19

Building Fermilab: A User's Paradise

ROBERT R. WILSON

Born Frontier, Wyoming, 1914; Ph.D., 1940 (physics), University of California at Berkeley; first Director of Fermi National Accelerator Laboratory and Professor Emeritus, Cornell University; high-energy physics (experimental).

ADRIENNE KOLB

Born New Orleans, Louisiana, 1950; B.A., 1972 (history) University of New Orleans; Fermilab Archivist.

Building Fermilab was a many-faceted endeavor; it had scientific, technical, aesthetic, social, architectural, political, conservationist, and humanistic aspects, all of which were interrelated.[1] Because the emphasis of this Symposium is on the history of science, I intend to highlight the scientific and technical aspects of the design and construction of the experimental facilities, but these other considerations were also important in building the experimental areas (Fig. 19.1).[2] Neither the experiments made at the laboratory, nor improvements such as the Tevatron, made under the aegis of succeeding Directors, will be discussed here.[3]

Before becoming director of Fermilab in 1967, I had been a trustee of URA since its formation in 1965.[4] This experience had sensitized me to the growing number of particle physicists throughout the country who, with no accelerator at their home universities, had become dependent on sharing the use of larger accelerators constructed at national laboratories. It was they who started the revolt against the benevolent rule typified by the University of California's Radiation Laboratory at Berkeley and (on a smaller scale) by my own institution, Cornell University. In 1963 that arch-user Leon Lederman expressed the community's sentiments of wanting the next lab to be accessible by right for all users, that they would have a strong voice in decisions on what was built and how facilities were used, and that it would be a place where they would be "at home and loved."[5] On becoming Director, I was determined that

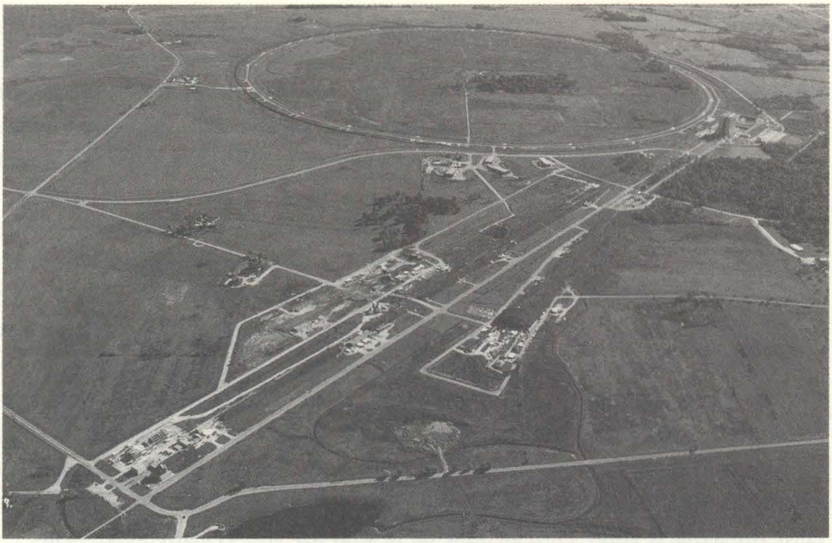

Fig. 19.1. A view from the north of the experimental areas, with the Neutrino Area in the left foreground, the Meson Area to the center right, and the Proton Area in the center left beneath the grove of trees where our buffalo roam. The Main Ring of the accelerator is at the top of this 1977 photograph.

Fermilab should become just that, a "User's Paradise." Easier said than done.

The above was implicit in the sentiments of the URA. It was also implicitly understood that we at the new laboratory would not set up fiefdoms of research under strong in-house physicists, such as the bubble chamber group under Luis Alvarez at LBL or some of the research groups at Brookhaven National Laboratory (BNL) on Long Island had been. This presented a serious problem, for if users were to make best use of the Laboratory, then they would need the assistance of a core of good Fermilab physicists, not only to set up facilities but also to provide the laboratory's help on the experiments being done. Our first estimate of the optimal fraction of Fermilab physicists participating in this work was that it would be about one-fourth.

In order to attract the best physicists to carry out that fraction of the experimental work, we decided to set up a Physics Department that we hoped would be equivalent in quality to that of a strong university. We would also promise that each research physicist hired could use up to 50% of his or her time doing undirected research.

Promises, promises; my conscience is still troubled. There were many physicists who, in the press of building facilities, never got the chance to do very much undirected research, if any. We did not keep books about who did what, but there was an unspoken agreement that if someone worked one full year on a facility, then the next full year could be spent on undirected research.

We never had to define full-time or part-time. Most of the physicists sorted it out for themselves. Some had a natural talent for administration, some did not. We tried to allow both to flourish. Those with administrative talent were of course much in demand in a laboratory just being built. Many of them managed to accomplish their laboratory tasks and do good research at the same time. How otherwise could they have been good physics administrators? In any case, I am exceedingly proud of that group of superb physicists who were "corrupted" into building Fermilab and making it work – but it was the search for new knowledge that motivated their efforts, just as it was for the visiting physicists.

The Berkeley design

The most immediate concern of our prospective users, apart from the accelerator itself, was the adequacy and the relevance of the experimental facilities that we were to design and then build at Fermilab. These facilities were originally specified in the 1965 Design Study of the 200-BeV accelerator made at the University of California's Lawrence Laboratory at Berkeley under the direction of Edward J. Lofgren.[6] It will be referred to here as the Berkeley Design. The people at Berkeley had done a superb job of bringing together all sorts of potential users to consult with them on the experimental areas, and their plans provided a solid foundation on which we could start the designs of our own facilities.[7]

A reduced-scope plan

It was not helpful that the scope of the Berkeley Design had been reduced in 1966 by fiat (AEC fiat via the Bureau of the Budget) to decrease the estimated construction cost from about $340 million to $250 million. Specifically, the number of experimental target stations were to be reduced from five to three, the designed proton intensity was to be reduced by a factor of 3, and a large bubble chamber (the principal method of event analysis of those days) was to be completely eliminated. The $250 million estimated cost of this "reduced-scope" project was the amount decided upon by Congress as they authorized funds for design studies in 1967. At the same time they challenged physicists to do better – but without extra money. As I was chosen to be the Director, it was "writ in blood" that we, at the very least, come up to that reduced-scope standard. Anything beyond that had to be done within the original $250 million – and not "one penny more."

Dark thoughts in the night

Of course before accepting the directorship of the project, I had thought long and hard about reducing the costs. It would not be just a matter of getting some clever ideas about the accelerator; every aspect would have to be reexamined from the point of view of cost. The buildings, the utilities and especially the experimental facilities seemed to me to be overly expensive in the Berkeley plan, and since more than half the costs would go for such things, I was determined to look particularly hard at them for cost reductions. The trouble was that the success of the lab would depend critically on the quality of the experimental facilities.

