feature article

Defending against nuclear weapons: A 1950s proposal

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The discovery of strong focusing inspired Robert Wilson's ingenious idea to use mobile particle accelerators as a countermeasure against atomic weapons.

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The aftermath of World War II—the "physicists' war"—was marked by the worldwide growth of physics. The cold war made physics and physicists integral components of national security, particularly in the US and the Soviet Union. University physics departments grew dramatically in size. Nuclear and high-energy physics in particular received lavish government funding, and the way was paved for rapid and remarkable development.

One key discovery toward the end of the war was the principle of phase stability, introduced independently in 1945 by Vladimir Veksler in the Soviet Union and by Edwin McMillan in the US. Prior to that development, to achieve the highest possible energy, protons were accelerated in cyclotrons, whose operation was limited to nonrelativistic energies of less than 20 MeV when the magnetic field was held constant. In Veksler's and McMillan's designs, both the magnetic field strength and the frequency of the accelerating voltage were varied with increasing particle energy to keep the particles in a trajectory of constant radius. Such a construction not only overcame the previous energy limitations, but made it much easier to create a homogeneous magnetic field and a better vacuum in the small region of the ring in which the particles traveled.

Phase stability stimulated the design and construction of a plethora of new accelerators, including the 3-GeV Cosmotron at Brookhaven National Laboratory and the 6-GeV Bevatron at the Berkeley Radiation Laboratory (now Lawrence Berkeley National Laboratory), both proton synchrotrons. In 1951 Cornell University finished constructing a 300-MeV electron synchrotron and was planning to increase its energy substantially during the next year. Great Britain and the Soviet Union were engaged in similar ambitious accelerator programs, and several continental European nations were exploring the possibility of cooperating to build a large accelerator.

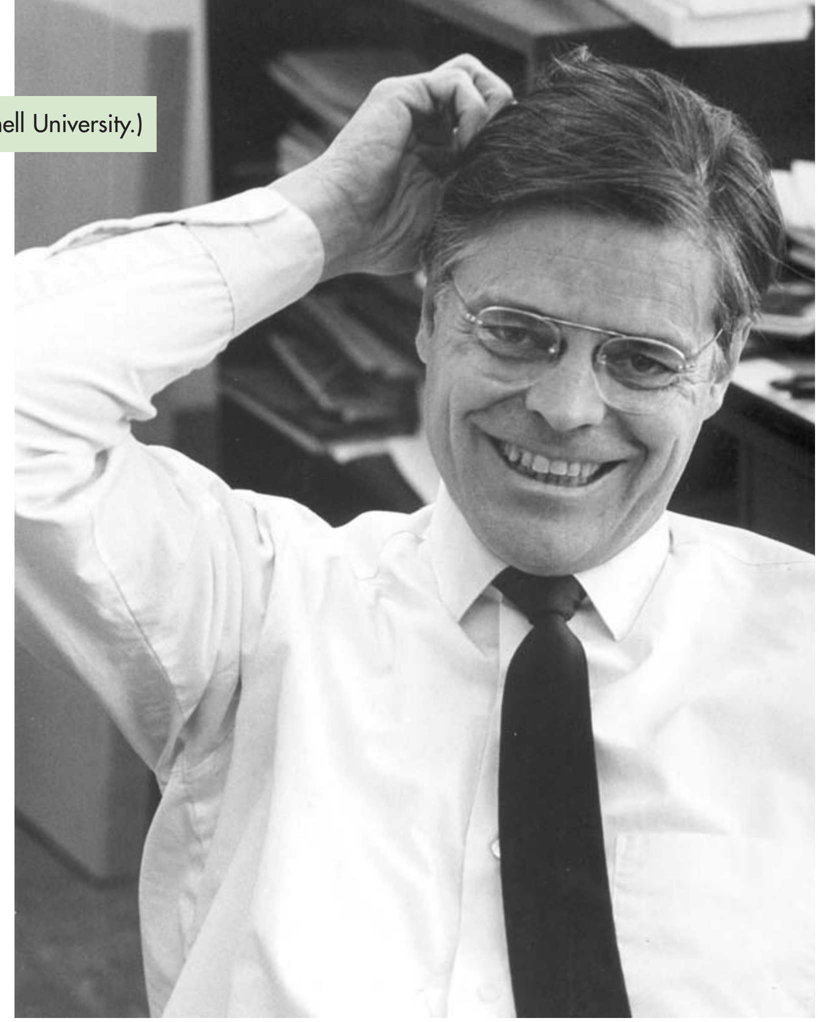
Even as those dramatic energy advances were being planned, the next limitation became apparent. Accelerating the particles would magnify their small deviations from a circular orbit, which could lead to beam loss. Higher energies could be achieved by increasing the size of the accelerator, the strength of the magnets, and the size of the aperture through which the beam traveled, but those quantities could not be increased without bound.

