The first large-scale application of superconductivity: The Fermilab energy doubler, 1972–1983

THE EMERGENCE IN the last half century of the research laboratory centered on a large technical facility presents new problems for the historian of science. The following study concerns one such problem: technological innovation in the new institutional context.[1] We focus

*Physics Department, University of Illinois, Urbana, Illinois, 61801, and Fermilab, Box 500, Batavia, Illinois, 60510. I thank the physicists, engineers and administrators who generously contributed their time and advice; May West and Adrienne Kolb, for archival and library assistance; and Francis Cole, William B. Fowler, J.L. Heilbron, Leon Lederman, Richard Lundy, John Ritchie Orr, Robert Seidel, Lee Teng, Alvin Tollestrup, Timothy Toohig, Robert R. Wilson and Ryuji Yamada for comments on preliminary drafts. All interviews referred to in this article were conducted by me between 1983 and 1985 and are deposited in the Milton G. White history of accelerators collection of the Fermilab archives.

The following abbreviations are used: AEC, Atomic Energy Commission; CERN, Conseil Européen pour la Recherche Nucléaire; *CL, Communications*, Physical Laboratory, Leiden University; DOE, U.S. Department of Energy; ERDA, U.S. Energy Research and Development Administration; ESCAR, Experimental Superconducting Accelerator Ring; FHC, Fermilab History Collection; GESSS, Group for European Superconducting Synchrotron Studies; KEK, Ko Enerugii Butsurigaku Kenkyusho; PMG, Project Management Group; *TM*, Fermilab technical memorandum; UPC, Underground Parameters Committee.

1. Several recent works touch on the problem. Peter Galison, "Bubble chambers and the experimental workplace," in O. Hannaway and P. Achinstein, eds., *Observation and experiment in modern physical science* (Cambridge, 1985), and "Bubbles, sparks and the post-war laboratory," in L. Brown, M. Dresden and L. Hoddeson, eds., *Pions to quarks: Particle physics in the 1950s* (New York and Cambridge, 1987); J.L. Heilbron, R.W. Seidel, and B.R. Wheaton, *Lawrence and his laboratory: Nuclear science at Berkeley, 1931–1961* (Berkeley, 1982); J. Krige and D. Pestre, "The choice of CERN's first large bubble chamber for the proton synchrotron (1957–1958), *HSPS, 16:2* (1986), 255–279; A. Needell, "Nuclear reactions and the founding of Brookhaven National Laboratory," *HSPS, 14:1* (1983), 93–122; R. Seidel, "A home for big science: The Atomic Energy Commission's laboratory system," *HSPS, 16:1* (1986), 135–175, and "Accelerating science: The postwar transformation of the Lawrence Radiation Laboratory," *HSPS, 13:2* (1983), 375–400; L. Hoddeson, "Establishing KEK in Japan and Fermilab in the U.S.: Internationalism, nationalism and high energy accelerators," *Social studies of science, 13* (1983), 1–48.

on the superconducting-magnet, high-energy particle accelerator pursued in the 1970s at seven large laboratories and achieved first at Fermi National Accelerator Laboratory (Fermilab). The Fermilab machine, known as the "Energy Doubler," and also as "The Energy Saver" and "The Tevatron,"[2] first delivered a 512 GeV beam in July 1983, and an 800GeV beam in February 1984, making it the highest energy accelerator in the world. The experimental high energy physics program at the Energy Doubler began late in 1983. By far the largest application of superconductivity—it employs over a thousand superconducting magnets in a circular tunnel four miles long—the Doubler is of particular scientific, technological, and historical interest. Study of its development offers insights into the interplay during the 1970s between the dreams and styles of individuals and the missions and motives of several subcommunities within and beyond the laboratory.

To identify factors crucial to the Doubler's completion and establish their relationship to the Fermilab setting, I will make comparisons with the ISABELLE superconducting accelerator project at Brookhaven National Laboratory, the Doubler's only technological competitor by 1978. In time, these comparisons should be refined on the basis of a more thorough historical analysis than has yet been made of the ISABELLE project, which played a pivotal role in the development of superconducting accelerators.

1. PREHISTORY

In 1913, two years after he had discovered superconductivity, Heike Kamerlingh Onnes wrote of its technological future: "This miniature coil may be the prototype of magnetic coils without iron by which in future much stronger magnetic fields may be realized than are at present reached in...the strongest electromagnets."[3] A further discovery by Kamerlingh Onnes and his associates dimmed the prospect of his envisaged economical 100,000 gauss superconducting

2. The names "Doubler" and "Energy Doubler" record the increase of energy of the Fermilab accelerator from 500 to approximately 1000 GeV (1 TeV); "Saver," which began to be used when ERDA began to support the machine, emphasizes the efficiency of the Doubler (the Doubler at 1 TeV uses half the energy of the original Fermilab accelerator operating at about 400 GeV); "Tevatron" proclaims arrival of accelerators in the range of one TeV.

3. H. Kamerlingh Onnes, "The sudden disappearance of the ordinary resistance of tin, and the supraconductive state of tin," *CL, 133d* (1913), 51–68, on 64; cf. Kamerlingh Onnes, "Report on research made in the Leiden cryogenic laboratory between the Second and Third International Congress of Refrigeration," *CL, suppl., 34b* (1913), 35–70, on 65, and P.F. Dahl, "Kamerlingh Onnes and the discovery of superconductivity: The Leyden years, 1911–1914," *HSPS, 15:1* (1984), 1–37.

magnets. They found that in lead superconductivity would disappear above a critical magnetic field of only a few hundred gauss. This is the dangerous and enigmatic phenomenon of the "quench," in which a superconductor suddenly returns to the normal state, releases its large stored energy, and sometimes melts itself. Quenching still challenged the builders of high energy superconducting magnets in the 1970s. A protective system to insure that Doubler magnets would not be destroyed when quenched was the object of a major research effort at Fermilab.

The road to high-field superconductors such as niobium nitride, niobium tin, and niobium titanium was opened during World War II by pioneering work in Germany. G. Aschermann, E. Friedrich, E. Justi and J. Kramer showed niobium nitride to be superconducting at 16.1° K, about the temperature of pumped liquid hydrogen.[4] Further work by many hands ultimately fulfilled Kamerlingh Onnes' hopes for superconducting magnets. Of particular importance was the experimental work by Berndt Matthias and John Hulm between 1949 and 1954. Around 1960, Hulm, Matthias, and J. Eugene Kuntzler built magnets out of high field superconductors at Bell Labs and thereby started a race to achieve even higher fields. In 1961 Kuntzler reported a critical field of 88 kilogauss, for niobium tin at 18° K, with average current exceeding 100,000 amperes.[5] Imbedding the superconducting alloy in high purity copper enabled solenoids to hold the high currents.[6]

High-field superconducting magnets began to impress particle accelerator builders during the late 1960s and early 1970s, when the 400–500 GeV proton synchrotron at the National Accelerator Laboratory (NAL, in 1974 renamed Fermilab) in Illinois and the Super Proton Synchrotron (SPS) at CERN were being designed. Leading high energy physicists were then concluding that frontier experiments in the 1980s and 1990s, for example a search for the W particle, would demand an energy or intensity of particle beams exceeding the capabilities of CERN and Fermilab. A few enterprising accelerator builders, including John Adams at CERN and Robert R. Wilson at NAL,

4. G. Aschermann et al., "Supraleitfähige Verbindungen mit extrem hohen Sprungtemperaturen," *Physikalische Zeitschrift, 42* (1941), 349–360.

5. J.K. Hulm, J.E. Kunzler, and B.J. Matthias, "The road to superconducting materials," *Physics today, 34:1* (1981), 34–43; J.K. Hulm, "Superconductivity research in the good old days," IEEE, *Transactions on Magnetism, MAG-19* (1983), 161–166; J.E. Kunzler, K.E. Buehler, F.S. Hsu, and J.H. Wernick, "Superconductivity in Nb_3 Sn at high current density in a magnetic field of 88 kgauss," *Physical review letters, 6* (1961), 89–91.

6. Solenoids have superconducting properties different from those of straight wires; W.B. Fowler, [History of the Energy Doubler], 21 Dec 1984 (FHC).

looked to superconductivity as an "elixir to rejuvenate old accelerators and open new vistas for the future."[7] Building large high-field superconducting magnets no longer appeared impossible; by then a few superconducting magnets had been operated successfully in bubble chamber detectors at the Argonne National Laboratory and at Brookhaven.[8]

In the early 1970s, three groups in Europe, three in the U.S., and one at KEK in Japan were studying the high-energy superconducting accelerator. Adams hoped that part of CERN's SPS could be made of superconducting magnets.[9] For this purpose, the Rutherford High Energy Laboratory in Great Britain, the Center for Nuclear Studies at Saclay in France, and the Karlsruhe Institute for Experimental Physics in West Germany formed the Group for European Superconducting Synchrotron Studies (GESSS). Each built several short (1 to 2 meter) prototype dipole magnets. But since CERN did not strongly support the idea of making part of the SPS superconducting, and since GESSS made slower progress than anticipated, the concept was eventually dropped. GESSS dissolved, having contributed substantially to the art of superconducting magnets.[10] The Japanese effort never entered the mainstream of research. As for Berkeley's Experimental Superconducting Accelerator Ring project (ESCAR), it contributed valuable research on dipole magnets and refrigeration, but came to an end in June 1978.[11] By this time, both the ISABELLE and Doubler projects were well underway. In July 1983, only weeks after the Doubler achieved its first accelerated beam, the DOE terminated the Colliding

7. R.R. Wilson, "The Tevatron," *Physics today, 30:10* (1977), 23–30.

8. Fowler (ref. 6). John Purcell built the superconducting coils for Argonne's 12-foot, and Fermilab's 15-foot bubble chambers, and some half dozen superconducting units in the range of 3 tesla for use as beam line magnets. A useful summary of the early history of superconducting magnets for high energy physics is included in P.J. Reardon, "High energy physics and applied superconductivity," IEEE, *Transactions on magnetism, MAG-13:1* (1977), 704–718.

