often overcomes humility, explaining, "it is generally an unequaled contest between the values of recognized originality and of modesty. Great modesty may elicit

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[42] Robert K. Merton, *On Social Structure and Science,* 311.
[43] Ibid, 289.

respect, but great originality promises everlasting fame."[44] For Merton, the tendency to favor originality "...goes far toward explaining why so many scientists, even those who are ordinarily of the most scrupulous integrity, will go to great lengths to press their claims to priority of discovery."[45] The emphasis upon original scientific achievement fostered by the social organization of science thus creates great pressure upon scientists to establish priority for a new discovery. Within this context, "one can begin to glimpse the sources, other than idiosyncratic ones, of the misbehavior of individual scientists."[46] In Perl's case, the pressure to produce an original discovery did not lead him to commit scientific fraud or engage in other extreme unethical behavior, but he recognized that the 'reward system' might slip through his fingers if he delayed publication.

Section 3.6 A Period of Doubt

Perl may have upset his colleagues by publishing too soon or neglecting to fully acknowledge members of the collaboration, but his surprising announcement meant that he also had to overcome the greater obstacle of proving that the new heavy lepton truly existed. Perl's first task was to convince his colleagues at SLAC and LBL that he had interpreted the electron-muon events correctly. Although Perl acknowledged that some of his colleagues were immediately supportive, in his view, the collaboration was composed of "a lot of arrogant people," which meant that he

[44] Robert K. Merton, *On Social Structure and Science,* 294.
[45] Ibid.
[46] Ibid, 304.

encountered some initial skepticism from his peers.[47] Fortunately, confirming Perl's results was a high priority for the collaboration, since the reputation of both laboratories was at stake if Perl was in error.

At Perl's request, Gerson Goldhaber of LBL re-analyzed the Mark I data by checking the electron-muon events carefully. After repeating Perl's calculations, Goldhaber no longer doubted that the electron-muon events existed, though he pointed out, "I did not believe this was proof for a new lepton, but he [Perl] was convinced."[48] In fact, because charmed mesons were of such great interest to Goldhaber's group, Goldhaber and several other physicists thought Perl's findings were related to charm mesons, which Goldhaber later found himself.[49]

Feldman was similarly persuaded by the data, explaining, "...the probability of it fluctuating to 24 is less than one in a million. Thus, the real issue was not statistics, but whether the misidentifications had been properly determined."[50] Richter recalls simply that the group "…put together a panel to check his results and they were verified."[51] This process took several months, but once the analysis was completed, there was "no question in the SPEAR collaboration that this thing was a heavy lepton—what we needed was independent analysis."[52]

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[47] Perl, OHI, 02 Feb 1988, 19.
[48] Personal communication from Gerson Goldhaber, LBL, 02 Mar 2006.
[49] Ibid.
[50] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," in SLAC-R-412, *The Third Family and the Physics of Flavor: Proceedings of the 20th Annual SLAC Summer Institute on Particle Physics (SSI 92), 13-14 Jul 1992*, ed. Lilian Vassilian, SLAC, 636-646; 636.
[51] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[52] Ibid.

Obtaining confirmation of the heavy lepton's existence from outside laboratories then became the most important step in validating the discovery, yet independent confirmation remained elusive for several years. Further exacerbating the difficulty of establishing that new heavy leptons had been detected was the recent discovery of the charm quark at SLAC, which meant that the Standard Model was widely considered to be complete. With the addition of the charm quark to the Standard Model, matter could be neatly subdivided into two families containing two quarks and two leptons. In other words, the model already seemed symmetric, but a third lepton would necessitate the existence of a third family, thus upsetting the assumed symmetry. As Richter stated, "with the psi and charm, you established the notion of two families, and along comes this damn tau."[53]

Thus Perl's announcement was not immediately appreciated as a groundbreaking new scientific discovery, and the delayed impact of the discovery on the physics community is partially explained by the ideological commitment to the two-family, symmetrical version of the Standard Model.[54] The period following Perl's

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[53] Personal communication from Burton Richter, SLAC, 30 Mar 06.

[54] The presence of strong social factors that contribute to the resistance to newly reported scientific discoveries has been examined in detail in Ernest B. Hook, ed., *Prematurity in Scientific Discovery: On Resistance and Neglect* (Berkeley: University of California Press, 2002). See especially Gunther Stent's essay, "Prematurity in Scientific Discovery," 21-33. A term coined by Stent, 'scientific prematurity' refers to a scientific discovery that cannot be linked to the canonical knowledge of the period. Prematurity is explored in this volume as a means of understanding the initial resistance to a scientific discovery, permitting the argument that an announcement of a discovery not yet fully accepted by the scientific community should instead be called a claim, hypothesis, or proposal. Using Stent's definition, Perl's discovery was 'unexpected', but not premature, since it could be linked to the canonical knowledge of that time, yet it challenged that knowledge. Following this reasoning, the physics

announcement was "...a very, very bad time. Because they couldn't find it and reports would come back that they hadn't found it. And I'd agonize with Gary [Feldman]."[55] The disappointing reports of the failure to confirm the anomalous electron-muon events came from the Doppel Ring Speicher (DORIS) storage ring at the DORIS electron-positron collider (DESY) in Hamburg, where two long-running experiments had the potential to either substantiate or invalidate the heavy lepton interpretation. Researchers overseeing one of the experiments, using the PLUTO detector, had been searching for muons for two years. The second experiment was known as DASP, an acronym for the double-arm spectrometer at DORIS, and the results from this experiment were equally important.[56] Unfortunately for Perl, there was no conclusive evidence supporting the existence of heavy leptons from either experiment, and the outside physics community was not ready to accept the new particle's existence based solely on Perl's results. Panofsky affirmed, "when Perl first published, the world greeted it with a firm 'maybe.'"[57]

Hoping for outside confirmation yet recognizing the necessity of building greater credibility for the discovery at SLAC, Perl and the other SLAC-LBL groups continued to produce new electron-positron annihilation data at higher energies throughout 1974. Perl noted, "we were running all the time in those days...so we built up from the original 24 until we had 160, 170...always consistent, always the same."[58]

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community initially resisted Perl's discovery because accepting his findings would require overturning the accepted two-family model.

[55] Perl, OHI, 02 Feb 1988, 20.

[56] Personal communication from Martin Perl, 30 Jul 2006.

[57] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.

[58] Perl, OHI, 02 Feb 1988, 19.

