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QUESTS WITH U.S. ACCELERATORS—50 YEARS THE HIGH ENERGY PHYSICS AND NUCLEAR PHYSICS RESEARCH PROGRAMS

HEARING

BEFORE THE

SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION

OF THE

COMMITTEE ON SCIENCE AND TECHNOLOGY U.S. HOUSE OF REPRESENTATIVES

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WITNESSES

QUESTS WITH U.S. ACCELERATORS—50 YEARS THE HIGH ENERGY PHYSICS AND NUCLEAR PHYSICS RESEARCH PROGRAMS

WEDNESDAY, JULY 23, 1980

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION,
Washington, D.C.

The subcommittee met, pursuant to notice, at 9:30 a.m., in room 2318, Rayburn House Office Building, Hon. Mike McCormack (chairman of the subcommittee) presiding.

Mr. McCORMACK. The meeting will come to order.

Good morning, ladies and gentlemen. The Subcommittee on Energy Research and Production is pleased to hold this hearing on particle accelerators, commemorating 50 years of U.S. research with these devices in such fields as high energy physics, nuclear physics, materials science and chemistry.

First, I would like to explain to those in the audience, unfamiliar with the term, what we mean by accelerators. They are machines that create concentrated beams of subatomic particles, which can be used by scientists much like one might use a microscope, to probe the smallest entities in nature. These beams also have practical uses in industry and medicine.

This year marks the 50th anniversary of the development of the first U.S. particle accelerator. In the last 50 years, particle accelerators, often called atom smashers, have evolved from the original laboratory gadgetry of the 1930s into the giant, mile-sized research facilities of today. This morning we shall hear about the fascinating history of these remarkable machines.

Each major advance in accelerators and particle detectors has produced new and important discoveries: about the fundamentals of matter and energy; discoveries concerning the forces in nature and the world of elusive subatomic particles, such as quarks and gluons; and the creation of new elements, beyond the dreams of the original alchemists. Of course, these discoveries were followed by numerous Nobel prizes for American scientists.

In addition, developments in accelerator technology have led to spinoffs with direct practical applications, such as in the field of energy technology.

For example, superconducting magnets now being installed in our latest generation of accelerators will be crucial components in our magnetic fusion reactors, which are coming very soon.

In another area, that of inertial confinement fusion, one of the most promising concepts makes use of ion beams fired from accelerators at pellets of hydrogen isotopes.

As we continue to advance the frontier of knowledge about the smallest constituents of matter, we need larger and more powerful machines.

The major research programs using accelerator facilities, the high energy physics and nuclear physics programs of the Department of Energy and the National Science Foundation, are truly in the realm of what we call big science, with a combined annual budget in excess of $500 million.

These are successful research programs, in which our Nation has been preeminent in the world, and in which we can be proud.

However, our preeminence is now being challenged by competition from abroad and is being compromised by the all too familiar combination of escalating operating and construction costs and constrained budgets.

Thus, as we shall hear this morning, our accelerator-based science programs are at a critical point in their history.

So, as we mark the passing of 50 exciting years of research with accelerators, it is timely to hold this hearing to explore how far we have come and where we are going from here and what it shall require.

This morning we are going to hear from two panels of distinguished witnesses. Our first panel is at the table now.

Dr. M. Stanley Livingston, a pioneer cyclotron developer at the University of California in Berkeley. He is a professor, retired, from MIT.

The next man is Mr. Craig Nunan, assistant to the president of the Medical Group of Varian Associates, Palo Alto, Calif.

On my left at the table is Prof. Stephen Brush, Department of History and Institute for Physical Science and Technology, University of Maryland.

We will have three other witnesses in a second panel—Dr. Wolfgang Panofsky at Stanford; Dr. John Adams from Geneva, Switzerland; and Dr. N. Douglas Pewitt from the Department of Energy.

Before we proceed with our first panel, I should like to ask Mr. Wydler, the ranking minority member of the subcommittee, if he would like to make an opening statement.

Mr. WYDLER. Thank you, Mr. Chairman.

Mr. Chairman, this hearing is part of a celebration in more than one way. The Congress is commending the scientific leaders who developed accelerators 50 years ago in 1930, but we also celebrate the fact that accelerators have stayed in the forefront of scientific research for the surprisingly long period of 50 years, and there are more years to come, obviously.

Certainly the Congress and the taxpayers share some credit for this since they have made commitments of hundreds of millions of dollars to build these machines. But the lion's share of the credit must go to the researchers themselves, who do the exacting scientific work. Equally important, they make their complicated work understandable to the public and to the Congress.

However, the Nation should not contentedly bask in the glories of Nobel prizes for accelerator work. Nor does it help that most of the work turns up more and more incomprehensible subatomic particles.

With new machines costing half a billion dollars or more we must wonder what the limits are to the size of future accelerator projects. Funding these projects will take bigger and bigger bites out of the total national resources allocated to research.

The United States appears to be slipping in the international competition of applying scientific advances to industrial product development. It is claimed that the Japanese and Germans do better, often capitalizing on U.S. discoveries.

It is important, therefore, that the Congress and the public pay close attention to the industrial and medical spinoffs from accelerator technology. We must not simply expect these applications. We must vigorously sponsor them.

It is inevitable that we will be comparing the U.S. and European accelerator programs. Certainly the Isabelle accelerator will help keep the United States at the fore in the world competition.

Of course, as one who lives on Long Island, I am very proud of the people who started the advances in accelerators, and I applaud the State of California for their early efforts in this regard, and I applaud those who are carrying it on today. But I cannot help thinking that to a great extent the future of accelerator development lies in my own Long Island, at the Brookhaven National Laboratory.

This Isabelle machine will make Long Island a center of excellence in high energy physics and attract highly skilled technical people and distinguished scientists to our New York area.

It will create hundreds of jobs during construction and hopefully spinoff some advanced technology firms during its innovative lifetime. These important aspects make it much easier for a politician to support, given the tremendous pressures from other programs competing for Federal dollars.

It is said that the European budget is twice the U.S. budget but that the United States accomplishes just as much. The question is, whether this is a tribute to U.S. scientific ingenuity or simply a transitory situation on the way to U.S. accelerator inferiority.

I look forward to getting insights on these matters from our witnesses today.

Thank you, Mr. Chairman.

Mr. McCormack. Thank you, Mr. Wydler.

Ladies and gentlemen, I should say our two panels are divided so that one will be dealing with historical trends and societal benefits and the other one will be programs in the United States and other nations, along with prospects and problems for the future.

I want to say to all the panelists that all of your testimony, as you have submitted it, along with your own biographies, will be inserted in their entirety in the record at the appropriate place.

I would like to recommend to you, having scanned the material, sincerely that you summarize your material, or we are not going to get through today.

So, I would like to recommend to you that each of you present your material in informal fashion, as you wish to do.

Dr. Livingston, would you proceed.

PANEL ON HISTORICAL TRENDS AND SOCIETAL BENEFITS OF RESEARCH USING ACCELERATORS: M. STANLEY LIVINGSTON, PIONEER CYCLOTRON DEVELOPER AT THE UNIVERSITY OF CALIFORNIA, BERKELEY, PROFESSOR (RETIRED), MASSACHUSETTS INSTITUTE OF TECHNOLOGY; CRAIG NUNAN, ASSISTANT TO THE PRESIDENT OF THE MEDICAL GROUP, VARIAN ASSOCIATES, PALO ALTO, CALIF.; AND PROF. STEPHEN BRUSH, DEPARTMENT OF HISTORY AND INSTITUTE FOR PHYSICAL SCIENCE AND TECHNOLOGY, UNIVERSITY OF MARYLAND AT COLLEGE PARK

[The biographial sketch of Dr. Livingston follows:]

M. Stanley Livingston - Biographical Sketch

Professor of Physics, Retired, Massachusetts Institute of Technology

1905 : Born, Brodhead, Wisc. May 25, 1905
1926 : A.B., Pomona College
1928 : M.A., Dartmouth College
1931 : Ph.D., University of California, Berkeley
1964 : D. Sci., Hon., Dartmouth College
1967 : D. Sci., Hon., University of Hamburg, Germany
1971 : D. Sci., Hon., Pomona College

1931-34: University of California, Berkeley
Associated with E.O. Lawrence in original development of cyclotron.

1934-38: Ass't. Prof., Cornell University
Associated with Prof. H. Bethe in first comprehensive survey of nuclear physics: Rev. Mod. Phys. 9, 245-390, 1937.

1938-70: Ass't. Prof., Assoc. Prof., Prof., Massachusetts Institute of Technology
Designed and built cyclotron for 16-MeV deuterons, Jour. Appl. Phys. 15, 2 and 128, 1944.

1944-46: Office of Field Service, O.S.R.D. (leave of absence), assigned to Operations Research Group, U.S. Navy Department.

1946-48: Chairman, Accelerator Project, Brookhaven National Laboratory
Design of 3-GeV Cosmotron and other accelerators.

1952 : Co-author, discovery paper on alternating gradient magnetic focusing, Phys. Rev. 88, 1190-96, 1952.

1954 : Author of book: High-Energy Accelerators, Interscience Publ., Inc., NY

1956-67: Director, Cambridge Electron Accelerator, Harvard Univ. and M.I.T., Design and administration of 6-GeV electron accelerator.

1962 : Co-author of book: Particle Accelerators, McGraw-Hill Book Co., NY

1966 : Editor of Book: Development of High-Energy Accelerators, Classics of Science series, Dover Publications, Inc., NY

1968 : Author of Book: Particle Physics - The High Energy Frontier, McGraw-Hill Book Co., NY

1968 : Author of Book: Particle Accelerators: A Brief History, Harvard Univ. Press, Cambridge, MA.

1967-70: Assoc. Director, National Accelerator Laboratory, Associated in design and construction of 200/400-GeV proton accelerator.

1971- : Consultant, Nuclear Regulatory Commission, Atomic Safety and Licensing Board Panel.

1971- : Consultant, Los Alamos Scientific Laboratory.

1980 : Chapter, "Early History of Particle Accelerators," Advances in Electronics, Ed. L. Marton, Vol. 50, Academic Press

Member: American Physical Society, American Institute of Physics, Sigma Xi, American Academy Arts and Sciences, National Academy of Sciences (1970).

Retired. Residence: 1005 Calle Largo, Santa Fe, New Mexico

July 1980

STATEMENT OF DR. LIVINGSTON

Dr. LIVINGSTON. Thank you.

Mr. Chairman, members of the subcommittee, I have been asked to start this discussion of the way particle accelerators have affected scientific progress by presenting a brief history of the early days of these machines.

The story starts with Ernest Rutherford at the Cavendish Laboratory in England in 1919, when he first disintegrated the nucleus of the nitrogen atom, using alpha particles from natural radioactive sources.

This opened a new era in science, and the dream of the alchemists was achieved. For the first time man could transmute atoms. However, scientists were quite aware that much more powerful tools were needed than the natural radioactive materials if they were to explore this new field of study properly.

For the next 10 years there were quite a few studies using natural radioactivities, and they measured the binding energies of the nuclei of the light atoms. They found them all to be many millions of volts. It gave the general impression that 1 million volts was about the least that you would need in order to disintegrate atoms.

None of the existing engineering machines of those days could go to more than a few hundred kilovolts. Several places started work on it. By 1926–27 Cavendish and several other places in Europe started.

A few of the younger associates of Rutherford, John D. Cockcroft and E. T. S. Walton, chose to use a kind of machine called a voltage multiplier. It was a machine in which you could multiply the voltage by charging capacitors in parallel and discharging them in series.

They did improve on that and were making considerable progress by the year 1930, which we are now talking about.

In the United States the physical sciences were largely limited to the classical fields which had been developed in Europe. There were very few scientists who were informed in this new field, which soon came to be known as nuclear physics.

However, by 1929 there were several individuals and several groups that were sufficiently impressed with the future in this field. For example, Robert Van de Graaff, on returning to Princeton from a Rhodes scholarship in Oxford, invented the belt-charged electrostatic generator. His purpose was to develop a machine with enough voltage to disintegrate atoms. He described it to the American Physical Society in 1931.

However, he went on to MIT. I can speak briefly of that. He did not do anything himself in developing it, leaving it mostly to a small group here at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington—Gregory Breit, Merle Tuve, Lawrence Hafstad, Odd Dahl. They had been working on voltage devices which proved not to be very practical to accelerate particles.

So when Van de Graaff's machine came along, they jumped on to it immediately and they built the first useful and practical accelerators on the east coast, using the Van de Graaff system.

By 1931 they had some voltage from this kind of machine. By 1932 they were well in shape and were able to start doing nuclear physics experiments.

The other place in this country which was well aware of the importance of these high energies was my own laboratory, where Ernest Lawrence was the leader, at Berkeley, Calif.

He invented the thing originally called the magnetic resonance accelerator, and is now known as the cyclotron, in 1929. He got the idea from a paper in a German journal written by a Norwegian physicist named Wideröe.

This article was published, and Lawrence told me later that although he could not read German very readily, he could see from the diagrams what was the principle of operation of Wideröe's experiment.

He realized what was there was a resonance between an electrical field made by a radiofrequency voltage applied to an electrode, which would make voltages at two ends of an electrode; particles going through would be accelerated twice, once going in and once coming out, if they had exactly the right entry speed and the time was right.

In other words, a resonance had to be established. The resonance principle that was in Wideröe's paper was the inspiration to Ernest Lawrence to start studying other devices. He thought, for example, of the chance of using a magnetic field to make particles go in circular orbits, and go through a pair of radiofrequency electrodes several times.

So, he derived the magnetic and electric field equations for that situation, and solved them, and found that the particles would go at the same period, no matter how much energy they achieved or how large the orbit. This meant that you could accelerate them all the way out.

He then decided what he needed was a system with crossed fields, radiofrequency electric fields between two electrodes—hollow—shaped, like half pillboxes, and a magnetic field perpendicular to that which would make them go in circles. This is the actual principle of the cyclotron.

Lawrence was quite aware of the field that he called nuclear excitations—those are the words he used to me in those days—which is basically nuclear disintegrations. He realized if he could get up to the million volt level, he could use these particles for this sort of excitation.

I am speaking here a little bit personally because I was there. I was going around as a graduate student looking for a doctorate thesis subject; Ernest Lawrence was a young, active assistant professor in those years. He suggested that I try and make this work.

There had been a previous trial by a graduate student of an earlier generation called Edlefson, and Lawrence had actually published one article with Edlefson about the principle of the machine, but they had not proven the method would work.

So, Lawrence suggested to me that I take this idea and demonstrate the existence of this kind of resonance of the particles in the magnetic and the radiofrequency fields. I accepted and immediately started to work. That was in the early summer of 1930.

I built a vacuum chamber out of a section of brass ring, with brass end places, waxed down with red sealing wax, and sealed with a torch between the poles, evacuated it, put hydrogen gas in.

I had a very low-powered radiofrequency system in which I got the help of a young chap named David Sloane, who was a radio ham operator and knew about such circuits. His help made it possible for me to make this try.

It was some time in early December of 1930 when I made the first observation of resonance. This kind of resonance existed and was observed this way: I had the beam of particles that were going out to the edge of the machine recorded, collected in a collector cup, where the particles had gone through several slits to define the way in.

So, at the periphery of machine I had this collector cup, and it went through insulators to a galvenometer. It was a very small current that I expected to get, and I needed a sensitive detector.

I would set one frequency on the radio system, by picking a coil with a certain number of turns, and then would tune the magnetic field through the proper calculated value.

It was in this way, sometime in late 1930, when I first observed resonance. I am told that that event is recorded in my data book, which is one of the exhibits in the Smithsonian Institute. I have asked for a copy.

I haven't had a chance to study it yet, but I haven't seen that data book for 45 years. So this will be an interesting experience.

As I say, in December 1930, I observed resonance of these particles. Next, I would change the frequency, and then go through the magnetic field and find another point of resonance with a different magnetic field, and then do it again.

I finally got a full curve, and this full curve was exactly as described by Lawrence's magnetic calculations, his basic equations of motion. So we knew this was it.

Lawrence then was absolutely certain that we had proven the point, we were off to the races, and so he was already starting to find funds for the next machine.

This first machine was small, about 4 inches in diameter. A replica is out here in the museum.

The next one was to be larger. It was to be 1 million volts. I was to do it, and Lawrence rushed me through my doctorate so I could hold an instructorship and carry on with him the next year.

We built the next machine out of anything we could find. Lawrence applied for and got $1,000 from the Research Council for his work on this big machine. I put it together in a hurry.

During the spring and summer of 1931, I was actually observing for the first time resonance in this machine. It went up to 1.2 million volt protons, the first time 1 million volt protons were attained in scientific history.

The vacuum chamber for this was also a brass box. It was sent by Lawrence in later years to England to Dr. Cockcroft, and he put it in the Museum of Science in Kensington, in London. So that one is located there.

The magnet disappeared. It was part of the laboratory stock, and other people used it. So that is not now in existence.

But then long before I even had that going, we got word that Cockcroft and Walton had actually disintegrated the atom with a much lower voltage than we had. They only had about 400 kilovolts, but they had the advantage of being with Lord Rutherford, having his inspiration, and knowing also what was going on in the theoretical field.

Certain people in the field of wave mechanics, early workers, one of them was Condon and Garney in this country and also George Gamov in London, both did it more or less simultaneously and proved that there was a chance that with a wave kind of understanding of the mechanisms of atoms, of nuclei, you could predict a penetration of this big barrier, millions of volts high, and that certain low-voltage particles could go through. In other words, you could observe disintegration at a much lower voltage of excitation.

So, they tried it and it succeeded. They were the ones in 1932 who first described the disintegration of the atom, for this Cockcroft and Walton got the Nobel Prize in later years.

We at Berkeley, meanwhile, were working to improve the operation of our 1-million-volt cyclotron out there. We did not have anything like proper detection equipment, and we were not going to use the kind of scintillation apparatus that they were using in Cavendish—Rutherford's technique.

So, we had to start making electronic devices. Lawrence got several people to help. All of us joined together to make electronic instrumentation, and before long we had that experiment ready to go.

Within about 2 to 3 months after we got the news of Cockcroft and Walton's disintegration of the lithium nucleus, we were also disintegrating nuclei.

We were the first laboratory in this country to do that, and we extended our work into other targets. So, as early as 1932 with the 1-million-volt machine, we had made a major start in the field of nuclear physics in this country.

Lawrence, of course, was working on the next step even before I had finished improving and developing this 10-inch, 1-million-volt machine. He was after something bigger, quite a bit bigger.

But this was in the midst of the Great Depression and he had to use a lot of substitutes and get a lot of gifts. He found a large magnet core that came from a Poulsen arc and a copy of that is in the museum here labeled Federal Telegraph Co. on the front of it.

It was given to Lawrence and we had it machined to make both faces 27 inches in diameter. About that time, I was asked in early 1932 to be in charge of the construction of that machine. It was located in a building called the radiation laboratory, known later as the old radiation laboratory, for which I understand the door is in the exhibit hall with only this title on it. It was the original one sent here by Ed McMillan, taken off the old building.

Nevertheless, that building was the center of activities for many years. It was in that building that the Lawrence laboratory began to get its big reputation.

Within 2 years, we developed this machine from its first start at 1 million volt protons to 5 million volt deuterons. Other young scientists joined the group and research students came over continuously from the physics department to join our group.

David Sloane did a great deal on his own with the assistance of some others, like Bernard Kinsey and Wesley Coates, to build linear accelerators following the principle of Wideröe's resonance.

They were not very successful as nuclear accelerators and were eventually abandoned. It was left for Alvarez to do it right after World War II, with a different style of machine.

Sloane designed, and Jack Livingood built, a unique radio frequency resonance transformer which used a resonant coil inside a large evacuated cavity, all of copper. It produced up to 1 million volts. You could insert electrons and get X-rays of 1 million volts quite easily.

Incidently, one of the things I did during that period was to build one of these X-ray machines over in the San Francisco hospital, of the University of California, and started the first million-volt X-ray laboratory in this country. It operated there for 20 years, and was the first of the deep therapy X-ray tubes. But that was on the side.

Later on, Henderson came back and built modern counting equipment, such things as the magnetic control circuits, to make them stable.

Incidentally, Malcolm Henderson invented the name cyclotron about 1933. It was first used as laboratory slang and then picked up by news reporters and popularized. So, the name didn't really exist until about that time.

Bob Thornton and Ed McMillan both came to Berkeley in 1934 to this laboratory. I think Ed McMillan had been there for some years before, working as a postdoctoral student. These are the only two present members of the Berkeley staff who overlapped my time at Berkeley.

Just a few comments about what went on.

We were the first accelerator laboratory to use deuterons; that is, heavy hydrogen ions. Professor Lewis of the chemistry department concentrated heavy water from battery acid residues and made from this heavy water the deuterium gas.

With that first beam of deuterium ions we began to observe new interactions and new particles and things so different and unusual that essentially every target we used gave new and important publishable results.

We built a variety of measuring instruments to observe all types of disintegration fragments. We built a target wheel, which was installed with a whole series of targets on it and operated by a greased stopcock joint, so that we could turn one after another in front of the beam, and installed a thin mica window so we could get our detection instruments in close.

Out of this situation a lot of things happened. For example, there were exiting times and some near misses going on. Early in 1934 Professor Lawrence brought into the laboratory a copy of a French journal, Comptes Rendus, which described the discovery of induced radioactivity by Curie and Joliot in Paris, using natural alphas on boron.

In that article they predicted that if they had only had deuterons, with a carbon target, they could have made the same activity.

Well, we had a deuteron beam. We had a carbon target on the wheel. We had all the counters you could ask for. It just took a little bit of wiring up, rewiring, so that we could turn the switch and operate the cyclotron beam against the target for 5 minutes and then turn it off and turn on the detecting instruments, and we got the click-click-click-click of induced radioactivity.

Within one-half hour of the time we heard of this new development, we were observing the same thing in the laboratory.

Professor Lawrence was a very wonderful chap to have around. He was very excitable, full of enthusiasm. He would literally dance when these interesting things happened. It was a wonderfully exciting time for young scientists to be there.

I left Lawrence laboratory in July 1934 to go to Cornell and build the first cyclotron outside Berkeley. Later I went to MIT, where I continued my accelerator design activities.

I will leave to others the description of how this great Berkeley laboratory continued to grow and to become as impressive and as important as it has.

Lawrence received the Noble Prize in physics in 1939 for conceiving the cyclotron. He is certainly one of the most important and impressive scientists this country has ever known.

But now I want to return to some of the other things going on.

There were several other places in which these things were happening. At Carnegie, as I said, Hafstad and Dahl were using the Van de Graaff machine, the first model that was successful. They were very soon right on our trail doing a similar sort of work and doing proton calibrations.

They did some of the very good early work. They started research operations about 1 year after we did in Berkeley, in 1933. They first built a 2-meter-diameter aluminum sphere mounted on three textolite legs, and inside it a 1-meter sphere for a voltage divider. This is in the museum hall on exhibit now.

An accelerating tube they built was formed of multisection tubes, which was a very important thing to hold the voltage.

Incidentally, Merle Tuve and Lawrence were high school classmates at a small town in South Dakota, so they knew each other, and our laboratories kept in very close contact. We knew what was going on in the other lab all the time.

Meanwhile, Van de Graaff had gone to MIT, where with the support of President Karl Compton he started the design of a really large generator. It was a monster. Two 15-foot-diameter spheres, each mounted on 6-foot-diameter cylinders of textolite, and each mounted on movable platforms so they could be pushed back and forth together, and intended to get 10 million volts or more between them.

The maximum steady voltages they obtained were 2.4 million volts on the positive and 2.7 million on the negative terminal, and made massive sparks. But their hope that they could put a discharge tube between the two and run particles along there didn't work out. It was not successful. They finally had to abandon it.

One of the reasons was it was in very unclean condition in the hangar. Too many seabirds got inside, and their droppings made the terminals spark much too readily. So they had to move it.

They moved it back to MIT, where the two were mounted side by side in a solid metal-domed building. They obtained maximum voltages of about 2.4 million volts, used it for many years as an experimental tool.

Then many, many years later it was abandoned as obsolete and was put into the Boston Museum of Science. It is still there and occasionally operated to develop massive sparks for visitors.

There were many other electrostatic generators built. The first round of designs were not so successful. They were air-insulated machines, much like Van de Graaff started. All were limited. However, a parallel development was successful. This was the pressure-insulated types.

Ray Herb, who is with us here, and his group at the University of Wisconsin did the original development of a horizontal design, and they developed in a few years up to 4-million-volt energy.

Also, the High Voltage Engineering Corp., a commercial firm started in Cambridge by Van de Graaff, an engineer named John Trump and others, built and sold a wide range of electrostatic generators and pressure-insulated generators of all different kinds.

These belt-drive pressurized generators have a long and successful history. They have been the most popular and useful in all the field of accelerators. There must be, I guess, 1,000 of them around the world, of which a fairly large share were made by the High Voltage Engineering Co.

Now, one more lab in the United States was at CalTech. Charles Lauritsen and Dick Crane started about 1930 to develop an accelerator out of a voltage source made up of transformers which had been developed by their engineering department as a means of testing out high-voltage devices.

They built quite a few tubes, got up to almost 1 million volts, and they started in 1930 to report the first results of a nuclear research program, using 1-million-volt protons.

Eventually the superior qualities of the electrostatic generator were recognized, and they changed theirs also to that type. CalTech laboratory has trained a notable succession of research students, and for many years was a major contributor to nuclear physics.

Now, I have gone through this just to show what happened before World War II. The only other thing that was developed before World War II was the betatron. Since it accelerated only electrons, and electrons were not successful in disintegrating atoms, I don't really call it an atom smasher in the normal sense.

We had to wait until later years when it became useful in the particle physics field to have such high voltage electrons. Although they did make an awful lot of X-rays for commercial and medical applications, it was not an atom smasher. Don Kerst built the first one in 1941 at the University of Illinois and later several others.

At the end of World War II, a new principle, synchronous acceleration, was discovered independently by Ed McMillan of the United States and Vladimir Veksler of the U.S.S.R. These were quite independent. In both countries they started programs of building these big synchronous machines.

The synchronous principle is what has led to the major big machines of the present day: the synchrocyclotrons, for example. The biggest one was 184 inches at Berkeley, and I think it went to something like 740 million volts.

There are also linear accelerators. Ed McMillan built the first electron synchrotron, which was successful and went to 300 million volts, and immediately displaced all betatrons from all their other uses.

The time that I again got interested was with the proton synchrotron. Two machines were built in the United States, one at Brookhaven, where I was in charge of the design of a 3-billion-volt machine, which we called the cosmotron, completed in 1952—and I had left by then to go back to MIT—and at Berkeley, where Bill Brobeck headed the design for a 6-billion-volt machine called the bevatron, and which was finished in 1953.

At least 8 or 10 other proton synchrotrons were built with energies up to 10 or more billion electron volts. Those are the bigger ones.

This story continues with a description of other types and aspects of the increasingly large accelerators of the present day. But they have all grown from these starts that I have described so far.

The voltages obtained from accelerators since the day when I started, 50 years ago, have increased by a factor of 1 million, which I think is a big factor by which man has increased his scope of knowledge in only 50 years.

Thank you.

[The prepared statement of Dr. Livingston follows:]

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Testimony Before the

Subcommittee on Energy Research and Production

of the

Committee on Science and Technology

U. S. House of Representatives

QUESTS WITH U. S. ACCELERATORS--50 YEARS

by

Dr. M. Stanley Livingston
Professor of Physics, Retired
Massachusetts Institute of Technology

July 23, 1980

I have been asked to start this discussion of the impact of particle accelerators on U.S. scientific progress, by presenting a brief history of the early days of these machines.

When Ernest Rutherford at the Cavendish Laboratory in England first disintegrated the nucleus of the nitrogen atom using alpha particles from radioactive sources in 1919, a new era opened in science. The dream of the alchemist had been achieved; for the first time man could transmute atoms. However, scientists realized that more powerful tools would be needed than the particles from natural radioactivity if this new field of study was to be effectively explored. For the next ten years studies using natural alphas were continued; binding energies within nuclei were measured to be several million volts. None of the existing engineering techniques for producing high voltage could approach this energy range. Nevertheless, scientists believed that high energy devices could eventually attain voltages sufficient to disintegrate the lighter nuclei. By 1926-27 work had started in the Cavendish and in several other laboratories to develop the necessary instruments. In his President's address before the Royal Society in 1927, Sir Ernest Rutherford emphasized the need for higher energy voltage sources, and described the status of voltage generating devices throughout the scientific world.

The general belief was that particles of one million volts energy (1 MeV) would be needed.

Most of the known engineering devices were of the "direct voltage" type, which developed high direct voltages. Particles would have to be accelerated through a single large potential drop. These machines were all limited by voltage breakdown, either by sparking at the terminal or by breakdown of insulation or the acceleration tube. Practical limits were reached by using terminals of large radius and by improving insulation. However, voltages were still well below the 1 million volts thought to be necessary. Other types of voltage sources produced oscillatory or surge voltages which were unsuitable for accelerating particle beams. One system, the "voltage multiplier," dodged some of the problems; in this circuit capacitors were charged in parallel and discharged in series, thus multiplying the voltage. Alternating current transformers produced high voltages which were rectified by rectifier tubes to give a direct voltage output of several times the initial value. Two young colleagues of Rutherford at the Cavendish, J. D. Cockcroft and E.T.S. Walton, chose to use and improve this circuit, starting about 1928.

In the United States fifty years ago, the physical sciences being studied were largely limited to the classical fields which had been developed in Europe. It was customary for American scientists to complete their training by studying abroad. Only a relatively few scientists were fully informed in the new field which soon came to be known as nuclear physics. However, by 1929 several individuals and groups were sufficiently impressed with the future in this field to start to develop voltage sources. For example, R. J. Van de Graaff, on returning to Princeton from a Rhodes Scholarship in Oxford, invented the belt-charged electrostatic generator; he described it to the American Physical Society in 1931. A group at the Department of Terrestial Magnetism of the Carnegie Institution of Washington led by Gregory Breit, started experimenting with several voltage sources, including disc-type electrostatic generators and Tesla Coils, searching for a possible source for particle acceleration, during the years 1928 to 1930. And, Professor Ernest O. Lawrence at the University of California at Berkeley invented the "magnetic resonance accelerator" (now known as the cyclotron) in 1929, and first described it in the literature in 1930. Also, several large commercial laboratories were studying high voltages for the development of x rays or for the high-voltage transmission of electric power; but their work was not aimed at producing positive ion beams for nuclear studies.

Cockcroft and Walton in the Cavendish Laboratory were the first to achieve sufficiently high energy ions to disintegrate nuclei, in 1932. Urged on by Rutherford they had chosen to develop a device of modest size and energy. They were also aware of theoretical predictions of the new wave mechanics that protons with energies well below the potential barrier had a finite probability of penetrating the barrier and disintegrating the nuclei, for light atoms. So they tried the experiment even before their voltage supply had been developed to its best performance, with only 400 to 500 kilovolts. They used the lightest practical target, (lithium metal) and a scintillation counting technique for observation similar to that used by Rutherford in his early alpha particle experiments. The scintillations they observed were from the alpha-particle fragments of the reaction: $Li^7 + H^1 \rightarrow He^4 + He^4$. This was the first disintegration of nuclei by particles accelerated in the laboratory. Their results are described in a series of papers in the Proceedings of the Royal Society, London, from 1930 to 1932, which are classics in nuclear physics and have brought enduring fame to their authors, who were awarded the Nobel Prize for Physics for 1931. A few years later a 1.25 MeV voltage multiplier system constructed by the Philips Company of Eindhoven, was installed at the Cavendish Laboratory and was used for nuclear studies for many years.

The next to produce accelerated particles which could produce disintegrations was our group at Berkeley, led by Ernest O. Lawrence. Lawrence conceived the idea of magnetic resonance acceleration, which became the cyclotron, early in the summer of 1929 while browsing through the current journals in the University Library. I learned this in private discussions with him some years later. In the German Journal "Arkiv fur Elektrotechnik" for 1928, in an article by Rolf Wideröe, he saw illustrations which attracted his interest. Although he could not read German readily, he recognized the principle of resonance being described from the illustrations. Wideröe's paper described an experiment in which positive ions of Na and K were accelerated twice by a radiofrequency voltage applied to a tubular electrode, as they traversed the two gaps at the ends of the electrode. This was an elementary, two-stage, resonance linear accelerator, the first of its kind and the origin of the resonance principle.

Lawrence was well aware of the new field of "nuclear excitations" and of the importance of developing new techniques for accelerating particles. He recognized that extension of Wideröe's resonance system to a long array to achieve really high energies would be highly unlikely. So he speculated on variations of the resonance principle, including the use of a magnetic field to deflect particles in circular paths so they would return to the electrode and re-use the electric field in the gap. He derived the equations of motion of particles in such a combination of magnetic and electric fields, and found that the particle would have a constant frequency of rotation independent of its energy or of the size of the orbit. He reasoned that by applying the correct resonance frequency to a pair of suitably shaped electrodes mounted in the uniform magnetic field, ions could be made to resonate with this field, and cross and recross the gap between electrodes many times, each time gaining more energy and traveling in a larger orbit. This is the mechanism of acceleration in a cyclotron.

Lawrence asked a graduate student, N. E. Edlefsen, who had completed his thesis and was awaiting his June degree date, to make a quick experimental study. Although Edlefsen did not observe true resonance, Lawrence considered the results promising enough to present the concept at a meeting of the AAAS in Berkeley that spring, and also submitted a brief article, with Edlefsen, in the journal Science.

I was a graduate student at Berkeley at that time, and in the early summer of 1930 I asked Professor Lawrence to propose a topic for an experimental thesis. He suggested that I demonstrate the validity of this resonance principle (now known as cyclotron resonance). I started experimental work that summer, using the 4-inch magnet used earlier by Edlefsen. I built a vacuum chamber out of a short section of brass ring, with brass end-plates sealed with wax. For an accelerating electrode I used a hollow, half-pillbox of copper mounted on an insulated stem, with the opening facing a slotted bar placed across the diameter. The chamber was evacuated by a mercury-vapor pump; hydrogen gas was admitted and ionized by electrons from a thermionic source at the center. The important difference was a collector electrode shielded so only resonant particles traveling in circular orbits could enter.

I first observed sharp resonance peaks in the collector current when the magnet was tuned through a narrow range, in November, 1930. A notation in my laboratory notebook described the event, which was the first observation of cyclotron resonance. This notebook, and the brass chamber described above, are now located in the accelerator exhibit in the Smithsonian Museum of History and Technology in Washington. Resonance peaks were observed for a wide range of applied radiofrequencies and magnetic fields. At the collector radius and for the highest field used, the computed energy of the hydrogen ions was 80,000 eV. This resonance was obtained with an applied rf potential of about 1000 volts, so the ions traversed a minimum of 40 turns or 80 accelerations to reach the collector. This clearly demonstrated that magnetic resonance acceleration would work and that high energy ions could be produced in this way. I took my Ph.D. degree in June, 1931 and Lawrence obtained for me an Instructorship in the Physics Department for the following year, so I could continue my development of another and larger accelerator.

The first practical cyclotron, which produced protons of 1.2 MeV energy, was built during the spring and summer of 1931. Lawrence obtained a grant from the National Research Council for $1000, which paid for the materials; the Physics Department Shop and my own labor supplied the rest. The magnet had 10-inch diameter poles and weighed about 1000 pounds. The vacuum chamber was a flat brass box (square this time). Radiofrequency power was supplied by an early water-cooled power tube supplied by the Federal Telegraph Company. By December 1931 this machine produced hydrogen molecular ions of 0.5 MeV and by January 1932 protons of 1.2 MeV. This was the first time accelerated ions of over 1 MeV energy had been produced. The original square brass vacuum chamber is now on exhibit in the Kensington Museum of Science in London.

We had barely confirmed our results and I was working to improve beam intensity when we received the issue of the Proceedings of the Royal Society describing the results of Cockcroft and Walton in the Cavendish on the disintegration of lithium nuclei with protons of less than 0.5 MeV. We did not have instruments for making observations of disintegrations at that time. But with the help of others we quickly built electronic counting instruments to do the job. Within three months after hearing the news from Cambridge we were able to observe and measure disintegrations from lithium and from several other light elements.

Lawrence was planning his next step even before I had completed the 10-inch machine as a working accelerator. This was in the midst of the "great depression" and funds were hard to obtain; he was forced to use many economies and substitutes and to solicit gifts to reach the next goal. In late 1931 he located a magnet frame with a 45-inch core from an obsolete Poulsen arc, at the Federal Telegraph

Company plant in Palo Alto. This magnet was rebuilt with cores machined to 27-1/2-inch diameter flat pole faces, and magnet windings were formed of copper strip wound in flat layers and immersed in oil tanks for cooling. This magnet was installed in December 1931 in a frame warehouse near the Physics Building later known as the "Old Radiation Laboratory," which was the center of cyclotron activities for many years. I was put in charge of design and construction in early 1932.

Within two years we developed this 27-1/2-inch machine from its first operation at 1 MeV protons to 5 MeV deuterons (heavy hydrogen ions). Other young scientists joined Lawrence's "Radiation Laboratory" and more graduate students came from the Physics Department. Research studies were continuously under way, whenever time could be spared from the development toward higher energies. David Sloane, with the assistance of Bernard Kinsey and Wesley Coates, built a sequence of linear accelerators of the resonance type following Wideroe's concept. Sloan and J. J. Livingood built a unique radiofrequency resonance transformer which used a resonant coil inside a large evacuated cavity and produced high rf voltages; electrons and x rays were obtained of over 1 MeV. Livingood also developed quartz-fibre electroscopes to observe induced radioactivities when they came along. Malcolm Henderson came in 1933 and built modern counting equipment and magnet control circuits. Incidentally, Henderson also invented the name "cyclotron," first used as laboratory slang and then picked up and publicized by news reporters. R. L. Thornton and E. E. McMillan joined the group in 1934 and made many important contributions. These are the only two present members of the Berkeley staff who overlapped my time at Berkeley.

We were the first accelerator laboratory to use deuterons. Professor G. N. Lewis of the Chemistry Department concentrated heavy water from battery acid residues and provided us with gas which we used in our ion source. With the first deuteron beams we observed new interactions and emergent particles with higher energies than ever observed before. It seemed that every target we bombarded gave new, important and publishable results. We built a variety of measuring instruments, to observe all types of disintegration fragments from targets. A target "wheel" was installed operated by a greased stopcock joint, with 8 to 10 separate targets of the light elements. We installed a "thin window" of split mica sheet waxed to a grid on a re-entrant tube so we could insert counting instruments close to the target. Studies were in process whenever the cyclotron would operate. Many results were obtained and a flood of publications came from this very new (and very raw) laboratory. This was indeed an exciting time and place for a scientist to be.