Ned Goldwasser

It was a great day for me when Ned Goldwasser agreed to join the project as Deputy Director. I was especially pleased because Ned's experience was complementary to mine. Having worked as a user at BNL and the Argonne National Laboratory (ANL), he had hands-on experience with a bubble chamber and with producing the particle beam that led the protons to it. He had served on the Ramsey Panel, so he knew the problems and he knew the people of the proton physics community to whom we could turn for help. I could count on him to fill in many of the lacunae in my own experience, which had been with modest electron experiments. Ned brought much to the design and use of the experimental areas, but he also participated in every aspect of running the laboratory.

Summer and fall, 1967

Criteria for a new design

The project began on June 15, 1967, when a small but determined group met at an Oak Brook office building to start creating the new laboratory, but this time along austere lines.[8] Our first problem with the design was just that, but at the same time I felt confident that we could build to a proton energy of 400 GeV (maybe even 500 GeV), that we might also exceed the number of experimental areas of the Berkeley Design, and that we might even exceed the originally designed intensity of protons – all within the $250 million limitation. Of course we had no way of knowing how much of this was bravado and how much was real, nor how much time it would take to turn that Illinois cornfield into a sophisticated laboratory equivalent to the Berkeley Laboratory that had taken many tens of years to evolve.

There were many intangibles. Who would join the lab to do the job? How long would it take before we knew how much money we would need? How much money would we really get and at what rate? How many years would we require for the construction? When should we start the experimental areas? The answers to most of these questions were pretty straightforward. We would make very sure to deliver the reduced-scope laboratory within our budget. Money would not be spent on anything nonessential until we knew what we were doing. However, there was one deviation right from the start: we planned to keep our options open to exceed the reduced-scope in every respect. The big question was, by how much?

Even as we were designing the accelerator, we also perforce had to design the experimental areas, if only roughly, because the cost and design of the laboratory would depend crucially on the location and characteristics of those areas. As an example, one of the economies I had expected to make over the Berkeley Design was to have the proton beam extracted from the accelerator at only one position, and then to put tremendous care in the extraction efficiency. The reason for this was that in the Berkeley Design much of the cost had been due to the effects of the radioactivity due to the protons that were not extracted. For example, the radioactivity of the air in the tunnel, the production of nitric acid in the tunnel, the need for cumbersome and expensive equipment for handling radioactive magnets, and the long delays during a shutdown while the radiation level decayed, all would require measures that were far from inexpensive. The solution would be not to lose any protons. My dream was that each proton that left the injector would be made to travel benignly near the center of the vacuum tube. Should a wayward proton strike an object, the resulting radiation would be detected by one of a large array of external detectors and the beam orbit would be adjusted or scraped off so that the offending protons would not strike that object any more. At that time this appeared to be pure fantasy for, quite apart from the beam lost during the acceleration process, the extractors then in use had an efficiency of only about 60%.

Al Maschke

If I was not up to solving the extraction problem, Alfred Maschke was. He had joined the laboratory that summer, coming from BNL.[9] Maschke had a profound influence on all the technical phases of the accelerator and its concomitant experimental facilities. Soon he had invented a new way of cleanly extracting the proton beam from the accelerator for which he claimed an efficiency of 99%, or even greater. I must say that this was pooh-poohed by the experts at the other labs – those at CERN seemed even to be offended by such an extravagant claim. But eventually it turned out just as Al had said, and his invention must have saved millions of dollars from not having tremendous quantities of radioactivity deposited in the Main Ring.

Maschke was in charge of the Beam Transfer Division, which had the responsibility for transferring the proton beam from one accelerator to the next and then to the Switchyard, where the beam would be directed to targets in the various experimental areas. He planned and did this brilliantly. It was tragic for me when in 1971, Al, a feisty guy, left the lab following a serious disagreement with me. I hope he will eventually receive the recognition that is his due.

Jim Sanford

Jim Sanford came to the 1967 Summer Study also from BNL. He brought with him a wealth of experience in experimental areas. I suppose that Sanford was most influential of all in developing the concept of a single external beam that could be switched to a multiplicity of areas where experiments could be done. He worked literally day and night during the summer study with such intensity, and so single-mindedly, that when he returned to BNL, we knew that we should follow his plans. We also knew that we should try our best to recruit Jim to be a permanent member of the Fermilab staff. He returned in a few months as Associate Director in charge of our experimental areas and eventually to coordinate the experiments that were being done at Fermilab.[10] Jim was basically a very conservative person, and was exactly what we needed to balance my own cavalier approach to problems.

It would be unfair to ignore the valiant and valuable efforts of my colleagues who were also developing concepts of the experimental areas, both independently of Sanford or in parallel with him. I am thinking of Art Roberts, Lincoln Read (who had also been an important advisor to me before I became Director), Winslow Baker, James Walker, Timothy Toohig, Edward Blesser, Richard Carrigan, Frank Nezrick, and many others.

Tom Collins

Tom Collins, another Associate Director, had mostly to do with the accelerator, but in addition to being an outstanding expert on all phases of the accelerator, he also played a prominent role in building the experimental facilities. This was because Tom had a deep interest and competence in architecture and the architectural-engineering aspects of the lab. Thus it was one thing to decide on the positions and functions of the experimental areas, but it was quite another to design and construct the tunnels and buildings and bring the necessary utilities to them. Tom was master of all of these aspects of our work, and his participation was crucial to how well the experimental areas functioned and to how much they would cost. Furthermore, he ran a weekly meeting in which those aspects of the laboratory then being built would be reviewed with regard to cost and schedule.

I must emphasize how hazardous these facilities were. A beam of 400 GeV protons at an average intensity of 2×10^{13} per pulse, typical of what we hoped to have, has a daunting one megawatt of power. This can melt a piece of metal almost instantly. Incident on a target, it can make the equivalent of 200,000 grams of radium, whereas one gram would be a serious amount. Clearly we had to handle this fearsome force with great respect and dispose of the radioactivity with great caution. We would have to control access to dangerous parts of the facilities with absolute certainty. Thus we needed a radiation officer and assistants having essentially absolute power. Miguel Awschalom and his assistants, Dennis Theriot, Robert Shafer, Peter Gollen, Larry Coulson, and Andrew Van Ginneken, served among the first radiation protection group. That their plans were good and their vigilance keen is attested to by their excellent record of preventing human exposure to harmful radiation.