Because the data revealed anomalous electron-muon events at each energy level, Perl recalled that "I and my colleagues in the SLAC-LBL experiment became more and more convinced of the reality of the electron-muon events and the absence of a conventional explanation."[59] To boost the group's ability to collect new data, Feldman built a new muon detection system during this time called the 'muon tower.'[60] Feldman was motivated to build the muon tower when he realized that in order to identify the charmed particles he sought in his own research, he would need to improve the muon-detecting capabilities of the Mark I detector. The muon tower soon became known as the 'Tower of Power,' for it aided not only in the search for charm, but in the first internal confirmation of Perl's electron-muon events.[61]

In addition to finding more electron-muon events, Perl began lecturing on the new particle discovery right away, recalling, "I went public the first time I tried it out…I tried it out at a meeting in Montreal, I think it might have been in 1975, when I tried it out, lectured on it, and people sort of listened, and I was fairly nervous."[62] Perl did in fact speak about the new discovery in 1975 at the Summer School of the Canadian Institute of Particle Physics at McGill University in Montreal, carefully noting that "the data analyzed here all comes from the LBL-SLAC Magnetic Detector Collaboration."[63] During the Montreal lecture, Perl suggested that the electron-muon

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[59] Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," in *Physics in Perspective*, 417.
[60] Ibid.
[61] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 638.
[62] Perl, OHI, 02 Feb 1988, 19.
[63] Martin L. Perl, "Anomalous Lepton Production," Proceedings of the IPP International School on the Experimental Status and Theoretical Approaches in

events could also be due to heavy mesons or intermediate bosons, but his true goal was to persuade his audience that the 24 electron-muon events indicated the existence of new heavy leptons.[64] He informed the audience that although his data had not yet been published, "…because of the interest in this work and its possible significance I believe it is worthwhile to present the data and my analysis of that data even though that analysis is still in progress."[65] Perl acknowledged that "in one sense the proposal of explanations for the eμ events is premature…in another sense the analysis is aided by hypotheses," referring to heavy leptons, heavy mesons, or elementary bosons as the hypothesized sources.[66] Perl's talk concluded with the general statement that "no conventional explanation" had yet been found for the events, thereby dismissing the notion that the events could be easily explained away. He then conservatively summarized his interpretation of the data while hinting at heavy lepton detection by observing, "the hypothesis that the signature eμ events come from the production of a pair of new particles—each of a mass of about 2 GeV—fits almost all the data."[67] Perl could not yet definitively identify the new particles as leptons, but recalled, "…I remember feeling strongly that the source was heavy leptons."[68]

Physics at High Energy Accelerators, McGill University, Montreal, June 16-21, 1975 (Montreal: McGill University, 1975), reprinted in Perl, *Reflections on Experimental Science*, World Scientific Series in 20th Century Physics, Vol. 14 (River Edge: World Scientific, 1996), 141.

[64] Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," 416.

[65] Martin L. Perl, "Anomalous Lepton Production," 141.

[66] Ibid, 157.

[67] Ibid, 168.

[68] Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," 417.

While Perl publicized the anomalous electron-muon events in Montreal, Feldman went to Europe to "spread the word" at a conference on neutrinos in Hungary and at the annual meeting of the European Physical Society in Sicily. Perl continued to report on the Mark I data through three additional high-energy particle physics summer conferences while simultaneously preparing a paper that now met with the approval of his colleagues. The first peer-reviewed paper on the electron-muon events was submitted to *Physical Review Letters* in August 1975, and appeared in the December issue with Perl indicated as the lead author, followed by the other members of the SLAC-LBL collaboration in alphabetical order. The paper echoed Perl's earlier statement at the Montreal conference that "no conventional explanation" had yet been found for the events.[69] Perl recalls that "we were not yet prepared to claim that we had found a new charged lepton, but we were prepared to claim that we had found something new."[70]

Continuing his efforts to draw attention to his experimental findings, at a hadron spectroscopy conference in Argonne in early July of 1975, Perl decided to call the new particle 'U' for unknown.[71] Reflecting on Perl's busy lecture circuit publicizing the new particle, Panofsky recalled:

> Perl actually did the opposite thing to Sam Ting. He did not keep the initial data secret but he went around to neighboring labs, including Berkeley, presenting his

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[69] M.L. Perl et al., "Evidence for Anomalous Lepton Production in e^+e^- Annihilation," *Physical Review Letters* 35 (1975), 1489-1492; reprinted in Perl, *Reflections on Experimental Science*, 193-196.
[70] Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers" in Perl, *Reflections on Experimental Science*, 17.
[71] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 637.

> data. He even presented seminars saying, 'I don't know what this is, it may be a sequential lepton but it may be a lot of other things.' So, he tried very hard to proselyte non-believers in his experiments. He used the very opposite social dynamics of keeping it secret…[72]

Richter also believed the so-called U particle was a clever rhetorical device that Perl employed to great effect, noting, "Martin played this masterfully…it was clear exactly what he thought it was."[73]

One of the last conferences of the summer of 1975 was the international Lepton-Photon Symposium, which was fortuitously hosted by SLAC that year. Due to the recent addition of the muon tower, Perl's group was finally able to report an internal confirmation of the electron-muon events with much lower misidentification probabilities than the earlier findings. Feldman notes that the muon tower data presented at the Symposium was "as close as we ever came to a 'golden event' in the Mark I detector. Still, outside confirmation was needed."[74]

External confirmation did not come in 1975, but Perl continued to make progress in strengthening the credibility of his experimental data. Even before the first paper was printed in late 1975, Perl began to work on calculations for the second paper, which led to a critical revelation. Feldman recalls that while sitting at his desk one day, "...I was taken completely by surprise when Martin Perl appeared at my door and said simply, It's a heavy lepton. I responded with some sage comment such as,

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[72] Panofsky, OHI, 11 Apr 1997, 16.
[73] Personal communication from Burton Richter, SLAC, 30 Mar 06.
[74] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 639.