There were other exciting moments and some missed opportunities. Chadwick reported the discovery of the neutron in 1932, produced from radium-beryllium sources. As soon as we had developed linear amplifiers and thin ionization chambers with which to observe single particles, we used a paraffin layer in front of the ionization chamber and were able to observe the recoil protons from neutrons. We became aware of the possible dangers of neutron exposure, and improvised shields of 5-gallon cans filled with water which were stacked around the magnet and chamber as partial shields. Early in 1934 Lawrence brought into the laboratory a copy of the Comptes Rendus which described the discovery of induced radioactivity by Curie and Joliot in Paris, using natural alphas on boron. They predicted that the same activities could be produced by deuterons on a carbon target. Now we had a deuteron beam in use, a carbon target in the chamber, and a geiger counter and counting circuits in service at that time. We quickly arranged the cyclotron to bombard the carbon target for 5 minutes and then turned the beam off and the counters on. The counters clicked as we studied the delayed emissions from the target. Within a half-hour after hearing of the Curie and Joliot results we were observing induced radioactivity!

I left Lawrence's laboratory in July 1934 to go to Cornell and build the first cyclotron outside Berkeley, and later to Massachusetts Institute of Technology where I continued by accelerator design activities. I shall leave to others the description of how the Berkeley Laboratory continued to grow, to attract many more scientists, and to become the famous center of science which was Lawrence's major contribution. Lawrence received the Nobel Prize in Physics for 1939 for conceiving the cyclotron, and became one of the most important and impressive scientists this country has known.

We now return to the story of the several other types of accelerators which were under development while the cyclotron was having its first rapid growth. The group at the Department of Terrestial Magnetism of the Carnegie Institution consisted of Gregory Breit, Merle A. Tuve, Lawrence R. Hafstad and Odd Dahl (from Norway). They tried several voltage sources including the Tesla Coil, which was formed of a long coil with spherical metal terminals immersed in an oil bath for insulation. Their most useful result was a multi-section evacuated tube within which electrons could be accelerated; this technique was later used in their electrostatic generators. However, after a year or more of experimentation they found that the oscillatory character of the potential developed in the Tesla Coil made it unsuitable for particle acceleration. They abandoned the Tesla Coil around 1932 in favor of the belt-charged electrostatic generator invented by Van de Graaff. These were the first electrostatic machines built specifically for particle acceleration.

First the Carnegie group built a 2-meter diameter spun aluminum sphere mounted on three Textolite legs, with which they could measure voltages and test principles. Next they used a 1-meter diameter sphere with an internal hydrogen ion source, and used a vertically mounted sectionalized accelerating tube to accelerate protons. In 1933 these were combined in one machine with the 1-meter sphere inside the 2-meter sphere as a potential divider. An accelerating tube brought a proton beam down through the floor into a basement laboratory where it was brought out through a thin window so its range and energy could be measured. The best voltage calibrations were obtained by magnetic deflection of the proton beam, using accurately placed slits. During this first year (1933) they initiated experimental measurements of disintegrations, and observed a series of nuclear resonances in light element targets, which became standards for energy calibrations in other laboratories. In following years this was a very productive laboratory, competing favorably with the cyclotron in output of scientific results. Incidentally, Merle Tuve and Ernest Lawrence were high school classmates, so the two laboratories kept in close contact.

Meanwhile, R. J. Van de Graaff went to the Massachusetts Institute of Technology in 1932 where, with the support of President Karl T. Compton, he started the design of a really large generator intended to produce well over 10 MV, located in an airship hangar at the Round Hill estate of Col. E.H.R. Greene near South Dartmouth, Massachusetts. The machine had two 15-foot diameter spherical aluminum terminals, each supported on a 6-foot diameter Textolite cylinder 24-feet long, and each mounted on a movable platform rolling on rails. Within each cylinder was a 4-foot wide belt for charging. The plan was to mount a discharge tube horizontally between the two terminals; one terminal would contain the ion source and the other a laboratory for observations. Both machines worked, producing high voltages which gave great sparks from the terminals and down the columns. The maximum steady voltages obtained were 2.4 MV on the positive terminal and 2.7 MV on the negative terminal. However, the difficulties of mounting an evacuated discharge tube between terminals were insurmountable, and the machine never performed as an accelerator. The hangar was abandoned in about 1938 and the two generators moved to M.I.T. where they were mounted in contact side-by-side within a metal-domed building. This generator was completed as an accelerator in 1940 and used to accelerate particles used in a research program. After years of service this original "Van de Graaff" generator became obsolete; it was given to the Boston Museum of Science where it is mounted in its separate building as a permanent exhibit, being operated occasionally to develop massive sparks.

Many other electrostatic generators were built. The first round of design was a group of air-insulated, vertically mounted machines, based on Van de Graaff's concepts. These were all limited to voltages well below their hopeful limits, operating at about 4 MV, and all were soon abandoned. However, a parallel development of pressure insulated generators were more successful. R. G. Herb and his group at the University of Wisconsin did the original development of a horizontal design and achieved 4 MeV energy. The High Voltage Engineering Corporation, a commercial firm started in Cambridge by J. G. Trump, Van de Graaff and others from M.I.T., built and sold a wide range of electrostatic generators over the years. They also built a pressure insulated generator at an early stage, with vertical mounting. The pressurized, belt-driven generators have had a long and successful history of service to science, in a sequence of machines of increasing energy. A large fraction of those in service in science laboratories were built by High Voltage Engineering Corp. The Van de Graaff type accelerator is the most widely used throughout the world; estimates of the total number built range into several hundreds.

At the California Institute of Technology, C. C. Lauritsen and H. R. Crane started about 1930 to develop an accelerator out of a voltage source called a cascade transformer, a system of three 250-kV transformers in cascade which would produce 1 MV peak voltage. They built x-ray tubes and also positive ion tubes which operated up to 750 keV energy. In 1934 they reported the first results of a program of nuclear research, using protons up to 1 MeV energy. Eventually, the superior qualities of the electrostatic generator were recognized, and the transformer system was replaced. The Cal Tech nuclear research laboratory has trained a notable succession of research students, and for many years was a major contributor to nuclear physics.

The descriptions above cover nearly all the accelerator developments underway in this country before the start of World War II. One further development, the betatron, accelerates only electrons. And since high energy electrons cannot disintegrate atoms, the betatron had little to contribute to nuclear physics. It was used only for the production of x rays, where it had a considerable commercial and medical application. The first 2.3 MeV "magnetic induction accelerator," given the name "betatron," was built by D. W. Kerst at the University of Illinois in 1941. Many scientists had earlier recognized the possibilities in this principle of acceleration by magnetic induction, which has the advantage of avoiding the problems of insulation breakdown which plagued the direct voltage accelerators; some instruments had been built but did not work, and a series of patents had been applied for covering several aspects of the principle. But none of them were successful. Kerst's success was due to a theoretical study of orbit stability and focusing by R. Serber, which pointed the way to the correct design. Kerst went next to the General Electric Company where a 20-MeV machine was built in 1942, and which led later to a 100-MeV machine at G. E. Kerst then returned to the University of Illinois to build first an 80-MeV "model" and ultimately a 300-MeV betatron which was the largest and last of this line.

At the end of World War II a new principle of "synchronous" acceleration was discovered independently by E. E. McMillan in the United States and by V. I. Veksler of the U.S.S.R. In both countries the end of the war brought a desire to restart the research programs in nuclear physics which had been postponed for the duration. This synchronous principle sidestepped the relativistic limitations in energy of the cyclotron and the betatron. Several types of machines were conceived to use this synchronous technique for resonance acceleration. The electron synchrotron is the simplest application. McMillan built his first one at Berkeley starting in 1945 for 300 MeV energy electrons. Many others were built at laboratories around the world for energies up to 1200 MeV, completely displacing the betatron.

The synchrocyclotron uses synchronous acceleration to avoid the relativistic energy limitation of the standard cyclotron. The first application was to the 184-inch magnet built at Berkeley before World War II but not used as an accelerator. In 1946-47 a team of scientists who had worked in Lawrence's war-time laboratory rebuilt the magnet as a synchrocyclotron, producing 200-MeV deuterons. In later years it was improved to yield 740-MeV protons. A dozen other large synchros have been built around the world, but the energy record is still held by the Berkeley 184-inch.

The proton synchrotron is the most impressive of the phase stable accelerators and produces the highest energies. The first machines were built in two U.S. laboratories. At Brookhaven National Laboratory I was in charge of design for a 3-GeV machine we called the cosmotron, which was completed in 1952. At Berkeley, Brobeck headed the design for a 6-GeV machine called the bevatron, which was finished in 1953. At least eight other machines of this type were built, with maximum energy of 12.5 GeV (billion electron volts).

Modern linear accelerators also use a type of synchronous stability, in which a narrow phase band of particles is stably accelerated. Most linacs are used as pre-accelerators or injectors into larger machines, varying in energy from 10 MeV to 200 MeV. A few much higher energy linacs have been built, of which the largest is the two-mile Stanford Linear Accelerator called SLAC which produces 20-GeV electrons.

It is beyond the scope of this review to continue with the description of other types and aspects of the increasingly large accelerators of the present day. They have all grown from the start discussed above. The voltages obtained from accelerators in this past 50 years go from a few hundred kilovolts to 500 billion volts, a factor of increase of over one million. This is a big factor by which man has increased his knowledge in only 50 years.

Mr. McCORMACK. Thank you, Dr. Livingston. A fascinating presentation.

Mr. Nunan, we should like to proceed directly to your testimony.

[The biographical sketch of Mr. Nunan follows:]

CRAIG S. NUNAN RÉSUMÉ

EDUCATION:

Univ. Calif. (Berkeley): B.S. 1940, M.S. 1949, Electrical Engineering.

Stanford Univ.: Executive Development Program, Summer 1965.

37 units in Engineering Science Ph.D. program, 1968-69.

EMPLOYMENT:

Varian Associates, Palo Alto, CA: 1955 to present. General Manager, Radiation Division until 1968. Technical Advisor to Medical Group President since. Developed therapeutic and diagnostic equipment for medical, industrial and scientific applications.

Chromatic Television Labs., Emeryville, CA: 1953-1955. Director of Research. Worked with Ernest O. Lawrence on his color television invention.

Univ. Calif. Lawrence Berkeley Lab: 1946-1953. Project engineer. Worked for Luis Alvarez and Edward McMillan on development of accelerators for physics research.

U.S. Navy Dept. Bureau of Ships, Washington, D.C.: Worked in Admiral Hyman Rickover's Electrical Section on magnetic and infrared systems.

Patents: 18 total, in above fields.

Publications: An occasional article, in above fields.

Family status: Married, 4 children (3 in graduate school).

Office: Varian Associates, 611 Hansen Way, Palo Alto, CA 94303. Telephone (415) 493-4000 ext. 3522.

Home: 26665 St. Francis Rd., Los Altos Hills, CA 94022 Telephone (514) 948-0565.

STATEMENT OF MR. NUNAN

Mr. NUNAN. I am going to illustrate my presentation with slides. I wonder if we could turn off some of the room lights.

Mr. McCORMACK. Certainly.

Mr. NUNAN. Mr. Chairman, gentlemen, my presentation is on spinoffs to society from accelerator technology. I am going to give you a view from industry of the usefulness of particle accelerator research.

Just as an aside, I think one of the major things that these laboratories have done is to train people, not just the research physicists, but the engineers who get a chance to work around them, because they get a global view that they would never get otherwise, a way of looking at things because they have been around physicists that they would not get in an engineering school directly.

Then, if they leave these laboratories and go into industry, they can apply this view. I think we should not overlook the teaching aspects of these great laboratories.

In this presentation, I will describe the evolution of medical accelerators from physics research lab to hospital and then mention some practical applications of particle accelerator technology, to several industries, which I have chosen because they are related to energy and defense.

There are many other applications of accelerators in society, which I will not cover.

First, the evolution of medical accelerators. (See fig. 1.) About 1,000 medical accelerators are installed in hospitals in the United States. Their total value is about $250 million. You go to your local general hospital, you probably will find a machine like this, treating patients.

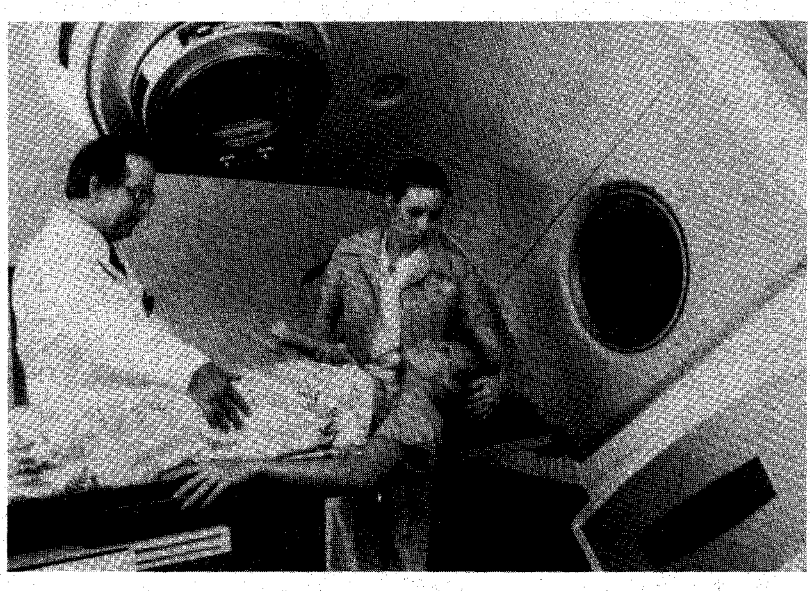

FIGURE 1

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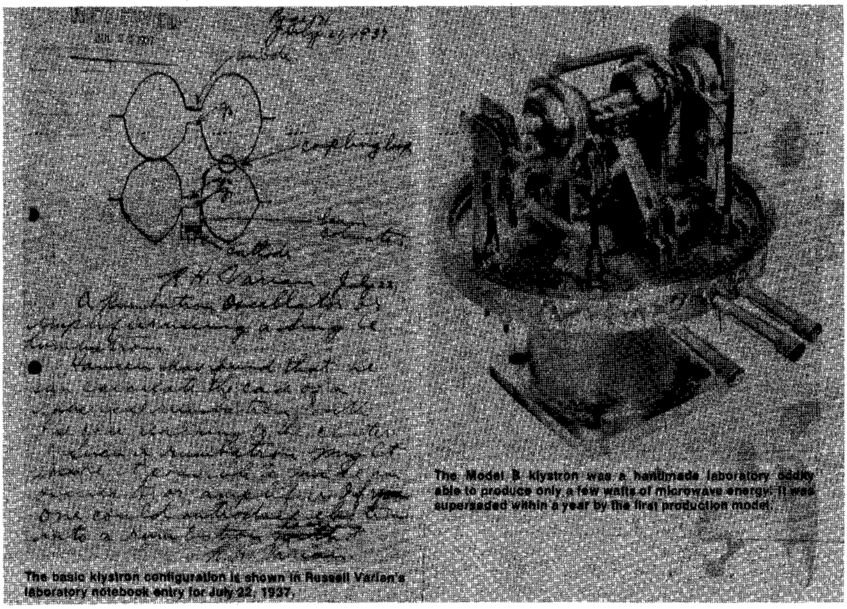
One person in eight at some time in his life will be placed under a machine like this for treatment of cancer. The story of how these machines evolved is an interesting one and may lend credence to the idea that investment in basic research today may lead to other unexpected benefits to society in the future.

In the 1930's, Bill Hansen in the physics department at Stanford University conceived the idea of using a resonant cavity to accelerate electrons. (See fig. 2.) He called his cavity a rhumbatron, after the Cuban dance, because of the way the waves bounced back and forth inside it.



The basic klystron configuration is shown in Russell Varian's laboratory notebook entry for July 22, 1937.

The Model B klystron was a handmade laboratory oddity able to produce only a few watts of microwave energy. It was superseded within a year by the first production model.

FIGURE 2

But Hansen needed a source of microwave power and nothing useful existed at the desired short wavelengths. The Varian brothers, Russ and Sig, joined Hansen at Stanford. With the handsome budget of $100 for materials, they proceeded to work on the problem and a few months later they invented the klystron, as shown here.

It employs two of Hansen's cavities. This type of microwave power source was subsequently applied extensively in radar during World War II. At the end of the war, Ed Ginzton and Marvin Chodorow returned to Stanford and developed a klystron 1,000 times more powerful than the wartime version.

Finally, Hansen's rhumbatron cavity, Varian's klystron invention and Ginzton and Chodorow's klystron improvement were combined to build an electron linear accelerator of 1,000 megaelectron volts energy. The Office of Naval Research supported this work.

These developments for physics research were then applied to medicine. Henry Kaplan of the Stanford Medical School combined his talents with Ginzton's to develop a 6 megaelectron volts electron linear accelerator suitable for treatment of cancer.

In subsequent years, scientists at Varian Associates invented the sputter ion vacuum pump, and Ed Knapp at Los Alamos Scientific Laboratory invented a new, highly efficient form of linear accelerator cavity system. Varian Associates combined these various ideas and others into a reliable radiotherapy machine suitable for widespread use in hospitals.

I want to mention a couple of dramatic examples of the significance to people of accelerators for cancer therapy. (See fig. 3.)

One of the advantages of radiation therapy over surgery for treatment of cancer is that the patient's function can be better preserved. This figure shows two patients who had cancer of the mouth. One was treated by surgery, resulting in severe disability. The other was treated by neutrons from a cyclotron and has retained entirely normal function of his mouth.

Plate VIII Two patients each with cancer of the mouth. Surgical excision (left) resulted in severe disability. Patient on right after treatment with neutrons has entirely normal function of his mouth and is able to wear dentures.

FIGURE 3

A second example is Hodgkin's disease. It is a form of cancer that typically occurs in young adults in the prime of life. Henry Kaplan made giant strides in its treatment.

After decades of clinical research and clinical machine development, the cure rate for Hodgkin's disease is 75 percent for all stages and 90 percent for early stages. Before the invention of particle accelerators, the cure rate for this disease was essentially zero.

We are here celebrating the golden anniversary of the invention of the cyclotron by Ernest Lawrence. (See fig. 4.) Other nations also celebrate particle accelerators. In England, the Medical Research Council just celebrated the silver anniversary of the installation of their cyclotron. This shows the Queen at the control console commissioning the machine 25 years ago.

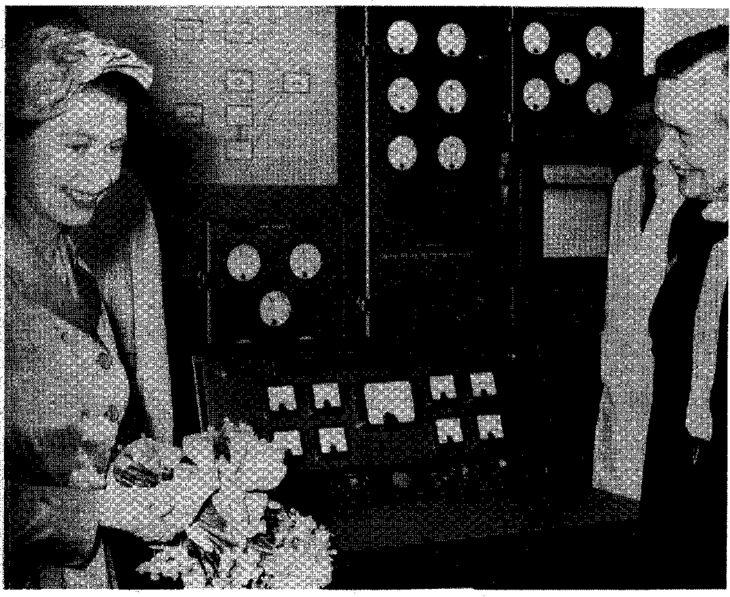


FIGURE 4

Since then, the British have used this cyclotron to make major strides in the development of radioactive isotopes for the diagnosis of disease and in the use of fast neutrons from their cyclotron for treatment of cancer. Their results indicate that fast neutrons are significantly superior to X-rays for treatment of several forms of cancer.

A number of medical research centers throughout the world are now conducting research on the treatment of patients with fast neutrons produced by accelerators.

Now, for some industrial applications. First, oil and gas industries:

There are hundreds of thousands of wells in the United States which were once drilled, cased with a steel liner, pumped, and then abandoned. It is now often economical to reactivate these oil and gas wells, but a survey must be run with a very small diameter accelerator down the thousands of feet of depth of the well to identify the oil-bearing strata, to bring the well back in again.

Next, the coal industry: With the current emphasis on gasification and liquefaction of coal, it has become important to measure the organic oxygen content of coal in order to control the amount of steam or hydrogen used in the conversion process.

In gasification processes, poisonous gases are produced and they must be removed before catalytic methanation takes place. Accelerators provide accurate and rapid methods of analysis which should prove to be of value in the future development of coal utilization processes.

Nuclear powerplants: (See fig. 5.) Present plans call for the completion of 130 additional nuclear powerplants over the next 20 years. The very thick steel vessels and pipes and other thick welded components used in building nuclear reactors and other heavy industrial plants are routinely inspected for flaws in fabrication by taking many X-ray films using a radiographic electron linear accelerator, such as shown here. This shows the accelerator in the background and the man holding the radiographic film cassette.

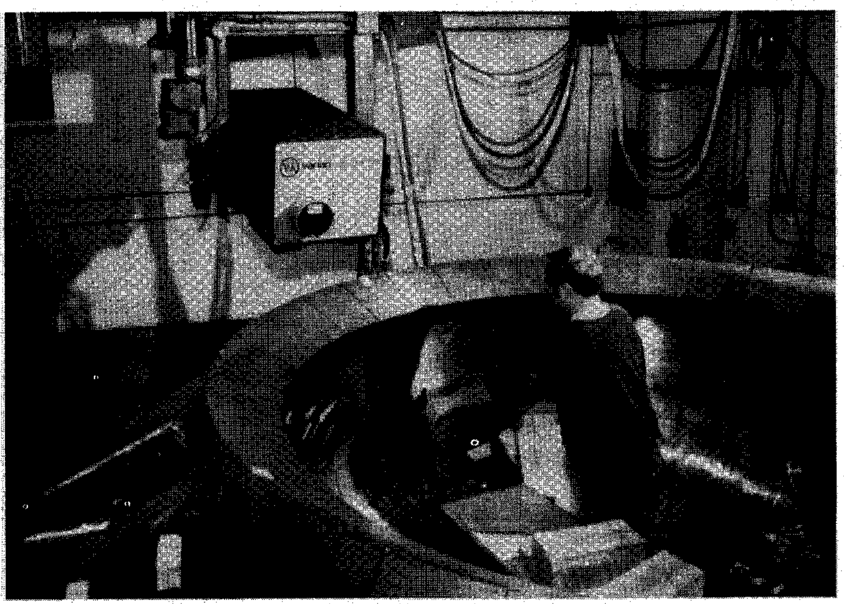


FIGURE 5

The steel is so thick that no other device than an accelerator could be used to take the large number of extremely fine resolution X-ray pictures in a reasonable amount of time. (See fig. 6.) This shows an accelerator inspecting a reactor vessel. The accelerator is way in the background, dwarfed by the size of the vessel it is inspecting. (See fig. 7.) This shows the basic elements of such a machine—the accelerator guide, gun, target, sputter ion vacuum pump and microwave power tube.

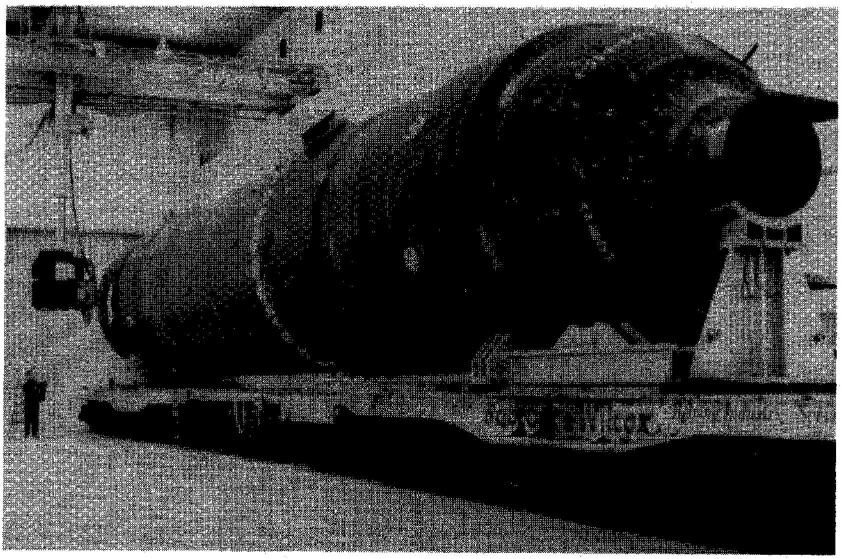


FIGURE 6

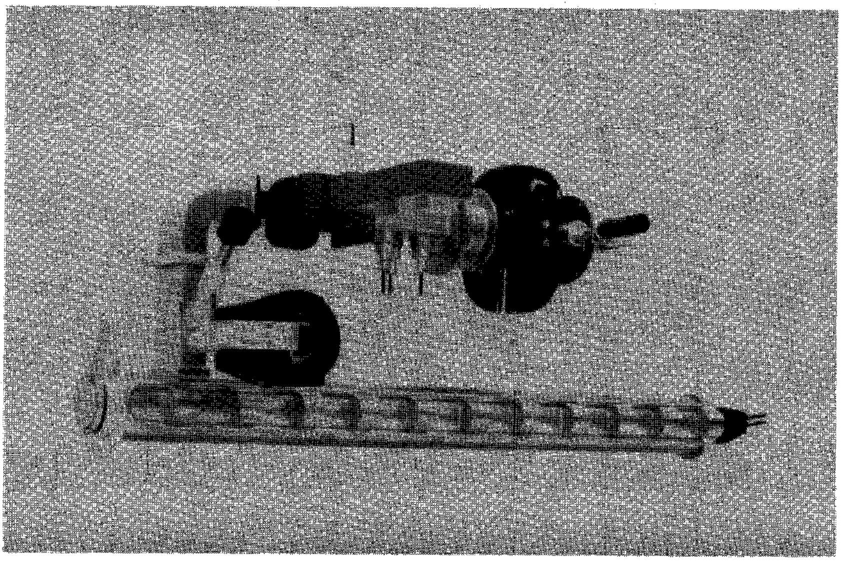


FIGURE 7

Of course, research particle accelerators have been used for many years to measure the neutron interaction characteristics of materials in order to learn how to design nuclear reactors and how to optimize their performance and safety.

Next, fusion energy research: (See fig. 8.) Soviet scientists have invented a new linear accelerator technique called radio frequency quadrupole focusing. This shows a section of this new type of accelerator, built at Los Alamos.

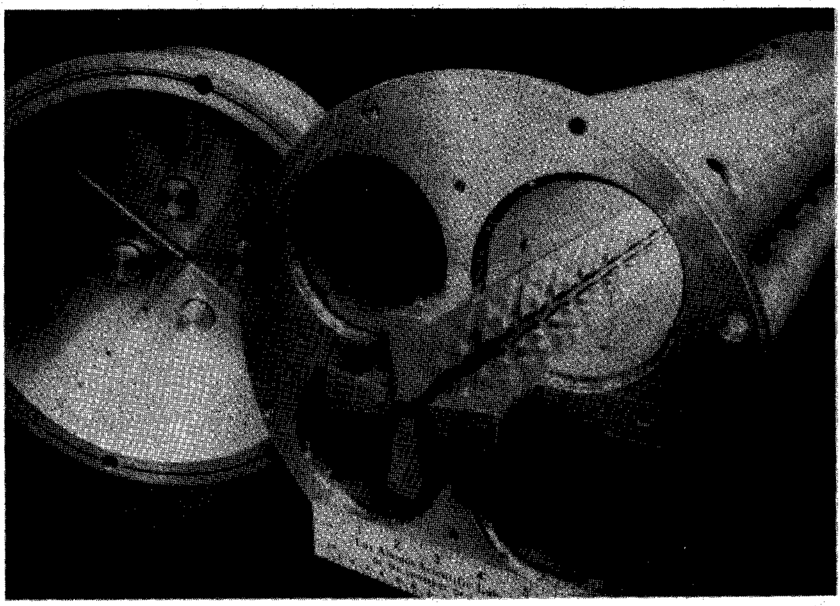


FIGURE 8

It facilitates the development of more compact and cheaper proton linear accelerators, which are being built for isotope production, for cancer therapy, and for irradiation testing of materials for fusion reactors.

(See fig. 9.)—The section marked RFQ shows where this Soviet invention will be used in a 160-foot long fusion materials irradiation test accelerator.

One way to produce fusion power is to implode small pellets of deuterium-tritium by a multiplicity of converging powerful pulsed beams. Laser beams, electron beams and heavy ion synchrotron beams are being studied for this purpose.

Another candidate is the induction linear accelerator, which is simply a long row of very fast electrical transformers and which is capable of producing an enormous current of heavy ions in very short bursts. Induction linear accelerators are also being considered for use as particle beam weapons.

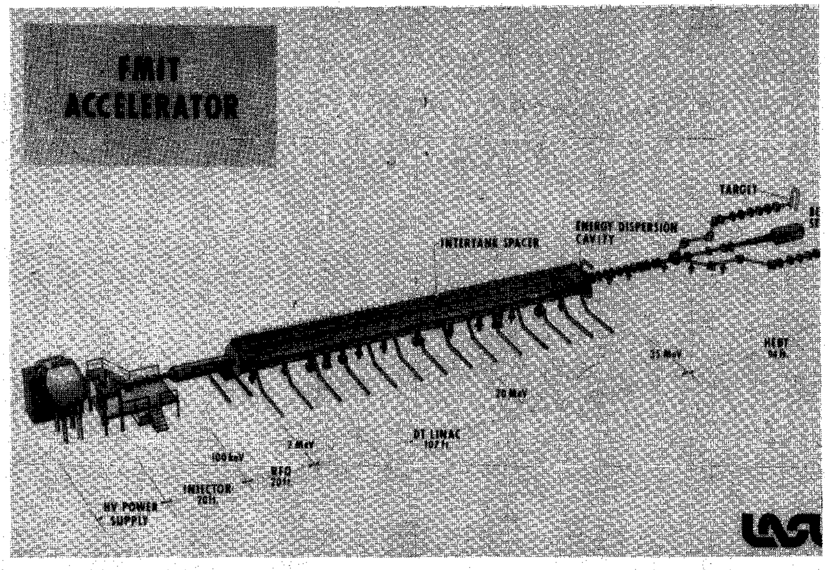


FIGURE 9

(See fig. 10.)—Regarding the microelectronics industry, standard integrated circuits are now readily available from foreign countries.

In order to maintain our technological lead in the world we must develop faster and more compact, very large-scale integrated circuits. Such advances can lead to a whole new generation of computers and digital electronics, opening up major opportunities for industrial and commercial and defense applications.

This can be done by using an electron beam lithography system, such as shown in this figure to write the circuit patterns on masks and X-rays from an electron storage ring to print these patterns on semiconductor wafers. The goal is to produce integrated circuits with 100 times present data throughput rates.

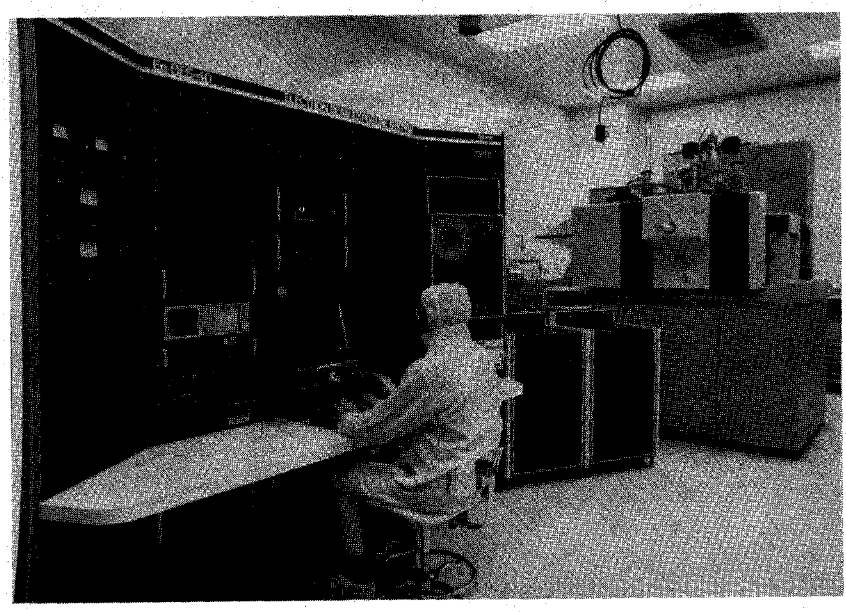


FIGURE 10

(See fig. 11.)—Integrated circuits go through at least one step of ion implantation during their manufacture. Some go through four to six steps. This shows an ion implantation system.

These machines might not normally be thought of as particle accelerators by a high energy physicist. Yet, they represent direct application of many of the techniques developed by these accelerator physicists.

They include an ion source, an analyzing magnet, an accelerator section, a beam focusing system, a beam scanning system, a vacuum system, and a target chamber to position the semiconductor wafers.

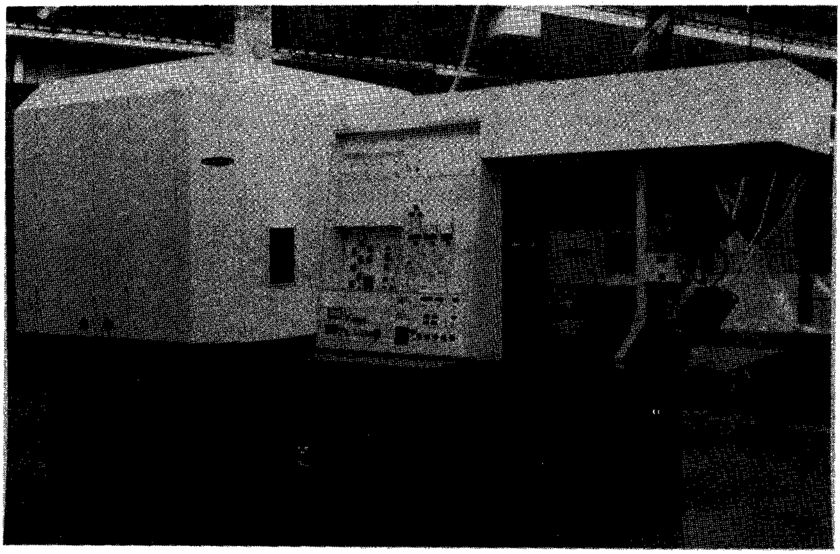


FIGURE 11

These are all equivalents on a small scale of the large-scale work in the major accelerator laboratories. About 1,000 of these machines are now in use. Their total value is about $250 million. They help to produce $8 billion a year worth of semiconductors.

(See fig. 12.)—This shows a very high beam current ion implanter for predeposition of silicon wafers. Note how much this machine looks like the typical particle beam experimental equipment in an accelerator physics laboratory.

The techniques of ion implantation may be extended to other industries. We now import more than 90 percent of certain metals used in high technology, and the world supply of these metals is dwindling rapidly.

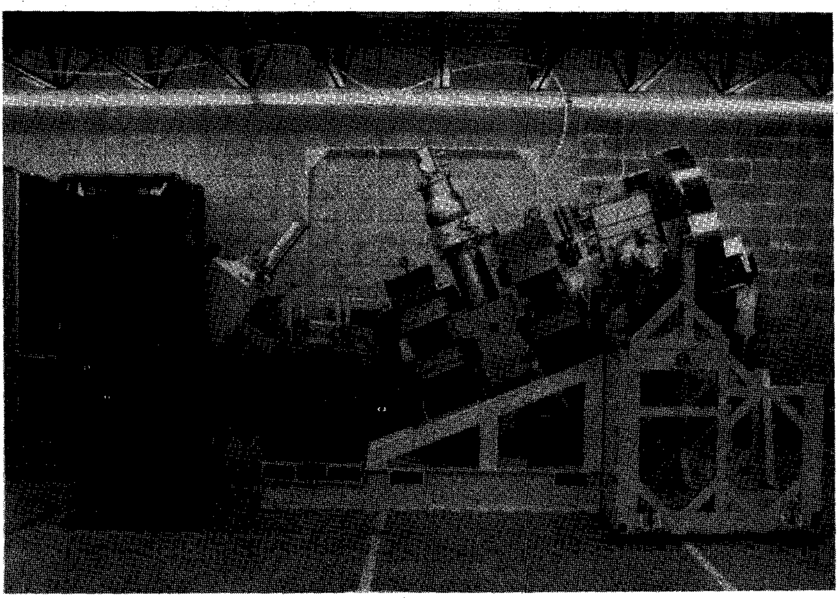


FIGURE 12

Substitution of other metals and limiting their use to the surface of the base metal is being tried using ion implantation. Reductions in wear of 1,000 percent have been achieved. Surprisingly, the wear resistance extends to depths as much as 100 to 1,000 times the ion implant range, thus, there is an enormous potential saving in the amount of scarce material required.

In summary, it should be evident from this brief survey that particle accelerators are essential to our way of life. Accelerator technology contributes directly to the health, the security and the economic independence of the people of this Nation.

There are major applications in health care—in the microelectronics industry, nuclear power industry and potentially in the oil, gas, and coal industries. Many other potential applications are in various stages of research. As high energy physicists develop new particle accelerator technologies, we can anticipate further spinoffs to society.

[The prepared statement of Mr. Nunan follows:]

A Report to the Congressional Subcommittee on Energy Research and Production

Spin-Offs to Society From Accelerator Technology

(Craig S. Nunan, Varian Associates, July 14, 1980)

Introduction

The goal of high energy physicists is to study the basic nature of matter and energy. Their past research has resulted in the discovery of essential new sources of energy. Their future research may provide this country's energy solutions 30 years from now. But their work is having a more immediate impact through spin-offs of new technology. In order to conduct their experiments these physicists develop various kinds of particle accelerators and associated experimental equipment and to do so they have to solve totally new technological problems. Industry maintains a continuous surveillance of these developments, always watching for opportunities to transfer a new technology from the physicist's laboratory to the practical world. Of course, there is also reverse spin-off, from industrial research back to the high energy physicist. This process of technological transfer back and forth works well in our free and open society. I will describe just a few examples of practical applications of particle accelerator technology in industry and health care.

1. Radiation treatment of cancer.

About a thousand electron linear accelerators are installed in hospitals in the United States. Figure 1 shows a radiotherapy machine being oriented with respect to the patient's tumor. One person in eight will at some time in his life be placed under a high energy radiation beam for treatment of cancer. The story of how these machines evolved is an interesting one and may lend credence to the idea that investment in basic research today may lead to other unexpected benefits to society in the future.

In the 1930's Bill Hansen in the Physics Department at Stanford University conceived the idea of using a resonant cavity to accelerate electrons. He called his cavity a Rhumbatron, after the Cuban dance, because of the way the waves bounced back and forth inside it.

