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Megascience in Particle Physics: The Birth of an Experiment String at Fermilab

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ABSTRACT

During the 1970s and 1980s, the experimental particle physicists at large laboratories responded to the limited funding context of that time. Unlike the "big science" in the postwar decades, when funding for research appeared unlimited and many parameters grew exponentially, the even larger-scale "megascience" of the 1970s and on was shaped by competition for limited resources at host laboratories. Experimental programs increasingly took the form of long-lived (typically ten- to twenty-year) strings of related experiments, invisible institutions creating research traditions within the laboratory. Focusing on how a particular study of charmed particles (experiment E-516) at Fermi National Accelerator Laboratory gave rise to one of the laboratory's earliest experiment strings (E-516-691-769-791), we explore why such strings evolved and how they led to new research practices. Because these strings produced conflicts and ironies that threatened to undermine fundamental aspects of the research, the emergence of experiment strings can be viewed as a limiting process of large-scale research in the 1970s and 1980s.

KEY WORDS: big science, experiment, Fermilab, high-energy physics, megascience, particle physics, twentieth-century physics

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The following abbreviations are used: ACP, Advanced Computing Program; CERN, European Organization for Nuclear Research; E-, Experiment; FA, Fermilab Archives, Batavia, IL; FNAL, Fermi National Accelerator Laboratory; GeV, giga eV; NAL, National Accelerator Laboratory; NP, E. Thomas Nash Scientific Papers, Fermilab, Batavia, IL; P-, Proposal; PAC, Program Advisory Committee; QCD, quantum chromodynamics; SLIC, segmented liquid ionization calorimeter; SMD, silicon microstrip detector; TPL, Tagged Photon Laboratory; TPMS, Tagged Photon Magnetic Spectrometer; TeV, trillion eV. All interviews referred to can be found in the oral history interview collection (OHI) of FA, the Fermilab Archives.

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The term "big science" came into use after several writers used it in the early 1960s to refer to what at the time appeared a boundless growth of research.[1] Many historians of science have since examined aspects of big science, typically attributing the phenomenon to new government funding sources created in the United States during or shortly after the Second World War that channeled bountiful support into many areas of science. Particle physics grew very rapidly in the first two postwar decades, when this field was entering its scientific adulthood. As many historians of science (including Peter Galison, Daniel Kevles, Robert Seidel, Silvan Schweber, and Peter Westwick) have argued, the field benefited in those early Cold War years from the association of military advantage with the expertise of the corps of highly trained particle physicists. Even today much of the writing about big science in particle physics refers to the first two-and-a-half postwar decades.

Much less examined is the big science seen in the decades following 1970 when funding limitations shaped research in new ways. The trends were notable

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[1] Classic references include Derek J. De Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963); Alvin M. Weinberg "Impact of Large-Scale Science," *Science* 134 (1961), 161–64; and Alvin M. Weinberg, *Reflections on Big Science* (Cambridge, MA: MIT Press, 1967). Writers have used the term in many fields, from genomics to weapons research, whenever that field's parameters (e.g., size, cost, time spans, scientific intricacy, complexity of apparatus, or numbers of scientists) appear large. However, the term is most often applied to the early Cold War decades, when such parameters appeared to be increasing exponentially. For the present view on big science and many references, see Catherine Westfall, "Rethinking Big Science: Modest, Mezzo, Ground Science, and the Development of the Bevalac, 1971–1993," *Isis* 94 (2003): 30–56; Peter Galison and Bruce Hevly, eds., *Big Science: The Growth of Large-Scale Research* (Palo Alto, CA: Stanford University Press, 1992); James H. Capshew and Karen H. Rader, "Big Science: Price to the Present," *Osiris* 7 (1992): 3–25; Wesley Shrum, Joel Genuth, and Ivan Chompalov, *Structures of Scientific Collaboration* (Cambridge, MA: MIT Press, 2007), 1–7.

Historians do not agree when big science began. Historians of technology often point to the big science done circa 1900 in early industrial laboratories, such as General Electric or Du Pont, e.g., George Wise, *Willis R. Whitney, General Electric and the Origins of U.S. Industrial Research* (New York: Columbia University Press, 1985) or David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: Du Pont R&D, 1902–1980* (Cambridge: Cambridge University Press, 1988). In particle physics it is accepted that big science emerged in nuclear physics research in Berkeley during the 1930s at the time of Ernest Lawrence's pioneering cyclotrons: J. L. Heilbron and Robert Seidel, *Lawrence and His Laboratory: A History of the Lawrence Berkeley Laboratory*, vol. 1 (Berkeley: University of California Press, 1989). Weapons historians look later at laboratories organized during or just after the Second World War: L. Hoddeson, P. Henriksen, R. Meade, and C. Westfall, *Critical Assembly: A History of Los Alamos during the Oppenheimer Years, 1943–1945* (New York: Cambridge University Press, 1993). Historians of space science may see the late 1950s or 1960s as the creation years for that field: Robert W. Smith, Paul A. Hanle, Robert H. Kargon, and Joseph N. Tatarewicz, *The Space Telescope: A Study in Science, Technology, and Politics* (Cambridge: Cambridge University Press, 1993).

in particle physics, where the size, complexity, and cost of most experiments continued to grow even as researchers found themselves facing stiffer competition for resources. The struggle for high-energy physics experiments to grow in a limited funding environment affected the physicists' scientific practices in paradoxical ways, as we will discuss below. Their day-to-day activities changed so much that we refer to the bigger big science of the 1970s and 1980s with a new name, "megascience." This term has previously been used by other writers to simply mean very big science.[2] We use it in a more restricted sense, to refer to the changed big science seen from 1970 on, when funding no longer appeared limitless.

As the name implies, megascience is in some ways even bigger than the earlier big science—in the size of its groups, budgets, bureaucracy, timescales, and data sets.[3] But there is a crucial sense, as we argue, in which this megascience is actually smaller than the big science of the postwar decades. As the physicists worked to make their precious resources go farther, especially by conducting follow-up studies in which aspects of the work were reused or extended, their experiments were often connected with each other in strings of related studies whose continuities (e.g., in the physics or their apparatus) reflected long-lived (typically fifteen- to twenty-year) traditions.[4] The subsequent experiments in a

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[2] See, for example, Jeff Hughes, *The Manhattan Project: Big Science and the Atom Bomb* (New York: Columbia University Press, 2002).

[3] The growth in scale of detectors in the late 1970s produced an explosion in the number of electronic channels in the particle physics detectors. The UA1 detector at CERN, for example, had some 50,000 channels, while the CDF and DZero detectors at Fermilab and the SLD detector at SLAC each had over 100,000 channels. The ALEPH detector at the Large Electron-Positron Collider (LEP) at CERN had about 700,000 channels, while the SDC and GEM detectors proposed for the discontinued Superconducting Super Collider would have had as many as 50,000,000 channels.