Oh, really?"[75] Perl now felt comfortable boldly stating his interpretation of the electron-muon events because his recent calculations had shown that the undetected particles in the decay reaction represented by the data were neutrinos. This new information was significant because if the missing particles were neutrinos, heavy leptons were the only other particles that could balance the decay reaction.[76]

It was a triumphant moment for Perl, and serves as further evidence of the preservation of small science practices at SLAC. From the initial graph of the first set of electron-muon events to the later analysis of the data involving the missing neutrinos, Perl was able to perform complex calculations without relying on large mainframe computers. Computers recorded and processed the raw data, but Perl made the detailed calculations by hand that led to the discovery of the new heavy lepton. He continued to produce and analyze data throughout 1976, and the group published the second paper on the electron-muon events that summer.[77]

The second paper was based on Perl's logical inference that the decaying particles had to be heavy leptons, and it should have marked a new victory in the struggle to establish the existence of the heavy leptons. However, as Feldman recalled, "one would think that July 1976 would have been the high point in the discovery of the τ. It was, in fact, the low point."[78] One of the factors influencing the low credibility of the discovery was that Perl and Feldman chose not to attend the international

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[75] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 639.

[76] Ibid.

[77] M.L. Perl et al., "Properties of Anomalous eμ Events Produced in e^+e^- Annihilation," Physical Review Letters 63B, (1975), 466.

[78] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 642.

Rochester Conference in the Soviet Union that year “for personal reasons,” so the Mark I results were presented by Bjorn Wiik of DESY. Wiik was the spokesperson for the DASP group, and as DESY’s most prominent experimenter, his report made a strong impression on the conference attendees that year.[79] Wiik emphasized the lack of confirming evidence in support of the anomalous electron-muon events, ending his talk with the devastating conclusion that there was no “convincing” evidence to demonstrate that heavy leptons had in fact been produced.[80] Wiik’s presentation reinforced the widespread suspicion within the high-energy physics community that Perl had mistakenly identified the particles as leptons because it was well-known that he had been searching for heavy leptons for years. After learning of the disastrous impact of Wiik’s talk, Feldman tried to convince Perl that they would need to attend future conferences in order to defend the results. According to Feldman, Perl reassured him by saying, “No, it’s not important. You see, that is the great thing about science. It doesn’t matter what people think or say. The truth comes out in the end.”[81]

Section 3.7 Outside Confirmation at Last

In 1976, M. Cavilli-Sforza headed another SPEAR experiment that produced the first outside confirmation of the electron-muon events, but although this confirmation was welcomed by Perl, it did not have great authority because it was carried out at SPEAR.

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[79] Personal communication from Martin Perl, 30 Jul 2006.
[80] Gary J. Feldman, “The Discovery of the τ, 1975-1977: A Tale of Three Papers,” 642.
[81] Ibid.

Using the muon tower of the Mark I, Feldman provided the second confirmation of the events in June. In a note to Perl, Feldman reported that he had found eight electron-muon events and seventeen muon-hadron events after correcting for particle misidentification. If the two anomalous signals derived from the same source, his results conformed with "almost exactly what one would expect for the decay of a heavy lepton."[82] These new results were published by Feldman and Perl in 1977, a pivotal year for the discovery of the tau lepton.[83]

Section 3.7.1 A New Name for Particle U

Encouraged by the new findings, Perl turned his attention to authoring the third paper on the heavy lepton discovery. Although the evidence was still mounting, Perl felt confident asserting that the unknown particle U was in fact a heavy lepton, and the time had come to properly name the new particle. Feldman urged Perl to change the name before submitting the paper, recalling, "...we had to do it now, because if we published one more paper with the name U, it would stick forever."[84] Perl agreed that the name had to be changed, and the other members of the SLAC-LBL collaboration were drawn into the discussion on suitable names. A lowercase Greek letter was in order, but "the problem was that most good Greek letters were already in use."[85]

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[82] Gary Feldman, quoted in Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 21.
[83] See G.J. Feldman, M.L. Perl et al., "Inclusive Anomalous muon production in e^+e^- Annihilation," *Physical Review Letters* 38 (1977), 117-120.
[84] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 642.
[85] Ibid.

The choices were narrowed down to lambda and tau, and there were valid reasons for choosing both names. Lambda had never been used for a particle name, while tau was already in use for the three-pion decay of the kaon. The tau symbol had been suggested by Perl's Greek graduate student Petros Rapidis because it meant "triton," or "third" in Greek, and thus would be more meaningful since the tau lepton was part of a third family of particles. Mulling over the two choices, Feldman remembers asking the group secretary Karen Goldsmith which symbol would be "more esthetic" because she would have to type the symbol that was selected to represent the new particle. Goldsmith decided on the tau symbol, and Feldman recalls that this was "the final piece of evidence that caused us to adopt τ as the name."[86] Perl announced the particle discovery as the tau lepton in March 1977 at the Rencontre de Moriond conference in France, and the name was well-received. Feldman notes that the name was featured prominently in the third paper submitted in August, but "…by the time of submission of the third paper, there was no need to explain it."[87]

A few months before the third paper was received, the long-awaited confirmation from DORIS finally came. Like the Mark I, the PLUTO detector at DORIS was a large solid-angle detector that was well-suited to detecting particles within the narrow energy range required to produce the desired electron-muon events. To Perl's great relief, researchers at DORIS found anomalous muon-hadron events in May of 1977 and published their results as definitive evidence for heavy leptons,

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[86] Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 643.
[87] Ibid. Also see the third paper, Perl, M.L. et al., "Properties of the Proposed τ Charged Lepton," *Physical Letters* 70B (1975), 487.

marking a tremendous breakthrough for the tau lepton discovery.[88] As Feldman notes, "as far as the rest of the world was concerned, it was the confirmation from DORIS that mattered."[89] This exciting confirmation was a major step towards validating Perl's work, but he recalls that "there was much to disentangle."[90] Two major hadronic decay modes, the pi and rho decays, had yet to be found. Finding these decay modes would provide truly persuasive evidence for the discovery because if the tau was a sequential heavy lepton as Perl believed, these decay modes would have to exist.