But Hansen needed a source of microwave power and nothing useful existed at the desired short wavelengths. The Varian brothers, Russell and Sigurd, joined Hansen at Stanford. With the handsome budget of $100 for materials they proceeded to work on the problem and a few months later they invented the klystron as shown in Figure 2. This type of microwave power source was subsequently applied extensively in radar during World War II. At the end of the war, Ed Ginzton and Marvin Chodorow returned to Stanford and developed a klystron 1000 times more powerful than the wartime version. Finally, Hansen's rhumbatron cavity, Varian's klystron invention and Ginzton and Chodorow's klystron improvement were combined to build an electron linear accelerator of 1000 MeV energy for physics research. The Office of Naval Research supported this work. During this period Henry Kaplan of the Stanford Medical School combined his talents with Ginzton's to develop a 6 MeV electron linear accelerator suitable for treatment of cancer. In subsequent years scientists at Varian Associates invented the sputter ion vacuum pump, and Ed Knapp at Los Alamos Scientific Laboratory invented a new highly efficient form of linear accelerator cavity system. Varian Associates combined these various ideas and others into a reliable radiotherapy machine suitable for widespread use in hospitals.

Henry Kaplan of the Stanford Medical School chose to specialize in the treatment of Hodgkin's disease. It is a form of cancer that typically occurs in young adults in the prime of life. He made giant strides in its treatment. Before the invention of particle accelerators, the cure rate for this disease was essentially zero.

Now, after decades of clinical research and clinical machine development, the cure rate for Hodgkin's disease is 75% for all stages and 90% for early stages.

One of the advantages of radiation therapy over surgery for treatment of cancer is that the patient's function can be better preserved. Figure 3 shows two patients who had cancer of the mouth. One was treated by surgery, resulting in severe disability. The other was treated by neutrons from a cyclotron and has retained entirely normal function of his mouth. This is a dramatic example of the benefits to people from particle accelerator technology.

We are here celebrating the Golden Anniversary of the invention of the cyclotron by Ernest Lawrence. Other nations also celebrate particle accelerators. In England, the Medical Research Council just celebrated the Silver Anniversary of the installation of their cyclotron. Figure 4 shows the Queen commissioning the machine 25 years ago. Since then, the British have used this cyclotron to make major strides in the development of radioactive isotopes for the diagnosis of disease and in the use of fast neutrons from their cyclotron for treatment of cancer. Their results indicate that fast neutrons are significantly superior to x-rays for treatment of several forms of cancer. A number of medical research centers throughout the world are now conducting research on the treatment of patients with fast neutrons produced by accelerators.

2. Nuclear medicine.

The greatest impact of the cyclotron on medicine has probably been in the development of radioactive isotopes. About two-thirds of all radioisotopes were originally discovered by use of cyclotrons. Small cyclotrons are now used for commercial production of radioisotopes for shipment to hospitals where they are used for diagnosis of disease. There is growing interest in installing small cyclotrons within hospitals to produce short lived radioisotopes of carbon, nitrogen and oxygen for nuclear imaging of patients because these elements occur naturally in the metabolic processes of the body.

3. Plastic products processing and sterilization.

Plastic packaging materials and plastic fittings are processed with electron accelerators to modify their thermal characteristics; when heat is subsequently applied, the plastic will shrink down tight around the item of interest, such as your prepackaged Thanksgiving turkey, or a cable fitting for an aircraft. Electrical cable insulation is processed in this way so that it will not melt when overcurrent surges occur.

Prepackaged disposable medical supplies such as syringes and surgical gloves and a host of other items are sterilized by radiation, such as from electron accelerators. Electron beam processing is replacing conventional manufacturing methods because of increasing concern about pollution of the environment.

4. Oil and gas and coal.

There are hundreds of thousands of wells in the United States which were once drilled, cased with a steel liner, pumped, and then abandoned. It is now often economical to reactivate these oil and gas wells, but a survey must be run with an accelerator down the depth of the well to identify the right place to pierce the casing to bring the well back into production. A very small diameter accelerator is lowered for thousands of feet down the well. It produces radiation which penetrates the steel casing and returns a signal characteristic of the elements in the surrounding formations.

With the current emphasis on gasification and liquefaction of coal, it has become important to measure the organic oxygen content of coal in order to control the amount of steam or hydrogen used in the conversion process. In gasification processes, poisonous gases are produced and they must be removed before catalytic methanation takes place. Accelerators provide accurate and rapid methods of analysis which should prove to be of value in the future development of coal utilization processes.

5. Nuclear power.

Present plans call for the completion of 130 additional nuclear power plants over the next 20 years. The very thick steel vessels and pipes and other thick welded components used in building nuclear reactors and other heavy industrial plants are routinely inspected for flaws in fabrication by taking many x-ray films using a radiographic electron linear accelerator, such as shown in Figure 5. The steel is so thick that no other device than an accelerator could be used to take the large number of extremely fine resolution x-ray pictures in a reasonable amount of time. Figure 6 shows such a machine inspecting a reactor vessel. Figure 7 shows the basic elements of such a machine, the accelerator guide, gun, target, vacuum pump and microwave power tube.

Of course, research particle accelerators have been used for many years to measure the neutron interaction characteristics of materials in order to learn how to design nuclear reactors and how to optimize their performance and safety. And accelerators are being used to measure the fissionable material in fuel rods, both for control of fuel and processing and for safeguarding against diversion for ulterior purposes.

6. Fusion energy research.

An important advance has been made by Soviet scientists in the invention of a linear accelerator technique called radio frequency quadrupole focusing. Figure 8 shows a section of this new type of accelerator, built at Los Alamos. It facilitates the development of more compact and cheaper proton linear accelerators, which are being built for isotope production, for cancer therapy, and for irradiation testing of materials for fusion reactors. The section marked RFQ in Figure 9 shows where this Soviet invention will be used in a 160 foot long fusion materials irradiation test accelerator.

One way to produce fusion power is to implode small pellets of deuterium-tritium by a multiplicity of converging powerful pulsed beams. Laser beams, electron beams and heavy ion synchrotron beams are being studied for this purpose. However, there are perhaps fewer problems to solve by using the induction linear accelerator, which is simply a long row of very fast electrical transformers and which is capable of producing an enormous current of heavy ions in very short bursts. Induction linear accelerators are also being considered for use as particle beam weapons.

7. Microelectronics

Standard integrated circuits are now readily available from foreign countries. In order to maintain our technological lead in the world we must develop more compact very large scale integrated circuits. Figure 10 shows an electron beam lithography system. It consists of a very finely focused and accelerated electron beam and a computer and laser controlled scanning system. It is used to write the circuit patterns on the masks which are then used to manufacture integrated circuits. The intense soft x-ray beam produced by synchrotron radiation from say a 500 MeV electron storage ring can then be used to expose the semiconductor coating through this mask in order to produce an integrated circuit which, if future research is successful, will operate at 100 times the data throughput of present standard integrated circuits. Also, electron beam lithography speeds up the integrated circuit design process, facilitating customized design for special applications. These advances can lead to a whole new generation of computers and digital elec-

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tronics, opening up major opportunities for industrial and commercial and defense applications.

Integrated circuits go through at least one step of ion implantation during their manufacture. Some go through 4 to 6 steps. Figure 11 shows an ion implantation system. These machines might not normally be thought of as particle accelerators by a high energy physicist. Yet they represent direct application of many of the techniques developed by these accelerator physicists. They include an ion source, an analyzing magnet, an accelerator section, a beam focusing system, a beam scanning system, a vacuum system, and a target chamber to position the semiconductor wafers. About 1000 of these machines are now in use.

Figure 12 shows a very high beam current ion implanter for predeposition of silicon wafers. Note how much this machine looks like the typical particle beam experimental equipment in an accelerator physics laboratory.

The techniques of ion implantation may be extended to other industries. We now import more than 90 percent of certain metals used in high technology, and the world supply of these metals is dwindling rapidly. Instead of producing expensive bulk alloys, substitution of other metals and limiting their use to the surface of the base metal is being tried using ion implantation. Reductions in wear of 1000% have been achieved. Surprisingly, the wear resistance extends to depths as much as 100 to 1000 times the ion implant range. Much higher ion doses are required than for implanting semiconductor wafers and the shapes of metal components to be implanted can vary greatly, so a whole new configuration of ion implantation machines must be developed.

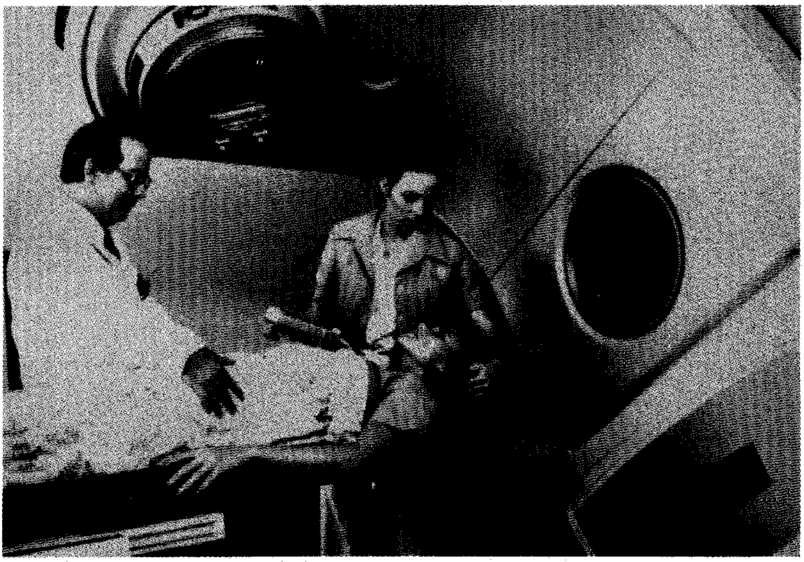
Summary.

In summary, it should be evident from this brief survey that particle accelerators are essential to our way of life. Accelerator technology contributes directly to the health, the security and the economic independence of the people of this nation. There are major applications in health care; in the microelectronics industry, nuclear power industry and plastic products industry; and potentially in the oil, gas and coal industries. Many other potential applications are in various stages of research. As high energy physicists develop new particle accelerator technologies, such as compact proton linear accelerators, induction linear accelerators, storage rings, and associated experimental equipment, we can anticipate further spin-offs to society.

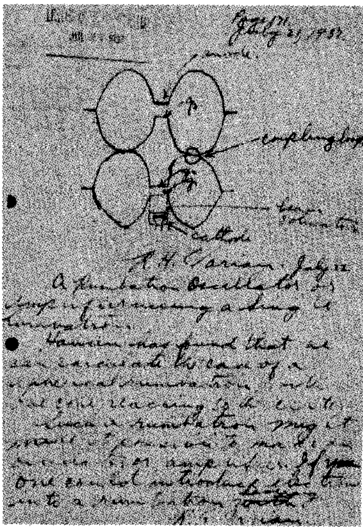
SLIDES

1. CLINICAL ACCELERATOR WITH PATIENT
2. KLYSTRON INVENTION
3. PATIENTS WITH CANCER OF THE MOUTH
(Photo courtesy of United Kingdom Medical Research Council)
4. QUEEN OF ENGLAND COMMISSIONING MEDICAL RESEARCH CYCLOTRON
(Photo courtesy of United Kingdom Medical Research Council)
5. RADIOGRAPHIC LINEAR ACCELERATOR AND X-RAY FILM CASSETTE
6. RADIOGRAPHIC LINEAR ACCELERATOR AND REACTOR VESSEL
7. RADIOGRAPHIC ACCELERATOR GUIDE, GUN, TARGET, VACUUM PUMP AND MICROWAVE POWER TUBE
8. RADIO FREQUENCY QUADRUPOLE FOCUSING LINEAR ACCELERATOR
(Photo courtesy Los Alamos Scientific Laboratory)
9. FUSION MATERIALS IRRADIATION TESTING LINEAR ACCELERATOR
(Photo courtesy Los Alamos Scientific Laboratory)
10. ELECTRON BEAM LITHOGRAPHY SYSTEM
11. ION IMPLANTATION SYSTEM
12. PREDEPOSITION ION IMPLANTATION SYSTEM

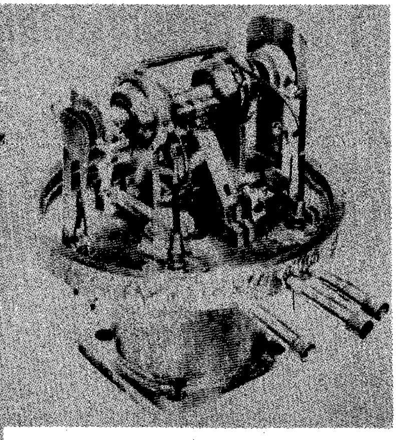
1. CLINICAL ACCELERATOR WITH PATIENT



2. KLYSTRON INVENTION



The basic klystron configuration is shown in Russell Varian's laboratory notebook entry for July 22, 1937.

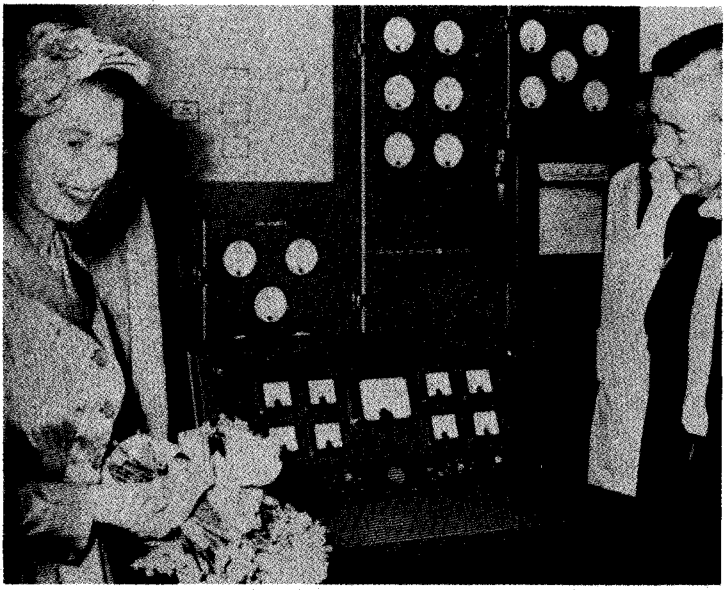


The Model B klystron was a handmade laboratory oddity able to produce only a few watts of microwave energy. It was superseded within a year by the first production model.

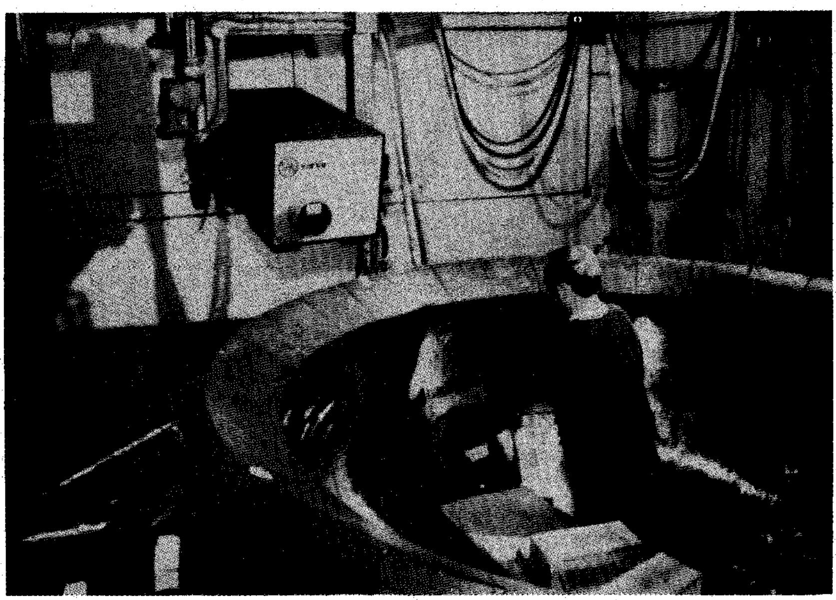
3. PATIENTS WITH CANCER OF THE MOUTH
(Photo courtesy of United Kingdom Medical Research Council)

Plate VIII Two patients each with cancer of the mouth. Surgical excision (left) resulted in severe disability. Patient on right after treatment with neutrons has entirely normal function of his mouth and is able to wear dentures.

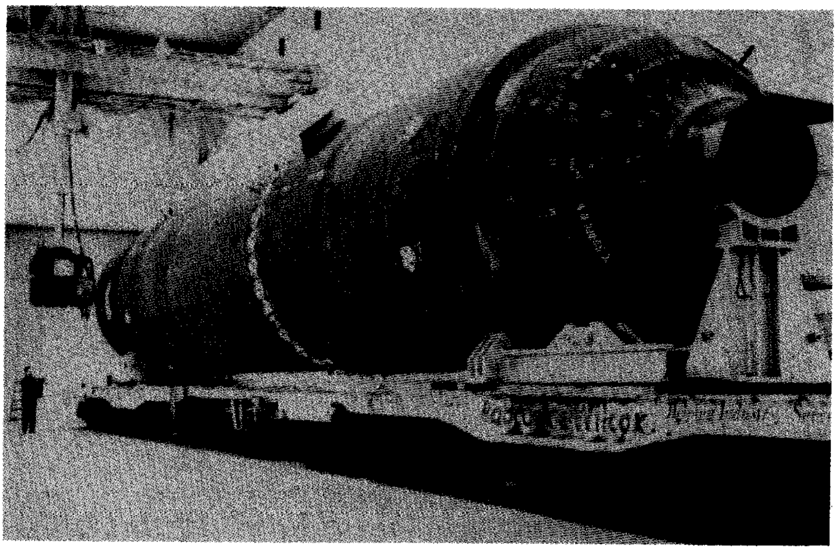
4. QUEEN OF ENGLAND COMMISSIONING MEDICAL RESEARCH CYCLOTRON
(Photo courtesy of United Kingdom Medical Research Council)



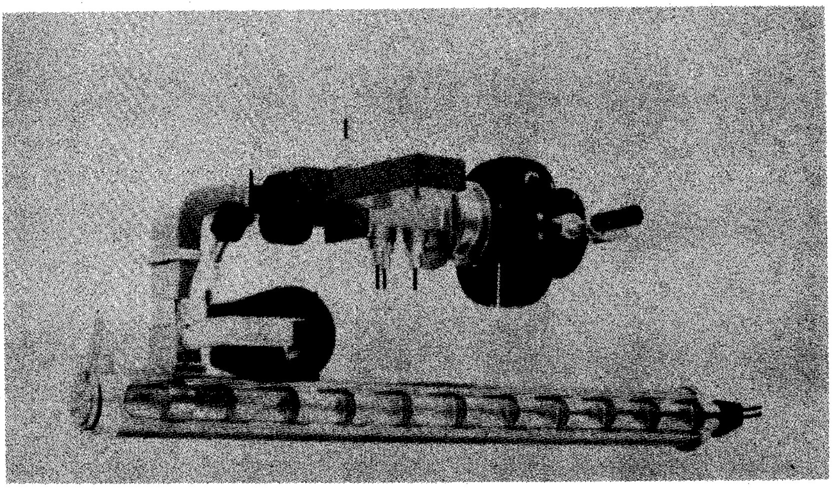
5. RADIOGRAPHIC LINEAR ACCELERATOR AND X-RAY FILM CASSETTE



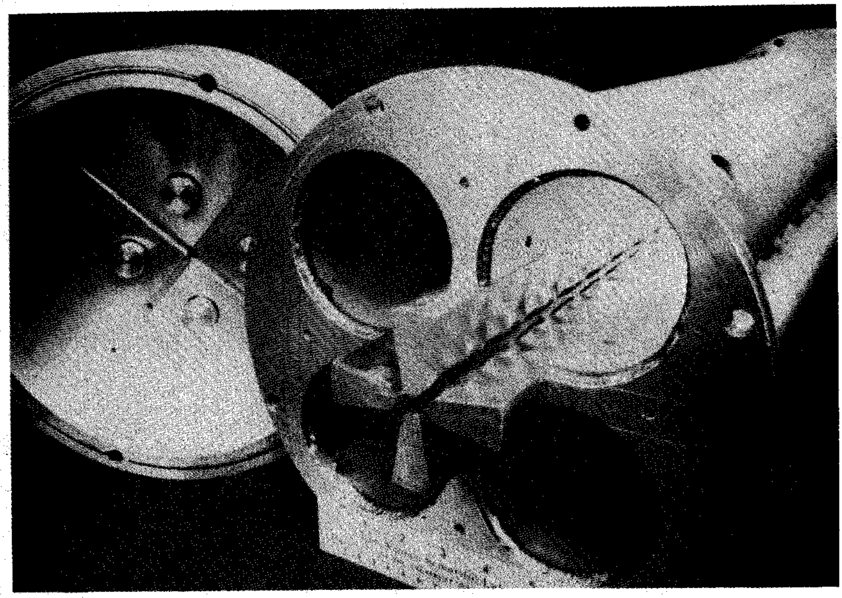
6. RADIOGRAPHIC LINEAR ACCELERATOR AND REACTOR VESSEL



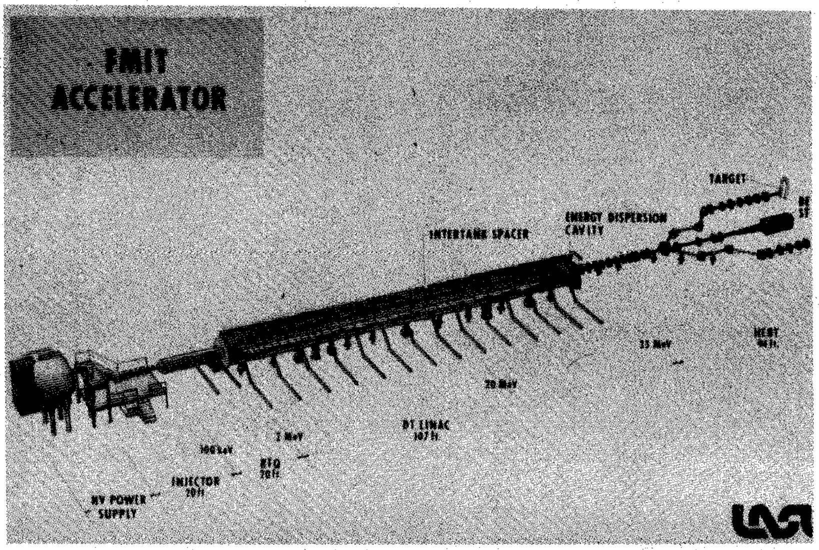
7. RADIOGRAPHIC ACCELERATOR GUIDE, GUN, TARGET, VACUUM PUMP AND MICROWAVE POWER TUBE



8. RADIO FREQUENCY QUADRUPOLE FOCUSING LINEAR ACCELERATOR
(Photo courtesy Los Alamos Scientific Laboratory)



9. FUSION MATERIALS IRRADIATION TESTING LINEAR ACCELERATOR
(Photo courtesy Los Alamos Scientific Laboratory)



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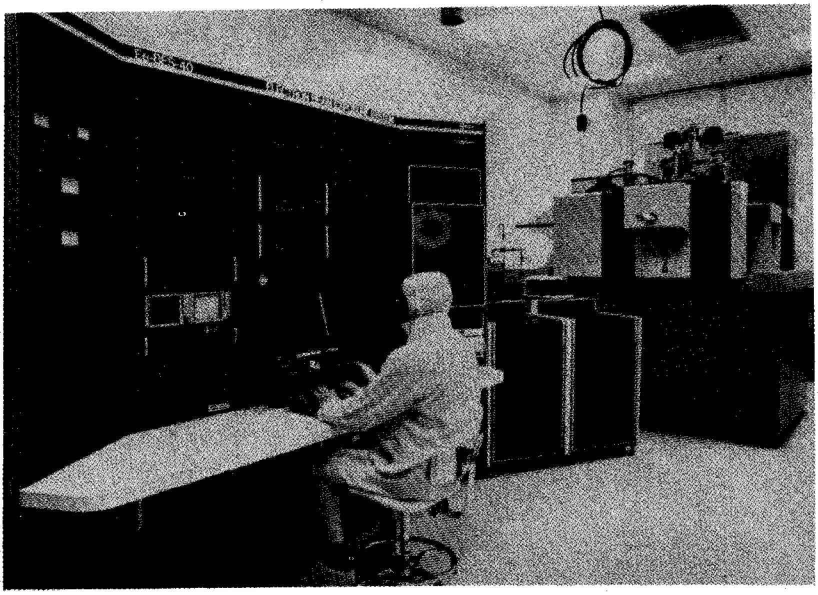
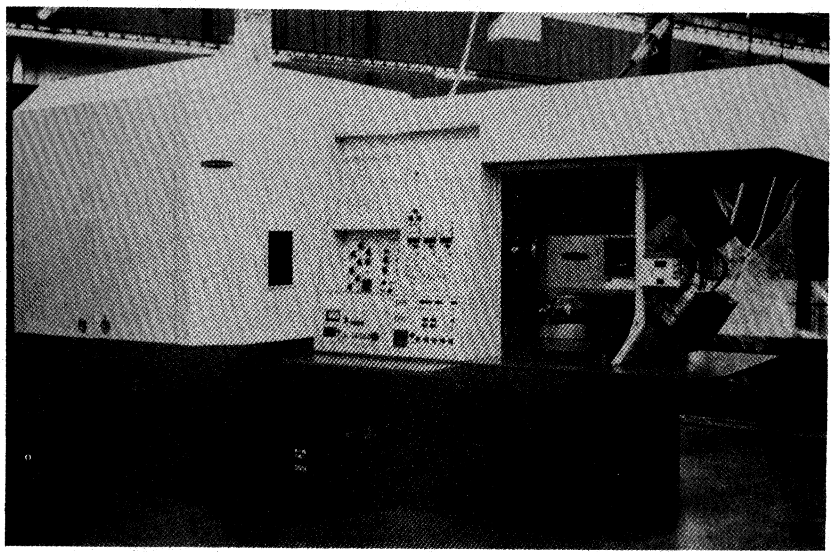
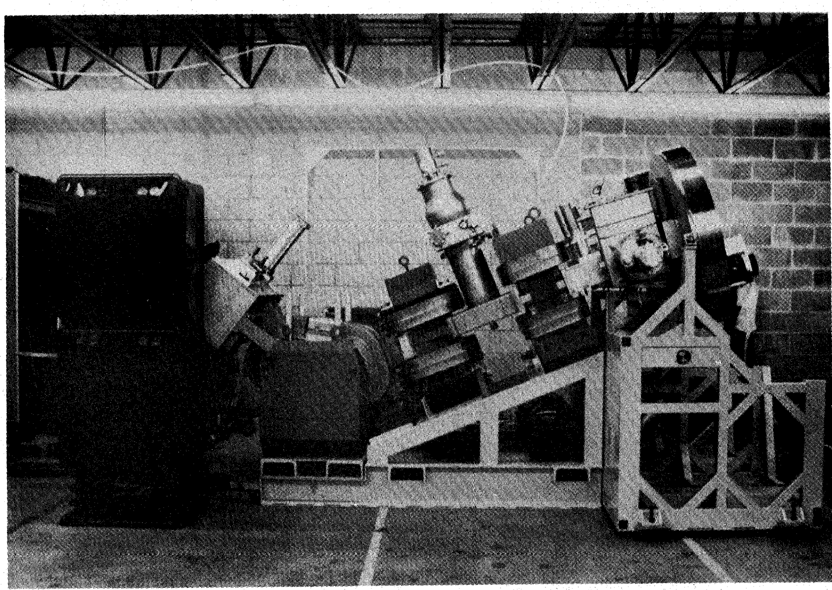


11. ION IMPLANTATION SYSTEM



12. PREDEPOSITION ION IMPLANTATION SYSTEM



Mr. McCormack. Thank you, Mr. Nunan. That was a fascinating presentation.

Our next witness is Prof. Stephen Brush, of the Department of History in the Institute for Physical Science and Technology at the University of Maryland.

Professor Brush, would you please proceed with your presentation.

[The biological sketch of Professor Brush follows:]

biographical sketch

Stephen G. Brush

Stephen G. Brush was born in Bangor, Maine (1935), received his A. B. in Physics at Harvard (1955) and his D. Phil in theoretical physics at Oxford University (1958) where he was a Rhodes Scholar. After several years of research in theoretical physics at what is now the Lawrence Livermore National Laboratory, he went to Harvard in 1965 to participate in the development of the Project Physics Course for high schools. He was also a lecturer in Physics and History of Science at Harvard (1966-68). Since 1968 he has been at the University of Maryland, College Park, and is presently Professor in the Department of History and the Institute for Physical Science and Technology. His book The Kind of Motion We Call Heat: A History of the Kinetic Theory of Gases (North-Holland) received the Pfizer Award for the best book on history of science by an American or Canadian author published in 1976. He is currently doing research on the history of geophysics and astrophysics in the 20th century.

Dr. Brush is a member of the Council of the History of Science Society and a member of the editorial board of its official journal, Isis; he is a Fellow of the American Physical Society, and associate editor of the American Journal of Physics; he has been elected a corresponding member of the Academie Internationale d'Histoire des Sciences.

STATEMENT OF PROFESSOR BRUSH

Professor BRUSH. Thank you.

Mr. Chairman, I would like to start with this transparency, which is simply a list of the major accomplishments in high energy physics which have resulted from or been associated with accelerators.

This continues the story which Dr. Livingston has told you, taking it up after 1950. It is interpreted from a longer document, which I think will be submitted to the committee, which goes into many more of these achievements in detail.

Now, I am not going to give you the history of these events. But what I would like to do instead is to look at the history of high energy physics and accelerators in the context of the development of physical science in a more general sense.

My presentation has two parts. The first part is on essentially the short-range trends and the second part, which is essentially the document, which was presented and circulated, discusses the long-range trends.

I don't think I will have time to go through all of the second part but it will be in the record.

What I am trying to do is to look at the subject somewhat as an outsider, and perhaps even trying to put myself in the place of Congress or the Government and ask: How do we evaluate the scientific value of research in a particular field such as high energy physics in relation to other fields?

I think this is especially important in a case such as high energy physics because the scientific results which come out of this, as illustrated here, are things which tend to be more and more abstract, more and more removed from any practical benefit.

Yet, at the same time, we know that it is essential that we must have support for basic research. I think it is very difficult for people who are not physicists to be able to assess the scientific value of these research results in physics and, at the same time, people within the field of physics generally do not go in for assessing what they are doing in relation to what is going on in those fields.

In other words, there is a great reluctance, I think, for any committee of scientists to get together—say, a physicist and a biologist and a chemist—and come to the conclusion that next year we should support research more in biology than in physics, and have the physicist agree to that.

Nevertheless, I think that at some point the scientific community does have to provide this sort of advice to the Government, rather than simply have each science come in and present the case for its own research and have the final decision left completely up to people who are not scientists or not familiar with the details of each field.

So, what I am going to try to do is to see how we might answer some questions such as the following:

First of all, how important are discoveries in high energy physics compared to those in other areas of science, particularly physical science? How has this situation changed within recent decades?

Another question which may be of interest to you is: What proportion of the discoveries in high energy physics and other areas of physical science have been made in the United States, and has there been a trend for the United States to lose its leadership to other countries?

Of the work which has been done in the United States, what proportion has been funded by various Government agencies?

Another question is: What areas of science may have become significantly more important in recent years, and which may have become of somewhat less importance? In other words, are there new areas of science which you should be aware of, which ought to be competing more strongly for funding with subjects like high energy physics?

Finally, how much does it cost to make a discovery in an area of science? In a sense, how much money do you have to put in to get a significant discovery?

Now, I am sure you will recognize all of these questions cannot be answered in any really definitive, rigorous sense, but at the same time I think that we need to make an effort to get some kind of a handle on issues like this. That is what I have tried to do here.

First of all, if one is going to ask the question about the importance of discoveries and so forth, one has to have some way of evaluating what is a discovery and what is an important discovery and what is not.

Of course, we have indicators like the Nobel Prize, we have sources like historical works on the sciences, in which after 10, 20, 50 or 100 years we can look back and say, well, this was obviously an important discovery.

But it is much more difficult to do this on a short-time scale to look at things that happened last year and say what was really important, other than simply by relying on the newspaper, what gets on television programs and what the scientists themselves say is important and what manages to get publicity.

One way which can be used, I think, even though it obviously has a lot of difficulties with it—and this is an area which has been investigated by many of my colleagues in the history and sociology of science in recent years—is what is called the citation count.

This is because when scientists publish papers, they refer in their footnotes to works of other scientists. If you turn this around and recognize that a given paper which is published now will be cited by other scientists during the next few years and will presumably be the basis for other research, then these citations in footnotes can be used in a fairly gross statistical sense to measure something like the impact or the significance of a scientific paper—at least in the short run. You cannot use it for talking about how important a particular paper is for many reasons. But I think if you look at fields of science as a whole and see what papers are being most frequently cited, you can get some idea of where new areas of science are emerging. You can get some idea of the change over time of the relative levels of activity in these fields.

So that is what I have tried to do. The data which happens to be available for this, is limited, but at least it is possible to get some fairly interesting results.

This is essentially based on what is called the Science Citation Index, which is prepared by the Institute for Scientific Information in Philadelphia.

They have recently published a list of the 100 most frequently cited papers in physical science in the year 1976—that is, papers published in 1976 and then cited in the followng years up through 1978—and at the same time a list of papers published in the 1960's, in which they then counted the citations to those through 1978.

I think it is very interesting to group these papers in the physical sciences into various areas and see what kind of changes have taken place.

Now, we are simply trying to get an overview of the field by putting these papers into broad, general categories. We are essentially talking about category No. 1—high energy and nuclear physics—of course, estimating how has that changed with respect to others.

I think one of the things that does become clear is that there has been a change in the direction from the atomic and molecular physics into nuclear physics, and especially into elementary particle physics.

Now, I think this becomes even more clear if you break it down into smaller categories.

In other words, we see that there has been a much larger number of papers in high energy physics and elementary particles and field theory in 1976 that attracted immediately great attention compared to the period throughout the 1960's; whereas the papers in nuclear physics are considerably less significant.

You can see that there are fields which go up and down. I don't want to put too much stress on this because 1976 is only 1 year. Perhaps this is not a typical year, and clearly this needs to have some further investigation. But at least one can get some idea of what are the new, emerging fields which might be competing with high energy physics.

Here are some examples of new types of research which are becoming quite prominent, attracting attention within the scientific community. In other words, apart from what gets into the newspapers, these are things being cited in the technical literature.

Now, of course one can look at things that were previously important in the 1960's and have gone down, and one can ask why, and one can say, for example that a field like geophysics may come back again and be very important if we have more eruptions of volcanos, and that stimulates a great deal more work in that area.

Now, another thing which one can do with this sort of list is to see how many of these contributions came from the United States.

Contrary to what I had been led to believe by many other sources, there has not been a marked decrease in the contributions from American laboratories in this period. In fact, both in the 1960's and 1976 approximately 80 percent of the most highly cited papers came from essentially American work—the fraction even increased a little bit.

I think part of the reason for the increase is the greater predominance of the very expensive elementary particle experiments which play a fairly sizable role. In 1976, 82 of these 100 papers were essentially done in American laboratories or primarily by American scientists.

Now, I think that the date base may be biased toward this, or it may be biased toward publications in the English language. In other words, we tend to cite only English language papers, and we may tend to cite ourselves more, whereas people in other countries may cite American works more than we cite theirs.

But, given that this is perhaps not a very reliable result, one can nevertheless ask, out of these 82 papers in 1976, where did they get their financial support from?

As I have already said, a large proportion of these—43 of the papers—were in the area of elementary particles, high energy nuclear physics and field theory, and of those 43 papers, a very large proportion—I think 41 of them—were American.

Then you will not be surprised to see that ERDA, which was the predecessor of the Department of Energy, supported roughly speaking 35 out of those 82, and NSF supported 22. Of course, there is a great deal of joint support and one has to correct for that.

Well, having that sort of statistic, then one can say, ERDA supported research which led to 35 discoveries defined in this very special sense. How much did it cost. NSF supported 22. How much did it cost them and so forth.

The result which one comes out with—and I am still attempting to pin down some of these figures more exactly—if one figures that NSF spent, let's say, at an annual rate in the years immediately preceding 1976, something like $100 or $110 million for research in the physical sciences, then you get a figure of something like $5 million per discovery for NSF-supported work.

As for ERDA—I find it even more difficult to get an exact figure for how much of ERDA's budget went for basic research, but something like $200 million seems to be a good order of magnitude, which would give something like $6 million per discovery.

So I think in general one can say that it costs $5 or $6 million in 1976 to support research leading to one discovery, which is a discovery that is significant in this particular sense.

Now, again, these are just order of magnitude figures. I don't think one can use this type of analysis yet to compare how much it costs to support a discovery in physics versus how much in chemistry or how much in astronomy.

Those discoveries relating to the Martian atmopshere, for example, were of course based on the Viking, and that was a rather huge expense of the space program and would have to be charged off to those discoveries. So that sort of thing is rather difficult to pin down exactly, but at least I think it gives you some idea.

The conclusion I would draw from these figures is as far as high energy physics is concerned, the rather large investment that was made by this country in accelerators especially during the 1960's has produced a good payoff.

It is very clear that results have come out of this, and that as far as the impact on the scientific community is concerned, there has been a definite result. I think then the question that you have to ask is, how do we evaluate the scientific impact of results in this particular field of science as opposed to others?

How does one justify supporting a field if its main impact, at least immediately is on science itself? The practical benefits of finding the quark, for example—may not be immediately obvious to the layman—even though the presentation of Mr. Nunan I think should convince you that certainly these accelerators have many applications.

Well, the way that high energy physicists tend to justify their research in many cases, faced with this sort of objection, is to say this field is more fundamental than any other area of science, it is the basis for other sciences, and therefore high energy physics must be supported because it enriches in some sort of a trickle down fashion all of the other sciences, which depend on the basic laws and concepts of fundamental physics.

I find this is a very interesting argument, if one looks at it from the historical point of view, and that is what I have attempted to do in the other document I have submitted to the committee. I will simply try to summarize this.

I think that one of the great achievements of high energy physics has been to establish itself as the most fundamental science. It did not inherit the status. It is not inherent in the nature of science that the search for the smallest constituents of matter should be the most fundamental thing that scientists can do.

In fact, in other times, in previous centuries, it has been other sciences which have claimed the status of being most fundamental, such as astronomy, for example, in the 16th and 17th centuries.

There has been a very definite period in the 20th century when atomic physics and the idea of looking for the most fundamental constituent did become established as the most fundamental, but this is not necessarily going to happen in the future. It may be some other area of science which will claim the status of most fundamental.

I think one can see some of the ways in which this might happen, even within high energy physics. When one sees physicists talking no longer about, "We must find the next smaller particle," it is not so much the emphasis on some basic single particle that is going to be forming everything else, but rather the emphasis on forces, that we must unify all the forces of nature; that the thing which is really fundamental is the forces between particles rather than the particles themselves because that is where we find the really simple concepts coming out.

I think this is the sort of thing which is going to change over time. It is one argument, perhaps, for saying that a science which does claim to be fundamental to other sciences should have its status reviewed and perhaps some of its major funding reviewed by representatives from other sciences.

I think if high energy physics is in fact fundamental to not only physics but also chemistry and astronomy and so forth, then the decisions as to what projects should receive major funding perhaps ought to be discussed by representatives from those sciences.