9. J.B. Adams, "The European 300 GeV programme," in M.H. Blewett and N. Vogt-Nilson, Conference on High Energy Accelerators, *Proceedings*, eds. (Geneva, 1971), 25–30. Figure 2 of this article refers explicitly to the superconducting portion at 1000 GeV.

10. Peter Smith, "Superconducting synchrotron magnets—Present status," in Blewett and Vogt-Nilson (ref. 9), 35–46; P. Turowski, J.H. Coupland, and J. Perot, "Pulsed superconducting dipole magnets of the GESSS collaboration," International Conference on High Energy Accelerators, IX, *Proceedings* (Stanford, 1974), 175–178.

11. G.R. Lambertson, W.S. Gilbert, and J.B. Rechen, *Final report on the experimental superconducting synchrotron (ESCAR)* (Lawrence Berkeley Laboratory, report 8211, March 1979). ESCAR, which officially began work in July 1974 as a pilot project for research on superconducting accelerators, fell because it was judged to be too slow to assist the full-scale projects at Brookhaven and Fermilab; at the same time Brookhaven's Mark 5 prototype magnet appeared to be capable of reaching 5 tesla.

Beam Accelerator (CBA), into which ISABELLE had by then evolved, in favor of moving ahead with the proposed 20 TeV Superconducting Super Collider (SSC). Shortly before, ISABELLE/CBA had convincingly overcome its severe technological hurdles.

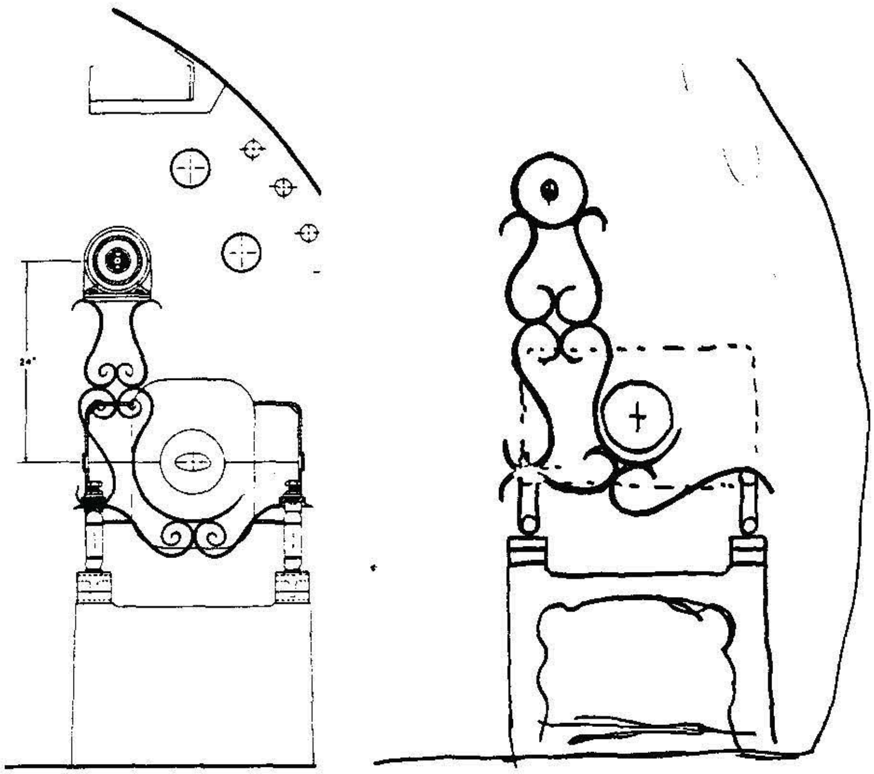
The early career of the Doubler, the only superconducting high-energy accelerator of the 1970s to be completed, may be divided into four periods. In the first, 1967–1972, a small group of scientists and engineers informally discussed the Doubler but did little research on it. In the second, 1972–1975, the Doubler effort was carried by a club of imaginative engineers and physicists, who found themselves severely limited by the Doubler's subordinate status to Fermilab's goal of completing the 200 GeV accelerator and starting its high energy physics program. Although adopted by the Laboratory, the Doubler project teetered technologically and financially on the edge of survival. During the third period, 1975–1978, intensive analytical research on the magnets and refrigeration system solved almost all the major technological problems. Nonetheless, the project had official authorization only for research and development, not for construction. In the fourth period, 1978–1983, operation, the Doubler became a large-scale professional, DOE-funded construction project. As the highest Fermilab priority, it was run by an army of scientists, engineers and technicians, who drew support from all sections of the Fermilab community.

2. A COOL TRILLION VOLTS

Early discussions, 1967-1972

The idea of achieving 1000 GeV using superconducting magnets was present from the beginning of Fermilab. At conferences held at Oak Brook (near Chicago) in the summer of 1967, physicists from numerous universities met with Fermilab's first director and principal designer, Robert Wilson, to help him plan the recently authorized NAL machine. Some physicists attending these meetings argued that an added ring of superconducting magnets in the main tunnel that housed the conventional magnets could be used to lengthen the time over which beam feeds out to experiments, or to store accelerated particles that could then collide with other particles emerging from the primary ring.[12] Most of those who met at Oak Brook considered these

12. Lawrence W. Jones, notes of discussions on superconducting magnets in relation to storage rings and colliding beams, O'Hare airport, 21–22 May 1967; A. van Steenbergen, "200–400 BeV accelerator summer study, National Accelerator Laboratory, Jul-Aug 1967," notes taken at the Oak Brook meetings (FHC); R.R. Wilson, "Colliding beams at Fermilab," *Workshop on producing high luminosity, high energy proton-antiproton colli-*



1 b 1 a

FIG. 1a Wilson's earliest sketch of a Doubler magnet, drawn using a pentel pen.

FIG. 1b A machine drawing based on Figure 1.

---

sions, 27–31 Mar 1978, Berkeley, Ca., (Lawrence Berkeley Laboratory Report 7574), 7–12, on 7; Wilson to Hoddeson, 11 Dec 1986 (FHC); author's conversations with Jones and Francis Cole.

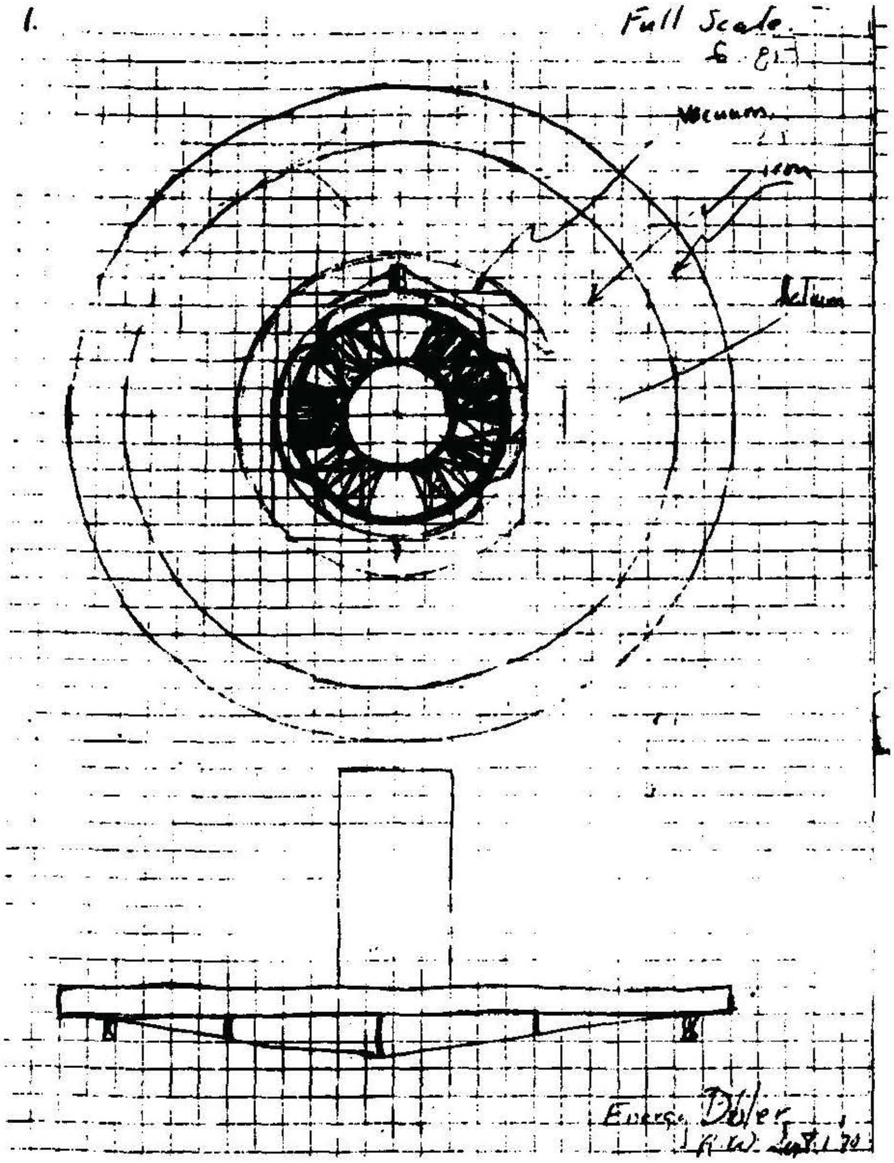


FIG. 2 Wilson's sketch in September 1970 of a Doubler magnet aperture of about one inch (soon to be increased to about 2¼ inch). "Full Scale" refers to the page of Wilson's notebook, 8 x 10½ inches.