[4] One of us (Bodnarczuk) pointed out the social structure that we refer to here as an experiment string in a March 1990 Fermilab internal publication, "The Social Structure of Experimental Strings at Fermilab; A Physics and Detector Driven Model," *Fermilab-Pub-91/63*, FA. See also Bodnarczuk, "Some Sociological Consequences of High-Energy Physicists' Development of the Standard Model," in *The Rise of the Standard Model: Particle Physics in the 1960s and 70s*, ed. Lillian Hoddeson, Laurie M. Brown, Michael Riordan, and Max Dresden (New York: Cambridge University Press, 1997), 84–93. A few other scholars subsequently picked up this notion in the course of studying multi-institutional collaborations at Fermilab and other laboratories, e.g., Joel Genuth, "Historical Analysis of Selected Experiments at U.S. Sites," *AIP Study of Multi-Institutional Collaborations: Phase I: High Energy Physics, Report 4* (New York: American Institute of Physics Press, 1992); Frederik Nebeker, "Strings of Experiments in High-Energy Physics: The Upsilon Experiment," *Historical Studies in the Physical and Biological Sciences* 25, no. 1 (1994): 137–64. For more about experiment strings at Fermilab, see Lillian Hoddeson, Adrienne Kolb, and Catherine Westfall, *Fermilab: Physics, the Frontier, and Megascience* (Chicago: University of Chicago Press, forthcoming), esp. chaps. 1, 11, and 12.

string were in some sense iterations of the original experiment, differing from it only in limited ways, e.g., a new aspect of the research problem, a modification of the apparatus, a new computing architecture, a new approach, or a new member of the collaboration. Forming invisible mini-institutions within the programs of large physics laboratories, such strings are among the most striking characteristics of megascience. Strings formed invisible mini-institutions within the laboratory.

Experiment strings can hardly be avoided in research areas that employ a large or expensive apparatus, such as a colliding-beam detector or a space telescope, which is intended to last at least for a generation. But when resources are limited, even experiments that in principle could be disassembled at the end of an experiment run tend to be extended through the formation of a string. At Fermi National Accelerator Laboratory (Fermilab), in Batavia, Illinois, the experimental program of the 1970s to the 1990s contained approximately a dozen experiment strings (the number depending on how one counts), most of them centered on expensive and complex detectors. By studying how in the early 1970s one large Fermilab physics experiment gave rise to one of the earliest experiment strings there, we seek insight into why such strings emerged so often at large laboratories after 1970 and how this phenomenon threatens to undermine certain fundamental aspects of large-scale research in high-energy particle physics.

The Fermilab experiment on which we focus, E-516, was a pioneering study of charmed particles (sometimes called "charm particles")—hadrons containing at least one "charm" quark, one of the six kinds of quarks in the theory of elementary particles known as the Standard Model.[5] This experiment along with its three follow-up experiments, E-691, E-769, and E-791, comprised a string, which, from the conception of E-516 through E-791's data analysis, spanned two decades (1971–92). In all four experiments, the beam of accelerated protons emerging from the laboratory's accelerator first struck a primary target located hundreds of feet upstream of the experiment hall known as the Tagged Photon Laboratory (TPL), the "stream" beginning at the accelerator, continuing on "down" through the TPL and into the experimental target and the detector, known as the Tagged Photon Magnetic Spectrometer (TPMS).

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[5] "Charm" quarks and "strange" quarks are in the middle generation of the particle physics theory known as the Standard Model. The other four quarks are: "up" and "down" quarks in the first generation; and "top" and "bottom" quarks in the third generation. For further discussion, see Hoddeson et al., *Standard Model* (ref. 4), 3–31.

The impact of the protons on the primary target created a secondary beam of particles, some of which were electrons. These electrons subsequently penetrated a thin metal foil just upstream of the TPL, producing a photon beam in the case of E-516 and E-691, and a hadron beam in the subsequent two experiments. Because the energy of the electron beam was measured ("tagged") with a particular energy for studying particle interactions of interest, the photon or hadron beam was also "tagged." Finally, a beam of these created photons or hadrons impinged on yet another target to produce the "charmed states" which were examined in the TPMS. The TPL and the TPMS were costly pieces of real estate; such experimental real estate will emerge as a central actor in this account of strings and megascience.

THE BIRTH OF EXPERIMENT 516

Our story of the birth of the E-516 string begins in the summer of 1971 at the National Accelerator Laboratory (NAL, later renamed Fermilab in 1974). Robert R. Wilson, Fermilab's founding director, was in the last stages of building the "Main Ring," the laboratory's original accelerator. Anticipating the start of experimental work using protons accelerated to high energy by the Main Ring, three groups of physicists submitted competing proposals to create the TPL. The laboratory assigned numbers to the three proposals, all prefaced by the letter "P" to indicate their proposal status. All three of the proposed experiments planned to examine one or more aspects of the photons to be produced in the TPL: P-25 proposed studying photon total cross-sections; P-144 aimed to study general features of photon-induced reactions in the 50 to 500 GeV region; and P-152 proposed studying production of the photon (i.e., photoproduction).

The influence of limited resources and a frugal philosophy concerning their use at NAL was immediately evident when NAL's management explained to the proposing groups that for budgetary reasons their three efforts would have to collaborate on a single TPL design that met the needs of all three teams. Samuel Ting, the spokesperson for P-144, promptly withdrew his proposal. The competition continued between P-25 and P-152, represented by their spokespersons, David Caldwell and Clem Heusch, respectively. When the laboratory approved both proposals, they became E-25 and E-152. Both now planned to "run" their experiments in the beam during a specified number of hours. When E-25 committed itself to building the beamline, the pipe in which the particle beam travels to their experiment, this group was awarded first use of the TPL while E-152 was awarded second use.

The experimental physicist E. Thomas Nash had worked at DESY, Hamburg's high-energy physics laboratory, with Ting, who sent Nash to NAL to help design P-144. When Ting withdrew P-144 from the TPL competition, Nash was out of a job. He decided to join NAL and become a member of E-25. While that experiment ran from 1971 to 1976, he proceeded to organize a new collaboration, which in time became E-516. Besides Nash, who would serve as spokesperson, the new group included George Luste from the University of Toronto, Rolly Morrison from the University of California at Santa Barbara, Jeff Appel from Columbia University, and Paul Mantsch from Fermilab. This team set as its initial goal to build a sophisticated spectrometer that could fully utilize the tagged photon beam in the TPL to study the production of charmed particles. This detector, the TPMS, would be the centerpiece not only of E-516, but of all three of this experiment's follow-up studies. To produce the charmed particles, Nash's team planned to shine a photon beam on a liquid hydrogen target. The computing architecture of the TPMS would let it select, record, reconstruct, and analyze charmed events. The aim was to elucidate such properties of charmed particles as production, cross-section, and mass. By early June 1976 the new team was ready to submit its "Letter of Intent" to the Fermilab directorate proposing the experiment.[6]

Even at the start, funding limitations influenced the experiment. To optimize its chances of receiving running time, Nash's group aligned itself with Wilson's well-known commitment to frugality in all aspects of the laboratory's activities.[7] The frugality extended to the use of computers, an essential data-gathering and analysis resource for any experiment employing a complex detector. Wilson ruled that NAL should offer experiments minimal computing facilities, not only "on-line" computing support, where the computers are physically connected to the experimental apparatus with cables, but "off-line" computing support, where the computers are not attached to the apparatus and are located elsewhere at the laboratory, or at another institution. For the

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[6] Thomas Nash to R. R. Wilson, E. L. Goldwasser, and the Fermilab Phyics Advisory Committee (PAC), 6 Jun 1976, NP.

[7] For a full explanation of Wilson's frugality philosophy, see Catherine Westfall and Lillian Hoddeson, "Thinking Small in Big Science," *Technology and Culture* 37, no. 3 (1996): 457–92.

scientists who were visiting the laboratory from other institutions, Wilson was adamant that the off-line computing support for these "users" should come from their home institutions. Respecting Wilson's limitation on computing support, the E-516 team planned not to take the typical "write-it-all-to-tape" approach, which gathered the most data but required more off-line computing support than the experiment expected. Instead, the group planned to reduce its data using an electronic trigger processor designed by Nash and Steve Bracker of Toronto. This device would turn the on-line recording system on only when the detector identified the signature of a charmed particle. Determined by the best available theory, the "diffraction model," this signature was a recoil proton moving at a certain angle with a particular momentum.[8] The reduced data recorded would then be sent to off-line computer programs, which would "reconstruct" the charmed particles.

