A history of the synchrotron

The events surrounding the origin of the synchrotron—the machine that made high-energy physics possible—narrated by a discoverer of the phase-stability principle that made the synchrotron possible.

Edwin M. McMillan

Speaking not as a historian but from a personal point of view, I would like to tell the story of the origin of the synchrotron as I saw it. The beginning, for me, was in the spring of 1945, when I was on the staff at Los Alamos, the wartime atomic-bomb laboratory. The Trinity test was in preparation, and I was already thinking about what to do on my return to Berkeley—from which I was on leave—after the war ended. I had spent a great deal of time and effort before the war on the design and operation of cyclotrons, I had a reasonably good understanding of the limits on the particle energies attainable by cyclotrons, and it seemed like a worthy goal to find ways to exceed these limits. The cyclotron, as you know, is a resonance accelerator; it pushes particles to high energies by the repeated application of a moderate voltage, which must be applied at the proper instant each time the particle comes around in its circular orbit.

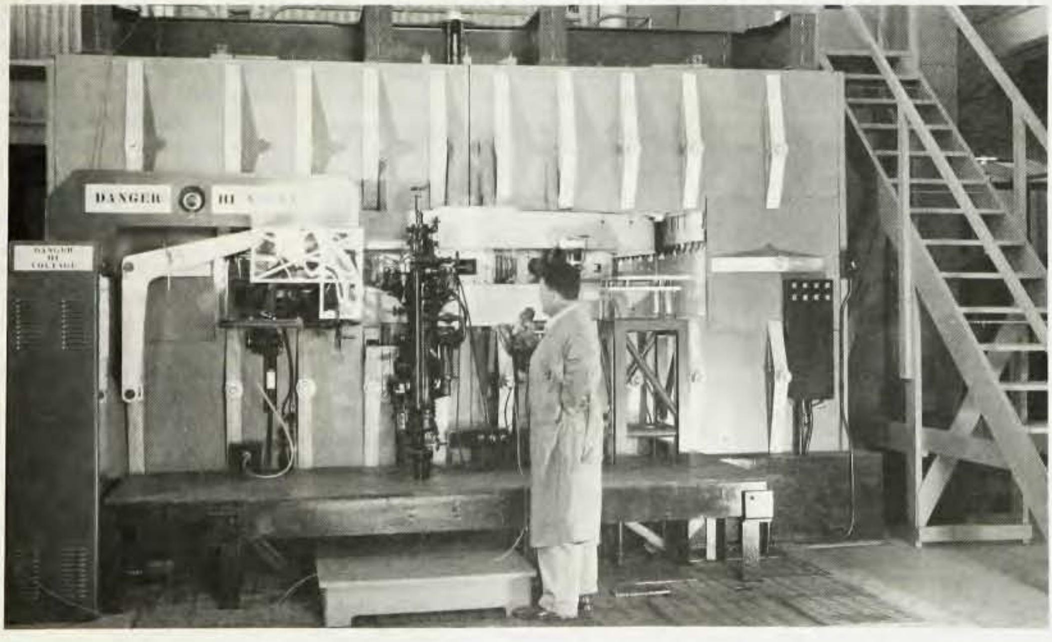
In the simple case of a particle of fixed mass in a uniform magnetic field, the frequency of rotation is constant and easily matched to a fixed accelerating frequency. But things are always more complicated in the real world. The mass of the accelerated particle is not fixed; it increases by the mass equivalent of the added energy. The magnetic field cannot be uniform or the particle orbits will not be stable. Bethe and Rose had pointed out these things in 1937, but at that time the economic limits on the size of machines were more important than limitations in principle. By 1945 this situation was reversing. One way to avoid the timing problem was to use an induction accelerator or betatron, in which the acceleration is independent of timing. So it happened that in May 1945 I started trying to design an air-core betatron. The reason for the air-core was that the absence of an iron core allowed the use of a high magnetic field and reduced the size of the machine for a given energy.

Edwin M. McMillan is professor emeritus at the University of California, Berkeley.