concepts to be beyond immediate technological feasibility; designing and building a 200 GeV non-superconducting machine was the immediate job at hand. Following these meetings, Wilson issued an informal edict prohibiting active work on a superconducting accelerator until the main accelerator was functioning.[13]

Nevertheless, Wilson insisted that adequate space be left "free in the tunnel of the main ring so that a second magnet system can be placed just above (or beside or below) the original magnets. The idea then is to place [in the tunnel] a second ring of superconducting magnets...the proton energy could be raised to 1000 GeV."[14] Richard Lundy, who worked on the main ring and later on the Doubler magnets, recalls that "Bob [Wilson] did enforce the idea that space be left clear...(although) it was never exactly obvious what would go in there." Claus Rode, who worked on the Doubler's refrigeration system, recalls that this space "was sacred territory."[15] Evidence of further planning for the Doubler during the period of Wilson's edict is the hemispherical balls above the conventional magnets in the original ring. These balls were added in 1971 as feet for the stands of the superconducting magnets. However, they were never used since the Doubler was built below the ring of ordinary magnets.[16]

In March 1971, when NAL's main ring appeared to be about to operate, Wilson described the Doubler to the Joint Committee on Atomic Energy: "The idea is to take the protons out of the present magnet ring and then inject them into the new ring of superconducting magnets..., [one] piggy back upon the other....We could then double the energy....If the protons were transferred at 500 BeV, the energy could become 1,000 BeV." He fantasized multiplying the idea: "One could install one of these rings after another taking the beam from one to the next, doubling the energy each time." Wilson estimated that "because the bore of the new magnets would be so small, because no new tunnel or buildings would have to be constructed, we can hope to be able to build such a device for less than $20 million, possibly even for less than $10 million. All of these considerations, it must be emphasized, are based only on the most

13. R.R. Wilson to NAL Users, 8 Dec 1972 (FHC).

14. R.R. Wilson, "An energy doubler for the 500 BeV synchrotron," Sep 1970 (FHC); P.J. Reardon and B.P. Strauss, ed., "Some preliminary concepts about the proposed energy Doubler device for the 200/500 GeV proton accelerator at the United States National Accelerator Laboratory, Batavia, Illinois," TM-421, May 1973, I-1-2.

15. Interviews with Richard Lundy and Claus Rode.

16. R.R. Wilson, "1000 GeV in the NAL Synchrotron," 1 Apr 1970 (FHC). The balls are clearly visible between the first and second magnets on the left in figure 5 of H. Edwards, "The tevatron energy doubler: A superconducting accelerator," *Annual reviews of nuclear and particle science, 35* (1985), 605–660.

preliminary of studies."[17] Seven years later, this optimistic estimate of the Doubler's cost—but not Wilson's reminder of its "most preliminary" nature—would be recalled as officials at ERDA and DOE reviewed proposals requesting twice this level of funding. The reviewers overlooked the fact that the Doubler design of 1978/9 was far more conservative than that of the experimental machine that Wilson boldly proposed in 1971.

On July 19, 1971, the Director of the Division of Research of the AEC, Paul McDaniel, requested that Wilson and his staff "perform the necessary work in the coming fiscal year to clearly define the scope of this undertaking and to ascertain whether the inclusion of energy doublers can be achieved within the $250 million authorized for this project [the NAL]." The report of the Accelerator Conference of September 1971 contained a half dozen references to NAL's proposed Energy Doubler.[18] That summer, Ron Oram, Robert Sheldon and Bruce Strauss built the first prototype Doubler magnet, a beam-focusing quadrupole a half meter long.[19] The first period of Doubler development had produced plans and untested ideas, a single constructed prototype magnet, but no authorization—a level of activity that could not have resulted in an operating machine.

Start up, 1972–1975

A short proposal dated February 24, 1972, by William Fowler, who had been working on superconductors associated with bubble chambers, and Paul Reardon, head of the business section of Fermilab, set down the first sketch of a Doubler project. They suggested enlisting industrial support "to augment in-house capability to achieve rapid development of [a] prototype system," as was done at NAL with the booster synchrotron's magnets and the refrigeration system of the 15-foot bubble chamber. Fowler and Reardon also proposed magnet

17. United States, 92 Congress. First Session, Joint Committee on Atomic Energy, *Hearings on Physical space research, nuclear, and nuclear waste management programs*, March 9, 16, 17, 1971, Part 3 (Washington, 1971), 1191–1247, on 1205, 1206, 1214. The first published mention of the Doubler is R.R. Wilson, "Future options at NAL Batavia," in International Conference on High Energy Accelerators 27 Aug–2 Sep 1969, ed. A.I. Alikhanian, *Proceedings* (Yerevan, SSR, 1970), 103–105.

18. McDaniel to Wilson, 19 Jul 1971; Wilson "Annotations from the September 1971 accelerator conference report with references to NAL energy doubler;" both in FHC.

19. R. Sheldon and B. Strauss, ".5 meter prototype energy doubler-quadrupole magnet," Jul 1971 (FN 235 FHC), who conclude: "This paper has shown that a suitable doubler magnet can be produced at a not prohibitive cost, and that further development work in this area would probably be rewarding."

tests in the "Proto-main," an experimental area that replicated 200 feet of the main ring tunnel, and beam tests in the booster ring in two years time; and they outlined a possible Doubler management group.[20] Although it underestimated the complexity of building the Doubler, this first written estimate of its needs and organization started the project.

The achievement of NAL's first 200 GeV beam in March 1972 allowed Wilson to lift his edict with a clear conscience. On September 1 he established an informal working group to meet twice a week and consider technical questions of building a Doubler. The meetings were open to anyone interested, with special invitations to a number of experts. Those invited to the first meeting were Fowler, Reardon, Strauss, Donald Edwards, Henry Hinterberger, Ernie Malamud, Donald Miller, Rae Stiening, Lee Teng, Donald Young, and Boyce McDaniel. Wilson's handwritten notes for this meeting include a sketch of the Doubler's placement at four possible locations.[21] Throughout this period, the group expected that the Doubler would be built above the main ring trajectory, most likely hung from the tunnel's ceiling. Early in the next period, the Doubler magnets moved below the conventional magnets, where they have remained.

The small Doubler effort thus initiated produced two dipole model magnets (the 3-foot Mark I Pancake and the 1-foot Mark I Shell), both successfully tested in January 1973.[22] Refrigeration tests in the Proto-main showed that it would be practical to cool long strings of magnets to liquid helium temperatures, a demonstration of considerable importance.[23] The 400-foot helium pump loop, the work of Peter VanderArend, Stanley Stoy, and Donald Richied, employed an old liquefier from an industrial plant in Hightstown, New Jersey.

The Doubler reached its first level of certification in January 1973, when the group joined the Accelerator Division under Reardon. Wilson dominated the discussions and controlled all the decisions. By November 1, 1974, the group had 30 members, Wilson as director, and Fowler as associate director. Most of the participants had other primary responsibilities and worked on the Doubler in spare time.

20. W.B. Fowler and P.J. Reardon, "Preliminary suggestions for starting the construction of prototype magnets and refrigeration systems for the proposed Energy Doubler," 24 Feb 1972 (FHC).

21. "*Confirmation of meeting announcement*," 29 Aug 1972, and Wilson's notes for meeting of 1 Sep 1972, both FHC.

22. "Pancake" refers to a flat magnet winding all in one plane, while "shell" refers to a more complex three-dimensional pattern. Energy Doubler Magnet Evaluation Monthly Progress Reports, Nov 1972 and Jan 1973 (FHC). Construction began on magnet P6C, a one-meter long, 5-cm bore pancake, during January 1973.