On receiving the Nash group's "Proposal to Study Photoproduction of Final States of Mass Above 2.5 GeV with a Magnet Spectrometer in the Tagged Photon Lab," the Fermilab directorate assigned number 516 to the proposal and it then became known as P-516. The laboratory also agreed to give this experiment, if approved, first use of its new spectrometer. With its newly acquired status as a numbered Fermilab proposal, the P-516 collaboration could now recruit additional physicists to help plan the experiment and design its TPMS.

APPROVAL FOR E-516

By the standards of its day, the TPMS was an unusually complex particle detector. Designed to detect eight to twelve charmed particle decay products, it had some 7,000 points where particles would interact. These were distributed over six detection regions of the TPMS (Fig. 1): (1) the recoil detector; (2) drift

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[8] Before quantum chromodynamics (QCD), the diffraction model was the prevailing theory. It was based on the framework of "vector dominance" championed in the 1960s by Jun John Sakurai. This theory specified that the strong interaction between particles could be explained in terms of the exchange of vector mesons. See the proposal for P-516, "Proposal to Study Photoproduction of Final States of Mass Above 2.5 GeV with a Magnet Spectrometer in the Tagged Photon Lab, "p. 11, and the accompanying footnote. For more details of the initial beam design, see P. Davis, R. Morrison, T. Nash, J. Prentice, J. Cumalat, R. Egloff, G. Luste, and F. Murphy, "The Electron Beam Test of October–November, 1974," *Fermilab TM-535* (6 Dec 1974), FA.

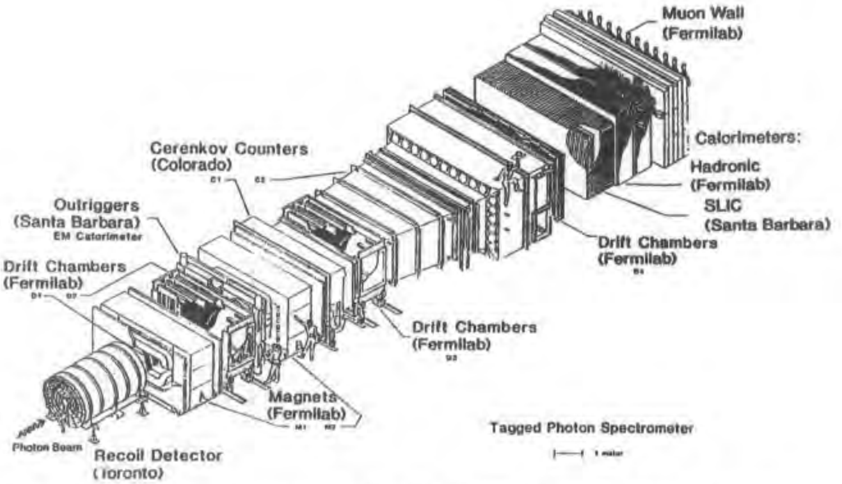

FIG. 1 The Tagged Photon Magnetic Spectrometer, 1977. *Source:* Courtesy of Fermilab Visual Media Services.

chambers; (3) Cerenkov counters; (4) electromagnetic calorimetors; (5) hadron calorimeter; and (6) the muon wall.[9] The recoil detector was used with the experimental target to trigger the spectrometer; the drift chambers were used to track charged particles by analyzing their trajectories and momenta; the Cerenkov counters were used to identify particle types by measuring their mass; the electromagnetic calorimeters, consisting of the segmented liquid ionization calorimeter (SLIC) and outriggers, measured the energy of photons and electrons; the hadron calorimeter measured the energy of uncharged hadrons; and the muon wall detected the presence and measured the trajectories of muons that penetrated the forward region of the spectrometer. The six regions, designed to function together as a unit, were each connected to a channel of electronics that fed into the trigger processor and then into the online computing system.

Because each region of the TPMS was built and maintained by one or more of the collaboration's member groups, the design of the detector shaped the collaboration's social structure, forming a physical organization chart. The University of Toronto group was in charge of the experimental target and

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[9] The TPMS spectrometer's design is discussed in J. Appel, D. Bartlett, S. Bracker et al., *The Tagged Photon Magnetic Spectrometer: Facility Design Report*, 9 May 1977, NP.

recoil detector; Toronto and Fermilab collaborated on the trigger processor; the Santa Barbara group was responsible for the electromagnetic calorimeter; the University of Colorado took charge of the Cerenkov counters; and Fermilab assumed the responsibility for the drift chambers, hadron calorimeter, and muon wall.

In presenting the proposal of their experiment, the P-516 collaboration took Wilson's frugality seriously, portraying the TPMS as a device built partly out of apparatus recycled from previous experiments. The collaboration planned to later ratchet up the laboratory's commitment of resources, including magnets, computing power, and beam time. Wilson likened this strategy for extracting resources to a "camel trying to get its nose under the tent."[10] In an effort to deemphasize the expense of the TPMS to the laboratory, its design report argued that it would be a general purpose facility intended for use in a variety of experiments by different groups. In practice this claim proved empty, for E-516 and its three follow-up experiments would hold on to this costly piece of experimental real estate, and the TPL, throughout the duration of the experiment string (except for one interlude when E-612 ran parasitically during E-516's scheduled operation).[11]

Wilson also insisted that his laboratory, including the experiments conducted there, meet the needs of the outside users. That was why he had pressed to have the laboratory named "*The* National Accelerator Laboratory" in the first place. He wanted to distinguish NAL from laboratories, such as Lawrence-Berkeley or Brookhaven, where users had complained that their needs were secondary to those of staff physicists. In line with Wilson's goal to make his laboratory respond to its users' interests, P-516 made sure that "in-house" NAL groups did not control the collaboration and that responsibilities were shared between in-house and outside user groups.[12] The outside users would benefit from their prominent status in the experiment back at their home universities, where the description of their responsibilities at NAL would contribute to the basis for determining their promotions and salaries.

NAL's powerful Program Advisory Committee (PAC) was the body of experts advising the director of the laboratory on whether an experiment deserved running time in the laboratory's proton beam, and, if so, how much time and

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[10] Robert R. Wilson, interview by Mark Bodnarczuk, 24 Sep 1992, FA.

[11] Thomas Nash, interview by Mark Bodnarczuk, 18 Jul 1990, FA.

[12] For more discussion of Wilson's democratic ideals see Hoddeson et al., *Fermilab* (ref. 4), chap. 5.

with what level of funding. This committee met twice each year, in June and November, to review the submitted experiment proposals, evaluating their importance, their plan, and how well their plan fit the available resources. The many considerations addressed by the committee included, for example, whether a collaboration involved enough people having the training and experience needed to bring the experiment to success.[13] Despite P-516's strenuous efforts to design an experiment suitable for running at NAL, Wilson's PAC did not consider P-516 ready for approval in November 1976, and he deferred the experiment. Recognizing, however, that once P-516 was approved, its team would need more time to build the TPMS than other groups typically needed to build their detectors, Wilson advised the collaboration to go ahead and design the TPMS while preparing to resubmit P-516 to the PAC the following June. By now the team consisted of fourteen physicists from four institutions.