The Atomic Energy Commission

How did our safety guys know what to do? For one thing they had experience at other laboratories as well as special training. But our source of confidence in what we were doing was also due to the AEC. I know it is now trendy to bad-mouth the AEC and its ensuing agencies, but we did work closely with them – and did depend upon their expertise and skills about safety. In this respect I must mention K. C. Brooks, Fred Mattmueller, and Andy Mravca, the AEC representatives in residence at the lab.[11] It was no accident that Glenn Seaborg, Chairman of the AEC, and one of my fellow students at Berkeley, kept in close touch with what we were doing. A host of other concerned well-wishers worked equally hard for us at the AEC Headquarters in Washington. Andy asked my permission to attend the meetings at which our construction plans were developed. By being present (not at all usual at other labs), he could make sure that our plans were consistent with the AEC safety standards and requirements, rather than having to go over them seriatim, which was guaranteed to consume months of our time. It was a good deal, and as a result he was able to gain approvals for us from the Washington office within days rather than months. No wonder we held the AEC in such veneration, respect, and friendship – they were very much on our team, or even better, we were on their team!

DUSAF and Parke Rohrer

DUSAF, the architectural-engineering consortium, did the design and construction of all of the conventional structures and utilities.[12] This was differentiated from the accelerator, experimental, and other scientific equipment. Here we lucked out. DUSAF had done the LBL plans which, to my mind, had been far too expensive. Happily, the president of the joint venture, Colonel William Alexander, a man of obvious integrity, promised to provide us with the kind of services we desired and demanded. He named Mr. Parke Rohrer to be the manager in residence, and for that appointment I shall always be indebted to him. Parke was exactly what we needed. He soon demonstrated tremendous expertise in architectural engineering, tremendous experience in administration, and unsurpassed character and compassion. If I am using superlatives, they are absolutely necessary in any appraisal of this remarkable man. He responded to our need to save money and also to our desire to create a workable and beautiful laboratory.

Parke had absorbed the ideals and needs of the laboratory we were to build so well that, instead of setting up a construction division to check all the DUSAF drawings and designs, I simply appointed him to be our Associate Director of Construction. He took this very seriously. I do not know how he managed to serve two masters, but serve us he did to our complete satisfaction – and, I'm quite sure, to the satisfaction of DUSAF as well.

Engineers

From the beginning we had outstanding engineers at Fermilab. For example, Don Young brought with him from Madison, Wisconsin, one group of engineers consisting of Glenn Lee, John O'Meara, Maxwell Palmer, Norma Lau, and Russel Winje. They stayed close to the building of the Linac and were not available to the experimental facilities. To provide for that need and also for making believable cost estimates, I called on an old friend, colleague, and teacher from the U.C. Radiation Laboratory of the thirties whom I knew when I was a student there, Bill Brobeck. Bill is a master engineer; he is Mr. Accelerator personified. He had designed cyclotrons before World War II, calutrons during the war, and the Bevatron after the war. He was renowned as the most conservative estimator of costs in the world, so I knew that if he estimated our technical costs, he would be believed. More importantly, I believed his estimates too!

Heeding my call for help, he came to Illinois and set up a commercial engineering group, using a few key people from his Berkeley firm as a core group and complementing them with a group of local engineers. Bill did important engineering for us. For example, he designed power supplies and made cost estimates of technical components as well. In a sense he did for our technical components what DUSAF did for our conventional facilities. It was because of these two groups that we were able to be "off and running" so rapidly.*

This is how it worked. We would furiously (and I hoped imaginatively) design a particular component or system of the laboratory complex. Brobeck, working quite independently of us, would price it out. It would inevitably cost too much. Since it was taboo to argue with Bill, we would

* Of course we were busy recruiting our own corps of engineers. To name a few of those heroes, but not all, were Dick Cassel, Hank Hinterberger, Hans Kautsky, George Mulholland, and Wayne Nestander, in addition to the engineers brought by Don Young from Madison.

go back to the drawing board, hoping to make reductions by making inventions or by cutting more deeply at what fat we could find. This would then go to the cost estimators again; costing less but still costing too much. So this process went on, iteratively, until the cost was within our limit. Every so often we would add up the total expected cost to see how we were doing. Still too much, but always closer to our $250 million goal. Finally, in a few months, we did joyfully hit it – but would we be able to build it?

Theorists

The theorists, too, were important to us in building the experimental facilities, as well as in helping us decide which experiments to do. It was crucial to me to engender a sense of doing physics at the lab at the earliest possible time, a sense that we were doing more than just building an accelerator. It would be easy, out on that Illinois plain, to lose touch with physics, to forget who we were, and why we were there. Theory was something that could be started immediately, and that would give us a sense of doing real physics.

My first scheme was to call on my old friend Bob Serber to come once a week from Columbia University to lecture about the most recent physics or, if he could not come, to arrange for someone else to talk to us. This was in the tradition of Bob at Los Alamos, starting off with a series of lectures in 1943 about neutron physics, or with his "Serber Says" lectures at LBL after the war. Bob has the knack of speaking simply and understandably to experimentalists without patronizing them. It worked again for us at Fermilab.

When Serber tired of his weekly flights from New York, Ned thought we needed a more permanent solution, and he arranged to have Sam Treiman come out from Princeton for a sabbatical year to set up a continuing theory department. Sam enlisted a group of young theorists, all of whom have had outstanding subsequent careers.[13] After his stint Sam asked J. D. "Dave" Jackson to take a turn. Dave then triumphantly recruited Ben Lee (a theorist of exceptional ability) to join the lab as a regular member. We attracted a steady stream of distinguished theorist-visitors. These included Maurice Jacob, William Frazier and Chris Quigg (Quigg stayed on to head the theory department). Eventually Bill Bardeen became the head of this distinguished group and the tradition of excellence continues. The theorists not only brought style and learning to the lab, they also fulfilled their promise to help with the choice of experiments. We have been proud and fortunate to have had such a strong group.

The users organization

I have already mentioned a number of institutions, URA, AEC, DUSAF and the JCAE, with which we interacted closely in building the research facilities.[14] There were others that were also important. Under the aegis of Goldwasser and Ramsey, the potential users of the laboratory organized a Users Organization in 1968. Their first meeting of about 170 members was held at the groundbreaking ceremony on December 1, 1968. The organization reported not to Fermilab but directly to the URA, consistent with the users' right to have their desired input into the top management of Fermilab. Before Jim Sanford became a member of the Laboratory staff, he was the first Chairman of this organization. This group proved to be of substantial value in facilitating communications between the users and us at Fermilab, especially with regard to the research facilities then under consideration. Their Executive Committee met not only with the URA trustees but directly with me and with other members of the laboratory. It was an effective method, if occasionally painful, for learning how short of our aspirations we frequently were. Indeed the users were positive, if forthright, in their criticism, as well as praise, for how else would we have known how we were doing? Every year they organized a general meeting of users. These occasions provided some of our best opportunities to speak directly to the users about our mutual hopes and plans.