So, I think it is possible for the scientific community to provide better advice to the Government, and I think it is possible to at least consider the idea that other areas of science are going to take over sometime in the future.

I don't think the time has come yet, but it may at some time in the future.

Thank you.

[The prepared statement of Professor Brush follows:]

The Scientific Value of High Energy Physics

Statement to the Subcommittee on Energy Research & Production,
Committee on Science and Technology, U.S. House of Representatives,
at the hearing on "Quests with U.S. Accelerators... 50 years"
July 23, 1980

by Stephen G. Brush, Professor,
Department of History and Institute for Physical Science & Technology,
University of Maryland at College Park

During the past half-century, accelerators have had an enormous impact on many areas of science and technology. Their best known contribution is to elementary particle physics (see Table 1); but accelerators have also been involved in chemistry, biology, and medicine, as the previous witness, Mr. Nunan, has shown.[1] The technique of radiation processing of industrial products is now applied to materials valued at more than $1 billion per year.[2]

In this paper I will discuss two aspects of the role of accelerators in the development of modern physical science: first, the increasing prominence of high energy/elementary particle physics in the past two decades, relative to other areas of physics, with suggestions about how the significance and cost of discoveries in different areas of science might be estimated; second, the justification of substantial funding for this kind of research on the grounds that it is "fundamental" to science, with remarks on the change in judgements of fundamentality from a long-term historical perspective.

I

During the last two decades there has been a shift of effort, resources, and interest from molecular, atomic and nuclear physics toward the study of elementary particles and their interactions, especially at the very high energies made available by accelerators. Since the results of this research

Table 1

U.S. MAJOR ACCOMPLISHMENTS IN HIGH ENERGY PHYSICS SINCE 1950

1955	Discovery of the Antiproton at the Bevatron. Segre and Chamberlain awarded 1959 Nobel Prize.
1956	Parity Violation Predicted in Weak Interactions. After confirmation in the Columbia Experiment, Lee and Yang awarded 1957 Nobel Prize.
1961	Particle Classification Scheme for Strong Interactions [SU(3)] proposed. Quarks predicted. Gell-Mann awarded the 1969 Nobel Prize.
1962	Muon Neutrino Discovered at the AGS.
1964	CP Violating Decays Observed at the AGS. Means Time Reversal Violation.
1967-75	Theory Unifying the Weak and Electromagnetic Forces Proposed. Experimental Confirmation followed. Weinberg and Glashow in the U.S. awarded the 1979 Nobel Prize.
1968	Scaling in Deep Inelastic Electron Proton Scattering Observed at SLAC. The Proton consists of Point Constituents.
1974	J/Psi Discovered Independently at the AGS and SPEAR. Confirms Force Unification and Charmed Quarks. Ting and Richter shared 1976 Nobel Prize.
1975	A New Heavy Lepton (Tau) Discovered SPEAR. Suggests a fundamental connection between Leptons and Quarks.
1975	Particle Jets Observed in Electron-Positron Annihilation at SPEAR. Confirms predictions of Quark-Gluon Structure of Hadrons.
1977	Upsilon Discovered at Fermilab. Heaviest Particle. Means the existence of a New fifth Quark.

(Compiled by Melvin Month, Department of Energy; selected from "US Major Accomplishments in High Energy Physics since 1945," prepared by Division of High Energy Physics, US Department of Energy, Washington, D.C., June 1979) seem to be more and more abstract and remote from practical applications (although the technology of accelerators has many spin-offs), it is very difficult for the non-physicist to evaluate them; and even physicists are reluctant to consider seriously how one should weight the importance of their own discoveries relative to those in other sciences.

It should no longer be necessary to make the case for government support of basic research, but it is necessary for the scientific community to provide some sensible advice about how the finite amount of money available for research should be spent. In particular, it would be useful to have answers to questions such as the following, in order to make decisions about allocation of research funds:

(1) How important are discoveries in high energy physics compared to those in other areas of science, and how this situation changed in recent years?

(2) What proportion of the discoveries has been made in the United States, and how much of this research has been funded by various government agencies?

(3) Which areas of science have become significantly more or less important in recent years? (i.e. the rate of discovery has markedly increased or decreased)

(4) What is the average cost for funding a discovery in high energy physics and how does this compare with the cost of funding discoveries in other sciences?

In order to answer any of these questions we must obviously have a working definition of "discovery," that is, some way of measuring the quality or importance of a particular scientific result. Strictly speaking this can only be done in retrospect; 50 or 100 years afterwards it may become obvious that a discovery was of great importance even though it was not recognized as such when it was first announced. One can make such a determination for previous decades or centuries by consulting systemic works on the history of science, as I have done for example for astronomy in the period 1800-1975.[3]

More often we tend to use the award of the Nobel Prize to certify the importance of a scientific discovery. But the number of prizes in a specialized field such as high energy physics is so small that this does not help very much in measuring our progress from one year or even one decade to the next. Moreover, there is always the suspicion that the people who decide on the prize will be biased for or against a particular field of research, and therefore their judgment cannot be used as an objective measure of the importance or progress of that field relative to others.[4]

Although there is no such thing as a completely objective measure of the quality of scientific research, several recent studies have show that for many purposes it is convenient to use the citation count, i.e.the number of times a published article is cited in other articles. There are a number of objections to the validity of the citation count when applied to individual papers or authors, but it seems to be a fairly good indicator of the significance or impact of papers when used statistically.[5] In any case I am not aware of any alternative method that is any more reliable, and the citation method is now much more practical than any other because the necessary data has already been compiled and published by Eugene Garfield in his Science Citation Index. Moreover, Garfield has recently summarized the information on physical science articles in a form that is appropriate for our purpose, and enables us to give at least a preliminary answer to some of the questions listed above. If this approach is considered worth pursuing, it could easily be carried out in a much more comprehensive fashion.

For the purpose of this statement I define a "discovery" as the research result reported in one of the 100 most frequently cited papers in the physical sciences, published in the year 1976, based on citations during the years 1976-78; or in one of the 100 most frequently cited papers in the physical sciences, published in the 1960s, based on citations during the years 1961-78.[6]

The two most-frequently-cited papers on high energy physics published in the 1960s were "Symmetries of baryons and mesons" by Gell-Mann (1962) and "A model of leptons" by Weinberg (1967); both were theoretical papers closely related to accelerator experiments. The most highly-cited paper published in 1976 was "Observation in e^+e^- annihilation of a narrow state at 1865 MeV/c^2 decaying to $K\pi$ and $K\pi\pi\pi$" by Goldhaber and 39 others. This was a report of an experiment at SPEAR by the SLAC/LBL group, confirming the existence of "charm." Each of these papers clearly represents a major scientific discovery by any criterion. On the other hand if you were to make a list of the most important papers in high energy physics published in the 1960s or in 1976, as judged by researchers, you would undoubtedly find several that do not appear on Garfield's list of the 100 most frequently cited papers.

An example of such a list is given in Table 1. Of the papers published in the 1960s, only two out of four (theoretical papers by Gell-Mann and Weinberg just mentioned) are identified by the citation count; apparently experimental papers are not cited very frequently after two or three years -- if they are important, their results are incorporated into the theoretical and review papers which are more frequently cited by later authors. This suggests that one should not try to use the citation method to compare the significance of theoretical and experimental papers within the same field.

A list of 1977 papers most frequently cited in 1977 and 1978, published by Garfield just after this hearing (Current Contents, 28 July 1980), shows that the announcement of the discovery of the upsilon particle at Fermilab heads the list. So in this case the citation count method is confirmed by the independent assessment presented in Table 1.

To give an idea of overall trends I have divided the 100 papers in each group into five general categories:

Number of highly-cited papers in:	1960s	1976
I. High energy physics, nuclear physics, field theory, elementary particles	27	43
II. Atomic & molecular physics, solid state physics, statistical physics, lasers & fibre optics	45	30
III. Astronomy, astrophysics, geophysics, supergravity	5	12
IV. Applied mathematics	3	0
V. Chemistry	20	15
Total	100	100

Here is a more detailed breakdown into subfields of physical science:[7]

Number of most-cited articles in:	1960s	1976
High energy physics, elementary particles, field theory	16	41
Nuclear physics	11	2
Solid State Physics	12	10
Geophysics	4	0
Applied Mathematics	3	0
Astronomy, astrophysics, supergravity	1	12
Statistical physics, phase transitions	4	3
Organic chemistry, biochemistry, biophysics	9	5
Inorganic chemistry	3	3
Physical chemistry	8	7
Atomic & molecular physics	28	11
Masers, lasers, fibre optics	1	6
Total	100	100

The list of most-frequently-cited 1977 papers is not completely comparable to the 1976 list because of a change in the way it is presented, but it indicates that papers in high energy physics, elementary particles and field theory make up an even greater proportion of the discoveries in physical sciences. When citations from 1979 to the 1977 papers are included, however, it appears that several papers in geophysics make the list.

It is not possible to tell from the published compilations whether the sizable jump in the number of discoveries in elementary particle physics is characteristic of the past five years or peculiar to 1976 and 1977, but I think this point deserves further investigation.

By looking in more detail at the individual papers on these two lists, we learn that three areas that were well-represented in the 1960s seem to have dropped substantially in significance in 1976: applied mathematics, crystallography, and nuclear physics. Several other areas, in addition to elementary particle experiments, are attracting much more attention in 1976: astrophysics, the Martian atmosphere, field theory, supergravity, phase transitions, lasers & fibre optics, and photochemistry.

How many of these discoveries could be credited to the United States? There is some ambiguity here because of the free flow of scientists between countries, and the participation of scientists from several countries in several reserach teams. My estimate from this data is that about 78% of the discoveries in the 1960s were made by people affiliated with institutions in the U.S., and that this proportion increased slightly, to 82%, in 1976. For 1977 it was down to 74%.

Within category I, the proportion of U.S. contributions increased from 20 out of 27 in the 1960s (74%) to 41 out of 43 in 1976 (96%). This change is mainly due to the shift in emphasis to the more expensive elementary-particle experiments within this category.

While these results are based on a single year in the past decade and therefore cannot be relied on very heavily, they do tend to contradict the impression one gets in publications such as Science Indicators 1978 that the American share of world research in the physical sciences has been declining during the past decade.[8] One can argue that the data base for the Science Citation Index tends to favor American publications, but I don't think that bias is likely to have increased between the 1960s and 1970s; hence citation analysis probably can give fairly reliable indications of overall shifts between countries.

Finally, we can easily determine the sources of funding of these discoveries since it is customary to acknowledge financial support at the end of every scientific paper. The only uncertainty comes from the fact that many papers list two or more sources of funding. Taking account of fractional-paper support and rounding off the totals, I estimate the following distribution of sponsors for the 82 American discoveries listed among the 100 most-frequently cited 1976 publications:

ERDA (predecessor of DOE)	35
NSF	22
NASA	8
DOD	5
HEW (predecessor of HHS)	4
Industrial laboratories (IBM, Bell, Corning, Dupont)	5
Miscellaneous private foundations or no sponsor mentioned	3
Total	82

Within category I, ERDA supported approximately 28 and NSF approximately 13 of the 41 U.S. papers on the 1976 list. The accelerators themselves were all ERDA facilities; out of 19 elementary particle experiments, 11 were done at

Fermilab, 5 at the SLAC/LBL/SPEAR complex, 2 at Brookhaven, and 1 at the Savannah River Plant operated by Dupont for ERDA. (Of the two European elementary particle experiments, one was done at CERN in Switzerland and the other at DORIS/DESY in Germany.)

We can now try to estimate the cost per discovery, on the basis of the support for basic research in physical science provided by these agencies during the two or three years prior to the publication of the results. The following data are provided by NSF's Division of Science Resource Studies.

	FY 1974	FY 1975
ERDA	$237 million	$243 million
NSF	119	138
NASA	160	198
DOD	54	52
Other	63	61
Total	$633 million	$692 million

(These figures do not include support for mathematics or geophysics.)

As a very rough estimate (using the averages of the above numbers) we then find that each discovery supported by ERDA cost 240/35 = $7 million; by NSF, 129/22 = $6 million; by NASA, 179/8 = $22 million; by DOD, 53/5 = $11 million.

Because of the different citation practices in different fields, one cannot legitimately compare the number of discoveries in two different fields of science on the basis of data from a single year; only the relative changes over a period of time are meaningful. Therefore we cannot say how expensive it is to fund high energy physics as compared to astronomy or chemistry, on a "per-discovery" basis, until a more extensive analysis for other years has been done.

My conclusion is that discoveries in physics published in 1976 cost about $7 million each, assuming that a discovery is defined as a result reported in one of the 100 most-frequently-cited papers in physical science. This number is arbitrary in the sense that it might be reduced to \$3.5 million, for example, if one changed the definition to include the 200 most-cited papers; but it does suggest the possibility of coming to a more definite conclusion by extending the analysis to other years and looking at changes over time.

It does not appear that high energy physics discoveries (funded primarily by ERDA) are significantly more expensive than those in other areas of physics. If 1976 is typical of the recent past, we are getting substantial dividends now from our past investments in this field.

II

Section I indicates that high energy physics is still a highly successful research field as judged by physicists. But the question of whether the field should continue to enjoy a high level of support relative to other sciences remains to be considered. We could probably show by the same methods that some area of biology or psychology is equally successful as judged by citations in the journals of life science or behavioral science, and in addition, its discoveries are more comprehensible, less expensive, and promise more immediate practical benefits.

In their attempts to justify funding for larger and larger accelerators, physicists argue that the study of elementary particles is so <u>fundamental</u> that it should be supported even if there were no immediate prospect of practical benefits to society. For example, R. P. Shutt of Brookhaven National Laboratory wrote in 1971:[9]

> Without particle physics, physics would have no true frontier, and in a larger sense, science would have no frontier... no truly new principles or laws of nature could be discovered any longer.

Recently this viewpoint has been stated as follows, in the words of science writer Nigel Calder:[10]

> Physics was always the master-science. The behaviour of matter and energy, which was its theme, underlay all action in the world. In time astronomy, chemistry, geology, and even biology became extensions of physics. Moreover, its discoveries found ready application, whether in calculating the tides, creating television or releasing nuclear energy. For better or worse, physics made a noise in the world, But the abiding reason for its special status was that it posed the deepest questions to nature.

These quotations probably reflect the opinions of many physicists. But physics was not always the "master science," and the study of elementary particles was not always the most fundamental kind of research; it only became so in the first half of the present century. Twentieth-century atomic physics, with the help of accelerators, earned the status of "most fundamental science." It is only by going back in history to a time when the search for elementary particles was not considered the most fundamental goal of science that we can appreciate the magnitude of that achievement, and at the same time recognize that some other science may in the future become the most fundamental.

Looking back to the origins of modern science in the 16th and 17th centuries we find that astronomy was considered the most fundamental science. This was the legacy of ancient Greek science which postulated that the heavenly bodies are perfect, move eternally in circular paths, and exhibit the true harmony of the universe, whereas everything in the terrestrial sphere is messy and complicated. As Copernicus wrote in his book, On the Revolutions of the Heavenly Spheres, which initiated modern science:[11]

> Among the many and varied literary and artistic studies upon which the natural talents of man are nourished, I think that those above all should be embraced and pursued with the most loving care which have to do with things that are very beautiful

> and very worthy of knowledge. Such studies are those which deal with the godlike circular movements of the world and the course of the stars, their magnitudes, distances, rising and settings, and the causes of the other appearances in the heavens; and which finally explicate the whole form.

In the 17th century, Galileo and Newton proposed new laws of mechanics which were confirmed in astronomy and applied also to the terrestrial sphere. Throughout the 18th century, astronomy maintained its high status as calculations of planetary and lunar motion became so accurate that they could be used in navigation. The last great triumph of Newtonian astronomical theory occurred in 1846 when deviations of the planet Uranus from its predicted path were used to locate a previously unsuspected planet, Neptune.

Early in the 19th century planetary astronomy, though still the most respected branch of science, seemed to offer little hope for further advances except through tedious numerical calculation. Chemistry and geology began to attract the attention of those who preferred a younger science in which major discoveries could still be made without excessive mathematical labor. Both were coming to be regarded as "fundamental" in the sense that they dealt with important problems and provided a firm basis for advances in other sciences.

Chemistry was a fundamental science as long as its "elements" -- hydrogen, oxygen, iron, etc. -- were thought to be qualitatively different kinds of matter. Chemistry (not physics) seemed to offer the best route toward understanding the atomic structure of matter, and stimulated research in the areas of heat, electricity, agriculture and nutrition. Physics, which was later to incorporate heat and electricity as subfields, did not even exist as a coherent science in its modern sense, including atomic physics, until the last half of the 19th century. Later the discovery of nuclear transmutation showed that one chemical element can be changed into another; they are not in fact "elementary" but can all be built up from hydrogen. The development of quantum mechanics showed that chemical bonds and reactions can be explained in terms of the physical properties of atoms. Thus chemistry by 1930 was no longer fundamental but reducible to a branch of physics.

Geology at the beginning of the 19th century was also a fundamental science. It dealt not only with the present strucutre of the entire earth, but with its past history; it was the only subject that attempted to describe changes through long periods of time, and even challenged theology for the right to consider the creation and age of the earth. It was fundamental also in the sense that it provided the basis for another science - paleontology, and later evolutionary biology.

By 1900 geology had greatly contracted its domain: the origin of the earth belonged to astronomy, and most of the inside was the province of a new science, seismology. But, more significantly, geology's claim to be able to establish a time scale for the development of the earth's crust had been demolished by physics. In the 1860s, the British physicist Lord Kelvin became intensely interested in the age of the Earth. He asserted on the one hand that this problem was one of the most important in science, but on the other hand that the geological theories and methods previously applied to it were unacceptable because they conflicted with basic principles of physics (especially those involving heat).

Kelvin's dictum was, in effect, that whenever physics and geology disagree, geology must give way because physics is more fundamental. The geoloists, intimidated by Kelvin's prestige and mathematical formulas, accepted his dictum and as a result lost confidence in the value of their own methods -- even after 1900, when Kelvin's results on the age of the Earth were found (by other physicists) to be wrong.[12] I find this episode very interesting because it shows quite clearly that physics was becoming more fundamental than another science through a direct confrontation.[13]

The high status of physics in the 20th century is of course primarily due to its own revolutionary successes -- Einstein's theory of relativity, Rutherford's experiments on the atomic nucleus, and the quantum theory of Planck, Bohr, Heisenberg and Schroedinger. Soon after quantum mechanics had elucidated the electronic structure of the atom, the accelerator provided "the key to a new world of phenomena, the world of the nucleus" as Lawrence and Livingston predicted in 1932.[14] Among many important results obtained with accelerators, I will mention only one that reinforced the fundamental character of atomic physics: the discovery (or "creation") of the antiproton in the Bevatron at Berkeley, by a team led by Emilio Segre and Owen Chamberlain.[15] This experiment confirmed a general principle of relativistic quantum mechanics (first proposed by Dirac) that every elementary particle has an anti-particle, so the world is symmetrical with respect to positive and negative charges.

By 1960s, elementary particle physics had become the most prestigious and lavishly supported area of science, and in many respects it retains that status today. Let us now ask: what might replace elementary particle physics as the most "fundamental" area of science?

The most obvious candidate would seem to be cosmology - the study of the development and strucutre of the universe as a whole - which has enjoyed a spectacular revival in the last two decades. Physicists do not seem to feel threatened by this development, in fact they welcome it since the most advanced cosmological theories and speculations involve large doses of relativity, quantum mechanics and even elemenatry-particle physics. But let us go further and consider what kind of change might replace the search for the most elementary particle with something radically different that would challenge the current philosophy and priorities of physicists.

In order to answer that question we have to recognize that <u>atomism</u> -- the assumption that matter can be analyzed in terms of elementary particles -- is a basic part of the scientific worldview that has prevailed in Western Europe and America since the 17th century. The previous worldview was "organic" or "holistic," emphasizing the <u>relations</u> between parts of a system rather than their separate structures. There have been periodic revivals of this worldview as reactions against the "materialist" or "mechanistic" tendency of Western science since the time of Newton. So far all of these reactions have failed because they have attempted the impossible task of repealing the advances made by science during the past four centuries.[16] When holism gains ground it is not by a frontal assault on science but by leading science in new directions. Here are some examples.

In high energy physics itself the proliferation of "elementary" particles has thrown doubt on the assumption that any of today's particles is really elementary. The most likely prospect is the quark. But it is apparently impossible to pry one quark loose from the others within a larger particle such as a proton, and study it separately. Thus attention has shifted to the <u>forces</u> between particles. The currently-fashionable goal of finding a unified theory of all forces, from which particles would be <u>derived</u> as secondary entities, seems to be an admission that the search for the most elementary particle has been given lower priority if not abandoned.[17]

If the primary goal of physics is to find a unified theory of all forces, it is by no means obvious that this goal can be reached only by doing experiments at higher and higher energies. I see an interesting historical analogy with the discovery of the relation between electricity and magnetism by Oersted in 1820: he was so firmly convinced, for metaphysical reasons, of the unity of electricity and magnetism within a holistic worldview that he managed to abandon the restrictive preconceptions of Newtonian science to do a very simple experiment with a current and a compass.[18] It was a new kind of arrangement of the apparatus, rather than simply increasing the strength of the charge or the magnet, that led to success.

Putting the emphasis on the unification of forces rather than the discovery of smaller particles might actually strengthen the argument for the fundamentality of high energy physics, since many more areas of science depend on these forces than depend on the properties of the elusive quark. In any case one can argue that if a science deserves to be called fundamental and is to be supported primarily for that reason, its funding might be reviewed by an advisory panel including representatives from those neighboring areas of science that are supposed to be based on it. Such a panel should be able to explain to the public the significance of the discoveries that have been made and the hypotheses that are to be tested.[19]

An even more radical breakdown of the mechanistic preconceptions of modern Western science is suggested by some interpretations of quantum theory. Perhaps phenomena at the atomic level do not have independent reality apart from human observation, or perhaps we cannot make a measurement on an atomic particle in the laboratory without in some way disturbing every other particle in the universe. The experiments currently being done to test these possibilities might produce a more fundamental change in our view of the world than any of the more expensive high-energy physics research now being proposed.[20]

An extension of this line of thought is the "anthropic principle": the postulate that the physical properties of the universe must allow the evolution of intelligent life that will observe and thereby confer reality on it. In this way one can explain why certain physical constants, such as the "fine structure constant" (a ratio involving Planck's constant and speed of light) have the values they do: if they were very much different, intelligent life could not have evolved to measure them.[21] These are now just wild speculations, disdained by most scientists even though they come from highly respected physicists like John Wheeler. If they are taken more seriously in the future we might have to elevate biology and psychology to the status of "fundamental science" now enjoyed primarily by physics.

To summarize: during the past 50 years, quests with U.S. accelerators helped to establish high energy physics as the most fundamental area of science. But this status was earned in a particular historical situation, and is not inherent in the nature of science. Leaders of the scientific community should be sensitive to the changing connections among the sciences brought about by new discoveries and insights. Even if they cannot always agree on priorities, discussions at this level (rather than on the technical "needs" of each specialty) should help Congress and the public to make reasonable decisions on the allocation of resources.

<u>notes</u>

1. A good though somewhat outdated review of these applications may be found in the book edited by L. C. L. Yuan, <u>Elementary Particles: Science, Technology, and Society</u> (New York: Academic Press, 1971). For a recent, brief overview of elementary particle physics see the first three pages of D. A. Bromley, "Physics," Science 209: 110-21 (1980).

2. J. Silverman, "Basic concepts of radiation processing" and "Current status of radiation processing" <u>Radiat. Phys. Chem.</u> 9: 1-15 (1977), 14: 17-21 (1979).

3. "Looking Up: The Rise of Astronomy in America," <u>American Studies</u> 20 (2): 41-67 (1979).

4. See Harriet Zuckerman, <u>Scientific Elite: Nobel Laureates in the United States</u> (New York: Free Press/Macmillan, 1977); H. Inhaber and K. Przednowek, "Quality of Research and the Nobel Prize," <u>Social Studies of Science</u> 5: 33-50 (1976).

5. J. R. Cole and S. Cole, <u>Social Stratification in Science</u> (Chicago: University of Chicago Press, 1973). F. Narin et al., <u>Evaluative Bibliometrics: The Use of Publication and Citation Analysis in the Evaluation of Scientific Activity</u> (Cherry Hill, N.J.: Computer Horizons, 1976). Eugene Garfield, <u>Essays of an Information Scientist</u>, 2 vols. (Philadelphia: ISIS Press, 1977); <u>Citation Indexing, Its Theory and Application in Science, Technology and Humanities</u> (New York: Wiley, 1979). E. Garfield, M. V. Malin & H. Small, "Citation data as science indicators," in Y. Elkana et al., eds., <u>Toward a Metric of Science</u> (New York: Wiley, 1978), PP. 179-207.

6. E. Garfield, "The 1976 articles most cited in 1976 and 1977. 2. Physical Sciences," <u>Current Contents</u> 19 (17): 5-16 (April 23, 1979); "Most-cited articles of the 1960s. 1. Physical Sciences," <u>ibid.</u> 19 (21): 5-15 (May 21, 1979). A somewhat similar analysis for 1972 chemistry publications may be found in "Report of the National Science Board to the Subcommittee on Science, Research and Technology of the Committee on Science and Technology, U. S. House of Representatives, regarding peer review procedures at the National Science Foundation," November 1977, pp.II-25f.

7. I have changed the classification of some of the articles in Garfield's lists as follows. For 1960s papers, the one by Kadanoff et al. has been moved from "molecular physics" to "statistical physics," and the one by Fisher has been moved from "physical chemistry" to "statistical physics." For the 1976 list, Kirkpatrick's paper in solid state physics and the two papers listed under "field theory in solid state physics" have all been moved to "statistical physics'; the paper by Letokhov and Moore has been moved from "atomic and molecular physics" to "lasers and fibre optics"; both papers in "chemical physics" have been moved to "atomic and molecular physics."

8. In physics, U.S. articles declined from 33% of the world literature in 1973 to 30% in 1977. During this period the "citation ratio" remained about 1.40 or 1.41, i.e. U.S. articles were cited about 40% more than their share of the literature. Science Indicators 1978, Report of the National Science Board 1979 (Washington, D.C.: National Science Foundation, 1979), pp. 150, 152. While recognizing the utility of citation counts as a measure of the influence of research publications, this report gives no hint of the extent of American domination of specialized fields such as high energy physics, revealed by Garfield's listings of "most-cited articles." For a general discussion and critique of the "science indicators" project see Y. Elkana et al., editors, Toward a Metric of Science: The Advent of Science Indicators (New York: Wiley, 1978).

9. Quoted from p. 31 of his article "Science, Physics, and Particle Physics," in Yuan, op. cit (note 1), 1-48. M. J. Moravcsik presents a well-reasoned analysis of the concept of "fundamentality" in his article "A refinement of extrinsic criteria for scientific choice" Research Policy 3: 88-97 (1974).

10. Quoted by J. E. Leiss, Associate Director for High Energy and Nuclear Physics of the Department of Energy, in his presentation to the

Energy Research and Production Subcommittee of the House Science and Technology Committee, February 26, 1980. See Nigel Calder, The Key to the Universe (New York: Viking, 1977), p. 14.

11. N. Copernicus, De Revolutionibus Orbium Coelestium (1543); quoted from the translation by C. G. Wallis, in Great Books of the Western World, vol. 16, p. 510.

12. J. D. Burchfield, Lord Kelvin and the Age of the Earth (New York: Science History Publications, 1975). The greatly diminished vitality of geology in the first part of the 20th century, due to many other factors as well as Kelvin's attacks, is shown not only by the scarcity of major discoveries in that period but also by morbid features of its published literature -- the turgid style of language, immense bibliographies, delays in publication of new results, etc. See the fascinating book by Henry Menard (now director of the U.S. Geological Survey), Science: Growth and Change (Cambridge, Mass.: Harvard University Press, 1971) which shows how one can detect the health or decay of a scientific discipline by looking at various quantitative indicators. Another phenomenon which may be more difficult to establish except on an anecdotal basis is the tendency of the best younger scinetists to move into a field they consider "fundamental" and desert a less prestigious field where they might have been able to make a major contribution. I can recall from my own days as a graduate student in the 1950s that the best students in physics were not encouraged to go into geophysics, even though with hindsight I can see that those were the days when the "revolution in the earth sciences" leading to plate tectonics was just beginning, and geophysics has since become much more respectable.

13. S. G. Brush, "Planetary Science: From Underground to Underdog," Scientia 113: 771-87 (1978). The fact that physics is considered more

fundamental than geology affects our perception of the history of science in previous centuries. For example, it is generally believed that Americans were indifferent to basic science in the 19th century, since they accomplished relatively little in physics; but in fact they were quite active and successful in the fundamental sciences of that time -- astronomy, the earth sciences and soem areas of biology. See D. J. Kevles, J. L. Sturchio and P. T. Carroll, "The Sciences in America, Circa 1880," Science 209: 27-32 (1980); S. G. Brush, "Looking Up: The Rise of Astronomy in America," American Studies 20: 41-67 (1979).

14. E. O. Lawrence and M. S. Livingston, "Production of High Speed Ions," Physical Review [2] 42: 20-35 (1935), reprinted in H. A. Boorse and L. Motz (eds.), The World of the Atom (New York: Basic Books, 1966), p. 1390.

15. See Boorse & Motz., op. cit., Chapter 82, for discussion of this experiment and reprint of the original report in Nature (1956).

16. Carolyn Merchant presents a provocative discussion of the change from holism to mechanism from the viewpoint of modern feminism and ecology in her recent book, The Death of Nature: Women, Ecology, and the Scientific Revolution (San Francisco: Harper & Row, 1980). I have reviewed the various "romantic" movements in relation to developments in science since 1800 in my book The Temperature of History: Phases of Science and Culture in the Nineteenth Century (New York: Burt Franklin, 1978) and my article "The Chimerical Cat: Philosophy of Quantum Mechanics in Historical Perspective," Social Studies of Science (in press).

17. This is stated most explicitly on page 33 of Steven Weinberg's article, "The Search for Unity: Notes for a history of Quantum Field Theory," Daedalus 106: 17-35 (1977). Bromley's article, cited in note 1, illustrates rather nicely the continuing tension between the conviction that particles like quarks must exist even though they can never be detected, and the desire

for a 'grand unification' of forces which will lead to observable consequences in high energy experiments.

18. R. C. Stauffer, "Speculation and experiment in the background of Oersted's discovery of electromagnetis," Isis 48: 33-50 (1957).

19. M. J. Moravcsik proposed such a panel for somewhat different reasons, to deal with a perceived "crisis" in high energy physics: see "The crisis in particle physics," Research Policy 6: 78-107 (1977). In my view interdisciplinary panels should be a regular part of the funding review process in all areas of science: physicists would have to justify their major projects to chemists and vice versa.

20. B. d'Espagnat, "The Quantum Theory and Reality," Scientific American 241 (no. 9): 158-81 (1979). A. Shimony, "Metaphysical problems in the foundations of quantum mechanics," International Philosophical Quarterly 18: 3-17 (1978).

21. J. A. Wheeler, "The universe as home for man," American Scientist 62: 683-91 (1974); "Genesis and observership," in Proceedings of the Fifth International Congress of Logic, Methodology and Philosophy of Science, ed. R. E. Butts and J. Hintikka, Part 2, pp. 3-33 (Boston: Reidel, 1977).

22. My research has been supported by the History and Philosophy of Science Program of the National Science Foundation. I am grateful to Dr. Robert Leachman of the subcommittee staff for asking some of the questions considered in this testimony, and to Dr. Henry Small of the Institute for Scientific Information for supplying some of the information that helps to answer them.

Mr. McCormack. Thank you, Professor.

Mr. Wydler?

Mr. Wydler. I have no questions. The panel's presentations were all excellent and gave us a little different perspective, something to think about.

I would like to give you something to think about, all the people in the room today, because you are all intimately connected with the future of accelerators and high energy physics; that is, the layman's point of view.

I think I can express that. I pretend to be at least a somewhat informed layman because I do sit and listen to scientists, such as I am today, talk about these matters. That is something the general public does not have the advantage of. But I still have the layman's point of view because I am a nonscientist.

It appears to me, it strikes me, as I listen to the discussion that has gone on here today, that the ultimate question that the public is going to ask is where does it all end. By that I mean will you continue to build bigger and better machines? That is fine, and they do bigger and better things, apparently, as we go along.

I know that we are building Isabelle on Long Island, and we are proud of that, and we look forward to some important results coming from that machine.

I also hear stories—I don't know how true they are—that CERN is already going to outdo us, they are going to build something enormously bigger, they are talking about a 20-mile wide or long or round machine that will upstage us, and go us one better.

I don't know if they are really going to build it. Immediately upon them announcing that, everybody thinks of how we can build one bigger than that.

The question that strikes me down at the heart of this whole thing is, Is it sensible to do that until we have really digested and used the information we already have?

In other words, if we just keep running forward and really don't take the facilities that we have available now and use them to the utmost, are we really sensible in just continuing on sort of a competition of who has the biggest machine.

I think the public is going to ask that question in the years ahead. It is going to be up to the scientific community to convince the people that they have indeed used all the information that we currently have and that is available and developed and utilized it to the extent that it is actually useful before the public is going to be willing to go just for continuously bigger and better machines.

That is the point that I would make to this group as a layman on something I think you all would do well to think about a little bit.

Mr. McCormack. Thank you, Mr. Wydler.

I think your remarks also speak to the concept of closer international cooperation, so that we are working together across the entire world in the field of energy physics, so that we don't unnecessarily duplicate expensive equipment, but work together for the common understanding of the subject.

I want to congratulate each of you gentlemen for your testimony this morning.

Dr. Livingston, Mr. Nunan, Professor Brush. I don't have any questions for any of you. There are obviously lots of comments one could make. But I am afraid if I took that time now, we would not get to our next panel. So I simply want to thank you and congratulate you for your testimony this morning.

We are going to have about a 5-minute recess and then we will pick up with the second panel.

[Brief recess.]

Mr. McCORMACK. May we come back to order again.

Our second panel deals with high energy and nuclear physics research programs in the United States and other nations, its prospects and problems.

We have three witnesses. I am going to introduce two of the witnesses and then ask a special guest to introduce the third one. I will go in reverse order at this moment and introduce first of all Dr. N. Douglas Pewitt, Deputy Director of Energy Research with the Department of Energy.

Dr. Pewitt, welcome. You are sitting on my right, on the left of the audience behind you.

On my left is Dr. John Adams, Director General at CERN. He has been a gracious host for us who have visited CERN. I can only say I recall vividly my visit to CERN, partially because about halfway between the hotel in Geneva and the CERN installation our State Department car was rammed broadside by somebody in a French Peugeot and totaled out his car and nearly totaled us out. So I have a very vivid memory of visiting CERN. That was an inelastic collision.

Our third guest is a guest who has been with us before, Dr. Panofsky. I am going to ask a good friend of ours, Congressman Pete McCloskey of California, who is very closely associated with SLAC, to do the introduction.

Mr. McCLOSKEY. Thank you, Mr. Chairman.

I am impelled to say that the Chairman knows my current politics started with opposition to the Stanford linear accelerator's high voltage energy line some 15 years ago as a private lawyer in Palo Alto.

Mr. McCORMACK. You mean you have been born again?

Mr. McCLOSKEY. No, I have been dealing with Dr. Panofsky both for and against over many years.

I just want to say two things.

One, that the abolition of the Joint Committee on Atomic Energy has focused the importance of this committee's work in the interests of long-range energy development for the United States.

I know of no more difficult question in a political year of this kind than the attempt to distinguish between the long-range energy research of this country, that does not produce immediate practical results, and the temptation to devote our time and our energies and assets to short-range research.

In that connection, representing Stanford University, as I do, and having the privilege of representing at least five Nobel Prize winners, as well as a group of other eminent scientists and scholars, I know of no greater individual in the United States or of more importance to the United States than Dr. Panofsky and the advice that he may give this committee and the Congress.

It is a privilege to introduce him. I think the marriage or the wedding or the meeting between this committee and Dr. Panofsky is of very great importance to the country and the world at large.

Thank you.

Mr. McCormack. Thank you, Congressman McCloskey. Are you going to be able to stay with us?

Mr. McCloskey. No, but I will read the key points of the testimony.

Mr. McCormack. Thank you.

Gentlemen, as with the previous witnesses we have your testimony before us. We will, without objection, insert it in its entirety in the record. You are each free to proceed as you wish. I would like to suggest that you may wish to summarize some of your testimony and make it as comfortable for yourself, be as informal as you like.

Dr. Panofsky, would you care to lead off.

PANEL ON HIGH ENERGY AND NUCLEAR PHYSICS RESEARCH PROGRAMS IN THE UNITED STATES AND OTHER NATIONS: PROSPECTS AND PROBLEMS: DR. WOLFGANG PANOFSKY, DIRECTOR, STANFORD LINEAR ACCELERATOR LABORATORY, STANFORD UNIVERSITY, STANFORD, CALIF.; DR. JOHN ADAMS, DIRECTOR GENERAL, EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH (CERN), GENEVA, SWITZERLAND; AND DR. N. DOUGLAS PEWITT, DEPUTY DIRECTOR OF ENERGY RESEARCH, U.S. DEPARTMENT OF ENERGY

[The biographical sketch of Dr. Panofsky follows:]

W. K. H. Panofsky

Director, Stanford Linear Accelerator Center

Professor, SLAC

Degrees

1938	A.B. Princeton University
1942	Ph.D., California Institute of Technology
1963	D.Sc. (Hon.), Case Institute of Technology
1964	D.Sc. (Hon.), University of Saskatchewan, Canada
1977	D. Sc. (Hon.), Columbia University

Experience

1942-3	Director, Office of Scientific Research & Development Project, California Institute of Technology, Pasadena
1943-5	Consultant, Manhattan District, Los Alamos, New Mexico
1945-6	Physicist, Radiation Laboratory, University of California at Berkeley
1946-8	Assistant Professor of Physics, University of California at Berkeley
1948-51	Associate Professor of Physics, University of California at Berkeley
1951-63	Professor of Physics, Stanford University
1953-61	Director, Professor, Stanford High Energy Physics Laboratory
1961-	Director, Professor, Stanford Linear Accelerator Center, Stanford University

Special Fields

X-rays and natural constants; accelerator design; nuclear research; high-energy particle physics

Activities

1945-60	Division of Military Application, U.S. Atomic Energy Commission
1954-58	Member, Physics Panel, National Science Foundation
1955-57	U.S. Air Force Scientific Advisory Board
1951	Consultant, Radiation Laboratory, University of California, Berkeley
1958	Consultant, Stanford Research Institute, Menlo Park, California
1960-64	President's Science Advisory Committee
1959	Office of Director of Defense Research and Engineering (member, Ad Hoc Group on Detection of Nuclear Explosions)
1959	WAE Foreign Service Office, Department of State: Chairman, U.S. Delegation (Geneva), Technical Working Group on High Altitude Detection; Vice-Chairman, U.S. Delegation (Geneva), Technical Working Group 2
1958-60	Member, High Energy Commission of International Union of Pure and Applied Physics
1958-60	Review Committee for the Particle Accelerator Division and High Energy Physics Division, Argonne National Laboratory
1959-61	Advisory Council, Department of Physics, Princeton University
1958-62	Advanced Research Projects Agency, Consultant
1963-66	Physics Survey Committee, National Academy of Sciences
1964	Advisory Committee, 200-BeV Accelerator Study, Lawrence Radiation Laboratory, Berkeley
1965-73	Consultant, Office of Science and Technology, Executive Office of the President
1965-73	Steering Committee, JASON Division, Institute for Defense Analyses
1959 -	Consultant, Arms Control & Disarmament Agency

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Activities - cont'd.