During their long wait to run, P-516's members built prototypes for components of the TPMS. They waited longer than expected because in the tight funding environment of mid-1977 Wilson had to defer P-516 again after the June 1977 PAC meeting. The size and complexity of the TPMS simply pushed the limits of the laboratory's resources, no matter that the group had described its experiment as frugally designed. The caution with which Fermilab proceeded with its approval of P-516 was further illustrated when the collaboration requested another $1 million that month to build new analysis magnets. Wilson and his deputy director Edwin Goldwasser asked that they first build prototypes and offer "a few specific examples of the kinds of measurements which you anticipate making during the initial operating period of the facility." The laboratory wanted evidence that the collaboration had the expertise on its team to succeed, and it also was concerned that P-516 might have underestimated the cost of its project in the interest of being approved in the prevailing tight funding context.[14]

Considerable jockeying took place during the group's long wait to resubmit its proposal. It was not possible for the P-516 group to set up and test its apparatus in the TPL because E-25 was still running there. When E-25 completed its run, the next experiment in line, E-152, moved in. E-152 was a University of California at Santa Cruz group which had been promised only one run in the TPL. It was especially vulnerable to displacement because it was the last modern high-energy physics experiment to be mounted by a single institution. Clem

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[13] Edwin L. Goldwasser to Thomas Nash, 19 Nov 1976, NP.

[14] Edwin L. Goldwasser to Thomas Nash, 29 Jun 1977, NP.

Heusch, the experiment's spokesperson, recognized E-516 as a prodigious threat, for once the enormous TPMS had been installed in the TPL the larger experiment would be unlikely to leave the TPL soon. He therefore proceeded to oppose the displacement by angling for additional time and the possibility for a follow-up experiment for E-152. Early in June 1977, he informed NAL's directorate that he planned to request additional running time on completing his run in the TPL. Anticipating Wilson's question of what measurements were planned with the additional running time, Heusch said he could not define his physics goals until completing his initial run. He played coyly on the laboratory's (and P-516's) promise to make the TPMS available to other users, claiming "that as much equipment as is technically feasible would be shared among different efforts."[15]

P-516 was threatened too. If Heusch were granted his request before the end of his first run, P-516 would be delayed for several years. Nash quickly offered technical reasons why E-152's detectors would not meet P-516's needs.[16] But Heusch won the round. Fermilab approved Heusch's request, granting 1,800 hours of additional beam time to E-152. Such maneuvering for the chance to run in the beam of a large accelerator would be characteristic of the new megascience.

By October 3, 1977, Nash had addressed all the issues raised by the PAC and the Fermilab directorate.[17] P-516 was finally approved in November 1977, more than six years after the experiment had been conceived.[18] In approving E-516, Wilson apportioned 1,000 hours of beam time to the collaboration. The first 400 were specified for an initial shakedown and engineering run during the summer of 1979 for testing all the apparatus and showing that it would be possible to deliver the design flux of 10^6 photons to the experiment target set about a mile from the accelerator. The second run, the first to gather actual data, would begin in November 1979 and continue into early 1980. The third and final run would start in October 1980 and continue until the accelerator was shut down in June 1981. The elaborate process of reconstructing the particle events seen in the TPMS would take yet another year and a half. This long process, which all large experiments suffered in gaining approval to run at NAL, caused Wilson to break from his earlier policy of approving numerous small

[15] Clem Heusch to Edwin L. Goldwasser, 2 Jun 1977, NP.

[16] Thomas Nash to Edwin L. Goldwasser, 15 Jun 1977, NP.

[17] Thomas Nash to Edwin L. Goldwasser, 3 Oct 1977, NP.

[18] Edwin L. Goldwasser to Thomas Nash, 15 Nov 1977, NP.

"quick and dirty" experiments rather than a few larger experiments of longer duration. Over time, this change helped the laboratory compete more successfully with other laboratories that more readily funded larger experiments.[19] As for Heusch and E-152, Wilson's approval of E-516 was bad news. E-152 lost its place in the TPL.

E-516'S COMPUTING CRISIS

Employing the trigger processor meant that the E-516 group would gather less data and be in a position to adhere to Wilson's frugality in computer support. The team therefore requested only a modest computer, a PDP11-55, for on-line processing of the acquisition and monitoring of the experiment's data.[20] But in the weeks following the approval of E-516, Steve Bracker, who had worked with Nash on the design of the trigger processor, began worrying that the requested minimal computer might not be adequate for the experiment. Nash pressed John Peoples, the head of Fermilab's Research Division, for more computing resources, and Peoples passed the request on to Al Brenner, the longtime head of the Computing Department; who controlled the computing resources offered to experimenters. Not only did Brenner support Wilson's philosophy of frugality in computing, he also understood, as did Peoples, that it was typical for experiments with large computing needs to appeal to frugality in their proposals but then request more support after approval.[21] Brenner upgraded E-516's on-line computer from the PDP11-55 to a PDP11-55T, but the upgrade amounted only to adding a tape drive; it did not increase the processing power. That limitation distressed some members of the collaboration. In late December 1979, two senior members, Morrison and Uriel Nauenberg of Colorado, warned Leon M. Lederman, who on June 1, 1979 had become Fermilab's second director, that E-516 was in danger of failing because of its inadequate computer support.[22] No action was taken, however.

The computing issue would blow up to crisis proportions during the second half of 1980, after the end of E-516's second run. By this time the level of anxiety within the collaboration was elevated because E-516 had already used two-thirds of its allotment of beam time but had not recorded any charmed

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[19] Westfall, "Thinking Small" (ref. 7).

[20] "The On-line System for Experiment E-516: How Big Is It?," 11 May 1978, NP.

[21] Richard Carrigan, interview by Mark Bodnarczuk, 18 Aug 1992, FA.

[22] Rolly Morrison and Uriel Nauenberg to Leon L. Lederman, 26 Dec 1979, NP.

particles. The collaboration members had not expected immediate evidence of charmed particles, but they did not imagine they would not see *any* such particles. Where were the charmed particles that physicists at the Stanford Linear Accelerator Center and at Brookhaven National Laboratory saw in their detectors?

E-516's failure to detect charmed particles during its first two runs was a grave problem that deserved extended discussion by members of the collaboration. But in the social setting of the high-energy physics laboratory it was not prudent to discuss such a matter publicly, not even at collaboration meetings. The approval of additional funds is based largely on performance, and competing experiments always lie waiting to displace experiments that are not making good on their promises. In July 1980, Nash wrote a memo warning the collaboration about the dangers of "loose talk." "Credibility is destroyed by the public statement of incorrect or, more commonly, variable results," he explained. "It takes judgment and a thorough, open and continuing discussion within the group of any work to determine when a result has stabilized and is dependable. Until this point is reached it is essential that results—and difficulties in the analysis—not be discussed idly." Nash also reminded his team that the "funding and beam time . . . are in such short supply that administrators are looking for any excuse to cut back resources—even if already granted. Under these conditions a slight comment about [trouble with] a couple of bad channels in D1 returns as 'Their drift chambers don't even work. What do they need all that intensity for?'"[23]

The physicists who had worked at NAL during the early years of its experimental program recalled the embarrassment that many at the laboratory had felt when, in an episode referred to as "alternating currents," the results reported from E-1A, the neutral current experiment of David Cline, Alfred Mann, and Carlo Rubbia, varied publicly. E-1A was competing with CERN to discover neutral currents and had claimed this major discovery, but it subsequently retracted this claim, stating that neutral currents did not exist, only to issue yet a second retraction, announcing that neutral currents did exist after all.[24] The episode taught experimenters at Fermilab a hard lesson: let results stabilize

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[23] Thomas Nash to E-516 collaboration, 21 Jul 1980, NP.