Discovery of phase stability

This design never got very far. One night as I was lying in bed thinking about the problem of getting high-energy particles, my mind returned to the concept of resonance acceleration. If there were only some way to keep the motion of the particles in step with the alternating electric field that was pushing them along! I was tracing out in my imagination the motion as it unfolded in time when I suddenly realized that it had a natural tendency to lock into step with the accelerating field, if certain simple conditions were satisfied. I felt like the inventor in a cartoon with a lightbulb flashing on over his head. I did not record the date of that night, but it must have been close to the first of July. The next day, I started to tell my colleagues at Los Alamos about my idea. I remember vividly the reaction

Berkeley synchrotron after completion in 1948. Walter Gibbins, who supervised the construction, is standing in front. The machine reached its design energy of 300 MeV in January 1949. Figure 1



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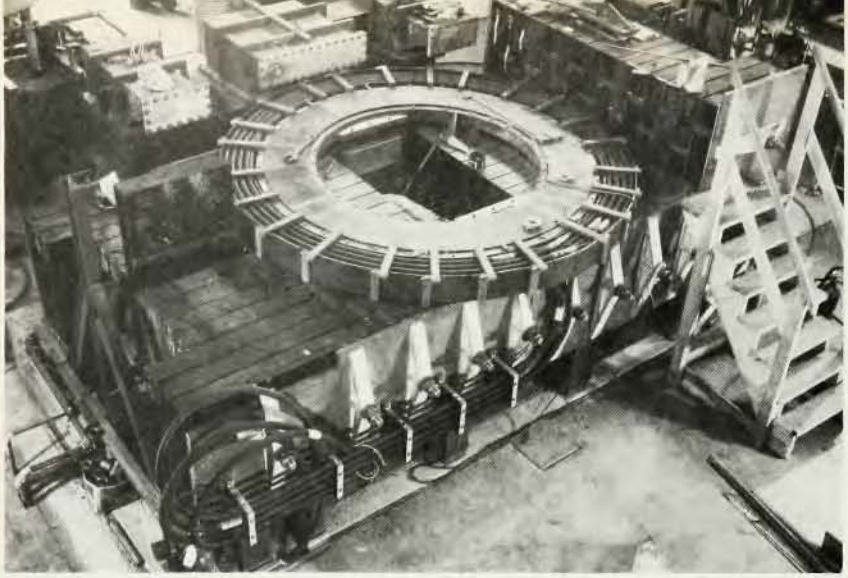
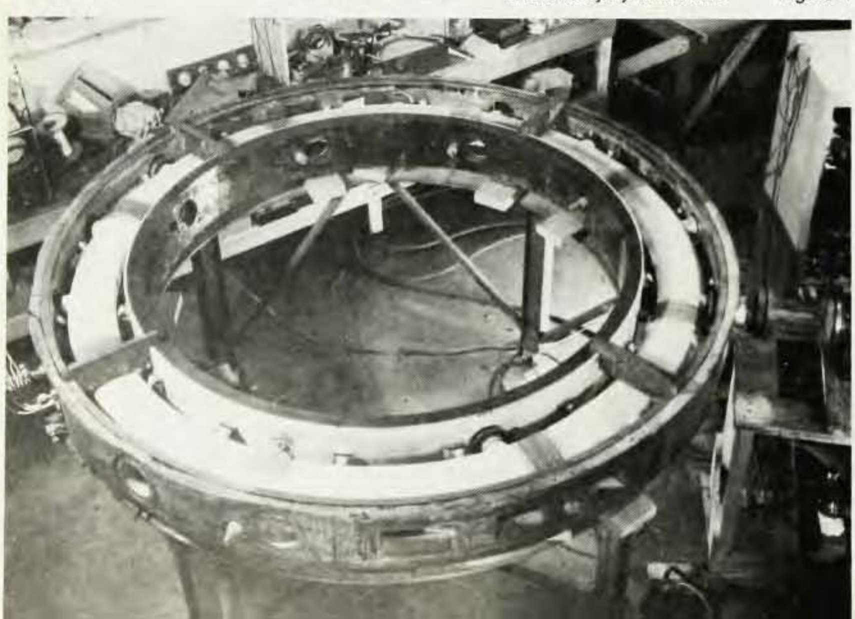