1967-70	Member, High Energy Physics Advisory Panel to the Atomic Energy Commission
1968-72	Advisory Committee, Brookhaven National Laboratory
1968-71	Advisory Committee, Cambridge Electron Accelerator Laboratory
1968-71	Advisory Committee, Physics Department, Univ. of Rochester
1969-71	Advisory Committee, Physics, Mathematics & Astronomy Depts., Calif. Inst. of Technology
1969-70	Co-Chairman, Stanford Mid-Peninsula Urban Coalition
1973-76	Board of Directors, Annual Reviews, Inc.
1977-	Board of Trustees, Universities Research Association
1977-78	Ford Foundation - NEPS (Nuclear Energy Policy Study)
1978-	General Advisory Committee, The White House

Societies

Phi Beta Kappa; American Physical Society (Fellow and 1974 President); Sigma Xi; National Academy of Sciences; American Academy of Arts and Sciences, Council on Foreign Relations

Awards

Guggenheim Fellowships (1959 and 1973); Ernest Orland Lawrence Memorial Award (1961); Richtmyer Lecture (1963); California Institute of Technology - Alumni Distinguished Service Award (1966); California Scientist of the Year Award (1967); National Medal of Science (1969); Franklin Institute Award (1970); Annual Public Service Award, Federation of Amer. Scientists-1973; "Officier" of French Legion of Honor (1977); Jessie & John Danz Lecturer, Univ. of Wash., Seattle (1979); Enrico Fermi Award (1979)

Publications

Classical Electricity and Magnetism (with M. Phillips), Cambridge, Addison-Wesley (1955); 2nd edition (1962); numerous scientific papers in professional journals

Personal Data

Name: Wolfgang Kurt Hermann Panofsky
Born April 24, 1919, Berlin Germany
Entered U.S. September 1934, naturalized April 1942
Married (Adele Irene DuMond)
Children: Richard Jacob, October 13, 1943
Margaret Anne, October 13, 1943
Edward Frank, April 19, 1947
Carol Eleanor, January 12, 1951
Steven Thomas, December 13, 1952
Home address: 25671 Chapin Ave., Los Altos Hills, Calif. 94022
Office address: SLAC, P. O. Box 4349, Stanford, Calif. 94305

STATEMENT OF DR. PANOFSKY

Dr. PANOFSKY. Thank you, Mr. Chairman.

Members of the committee and staff, I am pleased to be able, together with my colleagues, to give my views here as an individual citizen who has been involved with accelerator-related work during the entire post-World War II period.

The work which I am reporting about is important, both for cultural and material reasons.

On the cultural side, the quest to understand nature is as old as mankind itself, and it is from this growth in understanding that human civilization has developed.

In particular, when many decisions have to be made on how to use the forces that nature provides more effectively, while at the same time maintaining the quality of our environment, then we need more knowledge of the physical world.

As scientists, we have faith that on the average, however fallible individual decisions may be, man will make better decisions on how to live harmoniously with nature as his growing understanding of natural processes comes to replace ignorance.

On the material side, you have heard about many of the spinoffs of high energy accelerator physics. Let me display the first transparency to illustrate the historical thrust of this situation.

This slide shows what today the high energy physicist is doing. He is trying to penetrate the unknown. His motivation is better knowledge of the basic forces of nature.

In 1930–38 the nuclear physicists were also motivated only by trying to understand how nature works. He did not believe, in fact often denied, that there would be applications.

Yet today, we are facing both the benefits and the problems associated with all the applications of nuclear physics, and again our deeper knowledge about the processes enable us to make the wisest possible decisions.

If we go even back further into the turn of the century, at the beginning of atomic physics, again the physicists of those days were motivated only by understanding nature. Yet today, all of chemistry, quantum electronics, lasers, transistors, is resting on the results obtained at that particular time.

So, indeed the historical thrust is by far the strongest argument why we must maintain the cutting edge to the rate of accelerator-based high energy physics.

Now, large particle accelerators are a means to an end. They are the means toward the penetration into the unknown of high energy physics.

Now, however, we have here a very peculiar situation. Very frequently the way has been pointed toward progress in high energy particle physics by the means rather than the end; namely, it has been the accelerator technology which has made it possible to see how much penetration into the unknown we can achieve rather than establishing requirements for specific energies, specific experiments, and then design the tools.

So the high energy accelerator enterprise has been a very successful one in the United States.

The next slide shows a diagram which you will not be able to absorb here in detail, and which will go into the record. For those of you in the Congress who have had experience in watching the performance of high technology, large construction projects, I would like to mention here that all the accelerators which have been built have had an eminently successful record in terms of being built on schedule, within projected specifications, close to cost estimates, and in general as high technology enterprises go, being successful in the sense of responsible management and fiscal performance.

The next slide, please.

The next slide illustrates a very important point I would like to make here. This is a slide whose general format we owe to Dr. Stanley Livingston, from whom you heard during the last panel, known as the Livingston chart. It shows how the energy of accelerators has grown in time.

Now, there are some very important lessons to be learned from this chart. The first one is that this is still an extraordinarily dynamic field. The energy has gone up by a factor of 10 approximately every 7 years.

However, this increase has not been achieved by any one technology. Rather, what has happened is that as any one technology saturated, it came to a point of diminishing returns. One needed new ideas, new inventions to start a new technology.

This succession of new technologies, has resulted in a spectacular growth in attainable particle energies and, therefore, in an ever-increasing penetration into the world of the small to a finer and finer scale.

Equally important is the fact that the cost per unit of energy—gigaelectronvolt or megaelectronvolt—has drastically decreased during this time. If you correct for inflation, then the cost of today's say, 250 gigaelectronvolt machine is perhaps only a factor of 10 larger than the cost of the 4 megaelectronvolt electrostatic generators before the war.

Yet, during this time, the energy has gone up by a factor of 100,000. So therefore we have seen 4 to 5 factors of 10 improvement due to these generations of new technology which have been achieved by this succession of devices. This we have to keep in mind.

Another thing I would like to remind this committee about is that accelerators very rarely have been built for the right reason. In other words, when committees have been convened to project the needs of the future—and I might add that the field of high energy physics probably has been the most planned, reviewed and previewed among all the basic sciences—then such committees have generally pinpointed specific physics needs which would require new advances in accelerator technology.

Such plans, when they were reduced to practice, have indeed produced the results which were predicted. But the results which turned out to give the most profound, insight into the nature of things, were usually different from the ones projected for each installation.

If you look back in history as to what the main reason was for building the cosmotron at Brookhaven, you find its main impact was in a completely different field, although the predicted area was indeed fulfilled.

So, therefore, when elaborating on the need for new advanced facilities, the task should be approached with some humility. One should not presume that the most productive application of new facilities will precisely track the projection.

The field of high energy physics is indeed international, it is worldwide.

The next slide shows a map of the world as seen by the high energy physicist. I have plotted horizontally the attainable center of mass energy of different machines and vertically a number which I cannot explain here in detail.

It is called the effective luminosity, and it measures the data rate, the productivity quantitatively, that is how rapidly for a given reaction which is under investigation data can be produced which the physicist can use to interpret the phenomena.

Now, from this chart you can see a number of things.

First, the heavy entries—the circles and squares in black—are proton machines. The ones in open circles are electron and electron positron machines. The machines which go toward the upper edge of the slide excel by very high intensity; the ones which go over to the right excel by very high attainable energy. These things are complementary.

Let me draw a number of conclusions from this chart, again without going into detail.

First, the accelerator enterprise is a truly international one. Machines of many characteristics have been built and are being built on many continents, including the United States, Western Europe, the Soviet Union, and also now in China.

Second, there is not such a thing as a best machine for all high energy physics applications. The use of electrons and particularly electron-positron collisions offer certain powerful opportunities which cannot be met by machines accelerating protons.

Conversely, proton machines remain in the foreseeable future the means to reach the highest possible energy in relation to cost. Therefore, there simply cannot be a decision made as to what the best and most likely successful frontier is to push.

The above remarks demonstrate that new technology is an essential ingredient to progress in the accelerator arts. From that technology flows progress in high energy and nuclear physics as well as in many other applications which other speakers have described in greater detail.

It is possible to make some judgment as to what fraction of the total high energy physics budget should be dedicated to advancing technology and constructing new facilities. There must be a balance between innovation and exploitation.

In rough terms, exploitation of the new scientific opportunities made possible by a new accelerator requires 10 to 20 years. Accelerator research and development leading to new accelerator technology might take a substantial fraction of a decade before leading to a practical proposal. Subsequently, the lead time between a new initiative for accelerator construction and its fruition as a tool for research tends to be about 10 years.

Another thing to recognize is that operating costs of accelerators, including the research costs, are so high that the original construction cost is duplicated after a few years—the number differs in different machines from about 2 to 5.

If you put all these time scales and economic factors in an overall equation, and if it is a matter of national policy to keep continuity of thrust to maintain viability of this enterprise into the indefinite future, then you conclude from that that approximately 25 percent of the total investment, of the total budget in hand for physics research, should be spent on innovation—that is construction—and that of operating funds, a larger number than has been in the past, perhaps as large as 4 percent, should be spent on accelerator technology research and development.

So accelerator research and development and new construction, therefore, are essential to continue the high productivity of high energy physics research that we have heard so much from other witnesses.

It is also essential to continue the pattern of dramatic unit cost reductions, which is basically the reason why this exponential growth in the past has been possible. Unfortunately, budgetary pressures for the past 10 years tend to discourage just such innovation.

In contrast to the success of high energy physics experimentation, as reflected through novel and exciting scientific results, the financial support of the field has actually decreased.

Next slide, please.

The next slide shows the operating costs of the Nation's high energy physics program as measured in dollars of constant purchasing power, and you see that the operating costs in previous fiscal years have decreased as measured in fiscal year 1981 dollars from a value of about $320 million in 1968 to today's value, which in the President's budget was $242.2 million while the current House appropriation is about $328.2 million minus 2 percent.

Now, as a result of this decrease in funding, and also as a result of the technological evolution cycle which was indicated in my first graph, we have seen a life and death cycle of institutions, and of installations.

At the end of the war there were built seven large facilities which operated at the very frontier of energy. That number is now three supported by DOE and one by NSF. However, each one of these three facilities—the high energy facility at the Brookhaven National Laboratory, Fermilab, and SLAC—are at present both underutilized and undermaintained. That means it is not possible at this level of operating costs to exploit these facilities at capacity.

They are running, depending how you measure such a thing, at some such number as 50 percent or below capacity. Therefore, they are operating in a position of extremely high leverage.

That means a relatively small percentage of increase or decrease in operating costs has a highly disproportionate effect in terms of the total research output of that particular installation.

Now, let me make some comments on the matter to which Dr. Adams will address himself in greater detail; that is, the pattern of European and U.S. facilities as they have been evolving and as they will evolve in the future.

In the past the evolution of the European and U.S. facilities has generally followed parallel paths—both continents have acquired an alternate gradient proton synchrotron operating above 30 gigaelectronvolt energy.

There are also unique facilities on both sides of the ocean. The ISR at CERN is the only proton-proton colliding beam facility now operating, and SLAC in the United States is unique in terms of providing high intensity electron and X-ray beams at the highest energies.

Starting from this fairly symmetric pattern, we now see strong differences developing. Part of these derive from financial reasons and part of these are programmatic. Western Europe is supporting its program at nearly twice the rate as does the United States, as calculated using the official monetary exchange rates, and also as percentage of gross national product.

Thus, corresponding facilities in Europe tend to be considerably better supported, and this in turn has in recent times led to the evolution of considerably better equipment and utilization of the accelerators and their equipment on the European side.

On the programmatic side there is increasing diversification of facilities. This, I believe, is fundamentally a healthy development.

The European program is proceeding, emphasizing electron-positron colliding beam devices beyond the previously parallel developments in the United States and Europe, although I believe I can say that the United States has pioneered in these developments.

CERN is planning to construct a very large electron-positron facility which will extend the accessible energies by that method. Since the cost of such devices scales approximately with the square of the attainable energy, this new facility will be costly, approaching a sum equivalent to $1 billion.

Similarly, the German accelerator laboratory in Hamburg has proposed to add a new electron-positron facility of increased energy, but also intends to add a large proton ring in order to provide tools for the study of electron-proton collisions. In addition, CERN will engage in an unprecedented proton-antiproton colliding beam experiment next year. Such a development will be followed in Fermilab rather close in time.

On the U.S. side, the program is evolving in quite a different direction. Construction has started at Brookhaven for a proton-proton colliding beam facility designed for 400 gigaelectronvolt per beam, using superconducting magnets to provide the necessary magnetic fields.

At Fermilab the "Saver" project has been initiated which aims to introduce a second ring in the tunnel which now houses the 500 gigaelectronvolt proton machine which supports an extensive research program.

This second ring will again be superconducting and therefore can produce the same energy as the current ring at negligible consumption of electric power, a matter close to the concern of this committee.

The new installation also should permit an increase in proton energy to 1,000 gigaelectronvolt. This, in turn, can lead to a 1-Tev proton-antiproton colliding beam facility. Fermilab has proposed a progression of construction activities making use of the superconducting conversion and the consequent higher energy opportunities.

Both the European and American programs involve a balance of opportunities and risk. The opportunities are clear. The European program is intended to extend the eminently successful electron-positron colliding beam technique to higher energy, albeit at high cost.

On the other hand, the U.S. program will reach higher collision energies than those accessible under European plans, and therefore the U.S. program should be better poised for far-reaching discoveries.

However, there are risks on both continents. The success of the European plans depends critically on the attainable data collection rates which are possible with these colliding beam devices, and here the detailed understanding of all the phenomena which limit this rate is still incomplete.

The U.S. facilities programs depend on the successful development, design and construction of many miles of superconducting magnets. This is a very difficult undertaking with very high stakes. Not all problems are solved. Thus, the U.S. high energy program is now highly dependent on the successful solution of such engineering problems.

We are indeed in support of obtaining these solutions. I would recommend greater diversification to reduce the dependence of the entire program on the success of these solutions.

Yet, and let me emphasize this, this risktaking is nothing new. High energy physics instrumentation has always operated at the frontiers of technology. It has been the willingness of high energy accelerator physicists to push beyond the state of the art which has both advanced the energy region accessible to physics experimentation and has also made important contributions in providing new tools for many practical applications.

The pursuit of superconducting technology is no exception. It is characteristic that until relatively recent times a large fraction of the budget for superconductivity experimentation has been carried by the high energy physics program.

Let me summarize. The worldwide program in high energy physics has been one of the great successful adventures of modern man. It has, through new discoveries, profoundly affected man's view of the physical universe every few years.

The U.S. program in high energy physics in the past has been highly productive and has held a position of high prominence, both in terms of generating new insights into the nature of inanimate matter and technological innovation of accelerators.

The American program continues to be productive. However, the position of prominence is being replaced by a partnership in activity with Western Europe.

Going even beyond that, the current funding pattern is threatening even that partnership by producing underutilization and undermaintenance of facilities and leading to instrumentation not fully adequate to match the scientific opportunities offered by the basic accelerator facilities.

Plans pertaining to Europe and the United States are now becoming diversified, and the programs of both continents involve major challenges but also significant risks. From a global perspective I would conclude, however, that the expectation of future fundamental discoveries and technological advances looks very bright indeed.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Panofsky follows:]

TESTIMONY BEFORE THE SUBCOMMITTEE ON ENERGY RESEARCH AND PRODUCTION
OF THE
COMMITTEE ON SCIENCE AND TECHNOLOGY OF THE U. S. HOUSE OF REPRESENTATIVES

JULY 23, 1980

W. K. H. PANOFSKY

I am pleased to be able, together with my colleagues, to give you testimony on the state of accelerator technology in relation to high energy and nuclear physics. I am currently Director of the Stanford Linear Accelerator Center, one of the three large national facilities supporting the high energy physics community under the sponsorship of the Department of Energy. I am testifying as a private citizen who has been involved in accelerator-related work during the entire post-World War II period.

High energy particle physics is the study of the fundamental building blocks of matter and of the forces that act among them. It is the most basic of the sciences that seek to understand the nature of the world around us. This work is important for both cultural and material reasons. On the cultural side, the quest to understand nature is as old as mankind itself, and it is from this growth in understanding that human civilization has developed. In particular, when many decisions have to be made on how to use the forces that nature provides more effectively, while at the same time maintaining the quality of our environment, then we need more knowledge of the physical world. As scientists we have faith that on the average, however fallible individual decisions may be, man will make better decisions on how to live harmoniously with nature as his growing understanding of natural processes comes to replace ignorance.

On the material side, the justification for the pursuit of the most basic of the sciences becomes clear from the lessons of the past. Figure 1 is an attempt to summarize briefly the historical thrust of this scientific work. The present frontier is spear-headed by high energy physics, where we do not yet know what applications may be made of the fundamental knowledge that is being gained (although there has already been a great deal of technological "spin-off" from activities in this field). If we look back to the 1930's we find that the nuclear physicists of that time did not foresee practical applications of the knowledge they sought; their motivation was simply the desire to understand. Today we see that the knowledge gained at that time has resulted in nuclear medicine, nuclear power, nuclear explosives, and other important practical applications. If we go back even further, to the turn of the century, we find that the atomic physicists of the time were again simply trying to understand. Yet what they learned then is today the basis of all of modern chemistry, of electronics and of the properties of materials, to name just a few. The pattern shown in Figure 1 has been characteristic of the thrust of material progress throughout history, and I see no reason to doubt that it will continue in the future.

Large particle accelerators are a means to an end. The "end" I am discussing here today is high energy and nuclear structure physics. Yet it has frequently been the means, that is the accelerators, which have paced the progress which could be made in these fields. In other words, the path to progress in high energy physics has frequently been pointed by advances in the accelerator arts. It has been such progress which has also contributed so much to advances in applications of accelerators to many other fields of endeavor, as you have heard or will hear in related testimony.

The construction of high energy accelerators - and I am including both accelerators and storage rings under this term - has been among the most successful high technology enterprises in the United States. Figure 2 gives a tabulation of the comparison between promise and actual attainment of the accelerator projects sponsored by the U.S. Government. You will agree that, relative to other activities operating on the frontiers of technology, this has indeed been an excellent record. I give credit for this performance in part to the lack of separation between those responsible for accelerator construction and those responsible for their use in high energy physics. Although accelerator design has become a highly sophisticated field, it is still true, as it has been in the past, that many of the advances in accelerator technology have been led by those individuals who have been strongly motivated by using such accelerators for high energy physics research.

The growth of accelerator technology has indeed been dramatic. Figure 3 shows the pattern of this growth in chart form as originally presented by Stanley Livingston. Note that the energy (or equivalent energy in the case of storage rings) of the world's accelerators seems still to exhibit a purely exponential growth: The available energy has grown and continues to grow by about a factor of 10 every 7 years. Moreover, as the energy has grown, the cost of achieving this energy has fallen drastically. Today's largest accelerators such as the Fermilab Proton Synchrotron operating at 500 GeV are, perhaps, if inflation is taken into account, no more than 10 times more costly than the electrostatic accelerators of the 1930's which generated energies of one hundred thousandth of that amount.

These spectacular developments have been paced by a succession of rapidly changing technologies. As any one technology matured, thus leading to diminishing returns for further expansion, new technologies were invented by the physicists involved. Thus this explosive exponential growth which has been continuing from the 1930's until today cannot be attributed to any one technique but rather to a succession of a multiplicity of methods. At the same time these new technologies have infused many ideas and products into other applications and thus the economy.

Although the intended mission of accelerator construction has been to provide advanced tools for elementary particle physics, it should be recognized that from the point of view of research application rarely have accelerators been built for the "right reason." When committees convened by agencies of government or the National Academy of Sciences have assessed the needs for the future of high energy physics - and high energy physics has probably been the most planned, reviewed and previewed among the basic sciences - such committees generally pinpointed specific physics needs which would require new advances in accelerator technology. When such plans were reduced to practice these predicted missions were indeed fulfilled. Yet in most cases the most important discoveries made possible by the new accelerators have not been those for which the new facilities have been explicitly targeted. New discoveries occurred in unforeseen directions. Therefore, when elaborating on the need for new advanced facilities the task should be approached with some humility - one should not presume that the most productive application of new facilities will track the projections.

Let me now shift from these general remarks to describing the present worldwide status of accelerators. Figure 4 gives a map of the world's machines accelerating or storing either electrons or protons at the frontiers of energy. This figure shows the equivalent energy of these machines as well as the "luminosity," which is an index which is a measure of attainable data rate generated by the machine when observing a specific reaction.

A number of observations from this figure are in order:

1. The accelerator enterprise is a truly international one. Machines of many characteristics have been built and are being built on many continents including the U.S., Western Europe, the Soviet Union and also now in China.

2. There is not such a thing as the "best" machine for all high energy physics applications. The use of electrons and particularly electron-positron collisions offer certain powerful opportunities which cannot be met by machines accelerating protons. Conversely, proton machines remain in the foreseeable future the means to produce highest possible energy in relation to cost.

The above remarks demonstrate that new technology is an essential ingredient to progress in the accelerator arts. From that technology flows progress in high energy and nuclear physics as well as in many other applications which other speakers will describe in greater detail.

It is possible to make some judgment as to what fraction of the total high energy physics budget should be dedicated to advancing technology and constructing new facilities. In rough terms exploitation of the new scientific opportunities made possible by a new accelerator requires 10-20 years. Accelerator R&D leading to new accelerator technology might take a substantial fraction of a decade before leading to a practical proposal. Subsequently, the lead time between a new initiative for accelerator construction and its fruition as a tool for research tends to be about 10 years. Operating costs of accelerators are such that for a few years they tend to equal the expense of the original construction. Pulling all these economic and technical realities together, one would conclude that at least roughly a fourth of the expenses dedicated to a viable high energy physics program should be dedicated to new construction, and that a significant fraction of operating costs, say 4%, should be dedicated to accelerator R&D.

Accelerator research and development and new construction endeavors are essential to continue the high productivity of high energy physics research and to provide the means for the dramatic cost reductions in relation to performance which have been so effective during the preceding decades. Unfortunately, budgetary pressures for the past 10 years tend to discourage just such innovation. In contrast to the success of high energy physics experimentation, as reflected through novel and exciting scientific results, the financial support of the field has actually decreased. Figure 5 shows the operating costs of the high energy physics program of DOE (not including that of the National Science Foundation, which is about 10% of the nation's high energy physics program), and Figure 6 shows the total support of the field by DOE and NSF including construction. In contrast to the occasional assertion that support of the field has grown, and that a pause in this growth should be acceptable, these figures make it clear that in fact support in real dollar terms has diminished. Operating costs have shrunk steadily and this has been reflected in the discontinuation of the number of laboratories operating facilities across the nation at the frontiers of high energy physics research.

At the present time there are three high energy particle physics institutions (Brookhaven, Fermilab and SLAC) supported by DOE, and one facility (Cornell University) supported by the National Science Foundation which are still available for use by the total community of high energy particle physicists, based at some 80 institutions in the United States and many more abroad. This compares with a considerably larger number of frontier installations (7) after the war. If this continuing erosion of support is not halted and reversed, then there is danger that the number of institutions and thus the total geographical base, must again shrink - a move which would seriously damage the breadth, productivity and competitiveness of the program.

The total financial support pattern shown in the previous picture also shows a large gap (actually 9 years) in the initiation of new construction activities providing new opportunities for research. As I mentioned above, such a gap is highly undesirable if a program of continuing viability should result.

In the past the evolution of the European and U.S. facilities has generally followed parallel paths: Both continents have acquired an alternating gradient proton synchrotron operating above 25 GeV, a proton synchrotron operating near 400 GeV, and now both Europe and the U.S. have commissioned electron-positron storage rings operating above 30 GeV center-of-mass energy. There are also unique facilities on both sides of the ocean: The ISR at CERN is the only proton-proton colliding beam facility now operating, and SLAC in the United States is unique in terms of providing high intensity electron and X-ray beams at the highest energies.

Starting from this fairly symmetric pattern, we now see strong differences developing. Part of these derive from financial reasons and part of these are programmatic. Western Europe is supporting its program at nearly twice the rate as does the United States, as calculated using the official monetary exchange rates. Thus, corresponding facilities in Europe tend to be considerably better supported, and this in turn has in recent times led to the evolution of considerably better equipment and utilization of the accelerators and their equipment on the European side. It is well known that fiscal problems in the U.S. have dropped the average utilization of accelerators well below the 50% mark. As a result the running time available to experimenters, and presumably research productivity, are extremely sensitive to the level of support: Even relatively small increases or decreases in operating funding produce relatively large changes in output. Moreover, maintenance levels of U.S. accelerators have dropped below reasonably acceptable levels to assure longevity of reliable operation. In other words a relatively modest increase in operating funding (say 10-15%) would generate a disproportionately large increase in return (in terms of research productivity) for the taxpayer's past investment in facilities.

On the programmatic side there is increasing diversification of facilities. The European program is proceeding with electron-positron colliding beam devices beyond the previously parallel developments in the U.S. and Europe. CERN is planning to construct a very large electron-positron facility which will extend the accessible energies by that method. Since the cost of such devices scales approximately with the square of the attainable energy, this new facility will be costly - approaching a sum equivalent to 1 billion dollars. Similarly, the German accelerator laboratory in Hamburg has proposed to add a new electron-positron facility of increased energy, but also intends to add a large proton ring in order to provide tools for the study of electron-proton collisions. In addition, CERN will engage in an unprecedented proton-antiproton colliding beam experiment next year.

On the U.S. side the program is evolving in quite a different direction. Construction has started at Brookhaven for a proton-proton colliding beam facility designed for 400 GeV per beam, using superconducting magnets to provide the necessary magnetic fields. At Fermilab the "Saver" project has been initiated which aims to introduce a second ring in the tunnel which now houses the 500 GeV proton machine which support an extensive research program. This second ring will again be superconducting and therefore can produce the same energy as the current ring at negligible consumption of electric power. The new installation also should permit an increase in proton energy to 1000 GeV (or 1 TeV). This, in turn, can lead to a 1-TeV proton-antiproton colliding beam facility. Fermilab has proposed a progression of construction activities making use of this superconducting conversion and the consequent higher energy opportunities.

Both the European and American programs involve a balance of opportunities and risks. The opportunities are clear: The European program is intended to extend the eminently successful electron-positron colliding beam technique to higher energy, albeit at high cost. Since a large fraction of the discovery, and in particular the detailed exploration of the new quark families, has come from electron-positron colliding beam devices at lower energy, it is predictable that a highly productive future is in store. On the other hand, the U.S. program will reach higher collision energies than those accessible under European plans, and therefore the U.S. program should be better poised for far-reaching discoveries. However, there are risks on both continents. The success of the European plans depends critically on the attainable data collection rates which are possible with these colliding beam devices and here the detailed understanding of all the phenomena which limit this rate is still incomplete. The U.S. facilities programs depend on the successful development, design, and construction of many miles of superconducting magnets. This is a very difficult undertaking with very high stakes. Not all problems are solved. Thus the U.S. high energy program is now highly dependent on the successful solution of such engineering problems and I would recommend greater diversification to reduce that dependence.

Yet this risk-taking is nothing new - high energy physics instrumentation has always operated at the frontiers of technology. It has been the willingness of high energy accelerator physicists to push beyond the state of the art which has both advanced the energy region accessible to physics experimentation and has also made important contributions in providing new tools for many practical applications. The pursuit of superconducting technology is no exception. It is characteristic that until relatively recent times a large fraction of the budget for superconductivity experimentation has been carried by the high energy physics program. Yet once the engineering problems associated with large-scale superconducting systems have been solved, this technology should have far-reaching applications for such diverse items as superconducting motor windings, superconducting transmission lines, and many other activities resulting in energy savings associated with electric systems.

Let me summarize: The worldwide program in high energy physics has been one of the great successful adventures of modern man; it has, through new discoveries, profoundly affected man's view of the physical universe every few years. The United States program in high energy physics in the past has been highly productive and has held a position of high prominence, both in terms of generating new insights into the nature of inanimate matter and technological innovation of accelerators. The American program continues to be productive; however the position of prominence is being replaced by a partnership in activity with Western Europe. Going even beyond that, the current funding pattern is threatening even that partnership by producing underutilization and undermaintenance of facilities and leading to instrumentation not fully adequate to match the scientific opportunities. Plans pertaining to Europe and the U.S. are now becoming diversified, and the programs of both continents involve major challenges but also significant risks. From a global perspective I would conclude, however, that the expectation of future fundamental discoveries and technological advances looks very bright indeed.

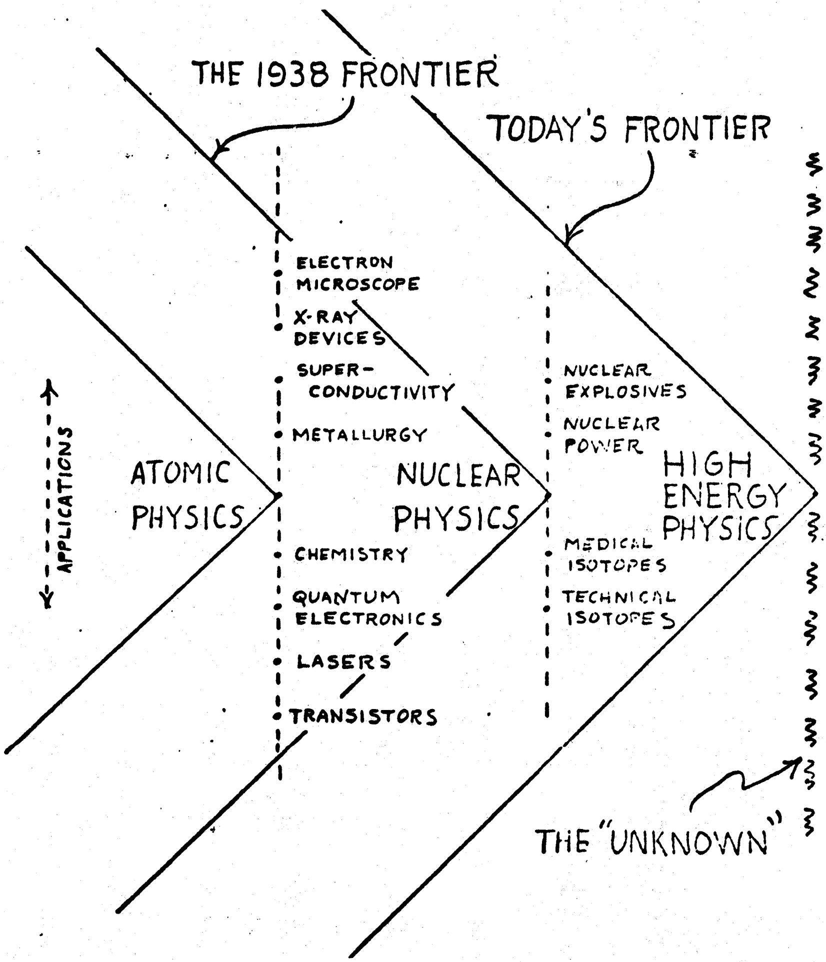

THE 1938 FRONTIER
TODAY'S FRONTIER
ELECTRON MICROSCOPE
X-RAY DEVICES
SUPER-CONDUCTIVITY
METALLURGY
CHEMISTRY
QUANTUM ELECTRONICS
LASERS
TRANSISTORS
NUCLEAR EXPLOSIVES
NUCLEAR POWER
MEDICAL ISOTOPES
TECHNICAL ISOTOPES
APPLICATIONS
ATOMIC PHYSICS
NUCLEAR PHYSICS
HIGH ENERGY PHYSICS
THE "UNKNOWN"

Figure 1

5/28/80

Figure 2

Record of Past HEP Projects

Device	Site	Const. Start	Init. Test	On Sched	Speci-fications	Cost Estimate	Cost in Then Yr $'s
Bevatron	LBL	1949	1954	Yes[1]	Met	Met	10,000,000
Bevatron Improvement	LBL	1960	1964	No	Met	Met	10,000,000
AGS	BNL	1953	1960	Yes	Met	Met[2]	31,000,000
AGS Improvement	BNL	1966	1972	No	Met	Over 3 %	49,000,000
Protron-Synchrotron	FNAL	1969	1972	Yes[3]	Exceeded	Under 3 %	243,000,000
2 Mile Linac	SLAC	1962	1966	Yes	Exceeded	Met	114,000,000
PEP	SLAC	1976	1980	Yes	Met[4]	Met	78,000,000
CEA	Harvard	1957	1962	Yes	Met	Met	10,000,000
PPA	Princton	1956	1963	No	Met	Met	12,000,000
PPA Addition	Princton	1961	1965	Yes	Met	Met	11,000,000
ZGS	ANL	1959[5]	1963	No	Met	Over 9 %	51,000,000
Bubble Chamber & Exper. Area	ANL	1966	1970	Yes	Met	Over 5 %	18,000,000

1. Work interrupted by other higher priority AEC construction.
2. Accounting principle changed during construction.
3. Three months ahead of original schedule.
4. Final performance not yet established.
5. Project rescoped in 1961.

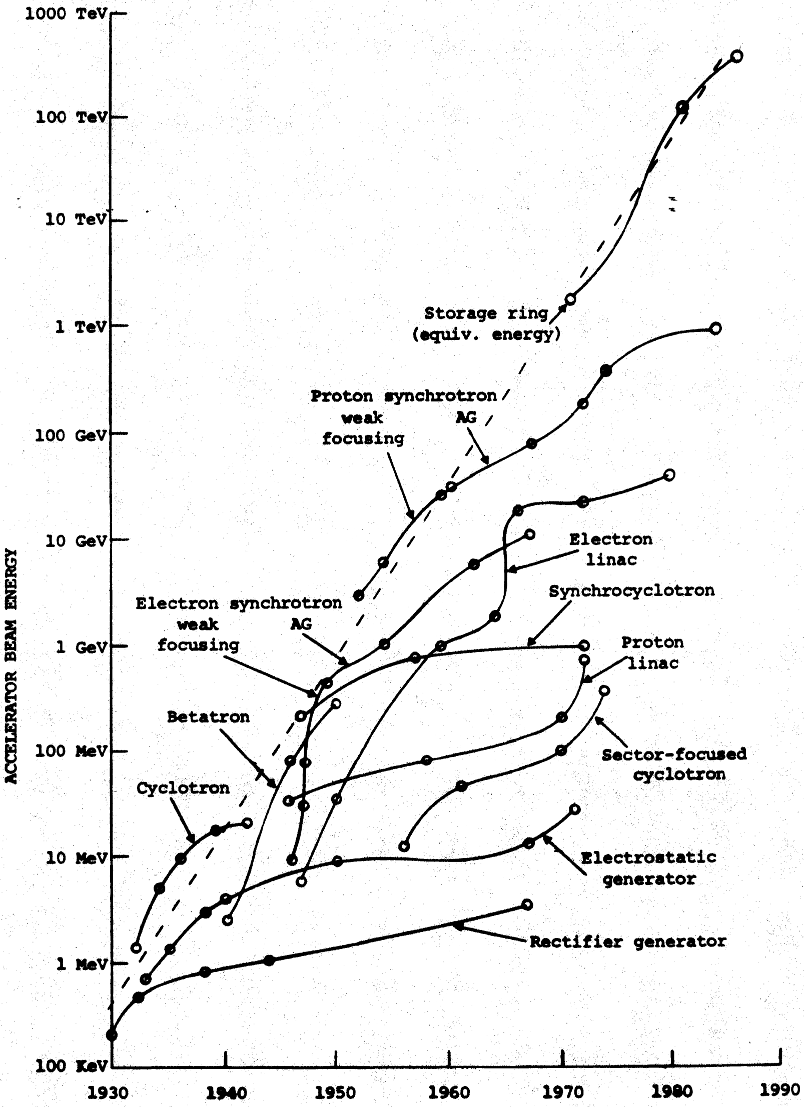
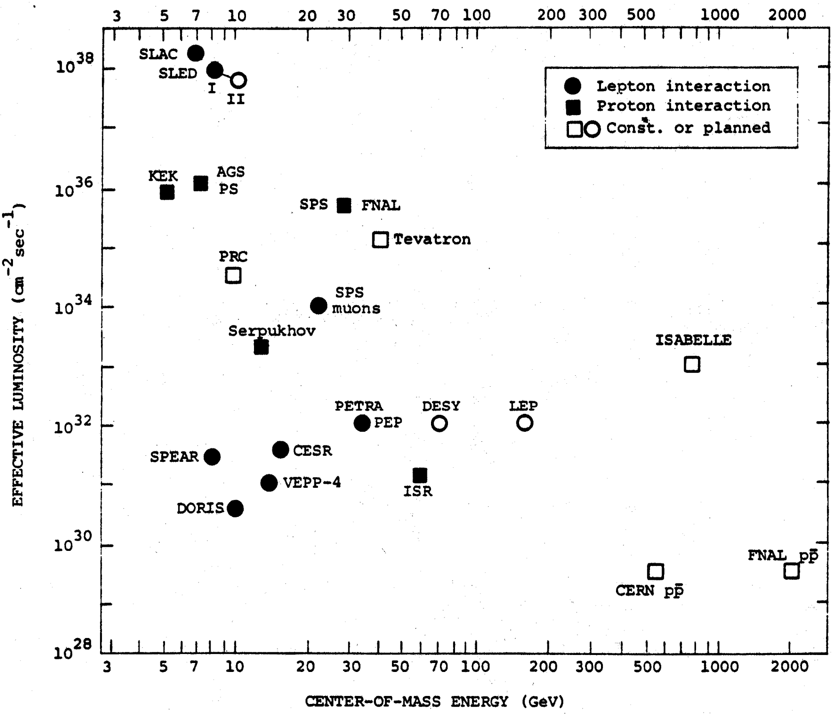


Figure 3

Energy growth of accelerators and storage rings



Effective luminosity vs. center-of-mass energy for the largest accelerators and storage rings now operating, in construction or planned. For accelerators, the target is assumed to be one meter of liquid hydrogen, except in the case of "SPS muons," where 50 meters of LH_2 is assumed.

Figure 4

January 1980

U.S. DOE operations funding in equivalent FY1981 dollars and "life and death" of U.S. facilities.