23. TM-421 (ref. 14), I-3; interviews with Donald Edwards and Claus Rode.

The group aimed at development of magnets and refrigeration, the two crucial components; it did not concern itself with arranging them in a working accelerator. Reardon reported in May 1973 that "the design effort is proceeding at NAL with the full understanding that the Doubler is not a critical project and has a low priority and that other more important activities take precedence whenever the needs arise."[24]

Viewing the Doubler as an experiment in building a superconducting accelerator—an attempt to demonstrate the problems involved, rather than as a final instrument for high energy research—Wilson estimated that the Doubler would cost about $20 million, roughly the amount left over from the building of the original Fermilab machine. On December 21, 1972, the Deputy Director of Fermilab, Edwin Goldwasser, wrote to AEC Chicago Area Manager, Frederick Mattmueller, requesting authorization to initiate the Doubler project and to "proceed within the construction funds currently available to the Laboratory." He added: "Due to other project demands the Laboratory has not yet been able to mount the type of effort" needed "to define the scope of the energy doubler device proposed..., to establish its feasibility and to ascertain whether it could be provided within the $250 million authorized for the [original NAL] project." Wilson expected that Fermilab would complete its definitions in January 1973, and stand ready to "initiate a more detailed design effort." On February 13, 1973, the AEC approved NAL's request to construct and test superconducting magnets. Fermilab's Board of Trustees encouraged proceeding with the Energy Doubler and preparation of a financial plan for the disposition of the balance of Fermilab's construction funds. However, the AEC declined to authorize expenditure of this balance on the Doubler. In October 1974, it removed $6.5 million from the original authorization to insure compliance.[25]

Despite difficulties of organization and funding, Wilson's group made important technical decisions. They fixed the material of the beam tube and cryostat. Wilson chose a warm iron magnet design, in which the iron was put outside the cryostat, rather than inside, as in the cold iron design selected for the Brookhaven magnets. The warm iron design, although delivering a somewhat lower field, cost less because the magnets were physically smaller and therefore easier to cool and to support. Niobium-titanium was selected for the magnet's wire. Immediately, some 15,000 pounds of niobium-titanium were

24. Ibid., I–4.

25. Edwin L. Goldwasser to F.C. Mattmueller, 21 Dec 1972; Mattmueller to Wilson, 13 Feb 1973; Robert F. Bacher to Wilson, 14 Dec 1973, all in FHC, FN-263; Universities Research Association, Inc., *Annual report*, 1974, (FHC).

purchased. The group decided to manufacture the superconducting wire in-house, since the small companies then able to heat treat and draw wire from the alloy could not afford to purchase a sufficient quantity of the costly superconducting material to meet the Doubler's needs, and their one-year estimate for commercial production of the wire was too long a wait for the Doubler group. The result was to spearhead the subsequent commercial production of superconducting wire.[26] Another crucial decision in this period was to arrange the wire like the twisted multifilament-strand superconducting cable developed in the early 1970s at the Rutherford laboratory, rather than in the monolithic conductor initially tried by the Doubler group. The Rutherford cable, variations of which are now used in all superconducting accelerator magnets, reduced hysteresis and eddy current loss through geometric cancellation.[27] Other advantages were an appropriate ratio of superconductor to copper and good packing fractions. In the summer of 1971, the ISABELLE group had opted for braided cable, which many judge to have been a fatal flaw.

To minimize magnet "training"—the phenomenon of magnets, quenching at higher and higher fields as they are ramped up to their limiting field—and for greater economy, the Fermilab group decided to wind its magnet coils into the three-dimensional saddle (shell) configuration favored by Hinterberger, rather than into a flat pancake as favored by Reardon and magnet facility head William Hanson. The saddle winding, while more complicated to make than the pancake, offered the advantage of substantially better field quality. In August 1973, two identical 29-inch saddle-wound dipole magnets were tested. The early saddle magnet design, composed of four shells of Rutherford-like cable, showed poor training—many quenches were needed before they attained full field. To track down this problem, Wilson had a single shell model made. That produced quite a low field but trained in a few quenches. On that basis he gambled: He authorized going from four shells to two, at the same time increasing the widths of the cable from 1/4" to 3/8" by increasing the number of strands in the cable from 17 to 23. With help from other improvements, this gamble paid off and by the end of March 1975 the new "D series" magnets were working well. Meanwhile, a large effort went

26. Interview wtih Bruce Strauss. Author's conversations with Alvin Tollestrup and William Fowler.

27. G.E. Gallagher-Dagitt, "Superconductor cables for pulsed dipole magnets," Rutherford High Energy Laboratory Report M/A25, Feb 1974; M.N. Wilson, "Rate dependent magnetization in flat twisted superconducting cables, " Rutherford High Energy Laboratory Report M/A26, Sep 1972. For discussion of problems of the braided cable see interviews with Robert Palmer and Michael Tanenbaum.

into designing circuits to detect and protect against quenches.[28]

Early in 1973, VanderArend and Fowler developed a "counterflow" refrigeration scheme. Single-phase helium liquid from a reservoir would be compressed, pass through the magnets for some 120 meters, and flow through a valve, turning two-phase through partial boiling into vapor, and cooling the helium stream. The two-phase system of helium liquid plus vapor would then be returned. The idea was tested in the Proto-main, with transfer lines containing heaters to simulate magnet loads, there being no magnets yet to work with. It was planned to implement the system with two dozen independent refrigerators installed around the ring and running around the clock.[29] Litigation between the CTI and Airco companies over the contracts for a prototype refrigerator delayed testing of this costly concept until a radically new one was developed.

In this second period, the work was largely research and informal engineering of the magnets and cooling system. Crucial decisions about materials and methods set the course of development. However the project still had second priority and the magnitude of the technological problems was so great that they could only be overcome with total laboratory commitment.

All out R&D, 1975–1978

In its first years, the group had worked largely in the spirit of the inventive engineer. In an attempt to inject further scientific discipline, Wilson added Darrell Drickey, but late in 1974 Drickey took ill and died. Two other physicists in the group, Reardon and David Sutter, made plans to leave Fermilab for other projects. The Doubler group was thus in need of new blood in the spring of 1975.

Alvin Tollestrup, a physics professor on sabbatical from the California Institute of Technology, provided what was needed. He arrived at Fermilab in April 1975, expecting to work on the Doubler magnets for about nine months, but he eventually joined Fermilab's permanent staff. He "brought a whole new technical light to the project which absolutely saved it,"[30] an emphasis on precise experiments that

28. Energy Doubler Design Study, 31 Jan 1974, 6–13 (FHC); B.P. Strauss and D.F. Sutter, "Evaluation of matched dipoles," 11 Dec 1973, TM-456; William Hanson to Henry Hinterberger, 28 Mar 1975 (FHC); Wilson to Hoddeson, 11 Dec 1986 (FHC).

29. P.C. VanderArend and W.B. Fowler, "Superconducting accelerator magnet cooling systems," IEEE, *Transactions on nuclear science, NS-20:3* (June 1973), 119–121; W.B. Fowler and P. VanderArend, "Refrigeration system for the NAL Energy Doubler," CCI reports/Energy Doubler, Subcontract No. 8073, 26 Apr 1976 (FHC); interview with Donald Edwards; W.B. Fowler and P. VanderArend, "The cryogenic system for the proposed NAL energy doubler," TM-421 (ref. 14), VII-1 to VII-9.

30. Interview with George Biallas.

brought greater insight into the critical elements of pulsed superconducting magnets. Tollestrup "had a remarkable ability...to break every problem down to the sophomore level....And when he could present it that way, then everybody would understand it."[31]

By early 1976, Tollestrup had made two pivotal contributions. The first, a rigid "collar" support for the magnet windings illustrates a social mechanism typical of cooperative research in large laboratories—the injection by outsiders of useful criticisms and fresh perspectives into ongoing projects. Superconducting magnets require strong mechanical support to provide good field uniformity and to prevent movement owing to the strong electromagnetic forces on the current-carrying conductors. Even slight motion generates heat that can result in a quench. In late 1975, the magnet support was an endoskeletal porcelain ring supplemented on the outside by a series of spiral steel bands. The group, particularly Hanson, had developed this structure over several years and had a considerable investment in it.[32] The newcomer, Tollestrup, calculating the forces on the magnets, realized that the banding support could not work for operating full-length magnets. Early in 1976, he proposed a new rigid external clamp, which Hinterberger designed into a successful exoskeletal steel collar. The group showed that it was possible to shape ("keystone") the wire without degrading the cable so as to pack densely when wound into a Roman arch-like structure supported and pre-stressed from the outside.[33] Subsequently, Hinterberger's design evolved into the final clamshell: Collars placed loosely on the magnets at room temperature are squeezed under 3,000 pounds per inch of length so that even after cooling the windings remain compressed despite the electromagnetic forces. This pre-stressing insured good results for all values of the magnetic field. The first 22-foot collared magnet, completed in December 1976, illustrates the flexibility of the Doubler project. According to Livdahl, Wilson "was willing to try things that didn't have a high probability of working but were worth some effort." But if the idea then looked poor, he was "perfectly willing to drop it."[34] At ISABELLE, prior to 1980, either no outsider questioned the

31. Interview with Philip Livdahl.

32. The investment in the spiral banding followed a proposal for a horseshoe collar, which Wilson deemed too expensive; interviews with Reardon and Livdahl.

33. Tollestrup, research notebook, Dec 1985–Feb 1986 (FHC). Drawing 1620–MA–96120 in the Fermilab drawing files shows the first collar design, probably by Henry Hinterberger. Keystoning was settled by Hinterberger, Strauss and Hanson, Romeo Perin of CERN made the first suggestion of the Roman arch in September 1975. R. Perin to G. Biallis, 22 Mar 1977 (FHC); I thank A. Tollestrup for calling my attention to this letter.