Lower yoke of the Berkeley synchrotron magnet and the coils that carry pulses of current from the condenser bank to excite the rectangular-design magnet. Figure 2

Capacitor bank for Berkeley synchrotron was assembled from surplus units at other installations. Figure 3



Vacuum chamber of fused quartz doughnut type was successfully used in Berkeley synchrotron. Figure 4

of Don Kerst, who said: "I am kicking myself that I didn't think of it." Soon I had a name for the locking-in phenomenon, which I called "phase stability" because the word "phase" is used to describe the timing relation, and a name for the accelerator that would use that principle, which I called the "synchrotron."

On 4 July I communicated my thoughts to Ernest Lawrence in Berkeley by a letter which concluded, referring at first to the air core betatron,

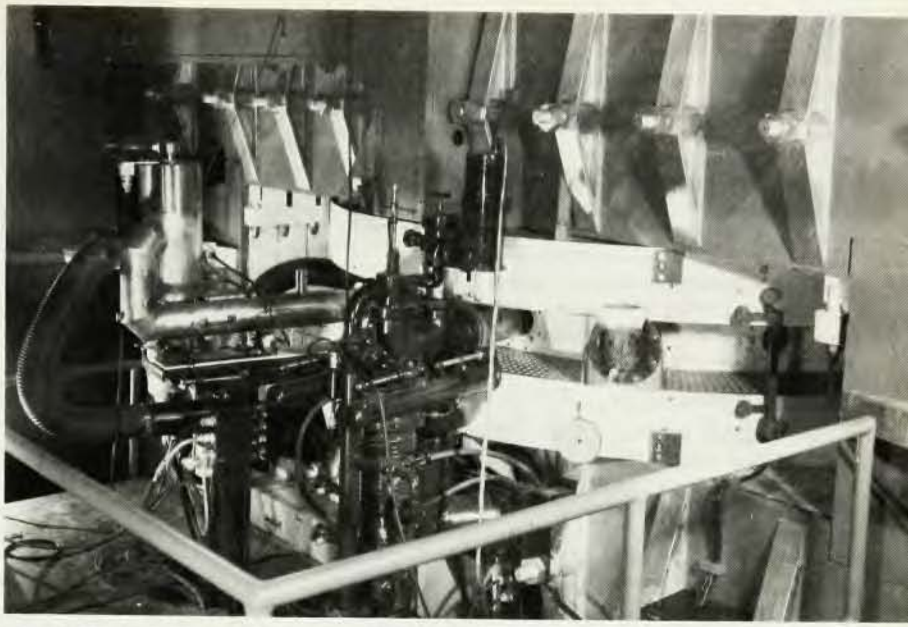
> In any case, it is pretty much of a "brute force" machine, and it is not the sort of thing that one would want to build if a neater way could be found to do the job. I believe that I have a much neater way of accelerating electrons. A brief description of its principle is enclosed. I will send further details.

The "neater way" was the synchrotron, already called that in the enclosed brief description, which starts:

> This is a device for the acceleration of particles to high energies. It is essentially a cyclotron in which either the magnetic field or the frequency is varied during the acceleration, and in which the phase of the particles with respect to the high energy electric field automatically adjusts itself to the proper value for acceleration.