When Perl attended the Photon-Lepton Conference in Hamburg during August of 1977, he informed the audience that while many of the predicted decay modes had been observed, finding the other decay modes was "a very important problem."[91] Describing the frustration of waiting for these decay modes to be observed, Perl lamented, "it was so clear how it ought to decay—it just took time to see all the decay sequences—the technology was always limiting."[92] Because the photon detection systems in detectors during this time were not efficient enough, it was difficult for experimenters to isolate the pi and rho decay modes. Fortunately, it was not long before new detectors, including the Mark II at SLAC, enabled physicists to make clearer distinctions between the two decay modes. By 1978, the pi decay mode was

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88 J. Burmester et al., "Anomalous Muon Production in e^+e^- Annhiliations as Evidence for Heavy Leptons," *Physical Review Letters B* (1977), 297-300.
89 Gary J. Feldman, "The Discovery of the τ, 1975-1977: A Tale of Three Papers," 642.
90 Martin L. Perl, "A Memoir on the Discovery of the Tau Lepton and Commentaries on Early Lepton Papers," 22.
91 Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," 423.
92 Personal communication from Martin Perl, SLAC, 28 Mar 2006.

measured using four new detectors, and the rho decay mode was found a year later.[93] Thus Perl observed that

> By the end of 1979, all confirmed measurements agreed with the hypothesis that the tau is a lepton produced by a known electromagnetic interaction and that, at least in its main modes, it decays through the conventional weak interaction. So ends the sixteen-year history, 1963 to 1979, of the discovery of the tau lepton and the verification of that discovery.[94]

Section 3.8 The Life Cycle of a Discovery

Perl clearly avoided attempting to pinpoint a precise moment of discovery, instead choosing to trace the beginning of the tau lepton discovery from his first investigations of the electron-muon problem upon arriving at SLAC in 1963 to the measurement of the hadronic decay modes in the late 1970s. Similar to earlier particle discoveries, the discovery of the tau lepton occurred over time as the evidence gradually accumulated.[95] Panofsky summarized the prolonged process of discovery by noting:

> It was a case where a large group worked at first, then Perl and a small group 'milked the tapes' doing analysis for several years, and he succeeded. The statistical

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[93] Martin L. Perl, "The Discovery of the Tau Lepton and the Changes in Elementary-Particle Physics in Forty Years," 423-424. The detectors used to find the pi decay mode were PLUTO, the Direct Electron Counter (DELCO) at SPEAR, the Lead-Glass Wall detector installed in the Mark I detector, and the Mark II detector. The rho decay mode was found by the DASP collaborators at DORIS and by the Mark II collaborators.
[94] Ibid, 424.
[95] For example, see Norwood Russell Hanson's *The Concept of the Positron: A Philosophical Analysis* (New York: Cambridge University Press, 1963). Hanson refers to the discovery of the positron as the discovery of three different particles because the discovery occurred in stages as the related nature of the work of physicists Dirac, Anderson, and Blackett gradually came to be recognized.

> evidence had to grow slowly and it took a long time to establish credibility. It shows the power of large groups doing great things but also the power of the individual to 'work offline'; it was a sequence of social structures all leading into one another.[96]

Just as determining when the discovery was made is somewhat debatable, assessing when the discovery was finally accepted within the high-energy physics community eludes a simple answer. Feldman marks the end of the discovery of the tau with the submission of the third paper in August of 1977. Perl also believes the existence of a third charged lepton was no longer contested by that summer's conference in Hamburg because he recalls, "it was pretty well accepted at that point."[97] Even following the Hamburg conference, however, many publications discussed the discovery in fairly conservative language. In a November issue of *Science*, science journalist Arthur L Robinson wrote that "a second recently discovered new particle, called the tau, is believed by many to be a new lepton or, in the jargon, a heavy lepton."[98] Proving that a new heavy lepton had been produced in the positron-electron collisions was said to be a difficult task because it would involve "...accumulating various, somewhat circumstantial data which, taken together, build up a strong case for the heavy lepton."[99] Perl would certainly have disagreed with Robinson's declaration that "all the results so far are consistent with the heavy lepton interpretation, but none are definitive."[100]

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[96] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[97] Personal communication from Martin Perl, SLAC, 28 Mar 2006.
[98] Robinson, Arthur L., "High Energy Physics: A Proliferation of Quarks and Leptons," *Science* 198 (1977), 480.
[99] Ibid, 481.
[100] Ibid.

Given the ambiguous nature of scientific discovery, it is perhaps impossible to ascertain precisely when the discovery was made or when it was ultimately confirmed and accepted.[101] One aspect of the history of the discovery of the tau lepton that cannot be questioned, however, is the central role played by Martin Perl, who single-mindedly sought heavy leptons for decades. Although the willingness to risk personal and professional merit by rushing to publication caused some initial controversy at SLAC and LBL, this decision effectively ensured that the discovery of the tau lepton would always be inextricably linked to Martin Perl.

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[101] The literature on the problematic nature of analyzing scientific discovery is vast; for more on the social context of scientific discovery, see Augustine Brannigan, *The Social Basis of Scientific Discoveries* (London: Cambridge University Press, 1981). Brannigan examines and rejects previous theories of discovery relying on a genius explanation or cultural determination. He focuses on how an event came to be constituted as a discovery rather than examining discovery from the psychological approach of determining how discoveries are made by individuals. Brannigan emphasizes the social construction of discovery, showing that discovery is dependent upon interpretations of the participants, which helps to explain why there is a lack of consensus among physicists involved in the discovery regarding when the tau lepton discovery occurred. See also Thomas Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: The University of Chicago Press, 1979), 165-177, for a discussion of the misleading language of discovery and the challenges of attempting to identify a scientific discovery.

Chapter Four: Conclusion: The Legacy of the Tau Lepton Discovery

Section 4.1 The Aftermath of the Discovery

Section 4.1.1 The Tau Lepton Transforms the Standard Model

The discovery of the tau lepton permanently altered the Standard Model of physics, firmly establishing that matter is composed of three families of particles rather than two. For many physicists, confirming that a third generation of matter existed was a major revelation, since theory now predicted that other particles were waiting to be discovered. Finding the tau lepton suggested the existence of an associated neutrino, which meant the symmetry established in 1974 with the charm quark discovery would have to be broken. Instead of four leptons and four quarks, the tau lepton discovery and its predicted neutrino would result in six leptons and four quarks. This unsettling asymmetry in the Standard Model gave physicists great incentive to search for new particles, however, and thus the tau lepton discovery indirectly led to the achievement of the modern form of the Standard Model.