Figure 5

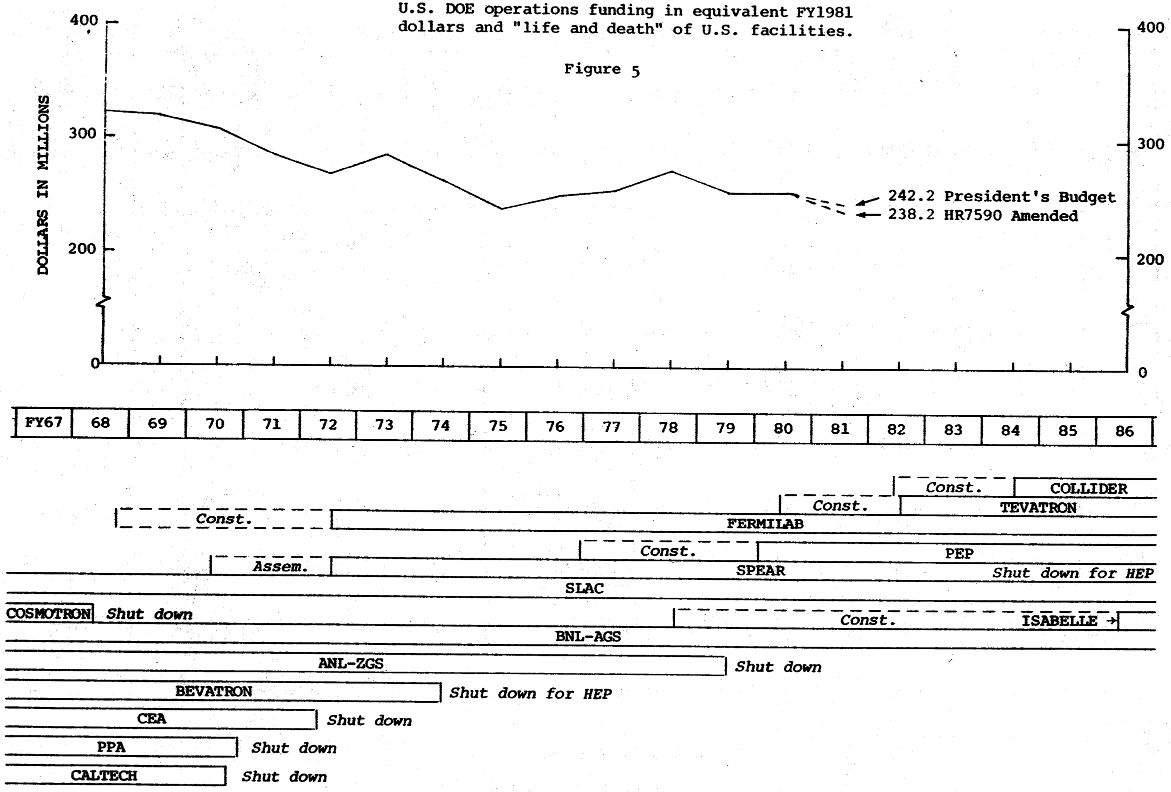
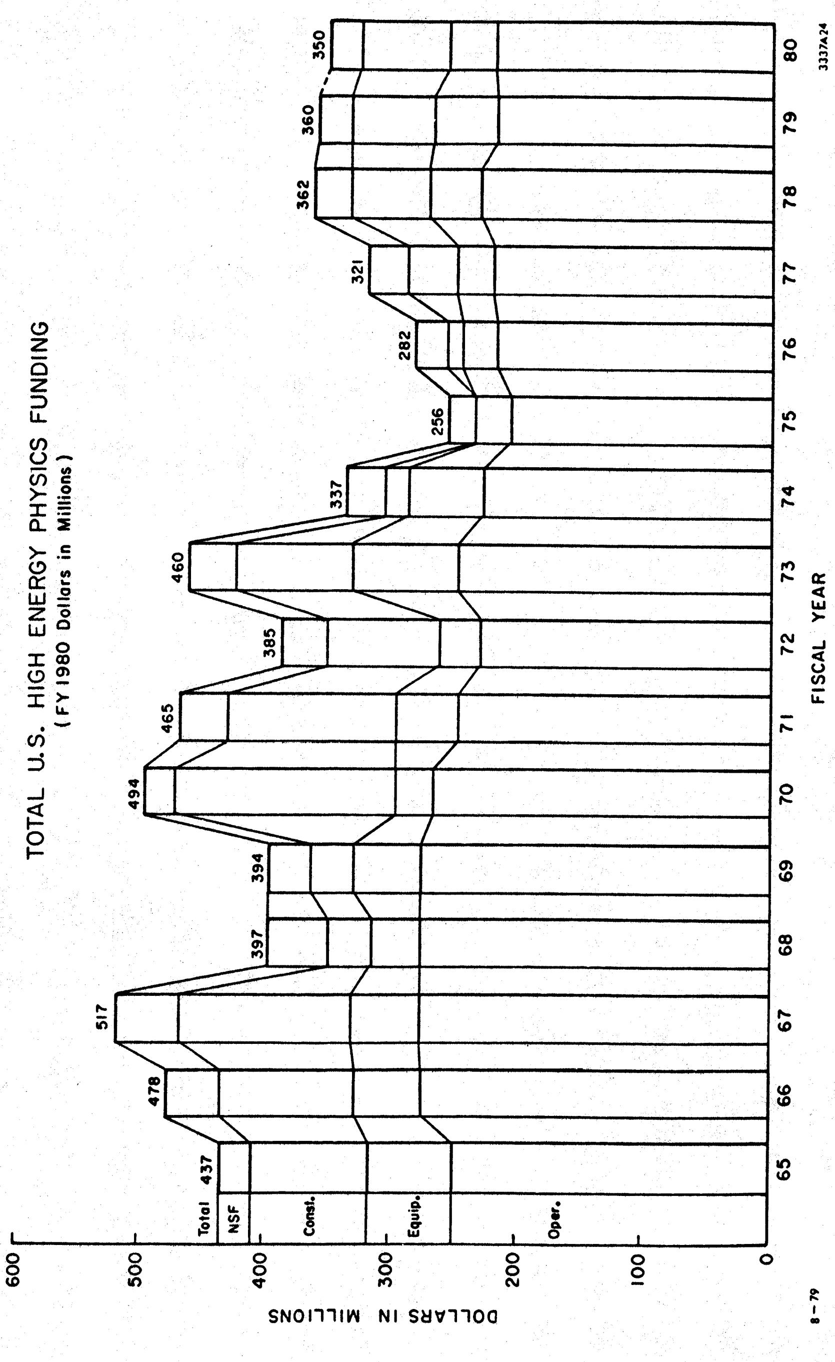


Figure 6

TOTAL U.S. HIGH ENERGY PHYSICS FUNDING
(FY 1980 Dollars in Millions)



Mr. McCORMACK. Thank you, Dr. Panofsky.

Our next witness this morning is Dr. John Adams, Director General of the European Organization for Nuclear Research, otherwise known as CERN, in Geneva.

Again, I want to suggest that you may wish to summarize or informally make your presentations. We are running fairly close to noon. We are liable to be interrupted with various schedules. It is entirely up to you how you make your presentation.

Mr. WYDLER. Before you start, Doctor, where do you get CERN out of European Organization for Nuclear Research?

STATEMENT OF DR. JOHN ADAMS

Dr. ADAMS. I am afraid that goes back to the past. CERN used to stand in the very early days for the Conseil Européen pour la Recherche Nucléaire, and then it was changed to organization, and nobody liked OERN, so we kept the old initials CERN.

Mr. Chairman, it is a great honor for me and the organization which I represent to be asked to testify before you today at this hearing of your subcommittee.

In your letter, you asked me to comment on a number of aspects of high energy physics as they are perceived in Western Europe and in other nations. Before doing so, I would like to make a few explanatory remarks of a historical nature.

Up to the outbreak of the Second World War the physicists in Europe made major contributions to this research, but the destruction caused by that war made it impossible for the individual countries of Europe to finance the laboratories and equipment necessary to pursue this research on a scale comparable with that of the United States.

The choice lay between an inevitable decline of this research in Europe or a joint international effort which hopefully would enable European physicists to contribute once again at the world level.

Fortunately, the desire for collaboration was such in Europe at that time that the second course was adopted and the European organization called CERN was set up in 1954 with its laboratory at Geneva in Switzerland.

I would like on this occasion to pay tribute to those eminent American physicists, particularly Professor Rabi, who played such a key role in the early history of CERN and the hundreds of American physicists who have collaborated with us in this research ever since. The very high standard of this research in the United States has been and still is the inspiration of our work in Europe.

It is interesting to see that other nations in the world have also seen the same importance in this research and when circumstances allowed have built up their own efforts.

The Soviet Union founded a laboratory at Dubna just after the Second World War, which subsequently became the Joint Institute for Nuclear Research, and later on the laboratories at Serpukov and at Novosibirsk.

Japan came in later with its KEK laboratory near Tokyo, and more recently the People's Republic of China has entered the field and is constructing a large accelerator near Peking.

I would now like to turn to the specific questions you have raised.

In the first place, you asked what purpose the European countries see in this research. High energy physics is regarded in Europe, as in the United States as one of the most fundamental researches since it deals with the basic constituents of nature and its discoveries illuminate our understanding of the universe we inhabit.

It is considered that advancing this knowledge by scientific research is an essential activity in Europe and in the long term useful, and the funding allocated to the research reflects this attitude.

Since the research is not oriented toward a particular mission, the funding allocated to it cannot be determined in terms of a specific application nor measured by cost benefit analysis.

Instead, it is usually expressed as a fraction of the wealth of the country pursuing the research and compared with the funds allocated to other fundamental scientific researches.

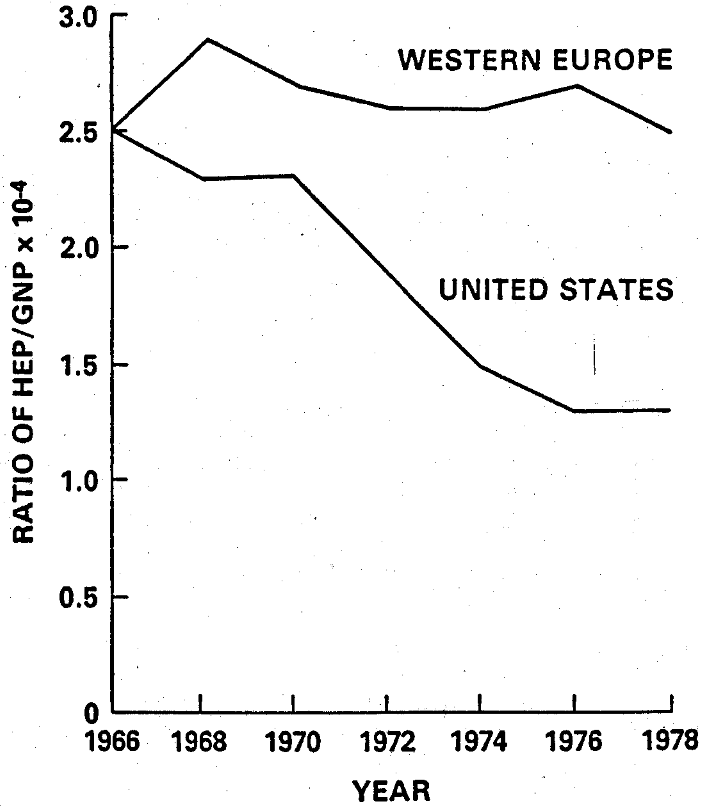
It is interesting to see that back in 1966 the CERN member states and the United States allocated the same fraction of their gross national product to high energy physics, namely 0.025 percent, but whereas the CERN member states have maintained this same fraction through to 1978, which is the last year for which we have official figures, the fraction allocated in the United States has fallen to about half the 1966 value.

I have a curve here which can be viewed. The top line is for the CERN member states and the lower line is for the United States you see they were the same in 1966, and you see the American curve has fallen to about half during this time.

[Graph referred to follows:]

HIGH ENERGY PHYSICS

EXPENDITURES RELATIVE TO GNP



FROM TESTIMONY OF J. ADAMS, CERN DIRECTOR GENERAL BEFORE HOUSE SCIENCE AND TECHNOLOGY SUBCOMMITTEE ON JULY 23, 1980

Mr. McCormack. May we have a copy of that?

Dr. Adams. Yes.

Mr. McCormack. We would very much like to have it for the record.

Dr. Adams. I hope it is correct. It is to the best of our knowledge. DOE can certainly check it.

Mr. McCormack. It delivers the message. Whether it is precise or not is less important than the message delivered.

Dr. Adams. I chose it for that purpose, Mr. Chairman.

Many comparisons have been made between European and American funding of this research, but they all suffer from the difficulty of equating the relative purchasing powers of the American dollar and the European currencies at any given time.

Several ingenious methods have been used to overcome this difficulty, including the notion of an equivalent dollar exchange rate, but I must admit that the results of these comparisons have often generated more heat than light.

The virtue of using the fraction of the gross national product as the basis of comparison is that it is independent of exchange rates and inflation on either side of the Atlantic and the figures I have just given you show a significant divergence.

Although I have no figures for the other nations in the world engaged in this research, I believe that the fraction of the GNP allocated to this research in the Soviet Union has not diminished during the years I have just quoted and that it has increased substantially in Japan. Since it was zero in China until a few years ago, there is no question that it is increasing rapidly in that country.

The financial contributions of the 12 member states of CERN to the cost of the international center at Geneva is also worked out in terms of their relative gross national products. The biggest contributor is Western Germany, which pays 25 percent of the costs, and the smallest is Greece, which pays less than 1 percent. So, the same idea of basing the research funding on the relative wealth of countries is maintained.

Another question you have raised is the relationships seen in Europe between scientific achievements and technical spinoff in this research, and the needs for technical education of students and scientists.

I am hardly competent, nor am I authorized, to speak for all the Member States of CERN on such a complicated issue, so I can only give you my personal views.

Clearly, the expectation is that the quality of the research made possible in Europe by the international research facilities built up at CERN is commensurate with the funding allocated to it.

To judge whether the research results are commensurate with the funding is not a simple matter of adding up the number of Nobel Prizes or the number of physicists involved. It is measured by the quality of the research results published and contributions to major conferences.

On this basis I believe that CERN has enabled the university physicists in Europe to contribute on an equal basis to those in the United States.

Technological spinoff from the research is certainly a recognized feature of this work in Europe, but by no means its motivation. A few years ago an economic study showed that the contracts for equipment placed by CERN did indeed have a considerable effect on European industry.

Over 100 firms were investigated who had manufactured all kinds of technical equipment for CERN, and it was found that for every dollar spent by CERN through its manufacturing contracts with the firms, the firms had subsequently gained $4 in sales of equipment or services due to new technologies, improved manufacturing methods and even organizational changes within the firm directly related to the original CERN contract.

Spinoff it seems is not just a simple matter of non-stick coatings for frying pans but a general improvement in the performance of industry and in its competitiveness brought about by the demanding needs of laboratories like CERN which touch every aspect of the manufacturing process.

With regard to the effects of the research on education, it will be obvious from what I have said that in Europe, as in America, the research is founded in the universities. Physicists who carry out research at CERN are university staff who also teach in their universities. In this way, education and research are directly related.

Now that universities are no longer expanding in Europe, it is very difficult for young physicists to find university posts and hence to follow a career in high energy physics research.

Nevertheless, we find that young physicists are still entering the field in much the same numbers as before, even though they are aware that after a period of research many of them will have to find jobs elsewhere.

The reason seems to be that these young physicists find that the very wide range of applied physics and technology involved in the research gives them a training and experience difficult to find in other subjects and enables them to get attractive jobs in industry later on.

Finally, you have asked me to comment on the world situation of accelerators outside the United States.

In the past there was some duplication of the accelerators in the three regions of the world most active in this research; namely, the United States, the U.S.S.R., and Western Europe.

For example, the 28 gigaelectronvolt proton synchrotron at CERN is very similar to the 30 gigaelectronvolt AGS machine at Brookhaven, and the 400 gigaelectronvolt machines at Fermilab and CERN offer very similar research facilities.

With the decreasing budgets allocated to this research and the increased size of the next generation of accelerators, such duplication is no longer possible.

The three largest accelerators now under construction or nearing approval are of different types, and there will only be one type in each of the three regions.

The Isabelle machine under construction at Brookhaven is a proton-proton collider over ten times bigger in energy than the one we have been operating for over ten years at CERN.

The UNK machine recently approved for the Serpukhov laboratory near Moscow is a 3,000 gigaelectronvolt fixed-target proton accelerator much larger than the 400 gigaelectronvolt machines at Fermilab and at CERN.

The LEP machine, for which approval is sought at CERN, is an electron-positron collider much bigger than the PEP machine at SLAC and the PETRA machine at DESY, Hamburg.

These machines, which will come into operation in the second half of the 1980s, will offer complementary research facilities and the physicists of the three regions seeking the facilities they need for their research will find them in different regions of the world.

How they might gain access to them is a subject which has been discussed during the last year in ICFA, the International Committee for Future Accelerators, which was set up by the International Union of Pure and Applied Physics.

Fortunately, high energy physics is a subject in which there is already a great deal of international collaboration and joint use of the present regional facilities so that this new step would be an extrapolation of existing practices.

Nevertheless, it is a major step and one which could lead naturally to another which would be a single accelerator complex serving all three regions which might be built toward the end of this century. This second step is also being discussed by ICFA under the terms of reference it has been given.

You may feel from all this that high energy physicists are rather optimistic in their planning, especially in view of the present political problems in the world. Yet, when I look back at the time when CERN was set up and at the political situation at the end of the Second World War, I feel our optimism is not misplaced.

In view of the size and cost of the accelerator complexes needed for this research, international collaboration is the only practical way of continuing with this research in the long term.

It may well be that some historian of the future looking back at our period of time may find that the most important spinoff from this research was the pioneering work it did in international understanding and collaboration.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Adams follows:]

DGE 15 July 1980

Testimony of J.B. Adams to the Subcommittee on Energy Research and Production of the United States Committee on Science and Technology

Chairman, Ladies and Gentlemen,

It is a great honour for me and the Organization which I represent to be asked to testify before you today at this hearing of your Subcommittee.

In your letter, you asked me to comment on a number of aspects of high-energy physics as they are perceived in Western Europe and in other nations. Before doing so, I would like to make a few explanatory remarks of an historical nature.

As you are aware, the origins of nuclear physics research date back to the 1930's with the discovery by Rutherford of the atomic nucleus, although the search for the basic constituents of matter and the laws governing their interactions has been a continuous theme throughout the recorded history of the human race starting with the ancient Greeks.

Up to the outbreak of the Second World War the physicists in Europe made major contributions to this research but the destruction caused by that War made it impossible for the individual countries of Europe to finance the laboratories and equipment necessary to pursue this research on a scale comparable with that of the United States. The choice lay between an inevitable decline of this research in Europe or a joint international effort which hopefully would enable European physicists to contribute once again at the world level. Fortunately, the desire for collaboration was such in Europe at that time that the second course was adopted and the European Organization called CERN was set up in 1954 with its Laboratory at Geneva in Switzerland. I would like on this occasion to pay tribute to those eminent American physicists, particularly Professor Rabi, who played such a key rôle in the early history of CERN and the hundreds of American physicists who have collaborated with us in this research ever since. The very high standard of this research in the USA has been and still is the inspiration of our work in Europe.

It is interesting to see that other nations in the world have also seen the same importance in this research and when circumstances allowed have built up their own efforts. The Soviet Union founded a Laboratory at Dubna just after the Second World War, which subsequently became the Joint Institute for Nuclear Research (JINR), and later on the Laboratories at Serpukhov and at Novosibirsk. Japan came in later with its KEK Laboratory near Tokyo and more recently the People's Republic of China has entered the field and is constructing a large accelerator near Peking.

I would like now to turn to the specific questions you have raised.

In the first place you asked what purpose the European countries see in this research. High-energy physics is regarded in Europe, as in the USA, as one of the most fundamental researches since it deals with the basic constituents of nature and its discoveries illuminate our understanding of the universe we inhabit. It is considered that advancing this knowledge by scientific research is an essential activity in Europe and in the long term useful, and the funding allocated to the research reflects this attitude. Since the research is not oriented towards a particular mission, the funding allocated to it cannot be determined in terms of a specific application nor measured by cost benefit analysis. Instead, it is usually expressed as a fraction of the wealth of the country pursuing the research and compared with the funds allocated to other fundamental scientific researches.

It is interesting to see that back in 1966 the CERN Member States and the USA allocated the same fraction of their gross national product to high-energy physics, namely 0.025%, but whereas the CERN Member States have maintained this same fraction through to 1978, which is the last year for which we have official figures, the fraction allocated in the USA has fallen to about half the 1966 value. Many comparisons have been made between European and American funding of this research, but they all suffer from the difficulty of equating the relative purchasing powers of the American dollar and the European currencies at any given time. Several ingenious methods have been used to overcome this difficulty, including the notion of an equivalent dollar exchange rate, but I must admit that the results of these comparisons have often generated more heat than light. The virtue of using the fraction of the Gross National Product as the basis of comparison is that it is independent of exchange rates and inflation on either side of the Atlantic and the figures I have just given you show a significant divergence. Although I have no figures for the other nations in the world engaged in this research, I believe that the fraction of the GNP allocated to this research in the Soviet Union has not diminished during the years I have just quoted and that it has increased substantially in Japan. Since it was zero in China until a few years ago, there is no question that it is increasing rapidly in that country.

The financial contributions of the twelve Member States of CERN to the cost of the international centre at Geneva is also worked out in terms of their relative Gross National Products. The biggest contributor is Western Germany which pays 25% of the costs and the smallest is Greece which pays less than 1%. The percentage contributions of all twelve Member States of CERN are given in the following table.

Contributions of the Member States of CERN

	%
Austria	2.39
Belgium	4.33
Denmark	2.32
France	21.43
Germany (Fed.Rep.)	25.00
Greece	0.36
Italy	12.55
Netherlands	5.62
Norway	1.69
Sweden	4.45
Switzerland	4.07
United Kingdom	15.79

Another question you have raised is the relationships seen in Europe between scientific achievements and technical spin-off in this research, and the needs for technical education of students and scientists. I am hardly competent nor am I authorized to speak for all the Member States of CERN on such a complicated issue, so I can only give you my personal views.

Clearly, the expectation is that the quality of the research made possible in Europe by the international research facilities built up at CERN is commensurate with the funding allocated to it. Perhaps at this point, I should emphasize that the vast majority of the physicists who carry out the research at CERN are not staff members of the Organization but university physicists from its Member States. It is estimated that about 2000 university physicists use the CERN facilities for their research. Of the 3400 CERN staff members, less than 100 are research physicists, the bulk of the staff being engineers, applied physicists, technicians of all kinds and administrators whose job it is to build and operate the accelerators and the larger experiments, operate the central computer facilities and run the Laboratory. To judge whether the research results are commensurate with the funding is not a simple matter of adding up the number of Nobel Prizes or the number of physicists involved. It is measured by the quality of the research results published and contributions to major conferences. On this basis I believe that CERN has enabled the university physicists in Europe to contribute on an equal basis to those in the USA.

Technological spin-off from the research is certainly a recognized feature of this work in Europe, but by no means its motivation. A few years ago, an economic study showed that the contracts for equipment placed by CERN did indeed have a considerable effect on European industry. Over 100 firms were investigated who had manufactured all kinds of technical equipment for CERN and it was found that for every dollar spent by CERN through its manufacturing contracts with the firms, the firms had subsequently gained four dollars in sales of equipment or services due to new technologies, improved manufacturing methods and even organizational changes within the firm directly related to the original CERN contract. Spin-off it seems is not just a simple matter of non-stick coatings for frying pans but a general improvement in the performance of industry and in its competitiveness brought about by the demanding needs of laboratories like CERN which touch every aspect of the manufacturing process.

With regard to the effects of the research on education, it will be obvious from what I have said that in Europe as in America the research is founded in the universities. Physicists who carry out research at CERN are university staff who also teach in their universities. In this way education and research are directly related. Now that universities are no longer expanding in Europe it is very difficult for young physicists to find university posts and hence to follow a career in high-energy physics research. Nevertheless, we find that young physicists are still entering the field in much the same numbers as before, even though they are aware that after a period of research many of them will have to find jobs elsewhere. The reason seems to be that these young physicists find that the very wide range of applied physics and technology involved in the research gives them a training and experience difficult to find in other subjects and enables them to get attractive jobs in industry later on.

Finally, you have asked me to comment on the world situation of accelerators outside the United States. In the past there was some duplication of the accelerators in the three regions of the world most active in this research, namely the USA, the USSR and Western Europe. For example, the 28 GeV proton synchrotron at CERN is very similar to the 30 GeV AGS machine at Brookhaven, and the 400 GeV machines at Fermilab and CERN offer very similar research facilities. With the decreasing budgets allocated to this research and the increased size of the next generation of accelerators, such duplication is no longer possible. The three largest accelerators now under construction or nearing approval are of different types and there will be only one type in each of the three regions. The ISABELLE machine under construction at Brookhaven is a proton-proton collider over ten times bigger in energy than the one we have been operating for over ten years at CERN. The UNK machine recently approved for the Serpukhov Laboratory near Moscow is a 3000 GeV fixed-target proton accelerator much bigger than the 400 GeV machines at Fermilab and at CERN. The LEP machine, for which approval is sought at CERN, is an electron-positron collider much bigger than the PEP machine at SLAC and the PETRA machine at DESY, Hamburg. These machines, which will come into operation in the second half of the 1980's, will offer complementary research facilities and the physicists of the three regions seeking the facilities they need for their research will find them in different regions of the world. How they might gain access to them is a subject which has been discussed during the last year in ICFA, the International Committee for Future Accelerators, which was set up by the International Union for Pure and Applied Physics. Fortunately, high-energy physics is a subject in which there is already a great deal of international collaboration and joint use of the present regional facilities so that this new step would be an extrapolation of existing practices. Nevertheless, it is a major step and one which could lead naturally to another which would be a single accelerator complex serving all three regions which might be built towards the end of this century. This second step is also being discussed by ICFA under the terms of reference it has been given.

You may feel from all this that high-energy physicists are rather optimistic in their planning, especially in view of the present political problems in the world. Yet when I look back to the time when CERN was set up and at the political situation at the end of the Second World War, I feel our optimism is not misplaced. In view of the size and cost of the accelerator complexes needed for this research, international collaboration is the only practical way of continuing with this research in the long term. And it may well be that some historian of the future looking back at our period of time may find that the most important spin-off from this research was the pioneering work it did in international understanding and collaboration.

Mr. McCORMACK. Thank you, Dr. Adams, for a fascinating statement. Very much appreciated.

I have already asked the staff to take advantage of the lead you have given us in that last slide and expand it to cover other major areas of the world.

The last witness on the panel this morning is Dr. N. Douglas Pewitt, Deputy Director of Energy Research in the Department of Energy.

Dr. Pewitt, welcome. We have your testimony. You may proceed as you wish.

STATEMENT OF DR. N. DOUGLAS PEWITT

Dr. PEWITT. Thank you for the opportunity to be here this morning. I will try to be very brief.

Within the Department of Energy, the Office of Energy Research is responsible for programs that provide support for approximately 90 percent of the national effort in high energy physics and nuclear physics research.

I currently serve as the Deputy Director of that Office. My perspective on these programs is somewhat different in that it is shaped not only by my current responsibilities in the Department, but by graduate education and research in high energy physics as well as a period as Chief of Science and Space Programs within the Office of Management and Budget.

Because of those different perspectives, I am keenly aware of the importance of accelerator progress to our continuing search for knowledge and for the need to plan and support programs over a sufficiently long period of time to permit us to sustain researchers and facilities for the time necessary to get results. I am also aware of the difficulties that you face in making appropriate budget tradeoffs between a number of programs, each important in its own way.

In the Department, the programs of high energy and nuclear physics differ in a fundamental way from other DOE basic research efforts. Although they may in the future lead to entirely new sources of energy in the future the high energy and nuclear physics programs do not support the energy programs of the Department directly. They do, however, play a crucial role in man's basic understanding of matter and energy. For these reasons, the Department supports these programs as a national trust.

The success of these programs depends critically on an understanding of the unique character of these programs and the willingness of both executive branch and congressional leaders to allocate funds based on that understanding.

I will not review for you the nature of the programs this morning from our perspective. I think that would be somewhat redundant. To save time, I think that I should just say that we are in a very exciting period in both high energy and nuclear physics in this country now. We are building on substantial successes in the recent past, and we are looking forward in the very near future to even more exciting developments in these fields.

However, these challenges continue to require additional costly and more sophisticated facilities to pursue these opportunities. At the same time, existing facilities have to be modernized or discontinued. With increasing demands for public dollars in every sector, there is growing concern about the budget tradeoffs between fundamental long-term research in high energy and nuclear physics, and other programs which offer more immediate payoffs, whether they be in energy or in other sectors.

In the past, these tradeoffs between fields of science produced budgets that varied greatly from year to year. This lack of funding stability was especially damaging because laboratory and industry contractors who carry out the programs are not-for-profit enterprises. As such, they do not have resources to conduct research without Federal support, and cannot maintain institutional capabilities during periods of reduced Federal funding.

The variable yearly budgets resulted in the past in periods of little or no new construction of accelerators or experimental facilities and had an especially detrimental effect on maintaining the core of very specialized talent. This instability was extremely damaging to the field and, more importantly, reduced our ability to mount major efforts when projects were approved.

In an effort to address this problem, we developed a long-range plan for the high energy physics program which would help to maintain world leadership. The present long-range plan is meant to internalize the budget tradeoffs within a particular field of science and avoid the yearly budget competition which had previously made long-range planning impossible. DOE and OMB have essentially agreed to a necessary level of funding which will be escalated yearly to reflect the effects of inflation.

We believe that the 1981 budget that was submitted to the Congress represents a reasonable adherence to that planning base. The stability inherent in the plan allows us to carry on an orderly phase-in of new facilities while at the same time conducting a program that is well-planned in advance, productive and at the forefront of research. It allows us to plan manpower requirements in advance and provides for new facilities required to sustain research progress.

The plan provides for operation of a three major laboratory system—the Brookhaven National Laboratory in New York, the Fermi National Accelerator Laboratory in Illinois, and the Stanford Linear Accelerator Center in California. These accelerator centers provide a unique set of geographically dispersed capabilities complementary to each other and will remain the mainstay of the high energy physics program.

The near 50-percent rate of accelerator utilization provided for in the plan, although clearly not what we would like, represents a tradeoff that was determined after detailed considerations of the balance within this total budget. It was developed in conjunction with the high energy physics community through the High Energy Physics Advisory Panel.

Like the high energy physics program, the nuclear physics program requires construction of large and complex facilities and operations of those facilities. Here, too, long-range planning and stable funding levels are necessary. Together with the NSF and the Department of Energy the Nuclear Science Advisory Committee has developed such long-range planning for basic nuclear research. The plan recognizes the central importance of accelerator facilities and related instrumentation. We are currently examining means to implement this plan, which would provide for construction, necessary equipment and funds for facility upgrading and for effective facility utilization.

Both the high energy physics plan and the nuclear physics plans, of course, will require periodic reassessment. We do this routinely through our advisory committees.

Absent the need to restrain Federal spending, scientific and technological needs would be our only guidelines for the programs. However, funding constraints are a reality we have to live with. There never will be enough funds to pursue every potentially fruitful line of research. That fact notwithstanding, I think some light can be shed on the funding difficulties which may lie ahead for the Nation's high energy and nuclear physics efforts.

Over the last 30 years, government spending, Federal, State and local, has grown significantly as a fraction of total output of the economy, from 24 percent of the GNP in 1950 to 35 percent by 1979. Federal spending was up from 16 to 22 percent over that same period. This has led to the current efforts to constrain Federal spending.

However, a growing fraction of Federal spending is relatively uncontrollable for items such as social security, medical care, interest on Federal debt, and payments on prior year obligations.

The balance, or controllable portion, of Federal outlays consists of discretionary programs, with defense being the major single item. However, controllable defense spending represents in 1980 only 14 percent of the total Federal spending. This is likely to grow as a fraction of Federal spending, as are the uncontrollable items, because of the changing demographics of our society. The balance of Federal spending, in other words, the nondefense controllable portion is only 11 percent, and is divided among such important areas as environment, health, education, agriculture, veterans benefits, energy, general science and space.

With constraints on total Federal spending a likely reality in the future, and the control of that spending important to control of inflation and the private sector's ability to form capital for productivity improvements which are important to maintaining our competitive position in the world markets, the outlook for the controllable programs is not good.

In the face of these trends and projections, the budget future for high energy and nuclear physics becomes a matter of legitimate concern. It is extremely difficult to balance the long-range benefits to be gained from basic research against the short-term, more readily definable needs in such areas as education, environment and energy. However, we think a persuasive case for stable or even increasing support of these programs can be made, justified by scientific and technological progress, even in the face of these competing demands.

As I mentioned earlier, we have taken steps to put high energy and nuclear physics programs on a stable planning basis. This stability should allow the programs to respond to new scientific and technological developments and to maintain our position as a world leader. It is an exciting time in the fields of high energy and nuclear physics. The challenges and the benefits are great. I am optimistic that we will be able to respond to these challenges. At the same time, we must recognize that there will be growing legitimate competition for the decreasing percentage of the Federal budget that is controllable. It is not yet clear what the ultimate effects of that competition on these programs will be if this enterprise, which is an important national undertaking, is to continue. The members of this committee will be key to that survival.

[The prepared statement of Dr. Pewitt follows:]

Testimony of N. Douglas Pewitt
Deputy Director
Office of Energy Research
Department of Energy
Before the
Subcommittee on Energy Research & Production
of the
House Committee on Science & Technology
July 23, 1980

Mr. Chairman and members of the Subcommittee, it is a pleasure to appear here today to discuss the High Energy Physics and Nuclear Physics programs of the Department of Energy, and the importance of accelerators to those and other programs.

Within the Department, the Office of Energy Research is responsible for Federal programs supporting approximately 90% of the national effort in high energy and nuclear physics research. I currently serve as Deputy Director of that Office. My perspective on these programs is, I believe, a broad one. It has been shaped not only by my current responsibilities, but also by a graduate education and research experience in high energy physics, and a stint as Chief of Science and Space Programs at the Office of Management and Budget. In that capacity, I was responsible for review of the research programs of the National Aeronautics & Space Administration, National Science Foundation, and the Department of Energy and for overall analysis of Federally-sponsored research and development. Because of the different perspectives resulting from such diverse responsibilities, I am keenly aware of the importance of accelerator progress to our continuing search for knowledge and of the need to plan and support programs that are sufficiently stable to permit us to sustain researchers and facilities over the time necessary to obtain results. I am also well aware of how difficult it can be to make appropriate budget trade-offs among a number of programs, each important in its own way. Today I would like to touch briefly on the current status of high energy and nuclear physics research, and then discuss the particle accelerators without which research in these areas simply cannot be conducted. Finally, I would like to describe some rather disturbing trends that may have major impacts on these fields of research.

In the Department of Energy, the High Energy and Nuclear Physics programs differ in a fundamental way from the roles of the other DOE basic research efforts. While the High Energy and Nuclear Physics programs do not directly support the Department's energy technology programs, they do play a crucial role in man's basic understanding of matter and energy. It should be noted, however, that development of such an understanding could conceivably lead to entirely new sources of energy sometime in the future. They also provide fundamental knowledge that can be used to meet the longer range needs of society. For these reasons, the Department supports these programs as a National Trust. In fact, the future success of the High Energy and Physics programs rests, in large part on the Executive Branch's and the Congress' understanding of their unique character and on their willingness and ability to allocate funds based on that understanding.

The High Energy Physics program includes experimental studies and theoretical analyses aimed at providing new insights into the constituents and structure of matter, the nature of the four known fundamental forces of nature, and the relationships among these forces. The goal of the Nuclear Physics program is to increase our understanding of the interactions, properties and structure of nuclei. As you are well aware, the principal experimental method of high energy and nuclear physics is to observe the results of violent collisions between nuclear and subnuclear particles. By studying these collisions and the properties of new particles created in them, we get valuable information about the constituents and forces governing all of the subnuclear particles involved. This information, when compared with theoretical predictions, hopefully advances our understanding. Theory sometimes points the way; at other times it has to be revised or refined to fit experimental observations.

The particle probes essential for these experiments are produced in intense and highly energetic beams by very large particle accelerators constructed and operated for the Federal government by university and national laboratory contractors. These research facilities are some of the largest and most complex research instruments ever built. To meet the demanding needs of the experimental research program, these facilities must continue to employ the most modern technology available. Continued development of the highly sophisticated research apparatus--the accelerators, colliding beam systems, and particle detection facilities--is essential. Without it, we simply will not have the tools needed to advance our knowledge in these fields.

This research, and the technology development which supports it, have a very close correlation. Research needs determine what new experimental capabilities are required. These needs challenge the technology effort to provide ever more advanced capabilities. At the same time, the existence of these new and improved capabilities stimulates the research program to attempt previously impossible experiments. Thus, there is a continuing challenge between the research and technology which is synergistic, and drives both to ever greater achievements.

In addition to increased scientific knowledge, this vigorous development of advanced technology has produced numerous and other specific spinoffs. The application of accelerators and accelerator-based technology within the Department is widespread. For example, accelerators are extensively used in the magnetic fusion program for neutral beam heating, for obtaining relevant nuclear cross-section data, for beam-foil spectroscopy experiments to obtain data required for analysis of impurities in fusion plasmas, and as a source of neutrons and other particles to study materials damage in future fusion reactors. They are used in materials research studies in development of synchrotron light sources, pulsed spallation neutron sources, and for accelerated materials damage studies with charged-particle beams. Use of accelerators has allowed development of extremely sensitive techniques of isotopic impurity detection important in scientific dating and in study of possible ground water contamination from potential nuclear waste management sites. Accelerators operated by the High Energy and Nuclear Physics programs are being used in clinical trials of promising new modalities for cancer therapy using heavy-ion, neutron, and pi-meson beams. In direct applications in energy technologies, ion accelerators are considered as promising drivers for inertial-ion fusion and for tritium and nuclear fuel breading.

A large fraction of the basic accelerator technology for these uses within the Department and for other applications throughout the nation have been developed with the support of the Department's High Energy and Nuclear Physics programs. The accelerator scientists and engineers in the universities and national laboratories supported by these programs have provided the trained scientific cadre required to exploit these applications. They represent a critical manpower resource of great value to many of the Department's programs.

The High Energy Physics program is currently in a period of great achievement. The most outstanding recent accomplishment has been experimental and theoretical progress toward a unified understanding of the strong, weak, and electromagnetic forces. Experimental discoveries during the past year have confirmed the new unified understanding of the forces. We are well on the say to unifying the fundamental forces of nature, a major task of 20th century physics.

In High Energy Physics, there is a strong sentiment that we are on the right track to deeper understanding of matter and energy. Our current goal is further progress toward testing the new theoretical predictions and making new discoveries which will either reinforce or require modifications of present concepts of matter and energy. In particular, the new experimental programs at the Stanford

Linear Accelerator Center, Fermilab, and the Brookhaven National Laboratory will be searching out new forms of matter, looking for new phenomena associated with the unification of the weak and electromagnetic forces, investigating the mechanism by which those elusive particles we call "quarks" are held together, and ascertaining the membership of the quark family.