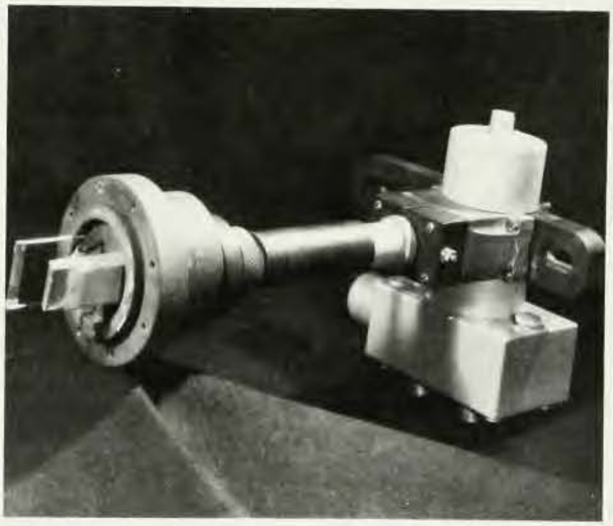
Today, the possibility of varying both field and frequency together would be specifically mentioned under the name "proton synchrotron," and the version with frequency variation alone would be called a synchrocyclotron. Lawrence and I had further discussion when he came to New Mexico to witness the Trinity test on 16 July, and he agreed that the construction of a synchrotron in Berkeley should be seriously considered. There were still some theoretical worries about the loss of energy by radiation (what is now called "synchrotron radiation"), and when the answer to this problem came—in the form of a calculation by Julian Schwinger, brought to me by I. I. Rabi—I went ahead with the publication of a Letter to the Editor of the Physical Review entitled "The synchrotron—a proposed high energy particle accelerator"; this was submitted for publication on 2 September 1945. (Rabi tells me that he persuaded Schwinger to make the calculation because of his concern over my problem.)

Later in September I returned to Berkeley. The war was over, but the Manhattan Engineer District was still providing funds for the Radiation Laboratory. General Groves was supportive of Lawrence's plans for conversion back to peacetime research activities, including the construction of a synchrotron, and design work was started at once, along with searches for surplus materials that might be usable. The actual directive authorizing construction was issued by the Manhattan District Office in Oak Ridge, Tennessee, on 29 August 1946. This authorized a total cost of \$500 000, of which \$225 000 was in the form of actual expenditures, while the rest represented the value of capacitors that existed as surplus at other installations, and that would be needed for storing energy to power the magnet. It did not include the building, for which \$61 052 had already been authorized under another directive. All of this went on before the formation of the Atomic Energy Commission; the synchrotron was authorized and its basic funding was arranged while the Army was still in charge.



Radiofrequency oscillator that supplied accelerating potential for electrons in Berkeley synchrotron is the brass structure at left with tube extending into center of machine. Figure 5

Target inside bore of vacuum doughnut consisted of platinum strip (at left) which produced x-rays when struck by electron beam; scintillating crystal on same mounting enabled measurements of the beam intensity. Figure 6



International efforts

Some time late in October of 1945 I got a telephone call from Charlotte Serber, who was then the librarian at Los Alamos. She reported that a Russian journal that had come into the library had in it an article, in English, describing an idea for an accelerator that was much like the synchrotron. I wrote to her on 30 October and requested a copy of that article, and thus I learned that Vladimir I. Veksler of the Soviet Union had developed the idea of phase stability in much the same way I had. A few months later, there appeared in the *Physical Review* a letter by Veksler complaining of my failure to give reference to his previous publications. In reply to this I sent a personal letter to Veksler and a letter to the editor of the *Physical Review*, in which I said: "It seems to be another case of the independent occurrence of an idea in several parts of the world, when the time is ripe for the idea." Veksler sent me a very friendly reply, dated 27 June 1946, in which he said:

> I fear that the English translation of my letter was somewhat more gruff than the Russian original. You are quite justified in saying that the history of science affords many examples of the simultaneous appearance of similar ideas in several parts of the world, as in our own case.

When Veksler used the word "simultaneous" he was being generous, as he had made three publications on the subject, his first being over a year ahead of mine, but when communications are almost non-existent, the concept of simultaneity is modified. I must admit that communications did not get much better for some time, and although it seemed likely to me that Veksler was building a synchrotron in Moscow, I had very few details about it.