Robert Wilson and strong focusing

In June 1952 Robert R. Wilson, then the director of the Newman Laboratory for Nuclear Studies (LNS) at Cornell, was invited to Copenhagen to attend a conference for "planning an international laboratory and organizing other forms of cooperation in nuclear research." The planned laboratory eventually became CERN.[1] The first two weeks of the conference were devoted to lectures on recent experimental and theoretical developments in high-energy physics. Several sessions during the third week addressed the properties of recently built and planned accelerators and advances in accelerator physics.

One of the speakers was Edouard Regenstreif of the United Nations Educational, Scientific, and Cultural Organization. Regenstreif had studied the Brookhaven Cosmotron and the Berkeley Bevatron, which were both under construction at the time. He reported on some preliminary work by Ernest Courant, Stanley Livingston, and Hartland Snyder at Brookhaven on a new technique called alternating-gradient focusing (also called strong focusing), which promised to alleviate the problem of beam instability. In accelerators such as the Cosmotron, the magnetic field through which the particles traveled had a very large radial gradient pointing toward the center of the accelerator. If some of the magnets were turned around so that the gradient pointed alternately inward and outward, it was expected that the beam would be focused in both the radial and vertical directions. The vacuum chamber in which the beam traveled could thus be made much smaller. Moreover, the dimensions of the magnet that produced the field could be reduced by an order of magnitude, so that the cost of building and running the accelerator would be greatly reduced.[2] Regenstreif indicated that in the Cosmotron's array of magnets one could cut down the aperture from the original 8-inch by 24-inch design to "two inches vertically and something like ten inches horizontally," without compromising the beam. Much higher energies could therefore be achieved within the practical limitations of accelerator size and cost.

On his plane trip back to the US, Wilson thought about the possible uses of strong focusing. As he later recalled at a May 1985 Fermilab symposium, "instead of scaling up, as the Brookhaven people wanted to do," he began "to scale down." He worked out that "scaling the Cosmotron down to 1 GeV



Robert R. Wilson circa 1956. (Courtesy of Cornell University.)

meant that you would come out with a ridiculously small aperture of about a third of an inch by perhaps three inches. That seemed very small. The magnet would also be very small in cross section."[3]

Strong focusing for nuclear defense

On 8 August 1952, Wilson wrote excitedly to Hans Bethe, his senior Cornell colleague who was at Los Alamos at the time, about the progress he had made using the Brookhaven results. First, Wilson put forth a design to build a small but powerful accelerator at Cornell. He anticipated that the machine would weigh about 15 tons and accelerate electrons to energies of 1.5 or even 2 GeV, with an intensity 100 times that of the existing 300-MeV LNS machine. That machine was built and would come on line in 1954 as the first strong-focusing accelerator in the world.