[24] Peter Galison has highlighted the move from table-top-size spark chambers to the mammoth spark chambers with which Fermilab's E-1A searched for neutral currents; Galison, "How the First Neutral Current Experiments Ended," *Reviews of Modern Physics* 55 (1983): 477–509. See also Donald Perkins, "Gargamelle and the Discovery of Neutral Currents," in Hoddeson et al., *Standard Model* (ref. 4), 428–46.

before announcing them. At the same time, however, gaining additional resources was contingent on publishing results.

By early December 1980, E-516 saw itself "in a very real crisis," as stated in correspondence exchanged between members of the collaboration. On December 9, seven weeks into the experiment's final run, Nash sent Morrison a letter expressing his concern that "there is more than a zero chance that we will come up empty handed."[25] This letter followed more than five years of preparation and an immense financial investment. The pressure on E-516 to announce results heightened when Lederman set February 1, 1981 as the deadline for submitting experiment proposals for the laboratory's higher-energy Tevatron program, based on its new superconducting accelerator, known in that period as the Energy Doubler. This program was projected to begin in two years.[26] In the midst of what appeared to be a failing experiment, Nash and his collaborators had less than two months to design a follow-up experiment to E-516, or lose its place in the TPL. Thus in the same letter to Morrison, in which Nash described the crisis of E-516, he added: "I would like to assure you that I (with your and others' help and advice) can put together a respectable document that will serve to hold our place in line."[27] Proposing a follow-up experiment before completing one was another common feature of the new megascience; it was a way to hold on to experimental real estate at a large laboratory. But such a proposal is usually made only when a collaboration has data to support its request. E-516 lacked such data, still the group could not afford to lose its "place in line."

By this time the cause of E-516's problems was becoming clear to some members of the collaboration.[28] Charmed particles were in fact penetrating the TPMS, but they were not being recorded to the experiment's magnetic data tapes, because E-516's triggering specification of a recoil proton was based on an earlier theory, the diffraction model. Within the framework of the theory that succeeded the diffraction model, quantum chromodynamics (QCD), the signature of a recoil proton would not dominate.[29] With this understanding, Morrison, Appel, and others in the group proposed running the TPMS with a less-biased

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[25] Thomas Nash to Rolly Morrison, 9 Dec 1980, NP.

[26] Norman Gelfand to Thomas Nash, 25 Sep 1980, NP.

[27] Thomas Nash to Rolly Morrison, 9 Dec 1980, NP.

[28] Rolly Morrison, interview by Mark Bodnarczuk, 3 Jul 1990, FA.

[29] Thomas Nash to Edwin L. Goldwasser, 3 Oct 1977, NP. The evidence for this diffraction model came from experiments performed at energies below 60 GeV, but the physics was different when the beam came from the Fermilab Tevatron, whose 800 GeV proton beam caused the photon beam to have an energy of about 300 GeV.

trigger, and they pushed to eliminate E-516's recoil detector with its tight triggering parameters from their follow-up experiment proposal. If adequate off-line computing were available, the team would be able to detect the as-yet-unseen charmed particles using solid-state detectors placed directly downstream of the experimental target. Nash, however, having invested years developing the recoil detector and its trigger parameters, was not ready to give these up. When he argued to retain them in the proposal for the follow-up experiment, the dissenting members found themselves in an uncomfortable position, because they opposed the collaboration's most powerful member, its spokesperson, the one with the political power to overrule group consensus. During January 1981, the circulating drafts of the collaboration's proposal for its follow-up experiment reflected the group's fragmentation over its experiment plan.

By early February the team had worked out a compromise in which the TPMS would retain the recoil detector and trigger processor, but the proposal would also discuss QCD and include a plan to study both diffractive and nondiffractive photoproduction of charm.[30] The directorate then assigned number 691 to this revised proposal for a follow-up to E-516 and requested the preparation of a statement of impact to help determine how much new equipment and computing resources would be needed and what effect the experiment would have on the resources of other proposed Tevatron experiments.[31] The assignment of an official proposal number to E-516's first follow-up experiment marked the birth of an experiment string, among the first such strings in Fermilab's experimental program in that period. The collaboration remained in an awkward position, however, because of its division over whether to retain the recoil detector with its tight triggering assumptions in the design of P-691. Morrison, Appel, and an increasing number of the others were opposed while Nash was in favor. The members argued about this issue for months.

THE RECONSTRUCTION SHOOT-OUT

After an experiment runs at a high-energy physics laboratory, the focus typically shifts toward data analysis. For E-516 and its follow-up experiments, this analysis involved reviewing the tracking system software, which "reconstructs" the

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[30] Jeff Appel to Thomas Nash, 30 Jan 1981; and Thomas Nash to Norman Gelfand, 4 Feb 1981, both in NP, which includes a description of the proposal submitted to Fermilab's directorate and designated as P-691.

[31] Norman Gelfand to Thomas Nash, 12 Mar 1981, NP.

particle events from the data gathered by the detector. In June 1981, when Nash reported to the PAC on the status of E-516, he had to admit that the experiment's tracking system reconstruction programs were not ready. As an experienced experimenter, Lederman understood instantly that the experiment had not even begun its in-depth data analysis and was therefore far from publication. After the meeting Lederman informed Nash that he was going to defer P-691 and would not grant approval until he received a clear statement of E-516's results.

E-516's crisis over its lack of data was compounded by a social storm that arose in the collaboration during 1981 over the experiment's particle reconstruction software. For over two years, Uriel Nauenberg and John Bronstein, two senior members of the collaboration, had been writing independent particle reconstruction software packages for the experiment's tracking system. While high-energy physics experiments rarely find the resources to build two detector subsystems that perform identical functions, the collaborators often write more than one software package for a subsystem. Such programs reflect the styles of their authors; multiple versions can cross-check each other. Neither Nauenberg nor Bronstein knew, however, that a junior member of the collaboration, David Bintinger, had been quietly working on a third software reconstruction package for the tracking system. Bintinger had not wanted to offend his senior colleagues, but as he neared completion of his package he found himself in a delicate situation. He showed his work to Nash, who appreciated its merit and who, as the spokesperson, had to decide how to present Bintinger's effort to the rest of the collaboration. Nash planned how to compare the quality and efficiency of all three programs. But before Nash could complete his planning, the news of Bintinger's program leaked.

As expected, Nauenberg and Bronstein were disturbed that Bintinger had moved in on their territory, and that Nash apparently supported Bintinger. Nash responded to Nauenberg and Bronstein in a formal letter. "David's right to pursue the direction of his interests is, of course, the same right you both had when you started your reconstruction efforts." Nash proposed settling the issue with a "shoot-out."[32] He asked Bracker, who had substantial expertise in computing hardware and software and was not involved in the dispute, to serve as referee. Bracker refused to examine any of the programs before he had developed quantifiable criteria for comparing them. In an effort to eliminate infighting over the definition of the criteria, he recorded his daily progress on the

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[32] Thomas Nash to Uriel Nauenberg and John Bronstein, 24 Sep 1981, NP.

task in a computer file accessible to all members of the collaboration. Eventually all agreed that Bracker had drawn up an acceptable basis for comparing the three competing software packages. At the shoot-out the three programs were run and evaluated with identical data sets. When the smoke cleared in November 1981, only Bintinger's program was left standing.[33]

THE E-516/691 INTERFACE AND THE COMPUTING REVOLUTION

To everyone's relief, in the early months of 1982, owing to the tuning of existing hardware and software, as well as help from Bintinger's new reconstruction code, the experiment began to show preliminary evidence for charmed particle reactions. The evidence came at about the same time that the laboratory notified E-516 that it would have to submit its annual report. The collaboration was able to claim that reconstruction and analysis of the results from E-516 yielded "encouragement in the charm channels that are our primary interest." And it could also submit the E-516 program report in requesting approval for P-691. It was nevertheless apparent on May 6, 1982, when the team requested approval of P-691, that after a year of data analysis, E-516 had managed to reconstruct only some 15% of its total data sample of 20 million triggered events.[34]

Insufficient computer support deriving partly from Wilson's policy of frugality was largely to blame for E-516's inadequate data. Had there been enough off-line computer support, the experiment could have avoided working with a faulty triggering assumption. Similarly, with regard to the P-691 proposal, Morrison and Appel's suggestion to run the spectrometer with a less-biased trigger could be implemented only if low-cost, high-power computing were available—computing that allowed all the evidence for the charmed particles entering the spectrometer to be written to magnetic tape. Lederman recognized the importance of improving Fermilab's computing facilities, but such correcting of the situation would prove to be a time-consuming process.