34. Construction notes on the Doubler magnets, Book 3, and interview with Livdahl.

commitment that had been made to braided cable, or none made his objections tell.

The second of Tollestrup's initial contributions, to wrap the magnet wire with kapton insulation, illustrates the empirical approach often taken during this period. Tollestrup and his co-workers were concerned about electrical shorts arising not only from solder used to attach coils, but also from tiny metallic chips produced during the preparation of the wire. They developed a way to clean the wire ultrasonically, but the shorts remained. Tollestrup decided to try to insulate the wire using mylar or kapton material. To his surprise, the kapton not only removed the shorts but solved a far more important problem: The magnets when made of kapton-wrapped wire required significantly less training. The kapton apparently formed a cocoon around the wire, which insulated against heat generated by outside friction as the wire moved; furthermore, the intrinsic slipperiness of the kapton reduced scraping.[35] Former ISABELLE researchers have suggested that wrapping their braided wires with kapton could have solved the high-field training problems of their earlier magnets.

Fermilab's cooperative approach to research influenced the Doubler magnet program in other ways as well. One example is the program of testing short samples of superconducting cable and wire for properties such as magnetization, hysteresis, and alternating current loss. This program was conducted for a time by a group under Ryuji Yamada, operating outside the Doubler program and sometimes in competition with it. Yamada's team showed that the ac loss of short magnets increases abruptly owing to mechanical deformation, that the Rutherford cable is superior to the other alternatives, and that the addition of solder greatly increases eddy currents (one source of quenches). The upshot: no solder is used for bonding strands in the cable of Doubler magnets.[36] Yamada's work resulted in the "zebra" conductor, a 50–50 mixture of strands coated with "ebonal" (oxidized copper) and of strands coated with "stabrite" (silver-tin), a conductor favorable for stabilization and having low ac loss. Yamada's group also built a computerized data acquisition bank, enabling study of correlations between large numbers of magnets and short wire samples.

The most novel large-laboratory aspect of the Doubler program was an in-house assembly-line factory then manned by a staff of about ten reporting to the ingenious engineer and inventor, Hanson. Wilson had built this facility for the development and manufacture of the

35. First magnet book, E series, 31 Dec 1975 (FHC); interviews with Tollestrup and Tanenbaum.

36. Magnet Measuring Group, notebooks (FHC); interview with Ryuji Yamada.

original NAL main-ring magnets. However, these ordinary magnets could conveniently be manufactured outside the Laboratory; the major role of the facility was to allow economical assembly of the magnets on site. (The serious crisis in 1972 surrounding the shorting out of many of the conventional magnets, due to their extremely thin insulation, was overcome by improving technique at the magnet facility.) The facility would play a crucial role for the superconducting magnets: In-house production enabled the Doubler group to make quickly over 100 sample magnets, observe their behavior, and obtain rapid feedback in the trial and error process of design. To attempt to understand the physical underpinnings of each particular problem (as the ISABELLE team did), to communicate changes to the manufacturer, and to wait for industry to respond was too slow and too costly. Tollestrup observed: "In a quantitative sense, you can't walk up to a magnet and predict that this will take three quenches to train and predict very well where the quenches will be....The short ones would train in a few quenches and the question with the 22-foot ones was: Is it going to take 22 times as many quenches to train? It turned out remarkably enough that it didn't, that they all trained in just a few quenches. So that was an exciting discovery. But it had to come from full-scale magnets." Karl Koepke recalled: "It took us close to a couple of months to build a 22-foot magnet in the early days, whereas the one-footers would be completely done in a week. So what we did was we built the short magnets, tested different cable, different insulation schemes, different geometry, different pre-load, to see what might work, no matter how wild the idea was, since it didn't take much effort to test it....And then whenever a short magnet tested well...we would then try to incorporate those design features in the long magnets."[37] The first 20-foot Doubler dipole magnet was wound at the facility in January 1976. The length of dipoles would shortly grow to 22 feet. Finally, in a major episode discussed below, the dipoles would be cut down to their final length of 21 feet.

Once the design was fixed, the magnet facility provided optimal tooling for the manufacture of many identical units. Only if the Doubler magnets were built exactly alike could test data predict the behavior of magnet strings: Developing the tooling for creating 1,000 identical magnets had to be an integral part of the Doubler program. Lundy reflected: "properties of the magnet were determined in complicated ways by the kind of tooling it was built with, and how it was built. We didn't understand the inner workings of that process....And so it was important to develop tooling to make a good magnet and

37. Interviews with Tollestrup and Koepke.

then not to change the tooling; in fact, in an almost superstitious way, not to tamper with it until we understood the effects."[38] At no other laboratory were these arguments taken into consideration until well into the 1980s, when Brookhaven abandoned its earlier philosophy of thoroughly studying a few prototypes in order to develop a perfect magnet for industrial production.

Tollestrup insisted that only one factor at a time be varied in the magnets tested at the facility so that the causes of new behavior might be identified. Brookhaven researchers, studying only a few magnets, could not afford such control. They never learned why their famous Mark V prototype reached a field of 5 tesla in 1976 against its design specification of 4 tesla. This fluke led to a doubling of ISABELLE's design energy, an objective unattainable by the ISABELLE magnets as designed before 1980 .

Tollestrup's group, particularly Robert Flora, also devised new tools in their effort to solve analytically such tough engineering problems of magnet design as field quality, mechanical constraints, reproducibility, and training. An example is the "scissometer" for measurements on magnet coils immersed in the liquid helium-filled dewars then in use: Crossed epoxy-impregnated glass fibers—the scissors—attached to the immersed coil indicate by their displacement how far the coil moves when magnets are excited. Continuing quench studies explored four main sources: mechanical motion, eddy currents, wire quality, and cryogenic effects. Stiening initiated the use of microprocessors for quench production, work later developed by Flora.[39]

Strings of up to 16 Doubler magnets were tested in an aboveground area nicknamed "B-12" because of its location near the B-12 station of the main ring. This area had been set up in the spring of 1975 for testing strings of magnets in an environment simulating the main ring tunnel. The immediate motivation was an experiment of Helen Edwards and Claus Rode in May to study beam transport through two superconducting magnets, one 3 feet and the other 10 feet long, hung from the tunnel ceiling. The cooling system had been connected to the magnets by a long (80 foot) helium transfer line.[40] Peter Limon conducted the first tests there on a ten-foot magnet in February 1976. Next came two ten-foot magnets, two 22-foot magnets, and a four-dipole string. Installation and vacuum problems, cryogenic

38. Interviews with J. Richie Orr and Lundy.

39. Documentation of these developments is contained in the W.B. Fowler Doubler collection (FHC).

40. Helen Edwards, "The energy saver test and commissioning history," International Conference on High Energy Accelerators, XII, *Proceedings* (Batavia, Ill., 1983), 1–9, on 6; also Rode interview.

operation, quench protection in magnet strings, power systems, refrigeration and prototyping of control systems, all were studied in the simulated tunnel environment.

The hundreds of quench tests carried out at B-12 were nerve-wracking. The tense, isolated, and continuous effort burned out physicists unused to "working where the wolves are howling and the blizzards are blowing, and where there is this long string of magnets which at any moment might do terrible things."[41] Limon remembered a "tremendous hissing noise...like a 747 taking off....The room was just filled with fog, with little pieces of superinsulation floating by." Koepke recalled: "As soon as a quench occurred we'd hear this bang. And then...this roar for about 3 or 4 minutes as the helium exhausted through these vents. Anyone standing outside would see this white vapor cloud coming through the cracks in the building and the doors as if the building were on fire....I got used to walking through that vapor cloud by crawling against the wall to keep my bearings, because you can't see anything."[42] Such tests continued until late spring of 1981, when magnet installation in the main ring took priority.

Meanwhile, progress was made on the refrigeration system. Early in 1975, while litigation held up work on the original cooling concept, VanderArend and Fowler had a new idea: to build a central helium liquefier and place satellite refrigerators and dewars around the ring. Every twenty-four hours the satellite dewars would be refilled with liquid helium trucked around the ring from the central liquefier. By consuming liquid helium and turning it into gas, which returned to the gas storage tanks at central for reliquefaction, the satellites would be smaller than the refrigerator units originally planned. For the central liquefier, surplus compressor equipment was found at the Defense Department's Santa Susanna rocket engine test station near Los Angeles. The new scheme was to save $8 million. Later, trucking was replaced by a helium transfer line. The central helium liquefier building was completed in 1978. To cut costs further, the construction of satellite refrigerator engines was carried out at the Helix Company (earlier CTI), while the construction of other components, including heat exchangers and distribution dewars, was partly turned over to a local machine shop, Frank Meyers Co.[43]

41. Orr interview.

42. Interviews with Limon and Koepke.

43. W.B. Fowler, D. Drickey, P.J. Reardon, B.P. Strauss, and D.F. Sutter, "The Fermilab Energy Doubler, a two-year progress report," IEEE, *Transactions on nuclear science, NS-22:3* (June 1975), 1125–1128; C. Rode, D. Richied, S. Stoy and P.C. VanderArend, "Energy Doubler refrigeration system," ibid., NS-24 (1977), 1328–1330; interviews with Claus Rode and Moyses Kuchnir, and author's conversations with W.B. Fowler.