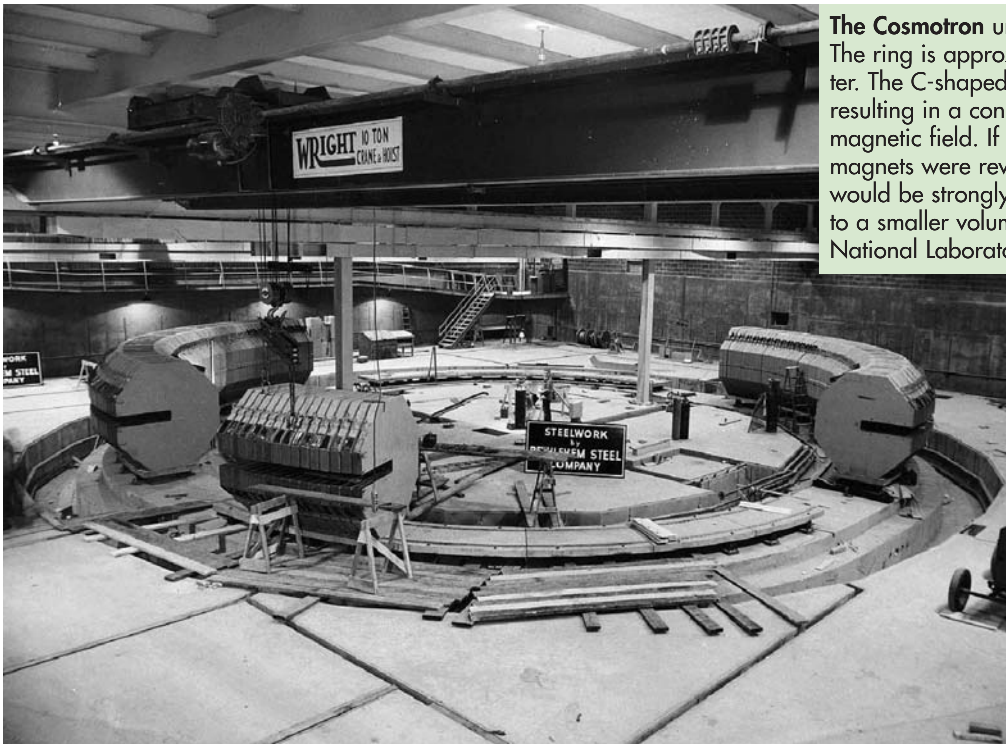
Wilson went on in his letter to Bethe to describe an idea, which he'd already sent to the Office of Naval Research and the Atomic Energy Commission, for another use of a scaled-down accelerator:

> As a result of this design the ONR representative, Mr. Leigh, became very excited about the possibility described in the accompanying document. I made the enclosed calculations and was not able to convince myself that it was not feasible. I reported these findings to the people in Washington in the ONR, the AEC (Paul Fine in the AEC).
>
> I am sending this to you because it seems to me that the people at Los Alamos are in a much better position to evaluate the possibilities than were the people in Washington. They struck me as overly enthusiastic, if anything. If the scheme should be at all feasible the development should not be at Cornell but should be at some place such as Brookhaven or Berkeley where they are set up to do such things on a large scale.
>
> In any case I do take the matter very seriously, but I do feel that someone who is qualified, such as yourself, should review the possibility. . . .
>
> Because of the possibility of classifying our work at Cornell you can understand my lack of enthusiasm for the success of this scheme. On the other hand, I feel driven to support it because of its obvious importance if it should turn out to be feasible.