I had even less information about the proposal that Mark Oliphant made in 1945 for the construction of a machine in Birmingham, England. There were some rumors among the British contingent at Los Alamos about such a proposal, but no one seemed to know much about it. Oliphant had talked about it with Lawrence during visits to Berkeley, but apparently in very general terms, so that Lawrence's knowledge of what Oliphant was planning was neither clear nor specific. During the design period of the Berkeley synchrotron, there was no interaction with the Birmingham group; it was only later that I found out that the original unpublished proposal, which contained little in the way of design detail or theoretical analysis, was for what would now be called an air core proton synchrotron. This was modified to an iron core design before construction was started in Birmingham.

The first electron synchrotron to operate was that of F. G. Goward and J. E. Barnes, who modified an existing 4-MeV betatron to give 8 MeV as a synchrotron at the Woolwich Arsenal in England in 1946. Incidentally, Goward told me later that they got the idea from my publication, which they saw before they saw Veksler's. The second synchrotron was that of Herbert C. Pollack and his colleagues at the General Electric Laboratory in Schenectady, which was made from parts originally intended for a betatron, and which gave 70-MeV electrons. It was with this machine that the phenomenon now known as "synchrotron radiation" was first observed in 1947. Even before these two pioneer synchrotrons, however, the principle of phase stability was shown to be valid by experiments conducted by J. Reginald Richardson and collaborators at Berkeley, using the old 37-inch cyclotron with the addition of a rotating variable condenser to modulate the frequency. The success of these experiments led to the redesign of the 184-inch cyclotron (its construction had been halted by the war) as a synchrocyclotron, using the synchrotron principle with frequency modulation, and it was brought into operation late in 1946.

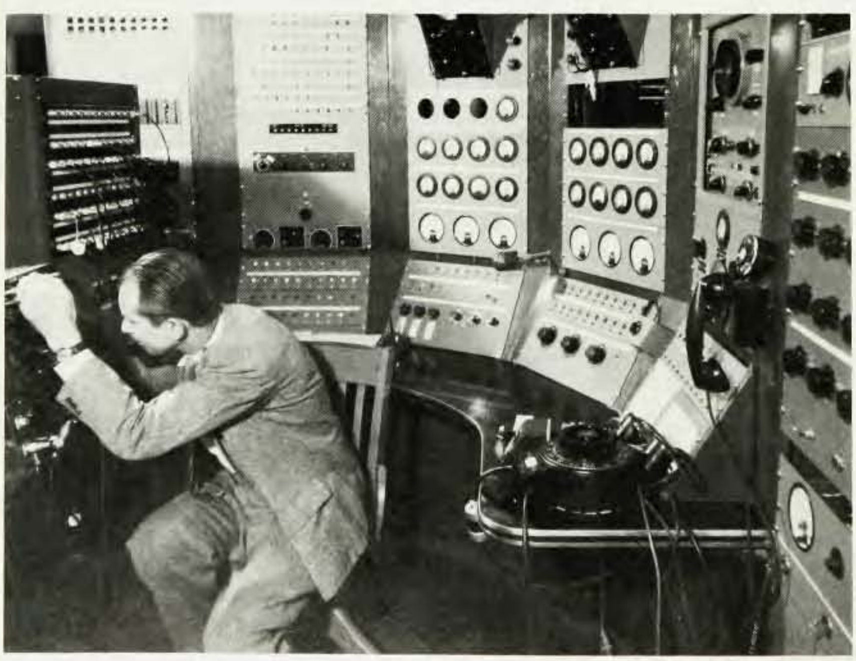
Berkeley synchrotron

Now let me return to the construction of the synchrotron at Berkeley. The design energy had been set at 300 MeV in the published letter, but no design details had been established; therefore we had much to do, and many people became involved (far too many to list here). For the magnet core, a rather conventional rectangular design was used (see figures 1 and 2). It was to be excited by the energy stored in a large capacitor bank (see figure 3) and discharged through the magnet by a set of ignitrons, giving pulsed operation, with a batch of electrons accelerated at each pulse. The original vacuum-chamber design, however, was far from conventional. It depended on the magnet pole tips and the plastic walls supporting the pole tips being made vacuum tight; this proved to be impossible, as the plastic used was too porous, and this design had to be abandoned. We went to a more conventional design that used a fused-quartz doughnut-type of vacuum chamber (see figure 4), and this arrangement worked out fine.