As they sought parallel structures to the organization of the first two generations of matter, physicists became interested in finding the two quarks that would restore symmetry to the model. The bottom quark, or upsilon, was discovered at Fermilab in 1977 in a collaboration led by Leon Lederman and involving Columbia

University and State University of New York at Stony Brook.[1] The top quark was also observed in 1995 at Fermilab, leaving only the elusive tau neutrino undetected.[2] In 2000, a Fermilab collaboration of physicists from the U.S., Japan, Korea, and Greece working on the Direct Observation of the Nu Tau (DONUT) experiment finally observed the tau neutrino, completing the lepton family.[3]

Section 4.1.2 The End of the "Mini-R Crisis"

In addition to introducing a new tier of matter to the Standard Model, the discovery of the tau lepton resolved the so-called "mini-R-Crisis." The mini-R crisis was a by-product of the "R-crisis," which was a troubling incongruency between theory and experimental data. R was the ratio of cross-sections for hadron and muon production in electron-positron annihilation, and the predicted values of R for quarks varied depending on color. In the 1970s, the consensus within the high-energy physics community was that R should be equal to a constant value, which was equal to the sum of the squares of quark charges. This constant value R was supposed to be energy-independent because according to the quark-parton model, muons and hadrons

[1] See Frederik Nebeker, "Experimental Style in High-Energy Physics: The Discovery of the Upsilon Particle," *Historical Studies in the Physical and Biological Sciences* 24, 2 (1994): 137-164; Pickering, Andrew, *Constructing Quarks* (Chicago: University of Chicago Press, 1984), 283.
[2] S. Abachi, B. Abbott, et al., "Observation of the Top Quark," *Physical Review Letters* 74 (1995), 2632-2637.
[3] Kurt Riesselmann, "DONUT Finds Missing Puzzle Piece," *FermiNews* 23 (2000), available at http://www.fnal.gov/pub/ferminews/ferminews00-08-04/p1.html, last accessed 06 Jul 2006.

produced in electron-positron annihilation only differ in their electric charges, and the total cross-sections were thus expected to have the same energy dependence.[4]

When the electron-positron storage ring SPEAR began operation in 1973, however, physicists realized that R appeared to rise linearly as a function of the square of the center-of-mass energy, s. Instead of the predicted R value of 2/3, 2, or 4, R ranged from a value of 2 to 6, creating the "R-crisis." With the J/psi discovery in 1974, however, the crisis ended when the new data showed that R was indeed constant within the predicted range.[5]

The J/psi discovery did not provide an explanation for another perplexing characteristic of the R value, however, which was soon termed the mini-R-crisis. At 5 GeV, R was expected to reach an asymptotic value around 3 ½ for 4-flavored, 3-colored quarks, but the experimental results showed that R became constant at the significantly higher value of 5. The mini-R crisis created skepticism over the charm discovery, and physicists struggled to account for the unexpected magnitude of R by suggesting new heavy quark models containing a greater number of quarks. The tau lepton discovery effectively ended the mini-R crisis by providing another explanation

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[4] For further discussion of the R-crisis, see Pickering, *Constructing Quarks,* 254-258; Michael Riordan, *The Hunting of the Quark: A True Story of Modern Physics* (New York: Simon & Schuster, 1987), 254-261, Peter Galison, "Pure and Hybrid Detectors: Mark I and the Psi," in *The Rise of the Standard Model: Particle Physics in the 1960s and Beyond*, ed. Lillian Hoddeson, Laurie Brown, Michael Riordan, and Max Dresden (New York: Cambridge University Press, 1997), 308-337; 327.
[5] Pickering, *Constructing Quarks,* 260.

for the higher asymptotic value of R because it revealed that R was inflated by heavy leptons instead of other quarks.[6]

Section 4.1.3 Unsettled Problems

While the tau lepton discovery expanded the Standard Model and resolved the mini-R-crisis, it did not provide many new answers to problems in high-energy physics. Perl initiated the search for the tau lepton out of a desire to resolve the electron-muon problem, writing in 1974 that the known properties of the muon and electron raised several important questions:

> Are there heavier charged leptons? If there are no heavier charged leptons, why are there two charged leptons? Are the charged leptons really point particles, or do they have a structure which has not yet been detected? Are the electron and muon related in any profound way, or are they simply unrelated particles…?[7]

One of Perl's primary goals had been to identify why there is a mass difference between the electron and muon, but the discovery of the tau did not change the fact that the Standard Model cannot account for particle masses. The inability to account for two charged leptons was replaced by the baffling reality that three charged leptons existed. As Perl later observed, "two sets of particles u, d, e^-, v_e, and c, s, μ^-, v_μ- seemed acceptable, a kind of doubling of particles. But why three sets? A question

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[6] Pickering, *Constructing Quarks*, 280-282.
[7] Martin L. Perl and Petros Rapidis, "The Search for Heav Leptons and Muon-Electron Differences," SLAC-PUB-1496, October 1974, reprinted in Perl, *Reflections on Experimental Science*, World Scientific Series in 20th Century Physics, Vol. 14 (River Edge: World Scientific, 1996), 52.

which to this day has no answer."[8] Ironically, the discovery of another charged heavy lepton raised new questions about the properties of these particles, so the electon-muon puzzle could have been renamed the 'electron-muon-tau puzzle' after Perl's discovery. Ultimately, finding the tau lepton contributed another puzzling dimension to the task of deciphering the mysterious code of fundamental particles.

Section 4.1.4 International Recognition: A Nobel Prize for Perl

The first evidence for the tau lepton was obtained in 1975, but Perl did not receive the happy news that he was to receive the Nobel Prize in Physics until 1995. On 11 October 1995, the Royal Swedish Academy of Sciences announced that it had decided to award Perl the 1995 Nobel Prize in Physics "for pioneering experimental contributions to lepton physics."[9] Perl shared the Nobel Prize with his good friend Frederick Reines, who was recognized for his detection of the neutrino.

Burton Richter and Samuel Ting had shared the Nobel Prize in Physics in 1976 for their joint discovery of the J/psi just two years earlier, yet twenty years elapsed before Perl was similarly recognized. Nobel Prize nomination information is classified for fifty years, so any explanation for the seemingly long period separating the discovery from the receipt of the Nobel Prize is highly speculative, but Perl has volunteered his own opinion about why the Nobel Prize Committee was reluctant to

[8] Perl, "Reflections on the Discovery of the Tau Lepton," Nobel Lecture, 08 Dec 1995, 187.
[9] Press Release 11 October 1995, Perl, Martin. *Les Prix Nobel: The Nobel Prizes 1995*, Editor Tore Frängsmyr, Nobel Foundation, Stockholm, 1996, available at http://nobelprize.org/nobel_prizes/physics/laureates/1995/perl-autobio.html, last accessed 22 Jun 2006.

award him the Nobel Prize in the years immediately following his discovery.[10] Basing his conjecture on the conservative nature of the Nobel Committee, Perl has suggested that “they rarely give it to one person, and they were waiting for another discovery to pair it with.”[11] Perl and Reines were in fact paired together for their “outstanding contributions to lepton physics,” but Reines’ work on the neutrino was carried out in the 1950s, so this is not a sufficient explanation.