Physics Advisory Committee

The Fermilab Physics Advisory Committee was a hard-working committee organized by Ned in 1969–70.[15] We came to depend on it heavily for advice on the research facilities and on the experiments and their priorities. I appointed the committee members, of which three came from the East coast, three from the Midwest, and three from the far West, in order to have a geographical balance. To have an equal representation of the different kinds of physics, each set of three consisted of one physicist who specialized in experiments that made use of electronic counters, one who made use of bubble chambers, and one theoretical physicist. They all had staggered terms of three years. They, as well as the Users Committee, made recommendations for their replacements, which I was careful to follow. It was all outrageously bureaucratic, but how else could we strive for a laboratory that would fulfill the Lederman dream of a Users' Paradise? Unfortunately, paradisios do not come cheap. It was about spending money that we necessarily had all too frequent disputes, for our funding was tantalizingly slow in coming.

Meeting the November 1967 deadline

It is hard now to remember the intensity of our labors to meet the November 1967 deadline that would allow us to complete the dread "Schedule 44," a detailed funding plan for the whole project that was required even to have funds for FY1968 authorized by Congress. Not only did we have to redesign the accelerator and the experimental areas, but the whole new design had to pass a URA review. This was held on October 12, 1967. We were also required to prepare and have printed and delivered by the first of January 1968 a complete Design Report for the benefit of the AEC and Congress. Had all this not been achieved, we would have suffered an automatic delay of one year in the project. No wonder we were absolutely ecstatic when we survived these, for this exercise gave us the confidence that we too might know what we were doing. Perhaps just as important was the fact that, in a project such as ours, time was money and a one year delay might have put our cost requirement out of reach.

Research areas, 1968–1969

Once the rough plans and costs of the experimental areas had been fixed and incorporated into our Design Report, our attention was focused on the accelerator. However, we did maintain a low level of design activity in the other areas. Our intention was to have as much input into our needs as possible from the physicists who would use the facilities.

This was rather successfully begun at a 1968 summer study at Aspen, Colorado.[16] About 75 users came to this meeting. Many of them were prepared with proposals or "letters of interest" for their experiments. There was by far more impassioned debate than mountain climbing and, alas, little fishing. The conclusion we reached from listening to the users was that they agreed with, or were neutral about, the single external beam concept. They thought, as we did, that the internal target area was not necessary. What many of them did care about was that we build a large bubble chamber. We returned to Chicago feeling that much

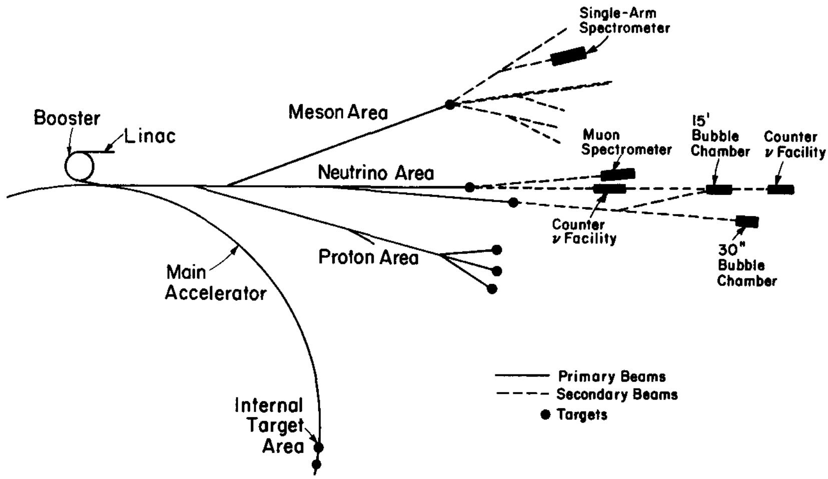

Fig. 19.2. Layout of the major experimental areas and beam lines at Fermilab.

had been learned from the experience, but that we had yet much to learn. The best part, perhaps, was that in Aspen we were removed from the hurly-burly of constant crises at the laboratory, and from endless telephone calls. This allowed us to concentrate on physics. It was not that we did not have many shorter meetings of users at Fermilab, but those were usually directed to some special end and did not have the breadth and depth of our Aspen meetings.

A three-area concept

Out of our deliberations, out of the recommendations of our users, we came to a consensus of what facilities we should build and when we should build them. This was not something we reached lightly. There was no point in building the best accelerator in the world – and we were trying to do just that – if the facilities were inadequate for the experiments our users would conduct; nor, by the same token, was there any point in building the most lavish experimental areas but an inadequate accelerator. No, the facilities had to match the accelerator, and both had to match what our users wanted – and what we both could afford to build and to use with our limited rate of funding.

As illustrated in Fig. 19.2, there would be three areas: a Meson Area, a Neutrino Area, and a Proton Area. These names were chosen to describe the general character, but not the exclusive character, of each of the areas. At first the areas had been given numbers instead of names, but I had a hard time remembering which was called what. The new titles were chosen not only to assist my feeble memory but also so that the names of the particles we were investigating would become familiar to the nontechnical people in the laboratory; I hoped that this might help to engender a sense of participation in the project by everyone working there. Indeed, we tried hard to infuse an understanding of what we were doing to everyone at the lab, and this did much to make an enthusiastic work force.

The Meson Area

The first experimental area to be constructed, the Meson Area (Fig. 19.3), was initially built to accept 200 GeV protons but with the potential of later raising the energy of the protons to 400 GeV. It would be primarily a facility to study secondary particles, such as mesons, that result when the 200 GeV protons from the accelerator strike a target.

The Meson Area lab building was a departure from those previously built at lower energy laboratories in that the whole area was not covered by one huge structure. Because of the higher energy at Fermilab, the range of the secondary particles, as well as the primary protons, would be vastly greater than heretofore, so a building extending from the proton target to the end of some of the envisaged experiments would require a distance of about one kilometer – prohibitively expensive. Instead we had one building, only a few hundred feet in breadth and length, in which targets and experiments would have the luxury of an overhead crane. Experiments extending beyond the building would be contained in corrugated metal tunnels that could be easily moved (but in practice seldom were) to correspond to the physical outlines of an experiment. The building itself, originating from my fevered brain, was a triumph of architecture (well, in my opinion), but it was something of a catastrophe from a practical point of view. I am ashamed to report that the users therein regarded it more as an Inferno than the Paradise I had hoped it to be. The roof, made of corrugated steel culvert plates, leaked seriously and continuously.