First acceleration of beam in Berkeley synchrotron was documented by film exposed to narrow cone of x rays produced by electron beam. Figure 8

Another serious problem was caused by irregularities in the shape of the magnetic field, due to remanence in the laminated iron pole tips. This was particularly bad at the instant when electrons were injected into their orbits, when the field was weak and the errors due to remanence were relatively large. Other groups who had started to build 300-MeV synchrotrons at about the same time—Robert R. Wilson at Cornell, Ivan A. Getting at MIT and Robert C. Haxby at Purdue—had the same problem, and a great deal of gloomy correspondence went on between the groups. At Berkeley, Wilson M. Powell, our expert on magnet design, set out to correct these field errors in detail with hundreds of little wires cemented onto the pole tips. This massive effort turned out to be unnecessary, however, and all of Powell's wires were finally removed. The shape of the orbit is determined primarily by the low harmonics of the azimuthal field distribution; in the system that was finally used, the field was corrected octant by octant with individual controls brought into the control room so that one could adjust the field shape during operation.

With these adjustments it would be possible to optimize a beam of electrons once it was found; the problem was to find the beam the first time, when we did not know where to set the adjustments. We were trying various things when, on 20 November 1948, a telephone call came in from Wilson at



Operator in Berkeley synchrotron control room adjusting controls while observing the signal from the "divining rod" scintillating crystal (see figure 6). Figure 7

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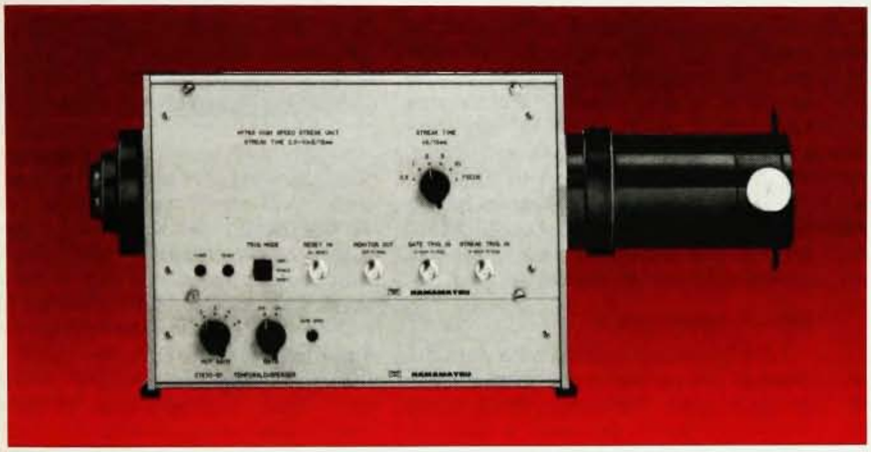

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Cornell; he told me that he had found a beam by operating the magnet at very low voltage. Three days later we found a beam at Berkeley, using the same procedure. Then the magnet voltage was raised bit by bit, optimizing the adjustments at each stage, and the full design energy was reached on 17 January 1949.

Figure 5 shows the oscillator that supplied the accelerating potential, and figure 6 shows the target that the electron beam was supposed to strike to make x rays. In this view, the actual target is the platinum strip at the left, which is inside the bore of the doughnut when the assembly is in place. Next to the target is a scintillating crystal that makes a flash of light when the beam hits it. This light would travel down a transparent lucite rod to the photocell in the box at the right. The signal from the photocell was displayed in the control room. We called this device the "divining rod" because it served to detect and measure the presence of a beam in the machine. I believe that this represents the first use of what is now called a "light pipe" in connection with particle detection; it was proposed by Emilio Segrè and built by Clyde Wiegand, and without it I don't know how we would have gotten the synchrotron into operation.