As this study has shown, the discovery of the tau lepton was not immediately confirmed and accepted, which suggests another reason for the twenty-year delay in Perl’s receipt of the Nobel Prize. The Nobel Prize Committee may have been hesitant to declare that Perl should be honored with the most prestigious award in science when the scientific discovery in question was not fully verified until the late 1970s. It should be noted, however, that that Nobel Prizes have frequently been awarded to scientists decades after the prizewinning work was first published. For example, the

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[10] The literature on the factors influencing the awarding of Nobel Prizes is vast; for a discussion of the historic development of the Nobel Prize and the political and value-driven nature of the nomination, evaluation, and selection process, see Robert Marc Friedman, *The Politics of Excellence: Behind the Nobel Prize in Science* (New York: Times Books/Henry Holt, 2001); see also Harriet Zuckerman, *Scientific Elite: Nobel Laureates in the United States* (New York: Free Press, 1977); Elisabeth Crawford, *The Beginnings of the Nobel Institution: the Science Prizes, 1901-1915* (New York: Cambridge University Press, 1984); Crawford, *Nationalism and Internationalism in Science, 1880-1939: Four Studies of the Nobel Population* (New York: Cambridge University Press, 1992), especially 125-146 on elitism as a factor in the national concept of science. For further insight into the deliberation process, see the first published census of the Nobel Prize nominees and nominators in Chemistry and Physics, Elisabeth Crawford, J.L. Heilbron, and Rebecca Ullrich, ed., *The Nobel Population, 1901-1937: A Census of the Nominators and Nominees for the Prizes in Physics in Chemistry* (Berkeley: Office for History of Science and Technology, University of California, 1987).

[11] Personal communication from Martin Perl, SLAC, 28 Mar 2006.

1990 Nobel Prize in Physics was shared by Jerome I. Friedman, Henry W. Kendall, and Richard E. Taylor for their contributions to the quark model based on work carried out in the late 1960s, so a twenty-year delay was not unprecedented or unusual.[12] In fact, a lengthy delay between discovery and award was a common pattern in the history of awarding Nobel Prizes.[13] In comparison to Richter and Ting's immediate recognition for the J/psi discovery, Perl waited twenty long years to receive the Nobel Prize, yet he shared this experience with numerous other prizewinners before him.

Perl was also awarded the Wolf Prize in Physics in 1983, an honor instituted in Israel by the Wolf Foundation in 1978 to living scientists and artists for "achievements in the interest of mankind and friendly relations among peoples."[14] The award was bestowed jointly upon Perl and Leon Lederman for "their experimental discovery of unexpected new particles establishing a third generation of quarks and leptons," an honor they shared with previous particle physics prizewinner Giuseppe Occhialini.[15]

Section 4.2 The Bigger Picture: Insights from the Tau Lepton Discovery

Section 4.2.1 Big Science through Small Science at SLAC

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[12] The Nobel Prize home page, available at http://nobelprize.org/index.html, last accessed 09 Jul 2006.
[13] For example, see Mary Jo Nye, *Blackett: Physics, War, and Politics in the Twentieth Century* (Cambridge: Harvard University Press, 2004) for the circumstances of the late Nobel Prize awarded to British physicist Patrick Blackett.
[14] The Wolf Prize home page, available at http://www.wolffund.org.il/wolf7.html, last accessed 09 Jul 2006.
[15] Ibid.

The discovery of the tau lepton took place roughly between 1971 and 1978, a period in which experimental physicists at SLAC were able to carry out Big Science research while continuing to enjoy the benefits of small science practices. If the primary drawbacks of Big Science are judged to be a loss of scientific freedom, a loss of control over experimental research, and an overall detachment between the experimenter and the experiment, we can see that physicists at SLAC were generally spared from this experience. SLAC was thus a unique Big Science facility, but what accounted for its ability to retain traditional elements of scientific practice during the 1970s, and how did the character of research at SLAC change over time?

Lab director Pief Panofsky was responsible for much of the small science atmosphere at SLAC from the early 1960s through the late 1970s. Because Panofsky was concerned about the loss of control over research he had witnessed at LBL, he was careful to ensure that physicists would not suffer the same fate at SLAC. As a result, throughout its history, SLAC's government contracts have not imposed stifling limitations on the scope or direction of research. According to Panofsky, as a national facility rather than a national laboratory, "...there is *accountability* to the government but no program *control* by the government."[16] Thus the research carried out at SLAC was not significantly constrained by federal funding, and Panofsky further enabled physicists to pursue research problems of personal interest by approving all reasonable proposals. SLAC physicists Joe Ballam, Burton Richter, and Martin Perl agreed that

[16] W.K.H. Panofsky, "Big Physics and Small Physics at Stanford," *Sandstone and Tile, Stanford Historical Society 14* (1990), 1-7; 7.

Panofsky provided an open working environment in which scientific freedom was encouraged rather than stifled.[17]

The lack of rigid governmental control coupled with the scientific freedom to choose one's own work at SLAC meant that Perl was able to pursue the search for heavy leptons even when such an undertaking was not thought to yield fruitful results. In other words, Perl's discovery of the tau lepton was made possible by the small science environment of SLAC. If government funding had been contingent upon meticulously justifying each device integrated into the Mark I detector, Perl's addition of the external muon detector would most likely have been rejected in the original proposal. Without the external muon detector, searching for heavy leptons would have been entirely unfeasible.

The discovery of the tau lepton also reveals that physicists did not necessarily become increasingly disconnected from the hands-on aspects of experimental practice with the shift to Big Science. Panofsky's focus on developing a strong in-house research program at SLAC with on-site technical development meant that SLAC physicists were accustomed to taking an active role in all stages of experimental work. As we have seen, physicists directly participated in the process of drafting the Mark I detector proposal and constructing the Mark I at SPEAR. The Mark I later underwent several important modifications to facilitate the detection of electron-muon events, and these new additions were designed and built by physicists in the SLAC-LBL

[17] See Chapter 1, Section 1.5.4, p. 25-28.

collaboration, so physicists were not removed from a direct interface with experimental apparatus during this period.