Early delays

Construction in the experimental areas was delayed because, just as it was going into the final stages of preparation for experiments, the

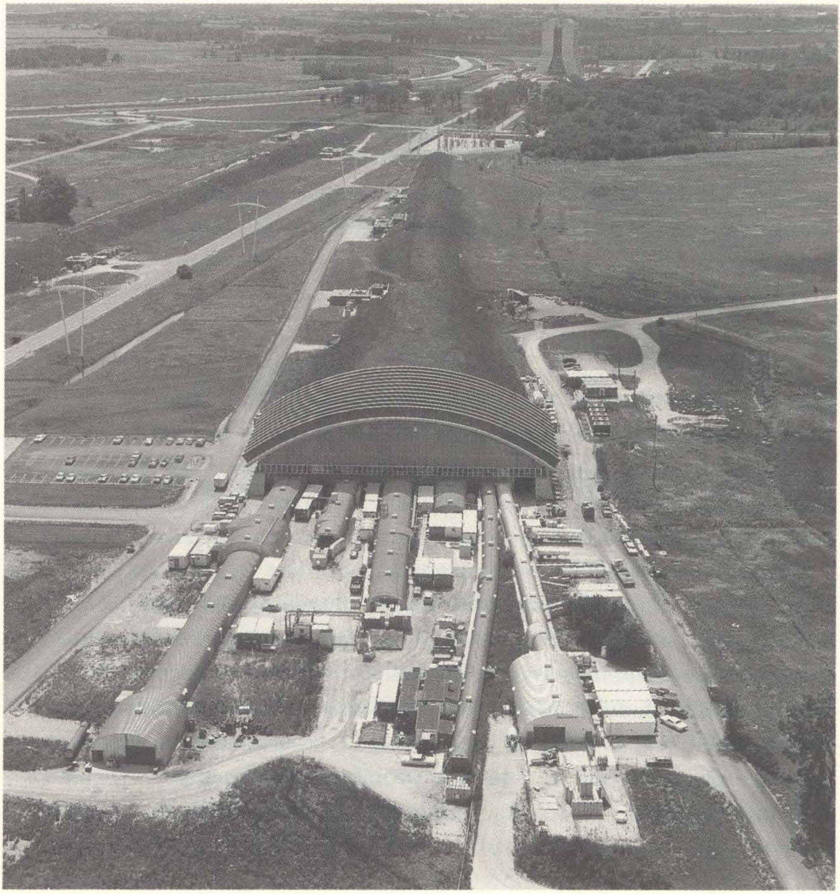

Fig. 19.3. The Meson Area, with its laboratory's distinctive roof, contains six particle beam lines.

accelerator went into a series of crises – primarily, many of the Main Ring magnets failed.[17] Two physicists, Rich Orr and Dick Lundy, heroically threw themselves and their comrades, who had been working on the experimental areas, into the breech (or was it the abyss?) to save the day. They were not the only experimental physicists who made this sacrifice and, with everyone on the project pulling together, the accelerator was indeed saved. Within a few months the accelerator belched out its first high-energy beam: 20 GeV in January 1972; 200 GeV in March 1972; and 400 GeV in December 1972. Then they all rushed back to bring

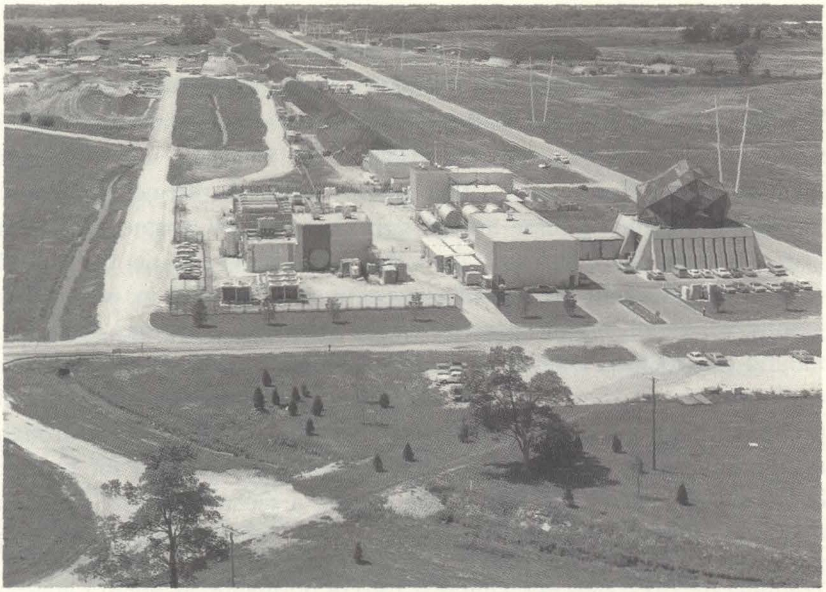

Fig. 19.4. The 15 foot bubble chamber sits at the end of the Neutrino Area. Adjacent is the Bubble Chamber Building, which is covered with a geodesic dome roof.

their respective experimental areas into operation for the patiently (??) waiting users.

But not all of the users were waiting. Consider Bob Walker and Alvin Tollestrup from Caltech experiment E-111 in the Meson Lab. With the Fermilab physicists still off in the accelerator abyss, they put their efforts into the messy work of bringing beams to targets and coping with the floods from my flawed design of the Meson Lab roof. By 1974 they brought their beautiful experiment about meson charge exchange to a successful conclusion.

Neutrino Area

The Neutrino Area, more than a mile in length, is directly in line with the direction of the extracted beam of protons. The bubble chamber was located near the end of the neutrino beam – about one mile from where the proton beam emerges from the accelerator – and protons as well could be brought from one end to the other. Although the Neutrino Area was primarily designed for the study of neutrinos, the muons that are made in the production of neutrinos were also extensively studied there.

A target train upon which proton targets and special magnets were mounted could be pushed into a tunnel in a long, high mound of earth. The pions and kaons that emerged from the proton target were formed into a nearly parallel beam that traveled the length of the 100 meter long evacuated decay pipe. The mesons decayed into neutrinos and muons, and some of these muons were deflected by magnets into the Muon Laboratory. At the end of the decay pipe, the neutrinos and other products of the proton collisions entered a long (800 m) absorption mound where all but the neutrinos were absorbed. Part-way down the mound was located a small "Wonder" building for low-energy neutrino experiments. At the far end of the mound the neutrinos emerged and passed through the 15 foot bubble chamber (Fig. 19.4) and then through a building where the neutrinos were detected by counters for further experiments. Off to the side of the mound a second, smaller (30 inch diameter) bubble chamber was located. High-energy protons could be led either to it or to the 15 foot bubble chamber.

After the intensity and energy of the protons from the accelerator had been increased, it was necessary to increase the absorptive power of the neutrino berm. Since we could not conveniently increase the length of the berm, we increased the average density by burying in it huge pieces of the aircraft carrier *USS Princeton*, recently decommissioned – swords into plowshares and all that. Pieces of the *Princeton*'s deck were used in a sculpture, "Broken Symmetry," located at the main entrance of Fermilab.

The Bubble Chambers

Many of the experimenters who would use Fermilab made it very clear to us at the first summer studies that bubble chambers would be highly desirable for research at Fermilab. Indeed the Berkeley Design Report had included some $60 million that, among other things, would provide for one 2 m^3 bubble chamber and one 100 m^3 bubble chamber, as well as for moving an unspecified, already constructed, large bubble chamber from another laboratory to the new site. Unfortunately, these plans had all been thrown out in arriving at the reduced-scope funds that were to be made available to us.