The accompanying document Wilson had mentioned was a plan for a scaled-down mobile accelerator—a "little bevatron"—that weighed a few tons and would produce an intense beam of 500- to 600-MeV electrons to be used as a defense against atomic bombs. The electron beam from the mobile betatron (or a neutron beam that it could produce through the bombardment of a uranium target) could be directed at an incoming atomic bomb to disrupt the bomb's detonation. In 1952 atomic bombs were still to be delivered by airplane and dropped by parachute to allow the aircraft that delivered them time to get away from the blast region. Wilson's idea was to spray the incoming weapon continuously with neutrons or gamma rays as it dropped below an altitude of one mile; the irradiating particles would cause the weapon to predetonate and thus produce a fizzle. As was later explained at an AEC conference held in early October 1952, "[A fizzle] application concerns the possible protection of localized targets by achieving a reduction of the magnitude of an atomic explosion. This would be accomplished by a directed beam of high-energy radiation and would result from the production in the weapon of sufficient neutrons at the time of assembly [of the critical fissile mass]. In some circumstances reliance would be placed upon the receipt of a radio or radar signal just prior to assembly, so that a short (≈ 200 microseconds) pulse of radiation may be used."

In mid-August Wilson sent letters summarizing the scheme to Henry DeWolf Smyth, one of the AEC commissioners; to J. Robert Oppenheimer, the chairman of the general advisory committee of the AEC; and to Ernest O. Lawrence, the director of the Radiation Laboratory at Berkeley and an influential senior adviser to the government on atomic energy, weaponry, and science and technology policy. All the letters were essentially identical in reporting on the device and included Wilson's document outlining the properties of the generated beams. All of them stressed that "the application of the Cornell design of a small Bevatron to this problem was pointed out by Mr. A. Leigh of the Office of Naval Research. "

Wilson's letter to Lawrence is shown in the box on page 39. In his letter to Oppenheimer, Wilson added that he was not interested in pushing the matter further, but that it seemed important enough to warrant bringing it to



The Cosmotron under construction in 1950. The ring is approximately 23 meters in diameter. The C-shaped magnets all point outward, resulting in a constant radial gradient in the magnetic field. If the orientation of alternate magnets were reversed, the resulting beam would be strongly focused and thus confined to a smaller volume. (Courtesy of Brookhaven National Laboratory.)

Oppenheimer's attention. Incidentally, Wilson concluded his letter by informing Oppenheimer of steps he had taken to bring Oppenheimer's brother Frank to Cornell. Frank had lost his position at the University of Minnesota in 1949 because he had lied about his membership in the Communist Party. Wilson told Oppenheimer

> I am trying to make it possible for Frank to come to Cornell as a Research Associate this fall. So far I have gotten the approval of the Dean and Ted Wright, our Vice President for Research, but the matter still has to go through the President and the Board of Trustees. I am not overly optimistic.

Wilson's caution was not unfounded; Frank Oppenheimer did not return to physics research until 1959, when he took a position at the University of Colorado.

Reactions to Wilson's proposal

Oppenheimer carefully studied Wilson's proposal and on 18 August 1952 sent it to Jerrold Zacharias, who was organizing numerous summer projects related to national defense.[4] (See also PHYSICS TODAY, July 2006, page 39.) In his letter Oppenheimer told Zacharias that Wilson felt "that this is a little too serious to be ignored; but he has a hunch that it will not be practical," and that Wilson had "a strong desire not to become involved in the undertaking himself." Oppenheimer also told Zacharias that he had "made a couple of numerical changes on the photo-neutron yield range quoted from [Joseph] Levinger, which bring down the number of neutrons by a factor of about 100, but still do not rule out the practicality of the business entirely."

Wilson continued working on the design of his compact 600-MeV synchrotron, and on 21 August he sent Bethe a detailed five-page statement concerning the design of the machine, together with blueprints. He had not marked the document secret because he felt "it is the application and not the particular machine that should be classified."

Evidently, Leigh told his superiors at ONR about Wilson's design and its capabilities as an x-ray machine and as a producer of beams that could cause fizzles in incoming atomic weapons. They in turn contacted various people at the AEC. Fine, the head of the division of military application of the AEC, asked Jane Hall, a staff member, to evaluate Wilson's proposal. Fine thereafter must have contacted Bethe, for on 8 September Bethe wrote a single-spaced 16-page letter to Fine evaluating Wilson's scheme. Bethe had received from Wilson a newer version of his proposal "in which electrons rather than protons were to be accelerated in the betatron and gamma rays rather than neutrons were to be transmitted through the air to the bomb."