As in the High Energy Physics program, the goal of the Nuclear Physics program is to increase our understanding of energy and matter. Currently, we know a great deal about the static properties of stable nuclei found in the earth and those unstable nuclei which differ from stable nuclei by the addition or removal of only a few protons or neutrons. The frontiers of nuclear physics now lie in exploration of the internal dynamics of nuclei and investigation of the response of nuclear matter to extreme conditions. With the new pi meson, electron, and heavy ion beams now available and those being developed, we are very optimistic that rapid strides in this area of science are being made and we are excited about getting on with the work.

New and more difficult challenges continue to arise in both high energy and nuclear physics. These challenges, in turn, require additional costly and more sophisticated facilities. At the same time existing facilities should either be modernized or discontinued. With increasing demands for public dollars in every sector, there has been growing concern about the budget trade-offs between fundamental long-term research in high energy and nuclear physics and other programs which offer more immediate payoffs--whether they be in energy or other sectors. In the past, these trade-offs between fields of science produced budgets that varied greatly from year to year. This lack of funding stability was especially damaging because the laboratory and industry contractors who carry out the programs are not-for-profit enterprises As such, they do not have the resources to conduct the research without Federal support. The variable yearly budgets resulted not only in periods of little or no construction of accelerators or experimental facilities vital to research progress in high energy and nuclear physics, but also had a significant detrimental effect on highly specialized contractor core personnel. In other words, this instability was extremely damaging to the orderly progress of the research and the ability of the programs to mount the major efforts required when projects were approved.

In an effort to address this problem, we developed a long-range plan for high energy physics which would help to maintain U.S. world leadership by providing a budget floor for planning purposes. The present long-range plan is an attempt to internalize the budget trade-offs within a particular field of science to avoid the yearly budget competition which, previously, had made long-range planning impossible. Now OMB and DOE have essentially agreed on a necessary level of base funding over a five-year period, which will be escalated in accord with inflation. That base for the Department of Energy High Energy Physics program is $300 million in FY 1979 dollars. The FY 1981 amended budget request represents the third year of what we view as reasonable adherence by the Department and the Administration to the DOE portion of the plan.

The stability inherent in this plan allows us to carry out an orderly phase-in of needed new facilities while, at the same time, conducting an on-going high energy physics program that is well planned in advance, productive, and at the forefront of research. It allows us to plan manpower requirements in advance, and provides for new facilities with the advanced capabilities required to sustain research progress. The plan also provides for operation by the Department of a three major laboratory system which is matched to the planned research. This consists of Brookhaven National Laboratory in New York, Fermi National Accelerator Laboratory in Illinois, and the Stanford Linear Accelerator Center in California. These accelerator centers provide a unique set of capabilities complementary to each other, which is a vital element of a comprehensive national research program. We expect that these facilities will continue to be the mainstay of the High Energy Physics program.

The near constant 50% rate of accelerator utilization provided for in the plan was determined after detailed consideration of the appropriate balance which must be achieved among many competing high priority needs, including adequate progress on the critically important new facilities under construction, the associated R&D needs, and the needs of university research programs. These considerations were assessed in conjunction with the high energy physics community and, in particular, the High Energy Physics Advisory Panel. This 50% level constitutes the best current overall balance for both short-and long-term needs of the U.S. High Energy Physics program. While it does limit the quantitive research output, we are confident that the highest priority research will be accomplished.

Like the High Energy Physics program, the Nuclear Physics program also requires construction and operation of large and complex accelerator facilities and detectors, with attendant long-lead times and commitments. Here too, long-range planning and stable funding levels are necessities. Working with the Department and the National Science Foundation, the Nuclear Science Advisory Committee has developed a long-range national plan for basic nuclear research. The plan recognizes the central importance of accelerator facilities and related instrumentation. It calls for a balanced mixture of new facility construction, upgrading of existing facilities and facility utilization. We are currently examining means to implement this plan which will provide an adequate level of funding necessary for the construction and capital equipment portions of the Nuclear Physics program, while accommodating the need to upgrade existing facilities and utilize the facilities effectively. Such a plan, if approved by the Department and agreed upon by the Executive Branch and Congress, would allow for a balanced, national nuclear physics program capable of producing fruitful research and maintaining our position as a world leader in this field.

Clearly the High Energy Physics plan, and the Nuclear Physics plan--once developed-- will need to be reassessed periodically to assure that all aspects of the programs are appropriately balanced to reflect new scientific and technological developments both nationally and internationally.

Absent the need to restrain Federal spending, scientific and technological needs would be the only guidelines for program plans. In fact, we fully recognize that funding constraints have, and will continue to have, significant impact on all aspects of these and other Federal programs.

Funding constraints are a fact of life for all Federal programs. As with most family budgets, there are seldom enough dollars to do everything we would like to do exactly the way we would like to do it. That truism notwithstanding,

68-818 O - 81 - 10

I think some light can be shed on the funding difficulties which may lie ahead for the Nation's High Energy and Nuclear Physics Programs.

Over the last 30 years, government programs at the Federal, State, and local level have represented a growing portion of the total output of the economy. In 1950, total government outlays comprised 24% of the Gross National Product (GNP); by 1979 that figure had risen to 35%. During that period, Federal outlays alone rose from 16% to 22% of the GNP in 1979.

Of those total Federal outlays in 1980, 58% are for the so-called "uncontrollable" and fixed cost items such as Social Security, retirement, and interest on the Federal debt. The demographics of our society will tend to increase these over the next several years. Another 17% are for prior year obligations. Thus, of the total Federal outlays in 1980, 75% are essentially uncontrollable and will tend to increase as a fraction of total Federal spending.

This projection is simply a continuation of current trends. We have seen the controllable portion of the Federal budget decrease from 35% in 1971 to 25% today. Controllable outlays for defense in 1980 amount to only 14% of the total, a fraction that also must increase if the U.S. is to have an appropriate defense capability. The balance of Federal outlays, 11%, is divided among such important non-defense areas as international affairs, natural resources and the environment, health, education, agriculture, administration of justice, general government, veterans services, transportation, energy, general science, and space. With constraints on total Federal spending important to the control of inflation and to the private sector's ability to form the capital necessary for productivity improvements aimed at maintaining our competitive position in world markets, the outlook for these latter controllable programs is not good.

In the face of these historical trends and projections, the budget future for the High Energy and Nuclear Physics programs becomes a matter of legitimate concern. It is an extremely difficult task to balance the long-range benefits to be gained from basic research against short-term, more readily definable needs in such areas as environment, education, and energy. However, we think a persuasive case for stable or increasing budgets for these programs--justified by scientific and technological progress--can be made even in the face of competing demands for the decreasing portion of the Federal budget that is still within our control.

As I mentioned earlier, we have taken and are taking steps to put the High Energy and Nuclear Physics programs on a stable planning and funding basis. This stability will allow the programs to respond to new scientific and technological developments and to maintain our position as a world leader. This is an exciting time in the fields of high energy and nuclear physics; the challenges and potential benefits are many. I am optimistic that we will be able to respond to those challenges. At the same time, we must recognize that there will be growing, legitimate competition for the decreasing percentage of the Federal budget that is controllable. It is not yet clear what the ultimate effects of that competition on the High Energy and Nuclear Physics programs will be. But if this enterprise--which is an important national undertaking--is to continue, the members of this Committee are key to its survival.

Mr. McCormack. Thanks very much, Dr. Pewitt.

You talk about the long-range plan. Have you submitted this long-range plan to the Congress? Has it been submitted to this committee?

Dr. Pewitt. I was in another part of Government when the plan was developed. I don't know whether it actually was submitted. OMB did not submit it at that time.

Mr. McCormack. Is anybody here that knows?

Let me suggest that it may be appropriate for you to consider entering into an informal discussion with us on this, which may result in some formal correspondence. It strikes me that one way to handle this whole business of funding major long-range research projects, to try to protect them from the vagaries of political atmosphere that change from year to year, is to set out a continuous 10-year program, and to make it optimistic.

It seems to me what you should be doing is working with the various professionals in the community, laying out what you think is a plan in real dollars, so you don't have to try to worry about what inflation will be, but put it in 1980 dollars and let the inflation rate take care of itself each year.

Make a projection for what the funding levels would be in a responsible program, not only at the three major laboratories as one category, but also for the universities as another, and maybe a miscellaneous category in addition. You would have some sort of a standard against which all budget requests and authorization appropriation could be compared.

I think it is obvious that remedial action is needed in the near future in this area. I think clearly we must be realistic as to what we can do and clearly have to consider the possibility of more complete international cooperation as we go into the next generation of machines.

I am struck by the fact that the administration request for this year, for fiscal 1981, for any high energy physics, was only 8 percent in real dollars over the actual appropriation level for 1980, which means it was a decrease when we count inflated dollars.

Nuclear physics was only 5 percent in real dollars. We would up appropriating, as far as the House is concerned, slightly less than that in each case, so that in each instance we have an actual reduction in funding for fiscal 1981.

I think some of this is a lack of courage, not on the part of anybody in particular, but in general. We are talking about two-tenths of 1 percent of the defense budget, I think it is important for us to remember that is what we are talking about—two-tenths of 1 percent of the defense budget.

Dr. PEWITT. If I may, this is somewhat reflective of the difficulty our institutions are having dealing with a highly inflationary economy. Projections are never that good. We have attempted and we are in the process this year of trying to do retrospective correction, so that the cumulative effect of underestimation does not eat us alive over a period of time. It is difficult to project inflation.

Mr. McCORMACK. I would ignore inflation and project in real dollars and let each year calculate the inflation effect. I think if I may steal an expression from Glenn Seborg, let us establish some islands of stability in all this confusion, and maybe we can keep the program going.

Dr. PEWITT. We will be happy to work with the staff.

Mr. McCORMACK. Thank you.

Mr. Wydler.

Mr. WYDLER. There is a little danger in what you are saying, Mr. Chairman. May I point out one problem. It is part of the whole problem we have here.

Just adding the inflation rate may not solve the problem. For example, to be parochial about it, I just was discussing with the people from Brookhaven problems on Long Island.

We happen to have a situation on Long Island where our electric power is generated almost exclusively by burning oil. The cost of oil has escalated way beyond the inflationary rates. The cost of the electrical power for the facility at Brookhaven is escalating way beyond the inflationary rates.

The result of that is that if you just added inflation and gave Brookhaven the difference and so on, they would still be hard-pressed to keep their machinery going. As a matter of fact, they have had to shut it down for a considerable part of the year this year, and I understand they are looking at a situation next year that will be even worse.

We find ourselves in this most ridiculous position that we cannot utilize our current facilities because we haven't got enough money, and we are building that expensive and new facility called Isabelle out there.

It is kind of illogical. I suppose history will work this out in some way, but that is the problem. It is a real one. The inflationary rate may not be the whole answer.

I just happen to know about that one because it is hitting us, because we happen to be oil burners in our electrical power, which is a stupid position to be in, but one which, you know, is symptomatic of what we go through in this country.

We don't want nuclear for one reason, we don't want coal for another reason, and you end up burning oil, and apparently we are stuck with that for a long time to come. We have to face the realities that exist there as a result of that.

Dr. PEWITT. Two points. No. 1, the plan that we developed was not a robust plan. It was a minimum plan, and for various reasons, including inflation, we have not kept up with the impacts of increased costs for conducting these programs.

With regard to new facilities versus operating current facilities, there has to be a tradeoff. This is a field that depends fundamentally on increased new capability. We cannot give away our future in this field to operate current facilities at levels we would like. It is a hard tradeoff. It is a tradeoff that is just part of the reality within which we have to manage this program. We are not happy about the utilization situation. However, I think the tradeoffs are properly balanced now.

Mr. MCCORMACK. Dr. Pewitt, I don't want to lecture anybody, but I do want to say quotations immediately came to mind as you were speaking. The first one is, "Make no little plans; they lack the fire to stir men's blood." The second one is, "Where there is no vision, the people perish."

I simply suggest to you that what you really should be doing is being a strong advocate for the program within the Department, and that if we fall, it should not be because you, in your role, have not come out with the strongest possible program. If we fail, let it be with the OMB or the Appropriations Committee.

I think we should be moving forward on the most aggressive program that the technology and the state of the art and the need for information calls for.

Dr. PEWITT. There are several ways to be a strong advocate within the Department. Not all of those ways are so public.

Mr. WYDLER. I think the doctor is an expert in OMB, having served with that group, and probably knows that whatever he sends to them is going to be cut. I am sure he realizes that. That is the nature of OMB. That is how they justify their existence, cutting budget. So, I presume he understands that.

Dr. PEWITT. I understand the nature of OMB.

Mr. MCCORMACK. Mr. Carney?

Mr. CARNEY. Thank you, Mr. Chairman.

I would like to take this opportunity to thank Dr. Adams for coming here and contributing so much to these hearings. They were most informative from my standpoint.

Certainly that slide was received very graciously by this subcommittee, which I happen not to be a member of, but I am a member of the full committee. I am sure the other members of the full committee would be very concerned with the amount of our gross national product that we are spending in research as compared to other programs.

I hate to go right from complimenting Dr. Adams and putting Dr. Pewitt back into the barrel, so to speak, but I am curious as to how you think we can maintain our position, our competitive position, when we are intending to stabilize our expenditures in research.

We are now spending 50 percent less than the European community. Do we have a 50 percent greater ability to utilize our money or are we 50 percent smarter than the European community? How is this going to work?

Dr. PEWITT. Dollar comparisons are not easily made on that simple a basis. I think historically, even during a period of declining real budgets, that the ingenuity and the multiplicity of institutions that we have has provided a great strength to our programs. It is a compliment to the community, not to the Federal support of that community, that in fact we still have a leading world capability. Certainly if the bureaucratic processes were to result in increased funding, I think that we would have a dominant program, perhaps. But the question of funding over the next several years, I think, is going to be tied more to macroeconomic considerations in formulating the Administration's budget than to detailed considerations of the needs of the field.

Mr. CARNEY. I asked a somewhat simple question and it was answered by a somewhat complicated answer that didn't answer that simple question. I have to compliment you on your ability to talk governmentese and bureaucratese. But you didn't answer my question. I don't know if you will today, though.

Thank you, Mr. Chairman.

Mr. MCCORMACK. Mr. Wydler?

Mr. WYDLER. Before we finish, I just want to say that I think all the witnesses we had today were excellent, added a lot to my knowledge, and I am sure the report will add to the knowledge of a great many people whose opinions are important in this respect.

I congratulate all the participants in all the events—the dinner, the breakfast, and the hearing. Outside of the fact that I put on a few pounds, that was the only minus I can figure for the whole proceedings. Otherwise, they were a total success.

Mr. MCCORMACK. Thank you.

If the day were young, we could do whatever we pleased. I would like very much to ask Dr. Adams, Dr. Panofsky and Dr. Pewitt to sit up here someplace and have everybody in the room ask them questions, and let them field the questions from the entire domestic and international group and have a roundtable.

Unfortunately, the pressures on us are extreme. We simply must terminate this hearing at this time. I invite you, of course, to take advantage of the fact that you are together to exchange information and to talk together about what is going on in the world.

If by your getting together you learn more and have a good time, then I think one of the major purposes of this hearing and last night's dinner will be achieved.

I do want to congratulate and thank all of you who have participated in this hearing this morning, and to congratulate all of you who have been involved over the years in this work.

Frankly, I am optimistic about the future of high energy research in this country and in the rest of the world. I am sure that the members of this subcommittee intend to insist on moving forward with strong support for the programs. Again, I want to thank you all.

We stand adjourned.

[Whereupon, at 12:30 p.m., the subcommittee adjourned.]

APPENDIX A

ANNOUNCEMENT OF ACCELERATOR 50TH ANNIVERSARY CELEBRATION

The Carnegie Institution of Washington
The President of the University of California,
The U.S. House Committee on Science and Technology, and
The Smithsonian Institution

Cordially invite you to festively celebrate

The 50th Anniversary of the Development of
the Cyclotron at Berkeley and
the Carnegie High Voltage Accelerators

and to honor the original developers and
accelerator-related Nobel Laureates.

Tuesday, July 22, 1980
Reception 6:30 pm
Banquet 7:30 pm
"Atom Smasher" Exhibit
Jubilee 9:00 pm

Reception Suite
Museum of History
and Technology
Constitution Avenue at
14th Street, N.W.
Washington, D.C.

Semi-formal

Congressman Mike McCormack, Chairman
Subcommittee on Energy Research & Production,
Committee on Science & Technology

announces

A hearing on
Quests with U.S. Accelerators . . . Fifty Years

Wednesday, July 23, 1980

9:30 a.m., Room 2325, Rayburn House Office Building

You are invited to breakfast preceding the hearing
8:00 a.m., Room B354, Rayburn House Office Building

RSVP by July 10
Card Enclosed

APPENDIX B

ACCEPTANCES TO THE

Accelerator 50th Anniversary Celebration
July 22, 1980

ORIGINAL DEVELOPERS OF ACCELERATORS IN 1930

Dr. (&Mrs.) L.R. Hafstad - Carnegie Institution Accelerator at Washington, D.C.
Former Head of General Motors Research Laboratories,
Former Director of Division of Reactors of Atomic Energy Commission

Dr. (& Mrs.) M. Stanley Livingston - Cyclotron at University of California-Berkeley
Former MIT professor

PIONEERS IN ACCELERATOR DEVELOPMENT

Dr. (& Mrs.) Ray G. Herb - National Electrostatic Corporation
Dr. (& Mrs.) E.J. Lofgren - University of California-Berkeley
Dr. (& Mrs.) Donald Kerst - University of Wisconsin
Dr. (& Mrs.) Gerald F. Tape - Associated Universities
Dr. (& Mrs.) Earnest Courant - Brookhaven National Laboratory
Dr. Arthur Roberts - University of Hawaii

NOBEL LAUREATES(FROM ACCELERATOR USES)

Dr. Samuel Ting - MIT
Dr. (& Mrs.) Edwin McMillan - University of California-Berkeley
Dr. Owen Chamberlin - University of California-Berkeley
Dr. Donald Glaser - University of California-Berkeley

CONGRESS

Mr. (& Mrs.) McCormack (D-Wa)
Mr. (& Mrs.) Young (D-Mo)
Mr. Ronnie Flippo (D-Ala)
Mr. Nicholas Mavroules (D-Mass)
Mr. John Wydler (R-NY)
Mr. Toby Roth (R-Wis)
Mr. Manuel Lujan (R-NM)
Mr. William Carney (R-NY)
Mr. (& Mrs.) Craig Hosmer - Former Congressman
Dr. (& Mrs.) Harold Hanson - Staff Director, House Committee on Science & Technology
Mr. (& Mrs.) Fuqua (D-Fla.)
Mr. (& Mrs.) Winn (R-Kan.)

HEADS OF MAJOR ACCELERATORS

Dr. John Adams - CERN,Switzerland
Dr. T. Nishikawa - KEK,Japan
Dr. Wolfgang Paul - University of Bonn, West Germany
Dr. H. Schopper - DESY,West Germany
Dr. Ian Butterworth - Imperial College of Science and Technology, England
Dr. Ronald R. Rau - Brookhaven National Laboratory
Dr. Louis Rosen - Los Alamos Scientific Laboratory
Dr. Alex Zucker - Oak Ridge National Laboratory
Dr. A. Bromley - Yale University
Dr. Thomas Collins - Fermi National Accelerator Laboratory
Dr. Lowell Bollinger - Argonne National Laboratory

SPONSORING ORGANIZATION REPRESENTATIVES

Dr. David Saxon - President, University of California
Dr. Bernard Finn - Smithsonian Institution
Dr. Christopher Wright - Carnegie Institution
Dr. Philip Abelson - Carnegie Institution (also Editor, Science Magazine)
Dr. (& Mrs.) George Wetherill - Carnegie Institution (Department of Terrestrial Magnetism)
Ms. Geraldine Sanderson - Smithsonian Institution

WITNESSES FOR CONGRESSIONAL HEARING

Dr. N. Douglas Pewitt - Department of Energy
Dr. Stephen Brush - Professor
Mr. (& Mrs.) Craig Nunan - Varian Corporation
Dr. Wolfgang Panofsky - Stanford University

GOVERNMENT

Dr. James Leiss - Department of Energy
Dr. A. Abashian - National Science Foundation

STAFF (ENERGY RESEARCH & PRODUCTION SUBCOMMITTEE)

Dr. (& Mrs.) Robert B. Leachman
Dr. Ezra Heitowit
Mrs. Mary Bly
Ms. Cady Barns

APPENDIX C

AGENDA FOR ACCELERATOR 50TH ANNIVERSARY CELEBRATION

Evening, July 22, 1980 -

Banquet festivities at the Museum of History and Technology, Smithsonian Institution:

Reception in the Dibner Library of the History of Science
Banquet in the Reception Suite

Congressman Mike McCormack, Master of Ceremonies
Commendations awarded to:

Dr. M. Stanley Livingston by Dr. David Saxon, President of the University of California

Dr. Lawrence Hafstad by Dr. Philip Abelson, Editor of Science
Dr. Merle Tuve and Dr. Gregory Breit in absentia

Statements by Dr. Bernard Finn of the Smithsonian, Congressmen Fuqua and Wydler

Dr. Arthur Roberts, University of Hawaii, played and sang his songs:

"The Cyclotronists Nightmare", "Take Away Your Billion Dollars", and "Particle Physics, You've Stolen My Heart"

Jubilee at the "Atom Smasher" Exhibit at the Museum

Morning, July 23, 1980 -

Breakfast at the Rayburn House Office Building for Congressmen, witnesses, banquet invitees, research directors in government agencies, and local university research directors

Hearing on "Quests with U.S. Accelerators...50 Years" in the Rayburn House Office Building

APPENDIX D

Commendation

We, the undersigned Members of the Congress of the United States, commend

Dr. M. Stanley Livingston

for his development of the underlying basic principle of cyclic resonance. Dr. Livingston developed this basis of the cyclotron accelerator on December 3, 1930 while working with Dr. Earnest Lawrence at The University of California at Berkeley.

During the past fifty years, Dr. Livingston has been at the forefront of the tremendous advances into the giant cyclic accelerators of today. Not only has he designed and supervised large accelerators, but he has also been the leading chronicler and educator about accelerators. His efforts have helped these technologies to serve the public in health, industry, energy, and defense.

Upon this fiftieth anniversary of his original development, it is appropriate that Dr. Livingston be accorded full honors for these great contributions to science and the Nation!

DON FUQUA	MIKE McCORMACK	JOHN W. WYDLER
Chairman, Committee on Science and Technology	*Chairman, Subcommittee on Energy Research and Production*	*Ranking Minority Member, Committee on Science and Technology*

Commendation

We, the undersigned Members of the Congress of the United States, commend

Dr. Lawrence R. Hafstad

for his role in the development of the Electron Accelerator. Together with colleagues at the Department of Terrestrial Magnetism of The Carnegie Institution of Washington, Dr. Hafstad achieved a magnetically-analyzed external beam of megavolt electrons on May 27, 1930. This started the very productive period of linear electrostatic-accelerator developments at the Institution.

Among his varied contributions to science for industry and the Nation, Dr. Hafstad has served in vital roles in the Atomic Energy Commission, first as Director of its Reactor Development Division and later as Chairman of the General Advisory Committee. Dr. Hafstad has remained active in other national science responsibilities.

Upon this fiftieth anniversary of the development of accelerators in the United States, recognition is given particularly to this original accelerator development, but also to Dr. Hafstad's 52 years of science leadership for the Nation!

DON FUQUA *Chairman, Committee on Science and Technology*	MIKE McCORMACK *Chairman, Subcommittee on Energy Research and Production*	JOHN W. WYDLER *Ranking Minority Member, Committee on Science and Technology*

Commendation

We, the undersigned Members of the Congress of the United States, commend

Dr. Merle A. Tuve

for his role in the development of the Electron Accelerator. Together with colleagues at the Department of Terrestrial Magnetism of The Carnegie Institution of Washington, Dr. Tuve achieved a magnetically-analyzed external beam of megavolt electrons on May 27, 1930. This started the very productive period of linear electrostatic-accelerator developments at the Institution.

Not only has Dr. Tuve provided initial inspirations for the earliest accelerator developments in the United States, but he continued faithfully in further developments and applications of accelerators. He provided distinguished direction to the Department of Terrestrial Magnetism in the scientific advances it has contributed to both the Washington area and the Nation.

Upon this fiftieth anniversary of the development of accelerators in the United States, recognition is given particularly to this original accelerator development, but also to Dr. Tuve's years of science leadership for the Nation!

DON FUQUA	MIKE McCORMACK	JOHN W. WYDLER
Chairman, Committee on Science and Technology	*Chairman, Subcommittee on Energy Research and Production*	*Ranking Minority Member, Committee on Science and Technology*

Commendation

We, the undersigned Members of the Congress of the United States, commend

Dr. Gregory Breit

for his role in the development of the Electron Accelerator. Together with colleagues at the Department of Terrestrial Magnetism of The Carnegie Institution of Washington, Dr. Breit achieved a magnetically-analyzed external beam of megavolt electrons on May 27, 1930. This started the very productive period of linear electrostatic-accelerator developments at the Institution.

Understandings in theoretical physics provided by Dr. Breit have stimulated many of the productive uses of accelerators. He has also been at the forefront of higher education in physics.

Upon this fiftieth anniversary of the development of accelerators in the United States, recognition is given particularly to this original accelerator development, but also to Dr. Breit's years of science leadership for the Nation!

DON FUQUA	MIKE McCORMACK	JOHN W. WYDLER
Chairman, Committee on Science and Technology	*Chairman, Subcommittee on Energy Research and Production*	*Ranking Minority Member, Committee on Science and Technology*

APPENDIX E

TELEGRAM FROM THE INSTITUTE OF HIGH ENERGY PHYSICS OF

THE PEOLPLES REPUBLIC OF CHINA

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DR WILLIAM WALLENMEYER
DOE
WASHINGTONDC/20585

GLAD TO LEARN THAT A MEETING WILL BE HELD BY
DOE CELEBRATING THE 50TH ANNIVERSARY OF THE INVENTION
OF ACCELERATORY BY DR LAWRENCE. WE WISH THE MEETING
A GREAT SUCCESS. REGARDS. INSTITUTE OF HIGH
ENERGY PHYSICS, ACADEMIA SINICA

COL 50TH

TWX INCOMING

APPENDIX F

WASHINGTON POST, JULY 23, 1980

Simply Smashing

By Elisabeth Bumiller

Round and round and round go the deuterons,
Round and round the magnet swings them,
Round and round and round go the deuterons,
Smack in the target goes the ion beam.

—From "The Cyclotronist's Nightmare" by Dr. Arthur Roberts

Bernard Finn, left, Dr. M. Stanley Livingston and Dr. L.R. Hafstad

Not apt to hit the Top 40, this song. Probably not something you'll hum in your head for days and days, either.

But to an atom smasher, it's downright catchy.

About 60 atom smashers—or, to be exact, 60 physicists who helped pioneer the development of nuclear accelerators, otherwise known as . . . atom smashers— had a party (an "Atom Smasher Jubilee," they called it) at the Smithsonian's History and Technology Museum last night. They had sherry in a charcoal-gray library, then dinner, and then were to listen to Roberts sing his deuteron song as well as "Particle Physics, You've Stolen My Heart."

"Romantic is one thing it isn't," said Roberts, a courtly gentleman who's currently immersed in a two-year feasibility study on the Deep Underwater Muon and Neutrino Detector. This gets highly complicated, but can best be explained in brief as a search for weak atomic particles outside the earth's atmosphere.

But back to the jubilee. The reason for it was the 50th anniversary of U.S. involvement in atom smashing, a scientific breakthrough that made possible, among others things, the atom bomb that was dropped on Hiroshima.

Which wasn't exactly tactful to mention last night. "We weren't responsible," said Dr. M. Stanley Livingston, a physicist who helped develop the cyclotron accelerator at the University of California-Berkeley. "I don't like to associate with that. Atom smashing explained and explored nuclear physics . . . Let's not disturb the history of nuclear physics by bringing that into it."

If this particular get-together was any indication, what physicists tend to talk about on their off hours is electron volts, tesla coils, bubble chambers and magnets.

But then, at least one of the few wives in attendance saw a clear advantage in being married to a nuclear physicist as opposed to, say, a doctor. "They aren't usually," she said, "called out on emergencies."

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A Sentimental Trip Down Accelerator Lane

No commemorative stamp has been issued, but the first 50 years of the particle accelerator in America was marked recently by a modest celebration in Washington. The "atom smasher" semicentennial took the form of a party at the Smithsonian Museum of History and Technology on 22 July and a hearing the next day on Capitol Hill.

Livingston, Hafstad, and cyclotron

Photo by Jane Walsh

Honored as "original developers" were L. R. Hafstad and M. Stanley Livingston. Starting in 1930, Livingston was a graduate student and then collaborator of Ernest O. Lawrence at Berkeley during the development of the first cyclotrons. At the same time, Hafstad was a member of a team at the Carnegie Institution's Department of Terrestrial Magnetism which experimented with accelerator technologies and settled on the electrostatic generator invented by R. J. Van de Graaff.

On hand also at the party were a number of physics luminaries including Nobel Laureates Owen Chamberlin, Donald Glaser, and Edwin McMillan of the University of California at Berkeley, all of whom won the big prize for work with the big machines. Major accelerator centers were represented by head men such as Wolfgang Panofsky of SLAC, the Stanford Linear Accelerator, and John Adams of CERN, the European Center for Nuclear Research. After dinner, speeches, and awards, a short sentimental journey was made down the hall to the Smithsonian's atom-smasher exhibit where samples of original hardware and memorabilia are on display.

The celebration was instigated by Representative Mike McCormack (D-Wash.), an energy subcommittee chairman, who also presided over the hearings.

The hearings focused partly on the past, with a discussion by Livingston and others of accelerator history. But the focus inevitably shifted to the present funding predicament of high energy physics and nuclear physics (*Science*, 1 August). The most arresting comment of the day came from CERN Director Adams who noted that in 1966 the United States and CERN member nations had allocated about the same fractions of their gross national products to the support of physics, about .025 percent, "but whereas the CERN member states have maintained this same fraction through 1978, the last year for which we have official figures, the fraction allocated in the U.S.A. has fallen to about half the 1966 value."

This prompted a question from the congressmen to Department of Energy officials as to whether the present "balanced" American program will keep the United States competitive, a question which, at the end of the hearing, was left hanging.

John Walsh

APPENDIX G

High Energy Physics Advisory Panel

Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics Program

July 1980

U.S. Department of Energy
Office of Energy Research
Division of High Energy Physics
Washington, D.C. 20545

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

July 15, 1980

Dr. Edward A. Frieman
Director of Energy Research
U.S. DOE, MS 6E084
Washington, D. C. 20585

Dear Ed:

I am forwarding to you the report of the 1980 HEPAP Subpanel which was formed in response to the Charge to develop a general strategy and long range plan for the U.S. High Energy Physics program over the next decade. In particular, the Subpanel was asked to assess the program balance between research, equipment, and construction over the period from FY 1982 to FY 1987 and to make specific recommendations for FY 1982. Two funding constraints were given as guidance to the Subpanel: the DOE/OMB Long Range Plan of 1978 - i.e., $325M in FY 1979 dollars for DOE plus NSF; and a funding level that is 10 to 15 percent higher. In addition, the Subpanel was asked to identify the physics opportunities that would have to be deferred, or that would be lost, under those constraints.

In addressing this broad and difficult Charge, the Subpanel worked both hard and effectively in the time available to it, and I greatly appreciate their efforts. HEPAP discussed the Subpanel report extensively at its June 30-July 1, 1980 meeting at DOE Headquarters in Germantown, Maryland. We endorse its general conclusions and recommendations as described below. In this transmittal letter I will convey HEPAP's own set of priorities on the main recommendations of the report, where they have not been expressed by the Subpanel. I also wish to express HEPAP's serious concerns about the future of the national High Energy Physics program in view of the gap between the current funding levels and the guidance given to the Subpanel.

High Energy Physics is in the midst of very exciting developments, both experimental and theoretical, that radically altered our views of the basic forces and elementary constituents of nature during the 1970's. Many of the key discoveries during the past decade were made in the United States. We are now entering the 1980's with a powerful and beautiful new conceptual framework being constructed which unifies three seemingly very different forces with one another: the electromagnetic forces, the weak forces of radioactivity, and the strong nuclear forces. At the same time many basic questions about the nature of these forces and the elementary constituents which they affect still remain unanswered. We see the possibility of confirming or denying our emerging picture of nature on the sub-nuclear frontier with decisive experiments using the new detectors and accelerators presently starting initial operation as well as the facilities now under construction. Other facilities currently being designed and analyzed would permit further crucial tests of the validity of this emerging picture. HEPAP recognizes that it is essential to formulate long range as well as short range plans in a field of such fundamental importance as High Energy Physics that requires large and expensive facilities as well as talented scientists in order to achieve its progress.

The national High Energy Physics program of the United States is heavily committed at this time to two major construction projects based on superconducting magnet technology at Fermilab and at Brookhaven. These are bold and exciting ventures that HEPAP endorsed in its earlier facilities reports. We again emphasize our strong support for these projects although they are turning out to be both technically more arduous and more expensive than previously anticipated. Though great strides have been made with the technology of superconducting magnets, some hurdles in this respect remain before both construction projects can be completed. These hurdles are described in the Subpanel report which notes that Fermilab, with its earlier commitment to substantial R&D, is further along toward its goal. When they are operating as proposed, the Saver/Tevatron I/Tevatron II at Fermilab and ISABELLE at Brookhaven will permit major advances in the research frontiers of elementary particle physics. We believe that this complex of new construction projects should proceed at this time with highest priority. The necessary R&D funds to assure their success must be provided during the coming year. At the same time we recognize that there is a great need to provide for high utilization of the forefront accelerator facilities in the ongoing research program--i.e., the Fermilab 400 GeV accelerator, the newly-commissioned PEP storage ring, the CESR facility at Cornell. As part of this recommendation, increased support is required for user groups to exploit these facilities for physics. The third component of a balanced research program--the detector and accelerator R&D that are essential for further progress--also requires significant support as emphasized by the Subpanel report. The technologies of very high charge-density bunches with low emittance in linear accelerators, very high-field superconducting magnets, and superconducting rf cavities were singled out by the Subpanel as being of great importance.

For the present we can meet the above goals of a healthy and balanced research program at the funding level of the 1978 DOE/OMB guidance. However, such a funding constraint still means the loss of very good science with existing facilities supported significantly below their potentials. In particular, at this level it is necessary to operate the currently interesting, important, and productive lower-energy fixed target programs at the Brookhaven AGS and SLAC linac with lower priority, thereby providing fewer opportunities for physics for considerable numbers of University and Laboratory-based user groups. This level of funding will also require deferral of other new construction initiatives. Specifically, the SLC electron-positron linear collider project has been proposed by SLAC, and other e^+e^- and ep colliders have been discussed. We would anticipate from projects such as these a very rich output of physics as well as advances in accelerator technology.

It is important to recognize that with only a modest (15 percent) increase in funding above the DOE/OMB guidance we could achieve our major goals of: timely progress toward completing current construction projects; major improvements in the level and efficiency of facilities operations, and correspondingly of valuable physics output from the ongoing programs; a strong program of advanced accelerator R&D. Simultaneously, an early new construction initiative such as described above, could be accommodated. This program would indeed ensure a broad-based, preeminent U.S. national program, both exciting in its own right and complementary in scope with Western Europe's very strong and advancing program. A U.S. program of such vigor remains our immediate and long range goal. As your Advisory Panel, charged with the responsibility of helping define a strategy and long range plan for a preeminent U.S. national High Energy Physics program, we can recommend no less.

The painful reality we must recognize and confront, however, is this: the High Energy Physics operating budgets for FY 1980 and, as currently envisaged for FY 1981, are well below the long range DOE/OMB plan of 1978. The best current estimate is that by the end of FY 1981 the accumulated shortfall since the initial implementation of this plan in FY 1979 will be about $45M. Budget stringency during this period has severely limited the R&D program underpinning the ongoing construction, and has also reduced the level of utilization of existing facilities to the point of causing a serious loss of physics as well as great inefficiencies and difficulties for the research groups. This loss will continue unless the funding is restored at least to the level of the DOE/OMB 1978 plan. Continuation of the current funding restrictions may force substantial reduction in the scope of the U.S. High Energy Physics program. If this occurs, the U.S. program will inevitably lose its eminence, due both to inadequate opportunities for U.S. scientists as well as to our inability to compete with Western Europe. We then will face the prospect of the U.S. High Energy Physics program being less and less competitive to share in making the major advances and discoveries on the sub-nuclear frontiers.

We hope that this will not be the fate of a U.S. program that has so distinguished a record of major accomplishments in the study of nature's most basic processes and constituents; a field that has flourished here for close to 50 years since its birth at the Lawrence Berkeley Laboratory under the initial leadership of E. O. Lawrence; and a field that has contributed so importantly to advancing the technology base of the U.S. In particular, pioneering advances in superconducting magnet and cryogenic technology, spurred by the Fermilab Energy Saver/Tevatron project and the ISABELLE project at Brookhaven, have important practical applications. This technology, which is so vital for further advances in the high energy frontiers, is also required for progress in the development of fusion energy and magneto-hydrodynamics, and will contribute to new techniques for energy conservation.

We expect that another review of the general strategy and plans for the U.S. High Energy Physics program may very well be desired a year from now. This summer proved to be an especially difficult time to formulate long range plans; hence, it was almost inevitable for the Subpanel report to focus heavily on problems, moods, and opportunities of the ongoing program. A year from now, however, there may be a considerable clarification of uncertainties that weighed so heavily on the deliberations of this year's Subpanel. For one thing, the current funding projections were very uncertain. Further clarification of the financial plans for High Energy Physics can be hoped for following completion of Congressional action on the FY 1981 budget and after discussion of the long range program plans between the Congress and DOE/OMB subsequent to study of the impending report of the General Accounting Office. In addition and most importantly, a year from now we may expect to have higher confidence in clearing the technical hurdles in our current construction programs based on superconducting magnet technology, as well as more definite information on Western Europe's plans for their High Energy Physics program. It may then be timely to accomplish an updated review of our plans and strategy for ensuring continued U.S. leadership in the quest for an understanding of nature's fundamental laws.

Sincerely yours,

Sid

Sidney D. Drell
Chairman, HEPAP

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June 24, 1980

Professor S. Drell
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Dear Sid,

Enclosed is the Report of the 1980 Subpanel on Review and Planning for the U.S. High Energy Physics program, which met at the National Academy of Sciences Study Center, Woods Hole, Massachusetts, from June 1-7, 1980.