Perl's calculations of the electron-muon event probabilities are also reminiscent of a pre-Big Science generation of experimental research. Perl and his colleagues relied on large mainframe computers to process the raw data, but detailed calculations were still done by hand, and these calculations proved critical to identifying the pair of heavy leptons buried in the data. As Panofsky observed, Perl's discovery represented a "classic example" of small science activity "...where a small dedicated group of individuals working in the traditional faculty and student pattern 'mine the computer tapes' in order to extricate the previously hidden information."[18] Even in a "large" collaboration, the hallmark of Big Science research, individuals were able to do independent work in much the same way as their counterparts working in small physics.[19] Although unquestionably established as a Big Science laboratory, the research environment at SLAC was a hybrid of Big and small science that flourished during the period of its most prolific particle discoveries in the 1970s.

This style of experimental work at SLAC was unique to the late 1960s and 1970s, however, since SLAC became a more typical Big Science facility in the early 1980s. One reason for this rather abrupt change was the necessity of abandoning the founding principles established by Panofsky. Panofsky had always wanted SLAC to remain a single-purpose laboratory devoted exclusively to high-energy physics, but

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[18] W.K.H. Panofsky, "Big Physics and Small Physics at Stanford," 4.
[19] The "large" collaboration refers to the SLAC-LBL collaboration that was composed of 20-30 members. See Chapter 2, Section 2.4.2, p. 25-26.

when the experimental successes at SLAC during the 1970s drew interest from outside fields, he was unable to maintain the original organization. Explaining this shift, Panofsky recalled:

> Our equipment proved extremely valuable for X ray physics and biology, molecular biology, solid-state physics, so we became multifunctional *driven by the technological opportunities*, not by purpose. Accelerator-based high-energy physics has become less important, and we're in a period of transition now. This started at the end of the 1970s as SLAC became in-demand with the SSRL [Stanford Synchotron Radiation Laboratory].[20]

Perl began to notice changes in the research environment at SLAC when the BaBar detector was built at SLAC in the 1980s, involving a massive collaboration consisting of 600 physicists and engineers from 75 institutions worldwide. Perl commented, "when we started BaBar, the B factory, I noticed things changing. Organization got internationalized, really Big Science, by necessity. I gradually became disenchanted."[21] SLAC essentially acquired the characteristics common to other Big Science facilities such as LBL beginning in the 1980s, becoming a multinational, multidisciplinary facility in which large collaborations were essential to conducting research on an ever-expanding scale.

Section 4.2.2 Exploring Scientific Communities

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[20] Personal communication from Pief Panofsky, SLAC, 29 Mar 06.
[21] Personal communication from Martin Perl, SLAC, 28 Mar 2006.

The discovery of the tau lepton provides an ideal opportunity to examine the interactions between the scientific communities of SLAC and LBL, since Perl's work was based on data from the SLAC-LBL Mark I collaboration. As discussed in Chapter 1, the image/logic distinctions at LBL and SLAC did indeed represent the dominant work carried out at each laboratory during this period, but the physicists at each lab did not strictly adhere to such rigid labels. Galison asserts that a deep tension existed between the image and logic traditions because pedagogical, technical, and epistemic factors prohibited physicists from embracing the 'opposing' tradition. According to Galison, logic physicists saw image physicists as inferior experimenters, while image physicists were suspicious of the logic tradition's selective use of data. From the evidence presented in Chapter 1, we can see that this representation of dueling image and logic traditions oversimplifies the dynamic nature of the scientific communities of LBL and SLAC. Even before the Mark I detector collaboration, both traditions were practiced at both laboratories, and physicists were not constrained by ideological commitment to a particular tradition. Reducing the defining features of SLAC and LBL to an image/logic divide fails to fully account for the versatility of individual physicists, who often crossed between the two traditions based on technological necessity. Surprisingly, although the technology-driven dependence of progress in science is one of the main themes of *Image and Logic*, for Galison, the necessity of technological innovation does not challenge the image/logic model. Yet we have seen that the decision to rely on either image or logic-based technology was generally not hampered by a preference for one type of technology over another, but was a function of which tradition was most appropriate for a given experiment. Therefore, the primary distinction between the scientific communities of LBL and SLAC was not the gulf between image and logic traditions, but the differences in the working environment at each laboratory.

This study has shown that LBL fit the typical Big Science profile while physicists at SLAC managed to practice Big Science while avoiding many of the disadvantages of large-scale scientific enterprise. Alvarez imparted a legacy of highly bureaucratized research within large, multidisciplinary groups at LBL, and physicists often experienced frustration over losing control of their research. By contrast, Panofsky fostered an open research environment consisting of small research groups. As a result, SLAC physicists generally felt comfortable proposing research programs of personal interest.

Despite these pronounced differences in laboratory culture, however, the two laboratories maintained a remarkably successful working relationship characterized by mutual respect throughout the Mark I collaboration. Each laboratory clearly possessed a distinct identity, but these communities were connected by their membership in the larger international high-energy physics community and thus shared many common research goals and interests. Competition was not a major issue for SLAC or LBL group members because the Mark I detector yielded a rich data set that provided ample opportunity for engaging in different types of research. From the ‘November Revolution’ to the discovery of the tau lepton, physicists made revolutionary contributions to particle physics at both labs.

Section 4.2.3 Priority and Discovery

Establishing priority for a new scientific discovery and confirming that the discovery has occurred is not a straightforward task for scientists, and the tau lepton discovery highlights several problematic questions concerning these fundamental elements of scientific practice. Scientific discoveries must be reproducible in particle physics, so announcing a new experimental finding is not taken seriously within the scientific community until it can be confirmed by outside sources. As the history of the tau lepton discovery reveals, the process of confirmation is highly variable. While the J/psi discovery was verified within days, it was several years before the tau lepton discovery gained widespread acceptance within the high-energy physics community.[22] As a result, there is no clear consensus on the precise date of the discovery. Most of the existing literature on the tau lepton refers to 1975 as the year of discovery based upon Perl's first analysis of the Mark I data. Perl and several of his colleagues, however, report the year of discovery as 1977 or even later, since outside confirmation continued to verify the tau's existence. Thus the tau lepton discovery reaffirms that

[22] The reluctance within the physics community to accept a new particle based upon entrenched conceptual views is not unique to the tau lepton discovery. See, for example, Norwood Russell Hanson's *The Concept of the Positron: A Philosophical Analysis* (New York: Cambridge University Press, 1963), especially 152-159, on the resistance to the positive electron hypothesis in the physics community of the 1930s. Just as the physics community of the 1970s was reluctant to accept the existence of a third charged lepton because doing so seemed to undermine the symmetry of the Standard Model theory, physicists in the 1930s found it difficult to envision a third fundamental particle in addition to the proton and the electron because it seemed to violate classical electrodynamics and elementary particle theory.

scientific discovery is usually a process rather than a moment in time that can easily be identified.