Bethe indicated to Fine that his reaction to the proposal was quite favorable, more so than to the recommendations of an ad hoc committee that had met in February 1950 to discuss countermeasures, or to the recommendations that Hall had expressed in a letter to Fine. Bethe added, "Clearly any assessment of the value of this device must wait for the successful construction of a bevatron of small weight and high intensity. . . . If [Wilson's] present optimism is justified and a machine [for pure research purposes] can be constructed which weighs of the order of 15 tons and delivers about 10^{14} electrons per second, I believe that it [the mobile bevatron] would be a very worthwhile device to be considered as a counter-measure."

Bethe noted that the value of Wilson's device depended on the method of delivery of atomic weapons. He thought that as long as atomic weapons were being delivered by planes, the most effective defense would be to destroy the carrier, and if that failed, to destroy the bomb by a missile carrying a small-yield atomic warhead. As he explained, "The destruction of the bomb by an electromagnetic device such as Wilson's bevatron . . . is considerably less attractive because it will still leave an appreciable yield to the atomic bomb," something on the order of 10%. However, Bethe noted that the situation would become rather different when atomic bombs could be delivered by guided missiles rather than by planes. Since it would be extremely difficult to destroy a guided missile by a counter-missile, the electromagnetic scheme offered great advantages. An electromagnetic beam could reach its target in a few tens of microseconds, during which time the missile would not move appreciably.

Bethe then analyzed two other aspects of Wilson's proposal: the particles used (neutrons or gamma rays) and the time needed to irradiate the incoming atomic bomb. Since the beam's range was the key to military interest in fizzling devices—a range less than 2 km would not be attractive for most purposes, whereas a range of more than 8 km would be more useful—Bethe summarized his evaluation in terms of range. He concluded that Wilson's bevatron could fizzle an implosion plutonium bomb at a distance of 2.5 km if the particles reached the bomb at the time the high explosive was detonated inside the bomb, and at a distance of 1 km if the bomb were irradiated continuously. Continuous irradiation could also fizzle a gun-assembled uranium bomb at a distance of 3 km. Moreover, Bethe thought that if Wilson's proposed intensities could be achieved, the mini-bevatron would be a promising device for scanning ships' cargo for smuggled bombs.

The AEC calls a conference

The AEC found Bethe's conclusions sufficient to warrant calling a conference to discuss the matter further. On 25 September 1952, George Kolstad, the acting chief of the AEC's division of research, sent a letter to a dozen prominent physicists from US universities and laboratories: Wilson and Bethe from Cornell; Zacharias from MIT; Snyder, G. Kenneth Green, and Leland Haworth from Brookhaven; Robert Bacher and Charles Lauritsen from Caltech; Robert Hofstadter and Wolfgang Panofsky from Stanford; Robert Serber from Columbia; and Maurice Shapiro from the US Naval Research Laboratory. In it Kolstad wrote

> During the past few months some new developments in the field of particle accelerators have revived the question of the use of such machines as military weapons. To examine and evaluate the ideas that have been put forward since the last conference on this subject several years ago and to recommend what action, if any, the government should take to exploit these ideas, the Division of Research is calling a one-day conference. If at all possible, it would be appreciated if you could attend.

The conference was called for 9:00am on Friday, 3 October 1952, and was held at the AEC building in Washington, DC. All of the physicists except Bacher and Lauritsen attended. In addition, the conferees included several high-level personnel from the AEC and the ONR, as well as several staff members from the AEC's division of biology and medicine.

At the conference, Wilson explained the subject with a technical discussion that emphasized electrons and photons and illustrated the application by assuming that the mini-bevatron produced electrons of 1 GeV. The conference delegates discussed Wilson's ideas, arrived at a consensus, and made the following recommendation:

> It is the opinion of the group that some increased emphasis should be given to the study of high energy physics and accelerator development and that the question of the military use of such accelerators should be kept in mind as high energy physics and accelerator development go forward. A small group of physicists should assume responsibility for relating available knowledge to the problem, correlating ideas as they arise, and making suggestions to the [Atomic Energy] Commission at an appropriate time. Further work should also involve the questions of signal detection, timing, and asymmetric detonation.