Funding remained uncertain throughout this third period while certain design changes increased the cost of the Doubler. In particular, calculations by theorist Lee Teng indicated that to use the Doubler in colliding-beam experiments required increasing the aperture of magnets from an elliptical opening 1¾ inches by 2½ inches to a circular opening 3 inches in diameter. (Richard Carrigan had made the first written suggestion about colliding beams in superconducting rings in the tunnel in 1971.) In November 1975, James Finks moved from the Fermilab business office directly into the Doubler group to handle an anticipated increase in contracts. This move was dictated by Wilson's policy to localize business dealings in the laboratory. By allowing scientists to make their own agreements with outside vendors, rather than delegating this chore to a central business office, Wilson expected to circumvent conflicts between projects having different priorities. This approach was highly effective.[44]

Several features characteristic of cooperative research in the large laboratory distinguished the work at the end of the Doubler's third period: the shifting of funds from larger established projects to smaller unproven ones, the setting up of parallel competitive groups, the use of the magnet facility for research and development. Most of the technological problems of the magnets and refrigeration were thereby solved. But the integration of the components into a working accelerator had not yet received much attention. Livdahl, who replaced Reardon as head of the accelerator and as a member of the Doubler group, recalls that until 1978 the many magnet problems made consideration of integration premature.[45]

Completion, 1978–1983

Between mid-1975 and late 1977 several alternatives were under discussion for new high energy facilities at Fermilab. Most involved colliding beams, for example, a small ring to accelerate electrons for collisions with protons from the main ring, or, as Carlo Rubbia passionately urged, a facility oriented toward antiproton-proton collisions in the main ring.[46] Wilson preferred to proceed with the Doubler,

44. Wilson, "Colliding beams" (ref. 12); interview with Lidvahl.

45. Interview with Livdahl.

46. Rubbia and co-workers submitted a proposal (No. 492) to construct an antiproton source at Fermilab and a second (No. 493) to search for new phenomena resulting from proton-proton and proton-antiproton collisions, but these were rejected by Fermilab's Program Advisory Committee in June 1976, *Fermilab proposals*, vol. 486–492 and vol. 493–515, held in the Fermilab library; *NALREP*, Jul 1976 (Batavia, 1976), 6–10, 47; *Fermilab research program workbook 1977* (Batavia, 1977), 90; Wilson, "Colliding beams" (ref. 12).

which he calculated would be much delayed if he followed Rubbia's advice and stopped to develop an antiproton source. CERN built Rubbia's colliding beam facility and with it eventually succeeded in finding both the W and Z particles, achievements recognized by the 1984 Nobel Prize in physics in which Rubbia shared.

Wilson now tried to obtain further funding for the Doubler. Writing on October 22, 1977 to James R. Schlesinger, the Secretary of Energy, about "the critical lack of support of the Fermi National Accelerator Laboratory," he complained that CERN, with a budget 2.5 times the size of Fermilab's, was threatening to "overwhelm us." The Doubler, he pointed out, offered America a chance to "regain the advantages of uniqueness" while reducing Fermilab's annual electric power bill by $5 million. The letter closed with these fateful words: "My own continued participation as Director will depend on a change in the laboratory's present dreary expectations for the future."[47] For Wilson, who over the last six years had invested his creative energy in the Doubler, the lack of funding for this machine meant becoming merely the administrator of a major laboratory.[48]

Wilson now had three strikes against him. For one, earlier he had given the impression that the Doubler technology was not yet ripe; in contrast, ISABELLE had been funded as a construction project.[49] Second, Wilson's conspicuously low budget for the Doubler made Washington fear that the project had not been thought through. Finally, Wilson's power base in Washington had diminished with the change from AEC to ERDA in January 1975, and ERDA to DOE in October 1978. Whereas the AEC had concentrated on nuclear-related technology, including accelerators, ERDA's mission covered the full range of the nation's energy needs. Accelerators were now competing with many other programs.[50]

47. R.R. Wilson to J.R. Schlesinger, 22 Oct 1977 (FHC). I thank Wilson for making this letter available to me.

48. R.R. Wilson, "My fight against team research," *Daedalus, 99:4* (1970), 1076–1087.

49. ERDA considered the ISABELLE project "the next natural step beyond the only other existing device of its kind, the ISR [intersecting storage ring] at CERN" and judged "that the technical level of achievement of the ISABELLE research and development program is such that the project is ready for funding." While recognizing extensive progress in the Doubler effort, the panel recommended authorization of only $12.8 million for Tevatron construction in FY 1979 and an additional $10 million for "the highest priority project(s)...that will enable Fermilab to begin the exploitation of the Tevatron for fixed target 1 TeV physics." ERDA, High Energy Physics Advisory Panel, Subpanel on New Facilities, *Report* (Washington, June 1977), 8–9, 12. At this point the ISABELLE researchers felt that they had triumphed over the Fermilab group.

50. Interview with Andrew Mravca.

On February 9, 1978, in an effort to obtain funding for the Doubler and Fermilab in general, Wilson threatened to resign as director. He had used this tactic successfully before. However, by this time he had antagonized so many people in powerful positions that the move failed, and in May 1978 the Fermilab Board of Trustees accepted his resignation. Wilson stepped down on July 17.

After an active search for a new director, the Board secured an elementary particle physicist from Columbia University, Leon Lederman, who reluctantly assumed the post of Director Designate in October 1978, while Livdahl continued as Acting Director. Lederman reviewed the options: either to terminate the Doubler and instead create a facility for carrying out proton-antiproton collisions in the main ring, as Rubbia had proposed, or complete what Wilson began and conduct higher energy fixed target experiments, proton-antiproton collisions, or proton-proton collisions in the Doubler.

Lederman organized a "shootout" that began at 9:00 a.m. on Armistice Day, 1978, and ended close to 3:00 a.m. the following day. "The ground rule was that everybody was to have all the time that was necessary for them to state their cases."[51] The result was a clear recognition that to accomplish one of the proton-antiproton options would require a major design effort over a long time, an effort comparable to completing the Doubler, which could be worked on simultaneously. Were Fermilab to build a proton-antiproton colliding facility based on the Doubler, the advantage over CERN's proton-antiproton collider (then at 540 GeV) would be a factor of four in the energy of collisions in the center of mass system, or sixteen in the laboratory. This large energy increase was the determining factor in Fermilab's decision to forego the race for the W particle in a collider based on the existing accelerator and to finish the Doubler.

Lederman then sought outside advice about the technology of the Doubler. He appointed "three wise men" to serve as a Doubler review committee: Matthew Sands of the University of California at Santa Cruz, Burton Richter of Stanford University, and Boyce McDaniel of Cornell, who served as chairman. This committee met three times between October 1978 and January 1979. They made useful technical suggestions and, most importantly, gave Lederman confidence in the Doubler's technological feasibility.[52]

In the meantime, a partial compromise with DOE was achieved in October 1978. DOE agreed to fund one-sixth of the Doubler—its "A sector"—as an R&D project. If this piece proved successful and

51. Interview with Livdahl.

52. Notes taken by Lederman's secretary, Judy Ward; Richter, Sands, and McDaniel to Lederman, 22 Jan 1979 and 18, 19 June 1979 (FHC).

Fermilab presented a realistic budget estimate, support at $38.9 million for the remaining five-sixths as a construction project was to follow.[53] Congress approved, but DOE held up the money pending its technical review of the project. Because of the many previous failures in superconducting accelerator technology, the newly formed DOE was very skeptical, as ERDA had been, about the Doubler's prospects. The magnet facility particularly provoked suspicion, since it had been customary to subcontract manufacture.[54] A test was proposed: Fermilab would have to demonstrate that 10 out of 12 full-scale Doubler magnets met all design specifications.

Meanwhile, a powerful internal force was gathering momentum. Wilson had excluded many of Fermilab's own professional accelerator community from participation in the Doubler. Furthermore, his method of design had created some tension. Livdahl recalls: "Wilson's style was not one of soliciting the opinions and ideas of other people in an open forum. He would tend to make design decisions on his own and then expect the people that were going to carry these decisions out to react to them with either better ideas or reasons why the design decisions weren't appropriate."[55] By the middle of 1978, Fermilab's accelerator professionals were pointing out that not enough effort had been devoted to designing a working accelerator. They were not disinterested. In 1978 the Doubler was seen as "the only show in town."[56]

In the year before Wilson's official resignation, a small group of accelerator physicists—including John Richie Orr, Helen and Donald Edwards, Thomas Collins, Rae Stiening, Lee Teng, Sho Ohnuma, Francis Cole, Alvin Tollestrup, David Johnson and Peter Koehler—had constituted itself as an informal committee, which in time adopted the name Tollestrup suggested of the "Underground Parameters Committee," or UPC, a name emphasizing the fact that Wilson had not formed the committee. After Wilson stepped down, Livdahl continued Wilson's practice of remaining separate from the UPC while supporting its activities.[57] The UPC's meetings were a

53. DOE, General Science and Basic Research-Operating Expenses and Capital Acquisition FY 1979, Congressional Budget Request, Construction Project Data Sheet; "DOE authorizes Fermilab to build superconducting accelerator," *Ferminews, 2:28* (12 Jul 1979), 1-3, on 2; T.R. James to John Deutsch, James S. Kane, and William Wallenmeyer, "Energy saver recommendations," 20 Mar 1977 (FHC).