The discussions that now ensued about E-516's inadequate computer support illustrate how the problems of a single almost failing experiment can bring major change to a large laboratory. Nash took the lead. Because of his involvement with the trigger processor for E-516, he was among the Fermilab physicists most disabled by the computing limitations. He devoted much thought to how best

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[33] Steve Bracker, "Bracker's New Track News," 3 Nov 1981, NP.

[34] Thomas Nash to Taiji Yamanouchi and Norman Gelfand, 6 May 1982, NP.

to correct this problem. On May 11, 1982, five days after submitting the E-516 progress report, Nash submitted another proposal to Fermilab, "A Program for Advanced Electronics Projects at Fermilab," explaining how experiments grow naturally until they reach a point at which their data is limited by "the amount of computing time they anticipate will be possible to squeeze out of the system." Clearly the more data an experiment is able to process, the more likely it is to succeed. Without referring to E-516, Nash explained the collaboration's difficulties in obtaining results in terms of the computing problems confronting all large Fermilab experiments. In almost a confession of E-516's near failure to detect the presence of charmed particles, he argued that E-516 was "up against the technological barrier of computing power limitations."[35]

As discussed by Robert Seidel in this issue of *HSNS*, Nash went on to propose a new program by which Fermilab could address the data problems of large physics experiments. The program focused on the development of multiple parallel-processing computing systems based on powerful, specially configured Intel 32-bit microprocessors. He explained that such systems, if properly configured, would offer experiments a thousand-fold increase in cost-effectiveness, for the reduction in cost and increase in computing power would allow experiments to use less-biased trigger assumptions and record more data to tape while at the same time accelerating the data analysis leading to publication. Nash's proposal, in time funded by Lederman's administration, offered a long-term solution to the laboratory's computing problems, but it did not address the serious problems E-516 faced as its members continued to analyze their data with less than adequate computing resources.

RISE OF THE E-516 EXPERIMENT STRING: THE BIRTH OF E-691

To argue for the support of P-691 based on E-516's too limited data remained a tricky business. Meanwhile the group within E-516 that opposed using the recoil detector grew. By the fall of 1982, a few members were exploring a new detector based on the silicon microstrip detector (SMD), a European technology invented between 1978 and 1980 by Robert Klanner. Allowing the simultaneous use of many more electronic detector channels, the SMD could measure particle tracks to an accuracy of 15 microns using fine-grained detectors of silicon mounted to form x, y, and z coordinates. One advantage of the

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[35] Thomas Nash, "A Program for Advanced Electronics Projects at Fermilab," 11 May 1982, NP.

new approach was its off-line filter for selecting events with short-lived particles, such as the D°, a charmed particle produced by the E-516 experiment.[36] But if the data were recorded to tape for later analysis, the new scheme would need even more on-line and off-line computing.

Lederman continued to view E-516's lack of published data with suspicion despite the team's excellent progress in developing the SMD. As it appeared that his continuing pressure on the group to publish its results had had no effect, in March 1983, more than two years after the end of E-516's final run, Lederman resorted to his most powerful incentive. He told E-516 to either publish its data or withdraw its proposal for P-691.[37] This ultimatum made the next three months a tense time. The group's annual progress report, due in May 1983, would be the basis for presenting P-691 at the critical PAC meeting held in June in Aspen, Colorado. Fortunately, after three years of mining their nearly barren data tapes, E-516 now had preliminary results to present. Nash asked for a brief extension of the May deadline to allow the collaboration to present its results in early June to the Friday afternoon "Wine and Cheese Seminar," Fermilab's joint experimental and theoretical physics seminar reserved for presenting the laboratory's most important results.

At this point, the E-516 collaboration had to change the way it worked. During the design, construction, and installation phases, the technical and financial demands of building and commissioning the spectrometer had forced members of the group to focus attention on the spectrometer region for which they had responsibility. Most members gained some knowledge of systems for which they had no direct responsibility, but they typically had only a rudimentary understanding of the entire detector. Now, as the collaboration approached publication, the team had to function as a whole, for reconstructing particle events drew on results from multiple regions of the detector. This process of bringing results together and coming to an agreement on what to publish intensified the interactions between groups, and the boundaries that had earlier modularized the collaboration became less rigid.

The pressure to publish the E-516 data was complicated by the need to proceed rapidly in planning P-691. With funding from Santa Barbara, the preliminary design for P-691 took shape under the leadership of Morrison and his postdoctoral fellow Paul Karchin. Soon another distinguished physicist joined

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[36] Paul Karchin, "Silicon Microstrip Detectors for the TPS," 18 Oct 1982, NP.

[37] Normal Gelfand to Thomas Nash, 3 Mar 1983, NP.

the effort. Morrison had, for some time, been encouraging Michael Witherell of Princeton to join the faculty of the University of California at Santa Barbara and work with E-516. After accepting, Witherell assumed the task of analyzing E-516's data tapes along with a graduate student, Don Summers, with whom Witherell had designed, constructed, and written the reconstruction program for E-516's segmented liquid ionization calorimeter (SLIC).[38]

The first piece of E-516's data that the group submitted for publication would appear in its paper on the decay of the D° particle. Kris Sliwa, a member of the group, presented this at the Friday Wine and Cheese Seminar on June 10, 1983. In his talk, "First Results from the Tagged Photon Spectrometer," Sliwa explained that analysis of the data was in progress and that more data would be announced soon.[39] The P-691 proponents hoped that Sliwa's presentation would ease Lederman's concerns and bring him to grant first stage (conditional) approval for the experiment. Hoping to assure Lederman that E-516's results would soon be published, the group submitted the overheads from Sliwa's talk with their design for P-691, which included hardware improvements in most systems and, despite some objections, E-516's trigger processor. Nash outlined the group's plan to use both the Santa Barbara group's silicon microstrip vertex detectors behind a short beryllium target and, perhaps simultaneously, its liquid hydrogen-filled recoil detector. Nash also revealed that Bintinger's charged track reconstruction program was being used to analyze E-516 data on the new Advanced Computing Program (ACP) resulting from his earlier computing proposal.[40] Although Lederman appreciated this convincing report, he informed Nash two weeks later that P-691's status would remain deferred until he saw concrete evidence that E-516's results would actually appear in a journal. But because Nash insisted that such publication was imminent, Lederman agreed to let him present the plans for P-691 at the November 1983 PAC meeting.[41] Meanwhile, in July, Witherell circulated drafts of his paper with Summers on the D°.[42] The plans for publishing E-516's results continued to be coupled with the designing of P-691.