Of course, eliminating the funds did not eliminate the need. At the first Aspen Summer Study in 1968 there had been general agreement among the users that a 25 foot diameter bubble chamber would be required to do the job of research then anticipated. Until it could be built, a 30 inch chamber from Argonne would be used.[18] We had also hoped to get the large bubble chamber being built by a group at BNL, but naturally they had a strong desire to keep it for their own research, as they did.

A collaboration was soon formed between the Shutt group at BNL (which was just finishing a 7 foot chamber) and our physicists (Nezrick, et al.), to design a 25 foot diameter monster chamber. Despite heroic efforts by Ned Goldwasser and those at Brookhaven, the elegant project was turned down by the AEC, almost surely because it would cost some $15 million, which was just too much for any funds they had then.

I felt that perhaps a lesser sum of money, to be provided from the hard-to-come-by Fermilab construction funds, might be afforded. Goldwasser and Sanford, together with the designers of the 25 foot chamber, eventually arrived at a more affordable design, this time a 15 foot chamber estimated to cost about $7 million. However, in the rush to the new design, I made an obligation to the experimenters that a 15 foot chamber would indeed be built. It turned out that the design of the chamber, for economic reasons, had been reduced to 14 feet. At that time, in 1969, wanting especially to maintain my credibility with the users, I insisted that we stay with the 15 foot size. So as not to have to make a new design, it occurred to me that a small one-foot long conical extension placed at the front end of the chamber would keep the sensitive path length to 15 feet within the chamber. The protuberance was sometimes, and with *lèse majesté*, referred to as "Wilson's nose"!

Bill Fowler, who had led the 15 foot design, came from BNL in early 1970 to head up the construction team. Russ Huson followed about six months later. They recruited a formidable group to do the job, gathering up people like Frank Nezrick, Hans Kautsky, and Wes Smart. John Purcell and his group at the Argonne Lab built the superconducting magnet for the chamber, no small job. Peter Van der Arend and his cryogenic company were responsible for the cryogenics through to operation. Bob Watt and his colleagues at SLAC took on the rapid expansion of the chamber. George Mulholland took over commissioning and operations. Safety was of overriding importance, for after all, the bubble chamber, full of liquid hydrogen, was indeed inherently dangerous. We lucked out in that regard by having Paul Hernandez at LBL serve as my safety officer. Paul had been LBL's chief engineer of Luis Alvarez's 72 inch chamber. He and our own very capable safety group did a magnificent job; there were no accidents.

Andy Mravca has to be celebrated for performing his usual miracles in the AEC. What he did with his consummate mix of science and bureaucratic savvy was just as necessary for the construction of the bubble chamber as it had been for the accelerator.

The 15 foot bubble chamber was started near the beginning of 1970; it was commissioned in September 1973 (remarkably fast for designing, financing, and then building it), and it ran successfully until it was turned off in 1988. That occasion was celebrated by a "15-Foot Fest" at which many of the participants in its construction and operation were able to attend and give voice to poignant memories. Happily these have been gathered into a delightful volume.[19] Therein a cogent review is given by Charles Baltay of some 17 experiments done with various mixtures of hydrogen, deuterium, and neon. Paul Hernandez of LBL also paid a poetic compliment: "after 34 years of Bubble Chamber connections ... I see the 15-foot bubble chamber as the 'Jewel in the Crown.' " It was indeed a good operation.

The Proton Area: life in the pits of the pits

The Proton Area (Fig. 19.5) was the last to be commissioned; it was intended for experiments using protons at the highest energies and the highest intensities. The proton beam from the accelerator could be split there so that it, or any fraction of it, could be guided into any of three underground well-shielded pits of the Proton Area. The pits are named Proton West, Proton Center, and Proton East.

These enclosures are indeed rough-and-ready places. They had the reputation of being, not Paradisios, but rather Purgatorios. Indeed, some of the users were advised by their older colleagues to "abandon all hope, ye who enter here!" I fear that I bear the responsibility for this fiasco.[20] In a frenzy of saving big bucks, I had a fantasy of not putting up (or down) any laboratory building at all. Instead the idea was that, once an experiment had been accepted, an outline of the necessary space would be drawn in an empty field at the end of one of the proton beams, then steel interlocking piles would be driven along the outline down to the necessary depth to protect against radiation. Then the experimental equipment would be lowered to a luxurious graveled floor, and finally a removable steel roof would be covered with the requisite thickness of earth. Once the experiment was finished, the pilings were to be pulled

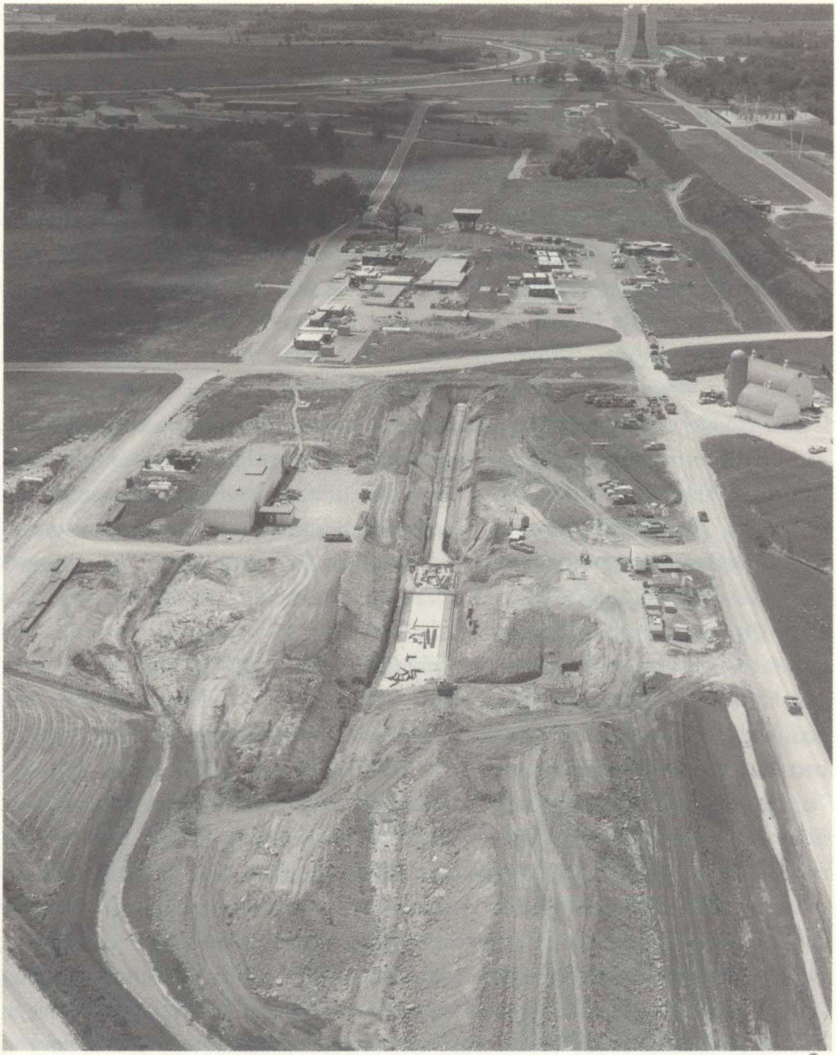

Fig. 19.5. A 1976 photograph of the Proton Area, which is divided into three subareas providing four particle beam lines.

up, the earth filled in, and then the next experiment would be ready to receive its tailor-made enclosure.