The delegates then addressed the problem of detecting concealed fissile materials and the possibility of using Wilson's mini-bevatron to examine the content of ships' cargo by detecting the delayed gamma rays from fission fragments produced when particles from the accelerator react with fissile material. Bethe and H. William Koch offered to evaluate the problem of examining diplomatic baggage. A discussion was also held concerning the use of an "electronuclear machine for anti-personnel weapon," the personnel being the aviators in an attacking aircraft, but the conclusion was that such an application was not practical. The concluding and principal recommendation of the conference was that "accelerator development should be strongly encouraged, directed to obtaining light-weight, high current devices; e.g. determination of maximum storable charge, including possibility of counter-currents for space-charge neutralization; multiple input possibility; [and] controlled output rate."

Wilson's letter to Ernest O. Lawrence

Dear Ernest,

In designing and modifying our synchrotron here at Cornell I have been impressed with the possibility of building very small electron accelerators which may have some application as a counter-measure against atomic weapons detonated in the air. It seems possible to construct a 600 million volt electron accelerator that will give a continuous beam of γ-rays with an intensity of 10^{14} effective quanta per second, that is less than 10 feet in diameter and weighs a few tons, that can be mass produced at a cost of $100,000 or so, and that consumes a low power of several kilowatts.

All this comes from scaling down the Brookhaven cosmotron and using a few tricks here and there. The injector would be a few feet of the Stanford linear accelerator which now operates at 10^{13} per pulse. Such a machine would produce 10^8 effective quanta per cm^2 per sec at one mile. According to Levinger's formula,* one complete absorption of one quantum produces 4×10^{-2} W neutrons, so our photons will produce some 10^9 neutrons per sec per cm^2 of absorption at one mile.

Conceivably, this might be effective in causing bomb fizzles in air bursts. It seems to me that the intensity might be such as having a range of as much as 5 miles. The enclosed paper gives further details.

If the above is at all feasible I would think that Berkeley would be an ideal place to develop such small accelerators. In any case I wanted to bring the matter to your attention.