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38. Don Summers, interview by Mark Bodnarkzuk, 19 Jul 1990, FA.

39. Kris Sliwa, "First Results from the Tagged Photon Spectrometer," overheads prepared for the Wine and Cheese Seminar on 10 Jun 1983, NP.

40. Thomas Nash to Leon Lederman, Taiji Yamanouchi, and Norman Gelfand, 13 Jun 1983; Thomas Nash to Taiji Yamanouchi and Norman Gelfand, 13 Jun 1983, all in NP; Thomas Nash, "Fermilab's Advanced Computer Program," 1984, *Fermilab Annual Report*, FA, 49–55.

41. Leon Lederman to Thomas Nash, 30 Jun 1983, NP.

42. Michael Witherell to the E-516 Collaboration, 24 Aug 1983, NP.

The members, most of them now back at their home institutions, could agree neither on the reconstruction program nor on the design of the new experiment. Multiple drafts were circulated without consensus while the scheduled November presentation to the PAC drew near.[43] By late summer 1983, the group's study of the small sample of charmed particles that had accumulated in the last three months of E-516's final data run had convinced everyone in the collaboration that charm was rarely produced diffractively and that the P-691 configuration should therefore not include the recoil detector. The new plan was supported by Karchin and Morrison's design of a beryllium target/SMD configuration, which would produce the same charmed particle event rate as their opposing group's hydrogen target scheme.[44] The group also agreed on an important change in the collaboration: that Witherell, rather than Nash, should act as the spokesperson for E-691. But they refrained from notifying Fermilab's management of this decision for fear that it might adversely effect the approval of P-691.

Witherell took responsibility for submitting E-516's results, "Study of the Decay $D^0 \rightarrow K^- \pi^+ \pi^0$" to the journal *Physical Review Letters*. On October 5, 1983, he set October 10 as the deadline for submitting the paper. "It has gone through six complete typings since it was put together in July," he wrote. He said he had tried to reach a consensus on all issues, but "I think it has hit the equilibrium point where for every change I make, I offend as many people as I satisfy." He decreed that from then on, "I will consider only changes that are typos, wrong numbers, or outrageous breaches of grammar inserted when the last changes were made."[45] Then he submitted the paper. Even before the paper was formally accepted, the collaboration informed Lederman that it would be ready to make its November presentation on P-691 to the PAC.[46] Nash was scheduled to present on November 10, 1983; the collaboration reassigned the task to Witherell. One week after Witherell's presentation, the three-times-deferred P-691 was granted "Stage I" (conditional) approval. Stage II (full) approval was contingent on Lederman's review of the readiness of the silicon microstrip detector (SMD).[47] Ten days later, Nash sent his formal letter to the

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43. "Inelastic and Elastic Photoproduction of J/Ψ (3097)," draft, 7 Sep 1983, NP.

44. Paul Karchin to the collaboration, handwritten note, 7 Sep 1983, NP.

45. Michael Witherell to E-516 Collaboration, 5 Oct 1983, NP; Donald J. Summers et al., "Study of the Decay $D^0 \rightarrow K^- \pi^+ \pi^0$ in High-Energy Photoproduction" *Physical Review Letters* 52, no. 6 (1984): 410–13, received 14 Oct 1983.

46. The TPS Collaboration to Taiji Yamanouchi, 15 Oct 1983, NP.

47. Leon Lederman to Thomas Nash, 17 Nov 1983, NP.

E-691 collaboration declaring the "rotation of spokespersons."[48] Nash would continue as a collaboration member, but the job of negotiating the problems that lay ahead would fall to Witherell.

THE E-516 STRING

The original E-516 experiment thus gave rise to an experiment string. It led not only to E-691, but to E-691's follow-up experiments, E-769 and then E-791. The four experiments in the string—E-516, 691, 769, 791—shared a number of structural components. In each, the beam of accelerated protons emerging from the Fermilab accelerator was used to produce either an incident photon or hadron beam. This beam of photons or hadrons would go on to interact in a target and create either photoproduced or hadronically produced charmed particles. E-691 went on to much subsequent success, obtaining 10,000 reconstructed photoproduced charmed particle events and the most precise measurements yet of charmed particle lifetimes. The proposal for E-769, the follow-up to E-691, was submitted to the laboratory in November 1985 and approved in December 1985. The experiment was completed in February 1988, having had a substantial time overlap with its predecessor, E-691. Using a beam of π and K mesons instead of a photon beam to study the charmed particles, E-769 would accumulate 6,000 reconstructed hadronically produced charmed events, the largest such sample in the world at that time. And the proposal for E-791, submitted to the laboratory in November 1987 and approved in June 1988, proceeded to analyze its data in 1992, accumulating 100,000 reconstructed hadronically produced charmed particles. The later experiments in the E-516 string thus greatly transcended the goals of the original experiment in the string, goals which in its time had been ambitious.

Numerous continuities in the E-516 string helped the laboratory economize on precious resources. The transforming of one experiment into another, even before the first had been completed, had made it possible for the collaboration to maintain possession of the TPL. Using the tagged photon beamline that the E-516 group had built for all four experiments removed the need, and hence the cost, for each of the subsequent experiments to design, construct, commission, and pay for a new beamline. With the TPMS as the basis for all three subsequent experiments in the string, a well-understood detector could be used

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[48] Thomas Nash to the E-691 collaboration, 28 Nov 1983, NP.

without expending the funds and time to perform the many tasks involved in commissioning a new spectrometer. Despite the struggle over reconfiguring E-516's trigger, the experiment's detector, the TPMS, showed remarkable structural continuity over the course of the string. And because most of the detector's subsystems remained the same through all four experiments, much of the software for reconstructing particle events could be reused with simple modifications of the code. The most significant difference between the detection apparatus of E-516 and the other experiments was that a silicon microstrip detector (SMD) replaced E-516's recoil detector and trigger processor; a simpler and less restrictive trigger could then be used because more off-line computing was available to handle the increased volume of recorded data. (E-691 used somewhat different hardware and software reconstruction packages, enabling it to more accurately study particle tracks and reconstruct the particle decays.)

The fact that E-769 and E-791 studied hadronic rather than photoproduced charmed particles had been planned as early as 1976, even before P-516 was approved, for during E-516's conception stage, plans had been made to later convert the original beam to transport pions. And pions were used in E-769's and E-791's studies of hadronically produced charmed particles.[49] By the time P-769 was submitted, it had been ten years since the design for converting the secondary beam to a pion beam had been established. But even using a hadron rather than a photon beam, the configuration for E-769 was so closely based on E-516's that the collaboration included a diagram of the E-516 configuration in P-769 and simply explained the modifications that had been made.[50] By the time of P-791, the design of the TPMS had become so well known to the Fermilab community that the proposal did not even include an image of it. E-691's ability to produce publishable physics results very shortly after the end of its data run was a direct result of the decision not to modify most of the components of the E-516 spectrometer. Although the TPMS did not become a facility for general use, as had originally been proposed, it did yield a productive and stable long-term program.

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[49] The earlier proposal was "Proposal to Study Photoproduction of Final States of Mass Above 2.5 GeV with a Magnetic Spectrometer in the Tagged Photon Lab," submitted 1 Oct 1976, NP.

[50] The difference between the two configurations included the addition of two banks of drift chambers to the tracking system, enabling greater redundancy in resolving the particle trajectories, and the upgrading of the on-line computing capability with a VAX11/780 for data monitoring, so that the PDP11/55, used in E-516 for both data aquisition and data monitoring, could focus entirely on acquiring data from the detector subsystems.