This history also provides insight into the often challenging task of establishing priority for a new discovery within a large collaboration. Scientists are strongly motivated to claim priority for scientific discoveries, which involves deciding upon an appropriate time to declare a new finding. Perl chose to prematurely publicize his interpretation of the Mark I data because he was unwilling to risk jeopardizing his chances of establishing priority. This controversial aspect of the tau lepton discovery, which has not been published in previous histories, raises important questions about the purpose of doing science. Do scientists pursue their work purely out of the desire to increase the body of scientific knowledge, or is scientific ambition characterized by the goal of achieving recognition? Historians recognize that a simple dichotomy cannot fully account for the multifaceted nature of scientific practice, and the motivation for individuals to pursue scientific work has been treated in numerous historical studies. For Perl, achieving priority was clearly more important than adhering to the shared standards of community behavior, and his actions certainly illustrate that establishing priority in science often takes precedence over personal or professional concerns.

Section 4.2.4 Shaping a Scientist: The Role of Pedagogy

Perl's quest for heavy leptons can be traced back to his graduate training under I.I. Rabi, and the history of the tau lepton reveals that there was a clear correlation between pedagogy and Perl's choices as a scientist. Rabi advised Perl to pursue high-energy physics, but importantly, to avoid "uncrowded" areas of physics.[23] This translated into an experimental philosophy that defined Perl's scientific decisions in such a way that he was committed to the search for heavy leptons, a decidedly uncrowded area of physics, for decades. In turn, Perl counseled his own graduate students to approach experimental physics in a similar way, which illustrates his belief that Rabi's recommendation promised great experimental success.[24] Perl also defied Rabi's principle of delaying publication until experimental results were carefully verified, however, when he chose to announce the discovery of a new particle in 1975. Thus Perl did not simply assimilate Rabi's teachings; he was selective about the suggestions he adopted and incorporated into his own scientific decision-making, so this work further highlights the important link between pedagogy and scientific practice recently explored by David Kaiser and others.[25]

Section 4.3 Suggestions for Further Study

This history has traced the discovery of a subatomic particle in the mid-1970s, but it has also been concerned with exploring several fundamental questions in the history of science. What do we mean by Big Science? When does discovery take place, and how do scientists establish priority for independent work within large

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[23] See Chapter 2, Section, 2.2 p. 9
[24] Ibid, p. 10.
[25] See David Kaiser, *Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics* (Chicago: University of Chicago Press, 2005); Kaiser, ed., *Pedagogy and the Practice of Science: Historical and Contemporary Perspectives* (Cambridge: MIT Press, 2005).

collaborations? I have attempted to provide insight into these questions through a critical analysis of the discovery of the tau lepton, but this thesis may also be a foundation for further historical investigation of related topics of interest.

One area of inquiry that warrants greater examination is Panofsky's role in facilitating the particle discoveries at SLAC through his directorship. Panofsky enjoyed a unique scientific career that ranged from working as a consultant for the Manhattan Project to planning and managing the world's largest particle accelerator. A recipient of numerous awards, including nine honorary doctorates, Panofsky is probably best known for his leadership abilities. As a hands-on director, Panofsky involved himself in every level of administration and fought to maintain SLAC's "single-purpose laboratory" status as long as possible. During the years of his directorship, 1961-1984, Panofsky oversaw the construction of numerous facilities and supervised research that led to several Nobel Prizes.[26] The archival sources consulted for this thesis reveal that Panofsky was skilled in political discourse as well, thus ensuring that SLAC would receive ongoing federal patronage.[27] A more detailed study

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[26] See the SLAC Archives and History website, available at http://www.slac.stanford.edu/history/nobel.shtml, last accessed 16 Jul 2006. The Nobel Prize-winning work carried out at SLAC under Panofsky's directorship includes the 1976 prize shared by Burton Richter of SLAC and Samuel Ting of MIT for the J/psi experiment, the 1990 prize shared by Richard E. Taylor of SLAC and Jerome Friedman and Henry Kendall of MIT for for contributions to the quark model, and Perl's 1995 Nobel Prize for the discovery of the tau lepton.

[27] For example, shortly after the J/psi discovery was announced, Panofsky seized the opportunity to immediately garner political favor and patronage. He sent a letter with a popularized account of the SLAC findings to the chairman of the U.S. Atomic Energy Commission, Dixy Lee Ray, and drafted similar letters to President Gerald Ford and Dr. John Neem of the U.S. Atomic Energy Commission thanking them for their appreciation of basic research in elementary particle physics. See Series IV: Stanford

of Panofsky's role as an institution-builder and administrator would shed further light on the scientific environment that supported the pivotal work in high-energy particle physics carried out at SLAC.

Another theme presented in this thesis that may be pursued at greater length in later studies concerns the recent trend emphasizing questions of pedagogy in the history of science. David Kaiser has argued that it is imperative to avoid "educational determinism," or the view that a simple causal link exists between learned and applied scientific practices.[28] I have suggested that Perl's scientific training shaped his development of a particular scientific style and thus the content of Perl's science was strongly influenced by pedagogical factors, yet the scope of this study does not permit a discussion that transcends educational determinism. Therefore, new avenues of research based on this work could expand upon the role of pedagogy in determining scientific content by examining the work of other physicists who trained under Rabi. Additionally, it would be worthwhile to assess the scientific styles of the generation of physicists who trained under Perl in order to further analyze the enduring nature of the skills and practices imparted by Rabi. In conclusion, this study examines many dimensions of scientific practice while leaving room for future historical investigation into the role of competition, community, and priority in the discovery of the tau lepton.

Linear Accelerator Center, Subseries I (Research Division, Groups A,B,C,D,E,F), Box 67, folder 2: SPEAR, New Particle, Papers and Correspondence, 1974-75.
[28] Kaiser, *Pedagogy and the Practice of Science,* 4.

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