Simple and inexpensive, is it not? I still find it difficult to understand why those users all stopped speaking to me. It is true that there were a few flaws in my logic. The rivers of ground water that flowed through their experiments, the walls of piling rusting away, the impossible access, and all without benefit of toilet facilities. But some of the users had their finest moments down in those proton pits – the discovery of beauty, the bottom quark, where else?! Alas, as far as I know not one piling has been pulled up, not one pit has yet been refilled with earth. How is one to interpret this?

To redress some of the inadequacies of the users' trailers, not to mention their visual blight, the director personally designed a luxurious building, the Proton Pagoda, a double stairway (à la the Vatican), and even toilets were eventually installed – alas, all too little and too late.

Internal target area

The Berkeley Design Report included a rather elaborate internal target area. We did not like it because radiation from the target might contaminate the Main Ring and a separate laboratory building would cost too much. During the summer study, after some debate among potential users, they recommended that we abandon any such area. A few years later we decided that a very thin target would not add too much to the radiation problem, and so we designated the straight section of the accelerator at section C-1 as a possible position for an internal target area, but that any laboratory space there would have to be improvised in the regularly enlarged part of the tunnel at C-1. Actually, the first experiment at the laboratory was an international collaboration of Soviets (V. Nikitin, et al.) working with a group from Fermilab (E. Malamud, et al.) and from Rochester University (S. Olsen, et al.). The Russians had fabricated a gaseous jet of hydrogen that constituted a very thin target when it was fired through the circulating proton beam of the accelerator – the group measured p–p elastic scattering and initiated a continuing and fruitful collaboration with Soviet physicists. This culminated, along with the physics results, in a 1974 performance by the Bolshoi Ballet in our auditorium! It also culminated in a small extension to the tunnel at C-1 to provide a little extra underground space for experiments.

Nooks and crannies

I had a bad conscience for having set up such a formidable bureaucracy to ensure fairness and scientific merit in the acceptance of proposals for experiments, so I tried to improvise a supplemental system of no bureaucracy at all. In this scenario any reputable physicist who could find a vacant nook or cranny for a modest experiment could, without any "by-your-leave," just go ahead and do it. Well, there were obvious flaws in this approach to Nirvana, and soon it was abandoned.

General remarks

I have emphasized the role of the users and of committees of users in the management of the Laboratory, for that had been one of our devices to realize a laboratory where the users "would be in charge" as well as being "at home and loved." But there was an even higher criterion of success to which we, and they, were beholden: the quality and quantity of the physics done. Alas, there is no easy formula, no democratic procedure, that would necessarily ensure our meeting this criterion. There was always a dichotomy between those experimenters who wanted to use the accelerator immediately after attaining 200 GeV and those whose experiments required the highest energy. It was pretty much up to the Director to decide on the basis of his own intuition what energy could be reached within the available funds. Since most of the users had urgent obligations to their students as well as obligations to raise funds for their research, my proclivity to go to the highest energy did not win me many popularity contests.

The accelerator produced its first beam at the design energy of 200 GeV in March 1972, less than five years after we had come to Illinois. Almost immediately our experimental program began. By July an energy of 300 GeV was reached, and then in December it went up to 400 GeV.[21] During that same period the intensity of the beam went from some 10^9 protons per pulse of the synchrotron to about 5×10^{12} protons per pulse – still less than the design intensity by a factor of 10. It took another four years of hard work before the intensity had been pushed up to within a factor of 2 of what we had planned. By then, however, the proton beam was running regularly at 400 GeV, and could, sporadically, run briefly at 500 GeV.

Other factors than just the proton energy and intensity were of equal importance in doing successful experiments; for example, the rate of the pulses and the shape of the pulses in time. Reliability seemed the hardest of all to attain. Far too many times we had to explain to an exasperated group of experimenters who had come from the ends of the Earth that the machine was broken and would take a few days to fix.

As in any adventure where high-spirited people are involved, tempers would occasionally flare and shrill voices would fill the air. Even so, the common goal of producing good physics would soon restore calm. Perhaps an occasional shot of adrenaline helped speed us on our way.

In the beginning we wondered, "Would the users come?" Indeed, they did come – thousands of users, doing hundreds of experiments. Did they feel "at home and loved?" Loved they were by us – in our fashion – but it was not always so evident to the pitiable users. The more relevant question now is whether they were able to use the above experimental areas to do important physics. That is for someone else to say, but I am satisfied that they did. Even one discovery such as the upsilon particle – and there were others, too – made all that effort worthwhile.

Did we construct a foundation upon which those who followed could improve? Apparently the answer to this is also in the affirmative.

It must be emphasized, however, that it was the skill and innovations and dedication and cooperation and good humor and hard work of the Fermilab staff that created the accelerator and its concomitant experimental areas – and then made that infinitely complicated system of tens of thousands of individual subcomponents work together as one system – a *miracol mostrare* – for the use of the experimenters to perform *their* miracles.

Notes

1 From the origin of the laboratory in 1967 until it was dedicated in 1974, it was called the National Accelerator Laboratory (NAL). At the dedication it was renamed the Fermi National Accelerator Laboratory (FNAL). Largely because of my dislike of acronyms, I called it Fermilab, a name that has stuck to it. I shall use that name throughout for simplicity even though it was not called that throughout most of the time about which I am writing.

2 Various aspects of building Fermilab have been described in the 1987 twentieth anniversary issue of the Fermilab Annual Report.

3 Leon Lederman, "Tevatron," *Scientific American 264*, Vol. 3 (March 1991), pp. 48–55.

4 The pronoun "I" is used throughout because this chapter presents the perspective and reminiscences of R. R. Wilson, but its preparation was a joint endeavor between the two of us. Universities Research Association, Inc. (URA) was created in 1965 by the Council of Presidents (of about 46 universities). Norman F. Ramsey was the first President of URA and H. D. Smyth was Chairman of the Board of Trustees, which had 15 members elected by the Council of Presidents from each of 15 groups of neighboring institutions.