Warmest personal regards,

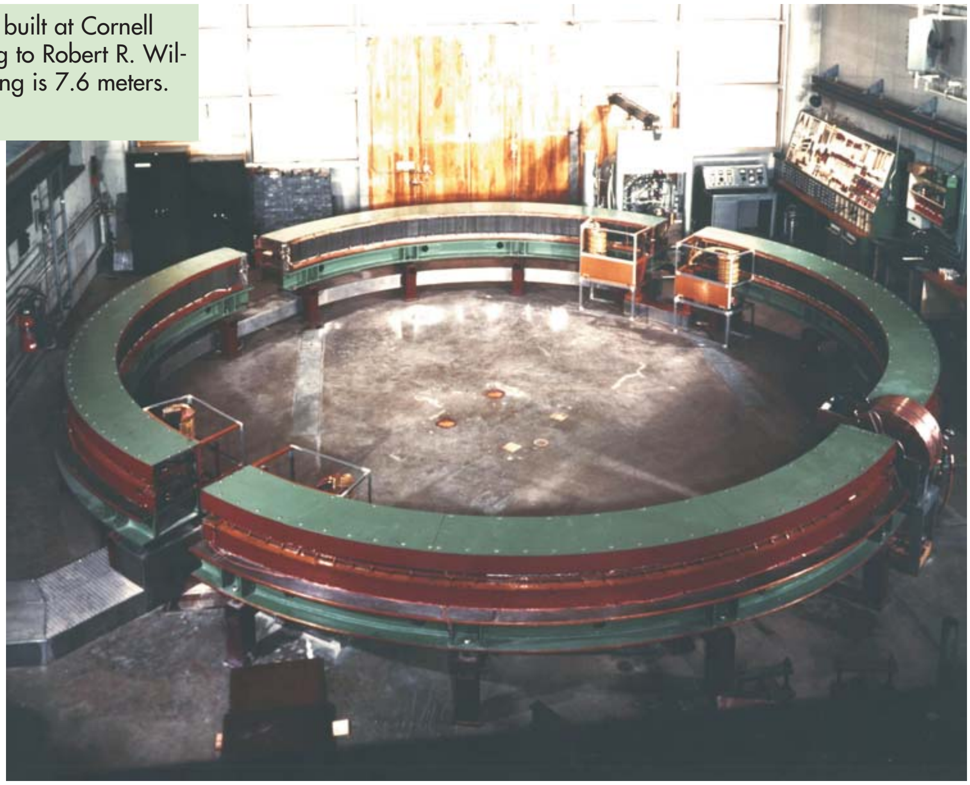
Yours sincerely,
Robert R. Wilson

*Joseph Levinger had calculated the neutron yield for photons absorbed in lead as a function of the photon energy W measured in units of 100 MeV. Wilson had assumed that W was measured in MeV. J. Robert Oppenheimer later corrected Wilson's error.

Accelerators and national security

I do not know what the subsequent history of Wilson's machine was. The AEC and the US Department of Defense continued to show interest in the idea at a 1953 meeting and in a subsequent report,[5] but there is no available unclassified documentation of the plan ever being implemented.

Connections have been made in the past between the support of high-energy physics and the interests of the US



The strong-focusing synchrotron built at Cornell University in the 1950s according to Robert R. Wilson's plan. The diameter of the ring is 7.6 meters. (Courtesy of Cornell University.)

government in fighting the cold war. In his lecture at the May 1985 Fermilab symposium, historian of science John Heilbron pointed out some of the many military benefits to government support of high-energy physics. To the physicist, Heilbron stated, the Berkeley Bevatron was built to produce antiprotons. But historians would add that the AEC built the Bevatron in the hope that the knowledge of nuclear forces might be exploited in new sorts of weaponry, and also to provide an opportunity to keep the experienced engineering staff at Berkeley together for mobilization in a national emergency. According to Heilbron, the Bevatron, despite its uniqueness in energy, is best understood as only the biggest of the many redundant accelerators commissioned at universities in the immediate postwar years by the Manhattan Engineer District, the ONR, and the AEC.[6]

It is well known that the AEC, the ONR, and the DOD continued to underwrite the construction of accelerators and support high-energy physics, both for the reasons outlined by Heilbron and for the contribution of high-energy physics to the development of pattern recognition and computing. But those issues should also be seen from a wider perspective.

Often dire consequences are attributed to the support that the armed forces, the DOD, the ONR, and the AEC gave to physics in the years following World War II. In some cases that may well be correct. But those organizations were responsible for defending the nation against what was considered to be a dangerous and implacable enemy, and the knowledge of physics and its capabilities clearly was a crucial factor in maintaining the balance of terror and avoiding a nuclear holocaust.

Individual scientists responded to the perceived threat in different ways. I have found Wilson's involvement particularly significant precisely because he refused to have anything to do with the design of nuclear weapons after the war. Although he was a pacifist before the outbreak of World War II, his assessment of the Nazi threat was such that he became deeply involved during the war in exploring the feasibility of nuclear weapons and in devising methods for the electromagnetic separation of uranium isotopes. He was one of the first physicists to arrive at Los Alamos in the spring of 1943 and eventually became the head of the physics research division there. He was also one of the first scientists to be deeply troubled by what had been accomplished at Alamogordo and what had been wrought on Hiroshima and Nagasaki.