54. Interview with Mravca.

55. Interview with Livdahl.

56. Interview with Helen Edwards, a leading member of this accelerator group. Her husband, Donald Edwards, in that same group, expressed himself similarly in an interview.

57. The best overview of the work of the UPC from late 1978 to early 1984 is contained in the 173 unpublished "UPC reports," available in the Fermilab publications

mechanism for promoting the involvement of the Laboratory's accelerator physicists in the design of the Doubler.

At one UPC meeting, Orr was asked to install in the main ring tunnel the Doubler magnets necessary for conducting a novel "sector" test invented by Collins. A 90 GeV beam was to be extracted from a point of the main ring, sent through a string of Doubler magnets, the " -sector" cooled by the first satellite refrigerator, and run into a beam "dump." This assignment proved to require a team of experts, since it depended on a suitable lattice for the magnets (their configuration in the accelerator). Orr asked Collins to help him to design the lattice. Helen Edwards, who had conducted the first beam tests at B-12, took on the problem of beam extraction. She also enlisted in the UPC her husband Donald, who had worked on the Doubler in its first years. Teng also joined. The A-sector test ran between December 1978 and February 1979. Thereafter, many similar string tests took place in the main ring tunnel while the work above-ground at B-12 was slowly phased out.

To help Helen Edwards prepare the official Doubler design report, the UPC began to list, outline, locate, and describe every element of the Doubler. Meetings were held almost daily during this effort.[58] One of many suggestions by the UPC was to add correction coils to the Doubler's dipoles in order to upgrade the good-field region for colliding-beam work. The coils would make precise alignment of the magnets unnecessary and could correct for undesirable shifts of the magnets. But adding these coils implied shortening each 22-foot dipole by one foot. [redacted] No decisive argument for shortening the dipoles was developed; 130 22-foot magnets had already been built, and no design existed yet for the correction coils. Still, the UPC continued to press for the correction coils. The dispute held up production and received much publicity in early 1979. Mulling over all the arguments, Lederman decided to accept the UPC specialists' suggestion to shorten the dipoles, and the 21-foot magnet program began in April 1979.[59] Lederman also accepted another UPC suggestion, to separate the trim-coil package and some diagnostic equipment from the quadrupole magnets, which again cost space in the lattice as well

office. The first crucial year of UPC meetings is, unfortunately, undocumented.

58. Lederman occasionally attended meetings. Wilson never did; that was part of the agreement. Interview with Orr.

59. A.V. Tollestrup, "Status of the Fermilab Tevatron Project," Apr 1979, TM-880 (FHC).

as money, but promised greater reliability. Not until the machine was turned on four years later was the wisdom of Lederman's decision widely appreciated.

Lederman's decision to shorten the dipoles was a turning point in the Doubler's development. By changing a long-standing feature to one favored by a group that had not designed the Doubler's components but would oversee their assembly into an accelerator, he at once brought the entire Laboratory behind the Doubler. Thereafter, the Doubler project was Fermilab's first priority. After May 1979 few essential design changes were made. On June 1, Lederman officially became Fermilab's second Director. That same month, Fermilab passed its ten-out-of-twelve magnet test at B-12.[60]

The Doubler was still without official authorization or funding. On June 26, 1978, Malcolm Browne presented Fermilab's case to the American public in a major article in the *New York Times*. He argued, as had Wilson eight months earlier in his letter to Schlesinger, that the United States was losing the competition in high energy physics to Europeans, who, he predicted, would soon discover the hypothetical W particle. Then the pitch: were Fermilab to develop its superconducting accelerator by 1983, it would have four times the energy of CERN in center-of-mass collisions and American high energy physics would be the 'first to enter an entirely new energy region.[61] Such accounts prepared the public for DOE's subsequent decision to fund the Doubler. Meetings of the High Energy Physics Advisory Panel (HEPAP) had prioritized construction projects: PEP at SLAC and ISABELLE at Brookhaven were first and second; the Doubler was third. The DOE review committee met in June and raised the earlier estimate of $38.9 million for the Doubler to $46.6 million to avoid the need to reauthorize the project were the $38.9 million exceeded by 25%. The official authorization came on July 5, 1979, and on July 23 Lederman and DOE officials Mattmueller and James E. Leiss met at DOE headquarters in Germantown, Maryland, to sign the official "Energy Saver Project Management Plan." The press emphasized that the Saver, as it dubbed the Doubler, would cut power usage at Fermilab by more than half, by $5 million of taxpayers' money every year.[62]

A successful working relationship now replaced the tensions between Fermilab and Washington, which had been exacerbated

60. "Summary of magnet data," 14 June 1979 (FHC).

61. Malcolm W. Browne, "Researchers race to find particle vital to atom theory," *New York Times*, 26 June 1979, C1.

62. Interview with Mravca; articles by Roberta Campbell in Batavia, Illinois, *Chronicle*, 18 Jul 1979; article in Naperville, Illinois, *Sun*, 18 Jul 1979, both FHC.

during the ERDA administration by Wilson's attacks on ISABELLE. Andrew Mravca, a member of DOE who had worked with the Laboratory a decade earlier, returned to Fermilab. His attitude was essentially that of his earlier boss at Fermilab, AEC official K.C. Brooks: "We're going to do whatever it takes to get this job to be a success...we'll roll over anybody who is an unnecessary or bureaucratic stumbling block." This attitude had disappeared with the change from AEC to ERDA to DOE. Indeed the DOE-Fermilab relationship had sunk particularly low point when Lederman became Director. Starting in the summer of 1979, the "two new boys on the block," Mravca and Lederman, worked hard to establish friendly relations by meeting frequently with DOE and keeping it informed of progress. For example, the laboratory met DOE's insistence on a detailed "project management plan," a mechanism then used in reactor development but not in high-energy physics, with the compromise of a Project Management Group (PMG) composed of everyone with major responsibility in the Doubler project. The meetings were held in an area called "the dungeon," which in 1972 had housed meetings to discuss the crisis of the shorting of many main ring magnets. Tollestrup acted as secretary and prepared detailed minutes that were sent to DOE.[63]

By summer 1979, the transition that began with Lederman's decision to shorten the dipole magnets was complete: Fermilab's accelerator professionals controlled the Doubler, and from this time Doubler jobs no longer lost automatically in priority conflicts with the existing accelerator. In July 1981, a significant administrative move dissolved any lingering friction between the Doubler and accelerator division: Orr, director of the Doubler project since early 1980, also became head of the accelerator, and Helen Edwards became his deputy. Meanwhile Lederman decided to enlarge the magnet facility and make it a more professional operation. In early June 1979, he replaced Hanson, whose health had become poor, with Lundy, a strong manager as well as a good scientist, who was then working with Fowler on cryogenics and magnet measuring.

Lundy solved the magnets' last major technological hurdle—the problem of inadequate stability in the orientation of the vertical magnetic field of the dipoles. The first twelve magnets in the 21-foot dipole series had been used in the test for DOE in 1979, and it was hoped, as Lundy recalled recently, that now "everything was known about the magnets, and all that remained was to build approximately 1,000 of them with no change."[64] However, certain of the tests had

63. Interviews with Mravca and Lundy.
64. Lundy interview.

not been carried out to the required precision. When Helen Edwards read the reports of more precise second measurements, she noted to her dismay that the orientation of the main field component inside the magnet yokes in the so-called vertical plane was not always the same from cooldown to cooldown. A small rotation had been noticed in September 1978 in the 22-foot magnets, but ignored.[65] The deviation Edwards recognized in the 21-foot magnets far exceeded that observed earlier. This problem stopped magnet production for several months while the magnet facility concentrated on building coils. Limon, who was on leave from Fermilab during 1979, recalls the response in Europe to Fermilab's problem: "People would come, and with enormous grins on their face—they were trying to restrain—would say, "Isn't it terrible what's happening at Fermilab?"[66]

The large variations of the vertical magnetic field's direction were traced to a change made in the cryostat design when the magnets were shortened. The space below the main ring magnets determined the maximum cross section of the superconducting magnets and accelerator considerations dictated a magnet aperture of 3 inches for passage of the beam. There was therefore little space left for the cryostat, which carried the cooling fluid and in which the insulating supports for the cold magnet also had to reside. The initial compromise configuration—32 sliding supports plus a single anchor—stretched the materials beyond safe engineering limits; the resultant movement of the cryostat when warmed up and cooled down knocked the magnets out of their vertical alignment. Furthermore, the magnet coil could not be centered accurately enough inside the warm iron.[67]

Diagnosis is not solution, however, and more than seven months after discovery of the problem it had not been licked.[68] Lundy then resolved the difficulty by brute force. He simply ruled that the cryostats would have four anchors instead of one.[69] The approach worked, although it increased the heat leak slightly and necessitated reworking of the magnets, which now required more refrigeration and more plumbing. The cryostat design had to be scrapped and redone, and approximately 100 magnets had to be remade. The centering problem was associated with the pre-stress put on the magnet support

65. Sho Ohnuma, "Field quality of dipoles in the tunnel," 11 Sep 1978, in R. Yamada, notebook, "Energy Doubler model magnets" A.V. Tollestrup in PMG, Minutes of meeting of 17 Jul 1979 (both FHC); interview with Orr.