Figure 7 shows a scene in the control room, with the operator (myself) watching the signal from the "divining rod" while making adjustments with his two hands. At the extreme right of the picture are the sixteen knobs (eight for the upper magnetic pole and eight for the lower pole) that controlled the magnetic-field corrections I mentioned earlier. As soon as a high-energy beam was found and allowed to strike the target, we could look for the x rays produced by the impact. The x rays would be expected to emerge in a narrow cone and to make a dark spot when they struck a photographic film. On 16 December 1948, when a sufficiently high energy was reached, we put a film in the path of the x rays and exposed for 80 minutes; the result is shown in figure 8. This film was signed by all present at the occasion.

Figure 9, taken ten years later, shows the "business end" of the synchrotron as it appeared during most of its life as a research instrument. The x-ray beam from the platinum target, which was inside the donut, emerged toward the viewer through a hole in a lead collimator, a little to the right of center. Two years later, in 1960, the Berkeley electron synchrotron was retired. It is now in the Smithsonian Institution, as part of a very fine exhibit of nuclear research equipment. The last figure shows me with Vladimir Veksler, taken at a meeting in Berkeley in 1959, and illustrates the fact that we did not allow our initial lack of communication to persist forever.

Proton synchrotron

Finally, let me consider the development of the proton synchrotron, again as seen from Berkeley. As I noted earlier, Oliphant proposed a machine of this type in 1945, but in Berkeley we had no clear notion at that time what was going on in Birmingham. William M. Brobeck, the chief engineer at the Radiation Laboratory, quite independently had the idea of designing a proton accelerator of the synchrotron type with a time-varying magnet field, but with the addition of a time-varying frequency to keep the orbit radius constant or nearly constant. This was some time in 1946, but because Brobeck apparently kept no records of the inception of the idea, the exact date cannot be fixed. I recall that Robert Serber and I were both consulting with Brobeck on the design, but we did not keep records either.

The earliest tangible record is a drawing by Brobeck dated 12 November 1946, labeled "10 billion volt proton accelerator." This drawing shows many features that were embodied in the Bevatron, such as the use of four straight sections in the orbit, allowing space for injection, acceleration, and ejection of the beam. There were also features that were changed, including the energy. Lawrence thought that the cost would be too high and insisted that the size, and therefore the energy, of the machine should be reduced. I recall that sometime during this stage of the design both Panofsky and I indepedently insisted with Lawrence that the energy should not be reduced below the threshold for making antiprotons, which is about 6 GeV. A drawing made in October 1947 and labeled "Study No. 2 of 50 foot bevatron" shows the next stage of development. The energy was to be 3 or 6.5 BeV, depending on the magnet gap and aperture used. The orbit radius, which was 80 feet in the original design, had been reduced to 50 feet, and that

Business end of Berkeley synchrotron—x-ray beam emerges toward viewer through a hole in lead collimator, to right of center. Figure 9

Vladimir Veksler to the left of the author at a meeting in Berkeley in 1959. Figure 10

became the radius used in the final design for the Bevatron.

The design work that I am describing was well known in other laboratories. I remember one occasion when Rabi was visiting Berkeley and was shown Brobeck's first drawing, with which he was greatly impressed, and was given a copy to take home. Thus it came about that when the time came to make serious proposals for construction to the Atomic Energy Commission, now in charge of funding for the laboratories, both Berkeley and Brookhaven were in contention. In November 1947 and February 1948 the General Advisory Committee discussed the matter at length, debating how many machines should be built, what size and where. The final decision of the Commission was to build two machines, one at Brookhaven to give 3 BeV and one at Berkeley to give a little more than 6 BeV. The formal authorization was sent to Berkeley on 20 May 1948. Note that by this date the electron synchrotron at Berkeley was still not yet operating, but the 184-inch synchrocyclotron had been running with great success for over a year, so there was no doubt that the principle was sound. And long histories of success also were achieved with the Cosmotron and Bevatron (as the machines at Brookhaven and Berkeley were called, owing to a lack of agreement at the time on a generic name) and with the still more powerful accelerators made possible by the later invention of strong focusing.

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