Having had his idea for using accelerators for nuclear defense, Wilson clearly felt it essential to develop the design and to explore its effectiveness. Like Dale Corson, Donald Hamilton, Edward Purcell, and others, he felt it important to cooperate to the extent of his ability and expertise to safeguard the nation, while at the same time safeguarding the integrity and openness of universities. But it should be added that even though Wilson didn't work directly on the design of nuclear weapons after the war, he was involved with secret work, primarily to use the knowledge of nuclear power for peaceful applications. He was a consultant at Los Alamos on "electrodynamic high density shock waves" in implosions and their possible use for a controlled thermonuclear device.[7] He evidently also thought intensely about "low pressure hydrogen reactors"[8] and even considered obtaining a patent "concerning a production of energy from sea water."[9]

Cancer therapy

When Wilson thought up his idea for a mobile accelerator, he probably also had in mind the application of such a device to cancer therapy.

In 1946, "to salvage what was left of [his] conscience" and to come up with an application of nuclear physics to save people instead of killing them[10], Wilson published in the journal *Radiology* an important and seminal paper called "Radiological Use of Fast Protons." In 1941, while at Princeton, he had accurately measured the range and energy deposition of 4-MeV protons in matter. He observed that most of a proton's energy was deposited near the end of its path. In 1946, while at Harvard, he extended those considerations to protons with energies up to 150 MeV and found similar results. That led him to the idea of using protons for cancer therapy. Carefully controlling the energy of the protons could cause most of their energy to be deposited in a well-delineated volume, such as a localized cancerous tumor inside the body. For example, 115-MeV protons would deposit most of their energy in a region 10 cm below the nearest surface, and 140-MeV protons in a region 15 cm below the surface. In treatment by x rays, on the other hand, photons interact with all the tissues along their path, and in fact deposit most of their energy near the surface of the body.

Wilson's 1947 *Physical Review* letter in which he announced his findings stated the case succinctly:

> It is evident that such protons have radiological applications since it is possible to treat a volume as small as one cm^3 anywhere within the body, and give to that volume several times the dose of any of the neighboring tissue. Thus 10^9 protons per cm^2 will produce more than 1000 r.e.d. [roentgen equivalent dose] in the last half cm of the range, but the skin dose will be less than 100 r.e.d.[11]

The first facility for proton cancer therapy was at the Harvard cyclotron that Wilson had helped to construct after the war. After 1952 Wilson thought of using his mobile bevatron design for medical applications. He kept up his interest in the field of proton therapy throughout his life. In the late 1960s, while designing the Fermilab Tevatron, he came up with a tabletop version of a proton accelerator for use in medical applications and explored the feasibility of commercial production of such an instrument. To date, more than 45 000 people have undergone proton cancer therapy.

After 1952, whenever Wilson spoke of such compact accelerators, it was always the medical application that he mentioned. He seemed to have forgotten—or chose to forget—the genesis of the life-giving machines.[12] He eloquently expressed his views on accelerators and national security in his exchange with Senator John Pastore during his testimony before the Congressional Joint Committee on Atomic Energy on 17 April 1969 in connection with the funding for the Tevatron:

> Pastore: "Is there anything connected with the hopes of this accelerator that in any way involves the security of the country?"
>
> Robert Wilson: "No sir, I don't believe so."
>
> Pastore: "Nothing at all?"
>
> Wilson: "Nothing at all. . . ."
>
> Pastore: "It has no value in that respect?"
>
> Wilson: "It has only to do with the respect with which we regard one another, the dignity of men, our love of culture. . . . It has to do with are we good painters, good sculptors, great poets? I mean all the things we really venerate in our country and are patriotic about . . . it has nothing to do directly with defending our country except to make it worth defending."

I would like to thank Dale Corson, Paul Forman, and Kurt Gottfried for their helpful criticisms, and Elaine Engst and David Corson for their assistance in the use of the Bethe and Wilson papers and for permission to use the quoted materials. I am indebted to Albert Silverman for pointing out Wilson's involvement with cancer therapy using proton beams.

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