5 Leon Lederman of Columbia University, then serving on the Good Committee to comment on the Ramsey Panel's report, presented the paper, "The Truly National Laboratory (TNL)," on June 25, 1963, at

Brookhaven's Super-High-Energy Summer Study, Brookhaven Report No. BNL-AADD-6 (1963).

6 "200-BeV Accelerator Design Study 1965," Lawrence Berkeley Laboratory Tech. Rep. No. UCRL-16000. After the submission of the above report, there were summer studies held in 1965 and 1967 to further explore the ideas of future users. See "200-BeV Accelerator: Studies on Experimental Use, 1966 and 1967," Lawrence Berkeley Laboratory Tech. Rep. No. UCRL-16830.

7 Other people who worked on experimental areas were Dennis Keefe (nominally in charge of this aspect of the Design Study), Robert Ely, William Wenzel, Willim Gilbert, George Trilling, Tim Toohig, Robert Meuser, and of course many others.

8 We had already decided to build as the injector for the synchrotron a near-copy of the 200-MeV Linear Accelerator (Linac) then under construction at BNL. Donald Young, the first NAL employee on May 22, 1967, was in charge of our Linac construction. He brought a group from MURA, the Midwestern Universities Research Association, which was then closing down. Curtis Owen, Cyril D. Curtis, John O'Meara, Glenn Lee, and Maxwell Palmer were the principal members of that group. Soon Philip Livdahl came from ANL to help. Margaret Kasak also came from MURA to be my secretary temporarily. Priscilla Duffield replaced Margaret when she returned to her home in Madison. Priscilla, a no-nonsense super-secretary with invaluable experience in many physics projects, had worked with Lawrence and Oppenheimer. She personally knew many of the people with whom we would be dealing and played an invaluable role in organizing me, and indeed everyone else! Don Getz, Assistant Laboratory Director, and Don Poillon, Purchasing Agent, were there, as was Frank Cole, from the Berkeley project. There were people from the AEC and from DUSAF (an architectural-engineering design team of four companies) . We occupied the tenth floor of the Oak Brook high-rise office building from June 1967 until September 1968, when we relocated to the Village of Weston site. On December 1, 1968, we held the groundbreaking ceremony for the project.

9 Acronyms, acronyms! There I go again, but I shall continue to refer to Brookhaven National Laboratory as BNL, to Lawrence Berkeley Laboratory as LBL, to Argonne National Laboratory as ANL, and to Stanford Linear Accelerator Center as SLAC; otherwise many physicists will have no idea to what I am referring!

10 Jim Sanford has written a good description of the experimental areas in his article "The Fermi National Accelerator Laboratory," *Ann. Rev. Nucl. Sci. 26* (1976), pp. 151–98. He graciously consented to my request to excerpt from his article.

11 K. C. Brooks was a gift from heaven, or rather from Glenn Seaborg, who brought him back from retirement just for us. Casey had a lifetime of experience in the AEC Construction Division. He was a doer and a good friend. His secretary Minerva Sanders and his deputy Fred Mattmueller also kept the fires burning. John Erlewine, Director of Construction for the AEC at Washington, D.C., was also a great source of support.

12 DUSAF, another tiresome acronym, but more justifiable than most since it stands for: Daniel, Mann, Johnson & Mendenhall; Max O. Urbahn; Seelye, Stevenson, Value & Knecht, Inc.; and George A. Fuller Company

– a real mouthful. The latter company had built the Washington Monument! Tom Downs, George Mitchel, Allan Ryder , George Adams, George Doty, William Rowe, etc., were members of Parke Rohrer's team of architects.

13 These have included Henry Abarbanel, Martin Einhorn, Steven Ellis, David Gordon, Emmanuel Paschos, and Anthony Sanda.

14 Well, I should have already mentioned the Joint Committee on Atomic Energy (JCAE) of the Congress. I had to report to them every year. They determined how much money we, or more accurately the AEC, got. Their Executive Director, John T. Conway, was crucially important for us. I got to know him quite well and also liked and respected the Congressional members of the committee: John Pastore, Chet Hollifield, John Anderson, Craig Hosmer, and Melvin Price. I tried to be punctiliously honest and direct with them, and they responded by being most friendly with me.

15 It was originally named "The Program Advisory Committee."

16 After the summer push to get a plausible design for the new laboratory, I was exhausted and went out to Aspen for a few days of fishing. There I ran into David Pines, who showed me around the Aspen Physics Institute. Never at loss for a good idea, David suggested that the Institute would be a good place for our next (1968) summer study that was already planned to be about experimental areas. Having caught a few fish, it was easy to convince myself of the wisdom of his idea – but would it play in Peoria? On my return to Chicago, Ned agreed that Aspen might add a bit of luster to what by many was considered a lackluster site. Although the idea was initially rejected by mid-level AEC officials, Ned felt that we could sell it to the Commissioners themselves on the basis of economy. The pitch was that if we held the Summer Study in the Chicago area, we would have to pay the travel expenses, etc., for whole families, if anyone came at all. If they did not come, it would be difficult to get the kind of user commitment that we wanted and needed. At Aspen we would need only pay the participant's travel because it is a great vacation spot. The attraction to families would be essential to the success of the program. This logic prevailed. The Commissioners overrode previous objections. Excellent people came, and the lab has been saving money there ever since!

17 I have tried throughout this chapter not to be defensive. However, Ned Goldwasser, to whom I passed a draft, has rebelled. He has insisted on the inclusion of the following note: "From the very beginning of the project Bob Wilson realized that to achieve the cost-savings that were required for the authorization of construction, it would be necessary to shave all designs to the bone. In every instance we would have to hew close to the line between a 'go' and 'no-go' design. We realized that in doing this, some systems would turn out to be under-designed and would have to be beefed up at some cost. But we were convinced that this would be far cheaper than designing a comfortable margin of safety for every component. The magnets turned out to be our principal Achilles' heel, but we remain convinced that had we designed conservatively, cost overruns would have seriously compromised the project or lost it altogether."

18 This was the Argonne–University of Michigan bubble chamber. Its

installation was made as a collaboration of Argonne and Fermilab under the general direction of Lou Voyvodic in the summer of 1971.

19 M. Bodnarczuk, ed., "Reflections on the Fifteen-Foot Bubble Chamber at Fermilab," (Batavia, IL: Fermilab, 1988). It is replete with many pictures and the usual enchanting drawings by Angela Gonzales.

20 It could be that it was Maschke who came up with this howler. If so, then he should step forward like a man and accept the blame.

21 Although 500 GeV protons had been produced in 1976 and used in bubble chamber exposures, the nation's energy shortage had reached crisis proportions, so the laboratory was not able to operate above the normal running value of 400 GeV.