66. Interview with Limon.

67. There were also problems with the anti-quench system and with the insulation in the quadrupoles; some 100 quadrupoles had to be taken out of their cryostats and reinserted. Lundy interview.

68. Tollestrup in PMG, Minutes of meeting of 26 Feb 1980 (FHC).

69. Tollestrup in PMG, Minutes of meeting of 18 Mar 1980 (FHC).

structures in order to insure that when the central magnet shrinks (some 20 mils) on cooling, sufficient force remains to center the coils in the iron. Especially when the magnet was warm, the pre-stress placed great force on the supports, which, to keep the heat leak low, were composed of a glass-epoxy composite; but this composite changes shape ("creeps") at room temperature, and a magnet maintained at room temperature therefore lost its pre-stress. Lundy came to the rescue with a "smart bolt," a support mechanism with a spring in it. The bolts cut down the applied force and decreased the creep. The smart bolts also reduced the quadrupole moment on the magnets almost to zero, since they could easily be moved until they induced just enough additional quadrupole moment to cancel the quadrupole moment in the core.

By July 1980, the magnet facility found satisfactory its measurements of the vertical field for magnets bolted according to Lundy's design. By September—fourteen months after the discovery of the vertical-plane problem—the design of the dipole magnets was finally fixed. Mravca recently reflected that had the DOE known about the problem of the rotating vertical plane field when they authorized the project, or "had it been uncovered during the review, there would have been a big question mark as to whether the project would have started....I doubt it would have.[70]

An important design principle in the Doubler was to develop small individually tested components and build up from these to the full ring. This approach enabled the Doubler to begin operating with essentially no commissioning time (most accelerators require about one year). The first piece of the Doubler to be installed was its A-sector, tested between January and June of 1982. (Due to the press of an upcoming 400 GeV program, there was time enough only to set up three-quarters of the sector.) The A-sector test of its cryogenics, power supplies, voltage-to-ground, quench protection, and pressure piping was a rip-roaring success.[71] The testers tried in every way to break the machine, and could not. "We ran it to the highest possible energies," Orr recalled. "We quenched the whole thing at once with the valves closed so we could get to the highest possible full ring quenches. We blew snow covers off the top of the relief stacks, the legend is as high as the [16-story] high-rise. We quenched the entire three-quarters of the sector with high voltage riding on top of the normally induced voltage on the magnets to see if we could cause electrical arcs....We couldn't break it."[72]

70. A.V. Tollestrup in PMG, Minutes of meetings of 30 Jul and 4 Sep 1980 (FHC); interview with Mravca.

71. H. Edwards (ref. 40).

72. Interview with Orr.

In June 1982, Fermilab's 400 GeV facility was turned off for the last time, marking the beginning of the final year of installing and completing the Doubler. Some installation had been performed during a shut-down in the summer of 1981 and for a month at the end of 1981. The full installation, led by Peter Limon, Thornton Murphy, and Larry Sauer, required many "tunnel rats" working simultaneously around the clock in the underground ring on electrical contracting, pipe fitting, leak checking, drilling, aligning, and connecting. All the activities were tightly interlaced. Murphy recalls that "in the first nine months of this effort, from June '82 until about January '83..., the installation effort was very frustrated by the problem of the magnet production schedule. We never knew exactly how many magnets would be available on what date to install." In time the magnet facility solved its production problems. Murphy coordinated activities in the tunnel, touring several times a week on a golf cart. He estimates that at the peak of activity as many as 200 people worked in the four-mile tunnel on a given day. "Their activities and travel patterns had to be understood to keep from getting a total traffic jam."[73] Fermilab met the temporary personnel crisis caused by installing the Doubler by shifting hands from elsewhere in the Laboratory. Only minor technical problems were encountered, for example, a high failure rate in vacuum seals, a problem soon corrected.

Among the less visible heros in the completion of the Doubler were Gerry Tool, who developed control and power systems, and Timothy Toohig, who coordinated the construction, seeing that service buildings, long runs of pipe and cable, utilities, cryogenics, and so forth, were built or modified before they were needed. He encountered Herculean problems in modifying the existing tunnel to accept the Doubler. Power lines were cut accidentally because drawings had not been updated. He kept colored progress charts on all aspects of the construction—on magnets, the central helium liquefier, refrigeration, power supplies, controls. By circulating updates of his charts and by asking questions—"where do you want the piping?," "where do you want the outlets?," "where do you want the ducts?"—Toohig often caused the construction jobs to drive the technical ones.[74]

The installation was finished in March 1983.[75] The first cooldown occurred in May, and the commissioning in June, Limon recalls, "went so smoothly as to be hardly worth mentioning." Lundy recently

73. Interview with Murphy.

74. Interview with Toohig.

75. P.F.M. Koehler to Saver PMG Members, 23 Mar 1983, "Minutes of Saver PMG Meeting on March 14, 1983," and attached Magnet Production Status Report (FHC); Edwards (ref. 40).

contrasted the Doubler "to the main ring, where a very spartan arrangement was tried. And when it wouldn't function, we were driven to add additional corrections and diagnostics, and then finally it worked. But here the needed corrections were available from the beginning and were just brought into play without any fuss or bother."[76]

The first beam injected into the Doubler, on June 2, 1983, made a full turn at 100 GeV. Two weeks later, the entire ring was ramped to over 500 GeV in that beam. On June 26, a coasting (non-accelerated) beam was achieved. On July 3, 1983, the first acceleration of beam in the Doubler to 512 GeV occurred. "We had a pleasant surprise. The machine was a lot better than we thought it would be."[77] A resonant extracted beam reached the switchyard area on August 12 at 700 GeV. That month ISABELLE was terminated.[78] Experiments using the Doubler began in October and a further record of 800 GeV was set on February 16, 1984. The Fermilab accelerator had become the Doubler.

3. CONCLUSION

When the Doubler was conceived, the technology necessary for success was not ripe; to complete the machine by late 1983 required a tour de force, possible perhaps only at Fermilab. We can identify ten crucial and characteristic factors: (1) Wilson's modern variation of the Edison approach of making many empirical attempts based on partial understanding, rather than a few based on more thorough understanding, with constant readiness to change suddenly to a new approach; (2) Tollestrup's modification of this approach—to vary only one factor at a time so as to be able to understand the effect of every change; (3) the on-site magnet facility, enabling rapid feedback in the R&D and in-house control of the tooling for construction of many identical models; (4) administrative unification of the Fermilab accelerator and Doubler managements in 1979, coupled with assignment of first priority to the Doubler project, which made available a large work force for any critical problem; (5) strong scientific leadership by the Laboratory director and his deputies, with the understanding that management and scientific skills as well as engineering skills are essential in a technological project as large as the Doubler; (6) insistence that researchers play a direct role, in collaboration with the business office, in

76. Interviews with Limon and Lundy.
77. Interview with Orr; Edwards (ref. 16).
78. HEPAP, Subpanel on New Facilities for the U.S. High Energy Physics Program, *Report* (DOE, Office of Energy Research Division of High Energy Physics, Jul 1983).

dealing with outside vendors; (7) taking a multiplicity of approaches to problems; (8) having frequent informal meetings to sustain communication; (9) viewing, from 1978, the magnets and other components as part of a total accelerator; (10) receiving, from 1979, strong support from the funding agency. The Doubler project differed from the other attempts to build a superconducting accelerator in the 1970s by a strong administrative commitment to the project by the laboratory (a commitment not fully made at Fermilab until mid-1979), a willingness to invest in the project before it was ripe technologically, a characteristic combination of cut-and-try engineering and thorough scientific research, and an ability to shift gears quickly at any stage.

ISABELLE's premature end is commonly attributed to individual technological errors such as the use of braided cable, but the Doubler too had its share of mistakes, for example, banding of the original magnets, insufficient anchorage of the cryostats, inadequate correction elements. The major difference between the Doubler and ISABELLE was in their organizations. All but the last of the ten factors listed above as crucial to the Doubler's success were missing from the ISABELLE project before its reorganization in the early 1980s. The ISABELLE approach of aiming at the perfect magnet through careful building and study of a small number of prototypes was diametrically opposed to the method employed at Fermilab based on rapid production and testing of large numbers of partially understood prototypes. (In 1977–1978 Fermilab built ten times as many magnets as Brookhaven did.) The Fermilab project was so organized that errors could be caught and corrected readily. Furthermore, although all of the early superconducting efforts, including those at Fermilab and Brookhaven, were begun by men distinguished for their inventive talent, from about 1979 Fermilab recognized that, in addition to inventive and scientific talent, managerial skill was essential to bringing the superconducting accelerator into being. The Doubler project was particularly distinguished by the degree of administrative commitment from the Laboratory from mid-1979. Fermilab realized two years earlier than Brookhaven that to complete the new technology required assigning it first priority. By 1980 Brookhaven had also learned this lesson, but could not then make up for its lost years. Other projects appeared more attractive to the DOE for support.