The experiment strings had a substantial social effect on the laboratory. With each iteration of the TPMS in the string, the laboratory could see its investment in the TPMS and E-516 bearing more fruit. The investment was seen as increasingly secure as the group developed a track record of success. Experiments in a long-standing string were more easily approved and were assigned higher priority than freestanding experiments. The later experiments in a string usually took less time to yield results, for by using a version of the same detector, the follow-up experiments could avoid spending time on the myriad of tasks surrounding the commissioning of a new spectrometer. In the competitive context of particle physics, the longevity of an experiment was a crucial resource for professors who needed access to high-energy beams to train graduate students. Strings also helped university-based physicists publish results more regularly and thus attract better graduate students. The accumulation of group expertise while doing an experiment became a resource for the follow-up experiments. Thus in the case of the E-516-691-769-791 string, which spanned two decades from conception to final publication, building the TPMS proved to be a good investment.

Wilson had celebrated risk as part of his frugality initiative, both to cut cost and for aesthetic reasons, but as larger, more complex, and more costly experimental facilities were needed during the Lederman era, from 1979 to 1989, the attitude toward taking risks shifted at Fermilab. Risk became something to avoid rather than to embrace, and strings helped to minimize risk by allowing long-term investments in most of the resources used in the experiments—e.g., protons, equipment, experimental halls, and people with relevant expertise. (Computers tended to be upgraded or replaced regularly and thus did not become a true long-term investment.)

Continuity in the physics goals of the experiments in the string also contributed to a unity of goal and approach of the experiments in the E-516 string. Rather than propose completely new physics measurements and risk rejection by the Fermilab directorate, the follow-up experiments focused on contributing to the continuous program of studying charmed particles. While each data set was used separately, the collaborations accumulated increasingly larger samples described by increasingly precise statistics, enabling more fine-grained measurements of charmed particles.[51] The experiment strings thus defined stable research traditions within the laboratory; in the case of the E-516 string, a tradition emerged for studying the production and behavior of charmed particles.

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[51] The first hadron experiment, E-769, actually demonstrated less charm than E-691, but it proved techniques that made hadron experiments more tractable.

The constitution of the groups also showed continuities, but for the E-516 string this was a less striking feature than in other Fermilab strings. Appel worked on all four of the experiments in the E-516 string, but Nash, Morrison, and Nauenberg left the string at some point. A few, like Summers, left and returned to the string, while others, like Witherell, joined at a later stage.

STRINGS AS A LIMITING PROCESS IN MEGASCIENCE

The continued growth of high-energy physics experiments in the 1970s and 1980s, in a context of limited resources, changed the way physicists worked, especially when their work was constrained by a philosophy of frugal use of its resources. Competitions arose between subgroups within the same experiment collaboration, as well as between different experiment groups. At stake were a variety of resources, from apparatus and working space to particle beam or computer support. In the case of E-516, the decision to conduct a high-statistics experiment in a limited computing economy almost caused E-516 to fail. The most dramatic outcome of these competitions was the formation of experiment strings as a way to retain or extend precious resources. By the time Lederman stepped down as Fermilab's director in 1989, experiment strings had become the relevant unit for research in Fermilab's experiment program.

The resources for experiments were negotiated in diverse economies within the laboratory. One economy was concerned with the protons that the laboratory made available to experimenters. The proton economy was one in which the demand always exceeded the supply. Once a team gained access to the proton beam at the end of a beam spigot, it set up a defensive posture in its effort to retain its access to the proton beam. Other resource economies at the laboratory centered on physics expertise, experiment real estate (both laboratory space and apparatus), computer support, and journal space. As a commodity, physics expertise tended to appreciate in value as the complexity of the physics and the experiments increased. A proper distribution of expertise was needed to design, fabricate, install, and operate both the apparatus and the computing systems.[52] And once a person had invested the time and effort to become

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[52] A study conducted by the High Energy Physics Advisory Panel (HEPAP) examining the United States research program in high-energy physics for the 1990s included a demographic study of "manpower considerations." "The U.S. High Energy Physics Research Program for the 1990s," HEPAP Subpanel Report DOE/ER-0453P, Washington, DC, Apr 1990, pp. 68ff, FA.

trained in a specific line of physics, it made sense to make this training serve him or her in related experiments.

Experimental real estate, including the experiment hall and the detectors, became an increasingly important subject of the wars within the laboratory, because the time needed to assemble and commission an experiment grew longer as detectors grew larger and more complex. Having invested the time, funds, and effort to develop and then erect a complex detector in an experiment hall, a group could not afford to give up this real estate. Thus when a group was awarded beam time it would move into an experiment hall with the explicit goal of performing an experiment and the implicit goal of not moving out for a long time. The size and complexity of the detectors began to shape the direction of the physics as well as the social structure of the field as the members of experiment groups matched their training and interests to the needs of the detector's subsystems. Physicists became typed according to the kinds of detectors with which they worked. Experiment proposals were judged, at least in part, by whether they could attract physicists or students having the right expertise for a particular experiment. This process even affected the rate of production of PhD candidates having the appropriate backgrounds for working on specialized aspects of experiments.

Computer facilities were a crucial resource, as our study illustrates. The more secure "write-it-all-to-tape" approach produced much more data but required immense off-line computing resources, which were simply not available in Wilson's frugally run laboratory. As computer resources were frequently updated, they had a less conservative influence than the other resources we have mentioned. Finally, the economy of journal space for published results played into the picture, because the laboratory saw publications as a return on its investment in the research. The laboratory director's power to approve or disapprove an experiment based on publication of previous work was a powerful management tool, and its influence was magnified by the fact that physicists recognized publications as the basis for being authorized for follow-up experiments and also for obtaining jobs, grants, raises, and promotions.

This study of how E-516 gave birth to an experiment string suggests that strings are the product of a tension felt by any large and expensive experiment conducted in a context where the resources are limited. This tension results from the opposition between a research effort's tendency to grow larger when adequately funded and the constraining influences that occur when resources are limited. With the possible exception of computing, all the resource economies of the laboratory contributed to the continuation of experiments beyond the duration they might have had in more bountiful times. While the particle physicists' desire to explore the frontiers of their subject propelled them to investigate new scientific territories, limitations on resources constrained their studies to a narrowing region of the frontier. The emergence of strings in the programs of experiment at large high-energy physics laboratories can thus be viewed as a limiting process in the evolution of large-scale research.[53] The experiment strings helped curb the unlimited growth of science that had concerned Derek J. De Solla Price, Alvin M. Weinberg, and others who wrote in the early 1960s about the worrisome implications of big science.

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[53] Similar continuities to those noted for the E-516 string at Fermilab can be seen in other strings at Fermilab, e.g., (E-82, 226, 425, 486, 584, 617, 731, 773, 799), (E-8, 361, 415, 440, 495, 505, 555, 619, 620, 756, 800), (E-21A, 262, 320, 356, 482, 616, 652, 701, 744, 770), (E-1A, 310), (E-537, 705, 771), (E-70, 288, 494, 605, 608, 772, 789), (E-36a, 186, 317, 381, 289), and (E-87, 358, 400, 401, 687, 831). At NAL's 15-foot bubble chamber, the major continuity seems to have been the chamber itself, used in (E-28A, 31A, 45A, 53A, 155, 172, 180, 202, 234, 341, 380, 388, 390, 502, 545, 546, 564, 632). Experiment strings emerged at other high-energy physics laboratories in the same time period as the E-516 string, e.g., the photoproduction experiments at CERN (NA1-1980 run, NA1-1983 run, NA14, NA14/2), as well as (WA4, WA57, WA58, and WA69).