Sincerely,

Sam Treiman

Sam Treiman

SUMMARY

High energy physics addresses the fundamental structure of matter, a central theme of science. The field is in a state of high accomplishment and promise. Striking experimental, technological, and theoretical developments over the past decade have combined to transform our understanding of the basic constituents of matter and of the forces that govern their interactions. The picture of protons and neutrons as composites built up out of more basic entities, the quarks, has been strengthened and elaborated. The strong forces, responsible among other things for binding of nuclei, and the weak forces, responsible for radioactivity, have come to be understood at a deeper level corresponding to the understanding of electromagnetism, pioneered by Maxwell in the 19th century, and of gravitation, pioneered by Einstein in the early years of this century. The weak and electromagnetic forces, and perhaps also the strong ones, now all appear to be unified in a common theoretical framework of great breadth and elegance. These theoretical advances have enriched the traditional connections between particle physics and other disciplines--nuclear physics, condensed matter physics, pure mathematics, the physics of the big bang universe, etc. The technological advances have similarly spread to other areas of science and technology. The cultural and technological attraction of high energy physics is reflected in the growing level of research activity and investment abroad, particularly in Western Europe and the USSR but also increasingly in Japan and the Peoples Republic of China.

The accomplishments in high energy physics spring from a variety of research approaches and experimental tools. At the heart of the effort are the high energy accelerators--the descendants of Ernest Lawrence's cyclotron. In the U.S. these are now centered at Brookhaven National Laboratory (BNL), the Stanford Linear Accelerator Center (SLAC), Fermi National Accelerator Laboratory (Fermilab), and the Newman Laboratory of Nuclear Studies at Cornell University. They serve the research needs not only of the high energy physicists at these centers but also of the wider community at the Universities. The current array of proton and electron accelerators in the U.S. constitutes a powerful and diversified set of research tools; and major new proton and antiproton accelerator projects underway at Fermilab and Brookhaven will extend our reach to higher domains of energy and surely to new discoveries.

The U.S. has long occupied the position of world leader in high energy physics, thanks to our national tradition of technological ingenuity, diversity, and generous public support. However, that leadership is now coming to be shared with Western Europe; and amidst all its high scientific achievements and promise, the U.S. program finds itself confronted with a number of present difficulties and future challenges and choices.

(i) The level of financial support in Western Europe now substantially exceeds that in the U.S. and it is necessary for the U.S. to rely increasingly on special effort and ingenuity to keep our program at least well represented at the major forefronts. Recent scientific developments point to the great promise for the coming decade of very high energy electron-positron and electron-proton colliders. Ambitious and costly projects along these lines are presently under

serious discussion in Western Europe. While it no longer seems financially possible for the U.S. to proceed in parallel by the scaling up of conventional technology, there is scope for more financially modest projects based on inventive new technology. Several attractive possibilities are under discussion in the community and were reviewed by the Subpanel.

(ii) In the current program, financial stringencies are imposing increasingly severe limitations on the exploitation of existing facilities, hence limitations on the opportunties available to the talented community of high energy physicists at the national laboratories and universities to mount experiments and follow out important scientific leads.

(iii) The new Fermilab and Brookhaven projects, which are based on pioneering superconducting technology, are turning out to be more arduous than originally anticipated. This is a serious challenge in its own right. It also impacts on the resources available to other elements of the national program.

Conclusions

The Subpanel was charged, among other things, to address the future directions of the national program: (a) on the basis of a constant dollar support level corresponding to the current financial plan; (b) on the basis of a 10-15% increase in the level of support. Since the situation is complex our evaluations and recommendations have to be read with care, in the context of all the supporting commentary. The following is a rough and compact summary of the main points.

1. General

(a) Utilization of the forefront accelerator facilities in the ongoing research program should be intensified, to exploit for physics the investments already made, and university groups should be given increased support.

(b) The new superconducting projects, the Energy Saver/Tevatron at Fermilab and ISABELLE at Brookhaven, must proceed with all deliberate speed. It is the responsibility of the entire high energy physics community to help in surmounting the difficulties now being experienced.

(c) Increasing support should be devoted to detector and accelerator R&D.

2A. Constant Level of Support

(d) The financial stringencies imposed by the present level of support will have to be accommodated by carefully planned reductions in the programs at the lower energy facilities.

(e) New construction initiatives will have to be foregone, at least for the nearer future until the time scales associated with the Fermilab and Brookhaven superconducting projects can be better assessed. Nevertheless, intensive R&D looking to the farther future should begin now.

2B. Increased Support Level

A modest (15%) increase in support, beginning in FY 1982, would make possible major improvements in the level of utilization and physics output for the ongoing programs. Moreover, it would provide the possibility of an early new construction initiative which addresses promising new areas of research. Specifically, the future U.S. program would be greatly strengthened by an electron-positron collider operating in the energy region between about 30 GeV and 100 GeV, where a rich output of physics is anticipated. Similarly, a facility designed to study high energy electron-proton collisions promises exciting physics opportunities. It may be that both the above goals can be met with a combined facility.

The Subpanel reviewed a number of attractive possibilities now under discussion in the community, in particular, the electron-positron linear collider SLC proposed for construction by SLAC. The Subpanel recommends that no action on this proposal be taken now but it recommends that it be considered, along with other emerging alternatives, by a similar panel convened within a year or at most two. At an increased level of support, we anticipate that the time would then be right for positive action on a new facility.

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SECTION I

INTRODUCTION

A High Energy Physics Advisory Panel (HEPAP) Subpanel was established in the spring of 1980 to address the Charge reproduced here in Appendix A. The Subpanel met at Woods Hole, Massachusetts, June 1-7, 1980. A list of the Subpanel members, consultants, and other participants at Woods Hole is provided in Appendix B. In preparation for its final deliberations the Subpanel carried out site visits to Fermi National Accelerator Laboratory (Fermilab), May 1-2; Stanford Linear Accelerator Center (SLAC), May 9-10; and Brookhaven National Laboratory (BNL), May 16-17. The Subpanel also heard presentations from Lawrence Berkeley Laboratory (LBL) at the SLAC site visit, and from Argonne National Laboratory (ANL) and the Newman Laboratory of Nuclear Studies (Cornell) at the BNL site visit. The input at the site visits was augmented by written responses to questions put in advance to the Laboratory Directorates concerning their current programs, future plans, and general opinions about the direction of the national high energy program. In addition, the Subpanel solicited the views of the entire high energy community, through a call for written statements from individuals and users groups and through wide-ranging discussions during the site visits. The responses were numerous and well thought-out and have figured into the deliberations of the Subpanel in an important way.

The 1980 HEPAP Subpanel is the fourth in a series of Subpanels formed from time to time to review the status of the national program in High Energy Physics and to address long-range plans. The earlier Subpanels (1974, 1975, 1977) were concerned chiefly with the issue of new research facilities required to advance the vitality of the program after a substantial hiatus in major construction projects. Several projects addressed by these Subpanels have reached completion or are now underway. In the meantime, numerous scientific and technological developments have opened up promising opportunities for further progress in the coming decade. It is timely, therefore, to review our present status with respect to facilities and to consider what new research tools are needed over the coming years for orderly exploitation of these opportunities. Beyond the matter of facilities, however, it seems especially appropriate just now to also address more general issues concerning the structure of the national program in High Energy Physics. This is so because an unusual number of problems, promising scientific prospects, and critical choices presently confront the U.S. high energy physics community.

The situation is surveyed briefly in the remaining paragraphs of this Section. More detailed discussion and specific recommendations appear in later Sections.

The accelerating pace of experimental discovery and theoretical development over the past decade have brought particle physics to a state of high promise and have influenced the pattern of facility construction addressed by the earlier Subpanels. The e^+e^- collider (CESR), which came into operation at Cornell in the fall of 1979, has turned out to be remarkably well-placed in energy to pursue the physics of the new "bottom" quark discovered somewhat earlier at Fermilab. The e^+e^- collider PEP is just now coming into operation at SLAC in a higher energy region appropriate for the physics of quark and gluon jets and other fundamental issues. The Energy Saver/Tevatron facilities under construction or planned at Fermilab are designed to provide a window on ultra high energies (the $p\bar{p}$ collider at 2 Tev in the center of mass) and to pursue varied physical phenomena induced by the high energy secondary beams of the fixed target Tevatron program. The pp collider ISABELLE under construction at BNL is designed to combine high luminosity and high energy (800 GeV in the center of mass) for study of hadron collisions.

The fixed target physics of the Tevatron project, when it comes into operation, will upgrade the 400 GeV program that presently constitutes the major high energy element of the national effort. The ongoing programs at the BNL Alternating Gradient Synchrotron (AGS), the SLAC electron-positron storage ring (SPEAR), and the SLAC linac carry much of the remaining current effort at lower energies. These latter facilities represent a valuable national investment already in place, and they cover regions of particle physics in which much important work remains to be done. Moreover, the SLAC linac serves as injector to the Positron-Electron Storage Ring (PEP), and the AGS is slated to inject protons into ISABELLE. The national program in High Energy Physics also involves experimental and research and development (R&D) activities not associated with high energy accelerators: e.g., cosmic ray experiments, searches for proton decay, neutrino experiments based on nuclear reactors, weak interaction studies at Los Alamos Scientific Laboratory (LASL) based on the Los Alamos Meson Physics Facility (LAMPF), quark searches, etc.

At all major levels -- experimental, technological, and theoretical -- the U.S. community has played a leadership role over the past several decades in the expanding science of particle physics. The facility construction projects undertaken in the 1970's were designed to sustain a vigorous U.S. position for the coming years, following a period of decline in real dollar funding and the phasing out of a number of lower energy accelerator facilities. The current level of funding for the national program is nominally governed by the DOE/OMB Long Range Plan of 1978. This is supposed to correspond to a steady annual funding level (DOE plus NSF) of $325M in FY 1979 dollars. It represents the lowest of several alternative levels of support which the 1977 Subpanel was charged to investigate for future planning. That Subpanel regarded the ongoing program and the ISABELLE and Energy Saver/Tevatron projects as workable on a "best effort" basis at this level, but it foresaw the possibility of strains developing.

Several problems have indeed arisen. For one thing, inflation in power and certain other technological costs has not been fully allowed for in the conversion to real dollar budgets, so that a deficit has begun to develop with respect to the DOE/OMB Plan. This has entailed a decrease in the utilization of existing facilities and has led to delay and in some cases abandonment of promising experiments. Moreover, the new superconducting technology involved in the ISABELLE and Tevatron projects is turning out to be more arduous than was anticipated in the reports of the earlier Subpanels. This is a challenge in its own right but it also impacts on the resources available to the ongoing program. Yet, for the longer run, the superconducting effort contributes to an important developing technology with potential applications outside of high energy physics; and for the physics program it provides an avenue for mitigating the power costs of accelerator operations while reaching higher fields and energies.

Another issue that increasingly confronts the U.S. community is the growing vigor of High Energy Physics activities abroad, particularly in Western Europe. The e^+e^- collider PEP has its counterpart in the PETRA facility at DESY in West Germany; the 400 GeV program at Fermilab is paralleled by a corresponding program at the SPS at CERN in Geneva; the pp collider ISR, at CERN, is without counterpart in the U.S.; and the overall level of funding in Western Europe substantially exceeds that supporting the U.S. program. Moreover, there is an increasing level of activity underway and planned in Japan, the Peoples Republic of China, and the U.S.S.R.

At CERN, a $\bar{p}p$ collider facility (540 GeV in the center-of-mass) is now under construction and is expected to come into operation well before the 2 Tev $\bar{p}p$ collider at Fermilab. Counterparts to the high luminosity ISABELLE facility and the fixed target Tevatron program are not presently envisaged for Western Europe. On the other hand, plans for a very high energy e^+e^- collider LEP at CERN (100 GeV in the center of mass in the earliest stage) and for an ep and e^+e^- collider HERA at DESY are now under serious discussion in Europe.

These developments abroad attest to the scientific attraction and importance of High Energy Physics. From the U.S. perspective, they have to be recognized as the inevitable limits which are coming into place on our long established predominance in High Energy Physics. At the same time, they provide welcome opportunities for increased scientific progress on the international scale, through collaboration, allocation of tasks, and friendly competition and cross checking.

As is reflected in the range of accelerator types described above, progress in particle physics calls for variety in tools and approaches. New facility projects are motivated from time-to-time by ongoing developments in the physics itself and in technology. Although it may no longer be feasible for the U.S. and Europe to pursue every new research opportunity in parallel, the sustained vigor of the U.S. program requires that we be at least well represented in the most promising lines of research. In present planning for the coming decade, the U.S. program stakes out the highest energies for the physics of hadron collisions, via the ISABELLE and Tevatron projects. Western Europe will probably emphasize the highest energies for e^+e^- and ep colliders, via the very ambitious and costly LEP and HERA proposals. There are fundamental scientific issues at stake in every one of these areas. In particular, recent advances in our understanding of the weak interactions strongly indicate that these interactions are mediated by gauge bosons, the Z^o and $W^{\pm}$ particles, with masses somewhat below 100 GeV. The several lines of approach bear on various aspects of these gauge particles: their existence, their production and decay characteristics, and the open possibility that there may be more than one class of such objects.

The e^+e^- collider approach is especially suitable for study of the expected rich variety of Z^o decay processes, including decay channels involving other anticipated (and perhaps unanticipated) objects such as "Higgs" particles, new quark and lepton types, etc. Similarly, the study of high energy ep collisions can extend the search for gauge boson effects to higher mass regions and it opens up new windows bearing on other aspects of the weak interactions. Although the European decisions regarding LEP and HERA have not yet been taken, the U.S. community, in planning for its own program in the coming decade, cannot fail to take into account the possibility of positive European decisions and vigorous implementation.

Several possible new projects, in varying stages of definition, have been proposed for the U.S. program and have been brought to the Subpanel for consideration.

(1) The Stanford Linear Collider (SLC) proposal for an e^+e^- collider at 100 GeV in the center of mass is based on ingenious new technology with promise for later relevance to ultra-high energy devices involving colliding linac beams. The project has provision for one interaction region. Compared to LEP, the SLC project is relatively inexpensive.

(2) The e^+e^- ring contemplated by Cornell at 100 GeV center-of-mass has four interaction regions and involves superconducting rf technology. Again, by the standards of LEP the Cornell project is relatively inexpensive, though much more costly than SLC.

(3) A possible ep facility, being explored by a Canadian consortium and independently by a U.S. group at Nevis Laboratory, Columbia University, was described to the Subpanel. It would involve a 10 GeV electron ring, used in conjunction with a high energy proton beam at Fermilab or BNL.

As with the Tevatron and ISABELLE, either of the above e^+e^- facilities would involve the challenge and opportunities of new technology. For the longer term it is clear that scientific progress rests on the development of advanced technologies, designed to extend the energy, intensity, and flexibility of accelerators and the precision and range of detectors and instrumentation. Much of the R&D effort in the current U.S. program is properly aimed at the short-term goals of existing or imminent projects. However, it seems increasingly important to upgrade the effort devoted to goals of longer range. This issue is addressed in more detail later in the report.

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The above discussion lays out some of the major issues that have confronted the Subpanel. High Energy Physics, in its intrinsic scientific dimensions, has accomplished major advances over the past decade. The ongoing program, the newly completed research facilities, and the projects underway, constitute a set of promising ingredients for further advances. On the other hand, budgetary constraints are exacting a serious price in the utilization of existing facilities and pose the risk of foreclosing the timely development of new facilities of major importance. This occurs in the face of increasing research investment and activity abroad.

Finally, an issue that is interwoven among all the others and that was the subject of much discussion by the Subpanel concerns the most important resource of high energy physics research, namely, the people who practice this science. The reduction in the number of accelerator laboratories that has taken place in the past decade and the reduction in utilization of the remaining facilities have combined to limit the opportunities for small groups of people to exercise independence and to mount and carry out experiments in a timely fashion. Moreover, the physics objectives themselves seem increasingly to demand large detectors, large collaborations, and long time scales. These trends are not peculiar to the U.S. program. On the shorter-term view one may have to accept the realities of the situation, in the interest of efficiency and scientific output. For the longer run, however, both efficiency and output depend ultimately on the skills and ingenuity of individual people, qualities that are enhanced by practice and independence. In our long-term planning, provisions have to be made to preserve independence and variety of opportunities, to engage fully the talented pool of high energy physicists at the universities, and to attract new, young talent. How these considerations are to enter into concrete decisions about the overall program, and with what weight, is a very complex problem.

SECTION II

STATUS OF THE FIELD

Introduction

During the decade of the 1970's, there has been remarkable progress in our understanding of the subnuclear world. Of the four basic kinds of forces, at best only two -- electromagnetism and gravitation -- could in 1970 be described in highly fundamental terms. The other two forces -- strong and weak -- were understood only in relatively incomplete and descriptive terms. The high energy behavior of the weak force could not be predicted, although enough was known to give hints of landmark energies at which new kinds of phenomena could be expected to occur. A great deal of information on the properties of the strong force provided by 1970 a body of organizing principles but no fundamental theory. Experiments were just beginning to point the way toward a description of the nucleon in terms of quark constituents, with some promise of simplicity of description at short distances.

In 1980 the situation is quite different. There exist candidate theories for both strong and weak interactions, each formulated at a level as basic as the formulations of electrodynamics and gravitation. The theory of the weak interaction is quantitative and predictive at very high energies as well as at low energies, and has enjoyed a remarkable degree of experimental success. The theory of strong interactions has likewise been successful in accounting for a broad range of strong interaction phenomena.

This revolution in our viewpoint on strong and weak interactions has also been accompanied by much greater confidence in the picture of nucleons as composites of point-like quarks. This picture was very much advanced by the landmark discovery at BNL and SLAC of the J/psi particle in 1974 and its subsequent description in terms of charmed-quark constituents.

The field in 1980 has progressed much further than anyone could have dared to anticipate in 1970, and a central feature of the program of the 1980's will be either to firmly establish the validity of the standard picture or show it to be deficient. If the present theoretical optimism is confirmed by the experimental program of the 1980's, we will have largely accomplished an advance in our understanding of the basic forces of nature comparable to the establishment of the theories of electrodynamics and gravitation.

The Standard Picture

The experimental and theoretical developments of the 1970's have led to a definite theoretical framework. In this standard picture the number and kind of building blocks (six kinds of quarks, three kinds of charged leptons, and three kinds of neutrinos) are specified, as well as the basic forces between them -- electromagnetic, strong, and weak.

The theory of quantum electrodynamics (QED) has been well developed for some time. The predictions of the theory have been tested ever more incisively by experiments at e^+e^- storage rings, so that it is known to be accurate down to a distance scale of order 10^{-16} cm, about one thousandth of the size of a proton.

The strong force acts between quarks and is described by a theory called quantum chromodynamics (QCD). QCD is structurally very similar to quantum electrodynamics (QED). Just as QED implies the existence of photons, QCD implies the existence of similar quanta called gluons. Gluons (like quarks) are only seen indirectly. Recently, indirect evidence for their existence has emerged from experiments at the e^+e^- storage ring PETRA. This will be a major subject of investigation for the PEP storage ring now being commissioned. Earlier evidence for QCD emerged from the lepton scattering experiments carried out at SLAC, Fermilab, and CERN during the 1970's. Experiments on production of lepton pairs at Fermilab and elsewhere have also provided evidence supportive of QCD.

The weak force, which is responsible for radioactivity, is described by a theory similar in many respects to QCD and QED. However, the analogs of photons and gluons, the intermediate bosons $W^{\pm}$ and Z^o, are expected to have large rest masses, of order 100 times the mass of the proton. The theory unifies to some extent the electromagnetic and weak forces at these large energy scales.

The rapid development of this theory in the 1970's occurred as a result of major theoretical breakthroughs and a large body of precision experiments at BNL, Fermilab, and CERN using muon and neutrino beams. The discovery of the J/psi at SLAC and BNL in 1974, and its interpretation in terms of the fourth, or charmed quark, was another important element. A beautiful and delicate experiment at the SLAC linac using polarized electrons also provided important confirmation. By now there is a remarkably consistent and accurate body of data in support of the standard electroweak picture.

The structural similarity of the theories of strong and electroweak forces has suggested a further unification. The simplest such extension involves extrapolation of present theory by 13 orders of magnitude to energies of order 10^{15} GeV, at which point the synthesis of strong and electroweak forces becomes manifest. While the energies required to directly explore this domain are inaccessible, there are observable low energy effects which do occur. In particular, the proton is predicted to be unstable, with a lifetime of the order of 10^{29} - 10^{33} years. Experiments with sensitivity in this range are already underway.

Despite these beautiful concepts and encouraging experiments, there are aspects of the electroweak theory which suggest it is incomplete. The most important aspect has to do with the masses of the $W^{\pm}$ and Z^o and of the quark and lepton building blocks. A general theoretical framework which describes the origin of these masses does exist, although there are differing versions of the details. In any event, it appears inevitable in such frameworks that there exists at least one extra particle (the "Higgs boson") with properties which make it very difficult to detect. The expected mass range is anywhere from a few GeV to 100 GeV.

The actual pattern of observed quark and lepton masses remains totally enigmatic. The understanding of this pattern is one of the greatest challenges for the theorist. Neutrinos may also have mass, a possibility that has been recently revived. Novel experimental techniques on a wide range of energy scales are relevant to the search for effects due to neutrino mass. The AGS, for example, is well suited to undertake these searches.

The number of quarks and leptons is also not understood. The third charged lepton, discovered at SLAC in 1975, and the fifth, or bottom quark, emergent from the Fermilab discovery of the upsilon resonance, were totally unexpected particles theoretically. Thus there is no shortage of profound theoretical problems, even within the standard picture.

Physics in the 1980's According to the Standard Picture

The facilities proposed for the 1980's, both in the U.S. and in Europe, should provide much better and more decisive tests of the standard picture.

The number of crucial tests of QCD is small. The higher energy e^+e^- colliders are especially useful in providing such crucial tests. Fixed target lepton-nucleon scattering and, if possible, ep colliding beams are another class of experiments which provide especially clean tests. The higher energies available in the 1980's at the Tevatron and elsewhere will be especially useful.

The many successful tests of electroweak theory have all been at energies small compared to its natural energy scale, given by the masses of the $W^{\pm}$ and Z^o. The clear central goal is the direct observation of these particles. The large value of the predicted masses make colliding beam facilities a necessity. ISABELLE and the $\bar{p}p$ Tevatron collider are well suited to this task. Electron-positron collisions would be a rich, especially clean source of Z^o's, allowing the accurate determination of Z^o properties. This is a prime motivation for e^+e^- colliders.

Another goal of electroweak theory will be to measure the single parameter of the theory (the weak mixing angle θw) more accurately at low energies as well as at high. This can be done in a variety of ways at all the fixed target machines.

In addition to describing the force laws, the standard picture anticipates the existence of a sixth, top quark, as yet undiscovered. If the mass is less than 100 GeV, it should be found at LEP or some other e^+e^- collider. For masses larger than that, it appears feasible to observe this quark in pp or $\bar{p}p$ colliders such as ISABELLE or Tevatron.

In addition, a third neutrino associated with the tau lepton is anticipated. The Tevatron fixed target program provides about the only opportunity for producing and studying this particle. Such experiments appear difficult, but attempts are already being made and will certainly continue to be pursued.

There is the chance that the single Higgs boson of the standard picture will be discovered if its mass is less than about 100 GeV. This should be possible with e^+e^- colliding beam machines such as LEP. If the mass is considerably larger, the task may have to be left to later generations of accelerators.

Finally, in the standard picture the puzzling phenomena of CP violation is correlated with the equally puzzling issue of the origin of quark masses and mixings. It is necessary to find additional observable effects which test this general idea. Precision measurements of K decay, now underway at Fermilab and BNL, and intense measurement of bottom quark properties at CESR or PEP may provide new information of vital importance.

The Program of the 1980's Without the Standard Picture

It may be fair to say that while the 1970 physicist was too pessimistic about the progress which would be made in the 1970's, the 1980 physicist may be too optimistic. Particle physics has, without fail, encountered major surprises in every new energy regime it has entered. Even within the basic framework of QCD and electroweak theories, unanticipated phenomena may be found. There may be no top quark. There may be rare decay modes of familiar particles into unexpected final states. There may exist new kinds of particles which we cannot even imagine. All machines -- high energy and low; pp, $\bar{p}p$, e^+e^- and ep -- have the potential for providing future surprises. However, very heavy particles may be produced only with high energy colliders. Any new particles with electroweak interactions will be produced at an e^+e^- collider. Any new particles with strong interactions will be produced at a pp or $\bar{p}p$ collider. Who knows what new particles will be produced in an ep collider, for no such machine has ever been built. Each kind of machine complements every other.

Even within the framework of QCD and electroweak theory, it may happen that quarks and/or leptons are themselves composites of simpler constituents -- a possibility which is suggested to some by the proliferation of families of quarks and leptons. Experiments on all three basic kinds of colliders, e^+e^-, ep and pp (or $\bar{p}p$) should be sensitive to compositeness on a very short distance scale.

Finally, we still do not know whether QCD and electroweak theories are really correct or, if correct, only have a restricted range of validity. Remarkable but controversial cosmic ray experiments have hinted at the existence of strong-interaction phenomena at collider energies, phenomena which could signal a radical departure from the picture at present energies. The existence of isolated, fractionally-charged quarks would likewise require, at the least, a major revision of QCD as it now stands.

The weak interaction likewise may not be described within the general gauge-theory framework which is now very widely accepted. Even in the unlikely possibility that the weak bosons do not exist, there is good reason to believe that the natural energy scale of the weak force must lie below 1 TeV, an energy scale accessible to the Fermilab $p\bar{p}$ collider. There are strong reasons to believe this scale in fact to be less than 200 GeV, a regime which is accessible to ISABELLE and especially to ep colliders.

Balance and Diversity of the Program

It should be apparent from the above discussion that diversity of approach is an invaluable asset. Many phenomena are best seen with probes involving electrons, both e^+e^- and ep. Others benefit from the extra center-of-mass energy available in proton-proton or proton-antiproton colliders. Still others require the high intensity and variety of beams in fixed-target accelerator facilities, high energy and low; while yet others, such as the proton decay experiment, do not need an accelerator at all.

Therefore, whether or not the present optimism is vindicated, a balanced and diversified High Energy Physics program for the 1980's will be a rich one and will have a basic impact on our view of the subnuclear world.

Intellectual and Technological Applications to Other Fields

Although the primary justification of High Energy Physics is the drive for a deeper understanding of the fundamental nature of matter there are a number of direct and indirect by-products of the field.

The enormous development of the theory of quantum fields has had major applications to solid state theory and to condensed matter research. The progress of elementary particle physics has provided tools for astrophysics and cosmology, for example in the study of neutron stars and in the extrapolation back to the early stages of the big bang universe. The recent development of a precise theoretical structure has reawakened the interest of many pure mathematicians in physics and led to fruitful exchange and mutual stimulation. The development of a quantitative theory of the strong interactions provides a foundation for nuclear theory. Indeed an increasing fraction of the present program in nuclear physics deals with QCD and electroweak phenomena. This might eventually lead to the prediction of novel nuclear phenomena or new applications.

The experimental accomplishments of the last decade were made possible by a remarkable series of technological developments. Some of these are listed below:

(1) The science and technology of producing, focusing, transporting, accelerating, and storing high energy particle beams has made great progress in the last decade. It is interesting to note that it was the high level of this technology which has made the heavy

ion approach to inertial fusion an exciting and hopeful approach to fusion energy. Indeed, the heavy ion ignited inertial fusion idea was initiated and developed by high energy accelerator physicists. The scanning electron microscope was inspired by methods of high energy particle beam technology and indeed was developed by a high energy physicist. We also note that the physics of particle beams has been applied to research in cancer detection and treatment.

(2) The applications of the synchrotron radiation emitted by the circulating beam in an electron storage ring were developed to a high level because of the importance of this device in high energy physics research. This has led to the controlled use of synchrotron light for a very large and important class of investigations in several fields, solid state physics, surface physics, biology, and micro-circuit fabrication.

(3) The development of particle detectors has continued at a remarkable pace. The resolution and data handling capability of high energy physics research instruments have increased enormously in the last decade. Multiwire proportional and drift chambers were developed and widely used. One important application of these instrumentation techniques is in medical tomography. In addition to the direct application of the detectors themselves, the ideas and system concepts in High Energy Physics are of value for applications. We note that the basic principle of the CAT scanner was developed by a physicist who was a High Energy Physics principal investigator at the time.

(4) The ongoing development of large-scale superconducting magnet projects has been, and continues to be, pioneered by high energy physicists. This technology has led to successful application in the construction of large magnets for the generation of electricity through magnetohydrodynamics (MHD) as well as magnets for fusion energy, both for confinement and for ohmic heating. The development of large-scale superconducting systems may also have important applications to the efficient transmission of electrical energy.

(5) The Winston cone, originally developed for a Cerenkov counter at ANL, has found application in the field of solar energy as a moderately concentrating, nontracking collector. This collector design has potential for good performance at low cost and is currently under industrial development.

High Energy Physics, both experimental and theoretical, may be expected to continue to generate new developments of importance on a wider scale such as those indicated above.

SECTION III

THE CURRENT EXPERIMENTAL HIGH ENERGY PHYSICS PROGRAM

The U.S. High Energy Physics program depends on particle accelerators and storage ring devices located at three national Laboratories supported by the U.S. Department of Energy (Brookhaven National Laboratory, Fermi National Accelerator Laboratory, and Stanford Linear Accelerator Center) and the Cornell University Newman Laboratory of Nuclear Studies supported by the National Science Foundation. Because the various facilities involve different kinds of particles, particle energies and intensities, and targets, the experiments performed at these facilities address many different aspects of High Energy Physics. This section outlines some of the physics activities and opportunities associated with the accelerators operating and under construction in the U.S. High Energy Physics program. Important experimental research supported by the U.S. program, but not associated with high energy accelerators, is also identified.

Fixed Target Program at BNL

The fixed target experimental program at Brookhaven National Laboratory is based on the 30 GeV proton synchrotron, the AGS, which provides not only intense external beams of high energy protons but also secondary separated beams of pions and kaons and very good neutrino beams. Many major discoveries have been made with the AGS throughout its distinguished history. Some of the highlights are: two neutrinos; the "Omega Minus" hyperon; CP violation; the J particle; and the charmed baryon. The physics program now focuses on: studies of the weak interaction via neutrino interactions and K meson decays; the search for neutrino oscillations; hadronic interactions via spectroscopy, dynamics and polarization studies; QED via measurements of muonic atoms with helium nuclei; and a strong program of intermediate energy nuclear physics studying kaon-hypernuclei and K^- - and hyperon-induced atomic X-ray emission.

The AGS has the highest average proton intensity available. It provides up to 10^{13} protons per pulse every 1.2 seconds for neutrino physics and, with a one second flattop, every 2.5 seconds for electronic counter experiments.

Fixed Target Program at SLAC

The present SLAC linac remains the highest energy and highest intensity electron accelerator in the world. The SLAC accelerator also provides positrons and longitudinally polarized photon beams. In normal operation the SLAC electron beam has an energy of 23 GeV, while in the SLED I mode it has achieved energies up to 33 GeV.

The fixed target program is now centered on three facilities -- the electron spectrometers, the multiparticle spectrometer (LASS), and the hybrid bubble chamber facility.

The electron spectrometers have been used in a long and productive program that has led to some of the important discoveries of the past 10 years (viz. the deep inelastic electron scattering and the parity violation measurements). The bubble chamber has had a very productive program of photo-production and hadronic interaction studies and is now being used to study charmed baryon production by monochromatic photons, and also to measure their lifetimes. The LASS spectrometer is being used for a programmatic study of the systematics of strange quark spectroscopy.

Future experimental topics involve more polarized electron scattering experiments to measure parity violation at large q^2, and Møller scattering and very high sensitivity polarized photoproduction experiments.

400 GeV Fixed Target Program at Fermilab

The physics program at Fermilab uses a 400 GeV proton synchrotron. Proton beams of > 2×10^{13} particles per pulse are produced with a repetition rate of one burst each 10 seconds. Proton beams can be directed simultaneously onto several targets, producing secondary beams of hadrons, muons, photons, and neutrinos. As many as 15 experiments can be run simultaneously, resulting in a program of great diversity.

In its early days (1972-74) the program was dominated by the exploration of a new energy regime. Experiments ranged from extensions of hadronic physics at lower energies to searches for quarks and monopoles. As the machine matured, the main thrust of the experimental program focused on the study of hadronic substructure, the interaction of subnuclear constituents with one another, and the investigation of new forms of matter such as charm.

A major discovery of the Fermilab physics program was that of the upsilon resonance, leading to its interpretation in terms of a fifth quark, the b-quark. The substructure of nucleons has been studied by means of muon and neutrino scattering. The structure of pions has been studied by means of dimuon production in pion-nucleon collisions. Static properties of hadrons such as hyperon magnetic moments have been measured. This program has also revealed unexpected dynamical effects such as the polarization of hyperons produced at large transverse momentum. Numerous measurements on the production and properties of charmed particles have been carried out. Notable are direct measurements of the lifetime of charmed particles.

SPEAR

The SPEAR electron positron storage ring at SLAC operates with a center-of-mass energy region between 2.5 and 8 GeV. The research program at SPEAR has produced a large number of spectacular results including the discoveries of members of the psi family, the charmed particles, the tau lepton, and the production of two-jet hadronic structures. With the commissioning of the higher energy storage ring PEP, the SPEAR facility is now operating 50 percent of the time for particle physics, the other 50 percent going into research with synchrotron light. There remains a rich area of work in the SPEAR energy range that focuses on the systematic study of states involving the c-quark. The recent discovery of the "charmed eta" proves the continuing vitality of the SPEAR program.

CESR

The Cornell electron-positron storage ring CESR, designed to operate at center-of-mass energies between 8 and 16 GeV, obtained its first luminosity in September 1979. During the subsequent 6 months, experimenters at both CESR interaction regions have obtained important results, including confirmation of the three narrow upsilon states and the discovery of what may be a broader upsilon resonance, probably above threshold for the pair production of mesons with b-quarks. The detailed study of states involving b-quarks, both mesons and baryons, including spectroscopy and decay properties, represents a rich and unexplored area to which the CESR facility has an ideally matched energy domain.

PEP

The PEP e^+e^- storage ring at SLAC, which covers the range from about 8 to 36 GeV in center-of-mass energy, is just in the process of commissioning. Although the same energy region has already been given an exploratory study at the German storage ring PETRA, one may expect that increased luminosity and a wide variety of detectors will allow the PEP program to make substantial advances in such areas as tests of QCD, studies of jet structure and fragmentation, measurements of interference between weak and electromagnetic currents, and searches for new particles.

Fermilab 1000 GeV Superconducting Accelerator and Storage Ring

A major new accelerator storage ring complex is being developed at Fermilab by means of adding a string of approximately 1000 superconducting magnets placed directly beneath the present 400 GeV accelerator ring. This allows for a variety of thrusts: large power cost savings can be realized by running the present accelerator up to 150 GeV and then transferring the beams into the superconducting ring for attaining 400-500 GeV (Energy Saver mode); the superconducting ring will also provide the opportunity to reach very high energies by colliding beams head on. This will involve producing and accumulating antiprotons and then simultaneously accelerating these antiprotons and a counter-rotating beam of protons in the superconducting ring to collide with each other at a center-of-mass energy of 2000 GeV (Tevatron I); the superconducting ring also allows for accelerating the primary protons up to 1000 GeV and then extracting them, thereby doubling the energy of the fixed target program involving neutrino, muon, and hadron collisions with high intensities and energies (Tevatron II).

The physics opportunities provided by proton-antiproton collisions in the superconducting ring are unique; nowhere else in the world will collisions at such high energies be available for experimental study. These collisions are expected to provide much new information on constituents of hadrons (quarks), the heavy carriers of weak and electromagnetic forces ($W^{\pm}$ and Z^{0} particles), and the interactions among these particles. Furthermore, this project represents a major thrust into the unknown with good prospects for major discoveries. The higher energies made available by the superconducting ring to the fixed target program make it possible to produce and study particles in new ways not possible with the 400 GeV program and to extend to higher energies many of the studies now carried out at 400 GeV.

ISABELLE

The major authorized project for the future is the 400 GeV on 400 GeV colliding proton machine ISABELLE at BNL. ISABELLE will provide an order-of-magnitude increase in energy available for new-particle production by proton-proton collisions. It represents a major expansion of the frontier of High Energy Physics. An extremely attractive attribute of ISABELLE is its very high intensity which will allow rapid data accumulation as well as exploration of rare processes. ISABELLE will therefore allow searches for the $W^{\pm}$ and Z^{0} particles, possible new members of the quark family, and most important, for the completely unexpected, which historically has accompanied the advent of nearly all new machines.

Non-Accelerator Projects

High Energy Physics never did and never will consist solely of experiments done at conventional accelerators. Much of importance can only be discovered in other ways. Current theories predict that protons live approximately 10^{31} years. Several U.S. experiments will test this prediction. These novel experiments involve very massive underground detectors. They may find proton decay, or reveal surprises such as new cosmic ray phenomena, unstable superheavy particles, or neutrino oscillations. Neutrinos may have mass, and may oscillate, one kind into another. This can be studied in many ways: accelerator neutrinos, intense radioactive sources, beam dumps, the electron spectrum of tritium beta-decay. There already exist hints of effects, and better experiments must be done. Reactors can be used to measure the neutron electric dipole moment and to look for nucleon-antinucleon mixing. Theory suggests the possible existence of unobserved forms of stable matter. Very heavy monopoles may be incident as cosmic rays; very heavy isotopes may be present on Earth. A variety of experiments is called for. Particle physics and astronomy have become closer than ever. Solar neutrinos and neutrino astronomy can tell us as much about neutrinos as about stars.

SECTION IV

ACCELERATOR FACILITIES

This section briefly describes the recently completed accelerator facilities, projects now underway, and some ideas for the future which have been discussed.

RECENTLY COMPLETED FACILITIES

CESR

In the fall of 1977, the National Science Foundation approved an e^+e^- colliding beam facility, CESR, at Cornell University. The design energy was 8 GeV per beam, expandable to 10 GeV. This machine was completed 1 year ahead of the contract period and first luminosity from colliding beams was obtained in September 1979, 2 years after approval of the project. Costs for the storage ring, a large magnetic detector, and computing facility were $400K less than the total budget of $20.7M.

The energy region for which this machine was designed has turned out to be a very interesting one because of the upsilon and related particles, and a productive experimental program is now underway, with the first interesting results already published.

PEP

A major e^+e^- colliding beam facility, PEP, was completed at the Stanford Linear Accelerator Center in April 1980 on time and at cost. This machine was designed for maximum luminosity at 15 GeV per beam and maximum energy 18 GeV, expandable to 24 GeV if a substantial upgrade were to be carried out in the future. This machine was constructed as a joint SLAC/LBL project.

In view of the 2 years earlier operation of the similar machine, PETRA, at DESY, the luminosity of PEP will be of critical importance. It is too early to tell what this luminosity will be but there is expectation that it will exceed that achieved at PETRA.

The experimental program at PEP will begin soon; a number of detectors are installed and others will be completed during the coming year.

PROJECTS UNDERWAY

Fermilab Energy Saver/Tevatron

(1) General

Although the Fermilab Energy Saver-Tevatron I-Tevatron II projects have been defined as three separate construction projects they represent one integrated program to be accomplished in three phases: the Energy Saver and related 500 GeV programs, the 1 TeV fixed target program, and a $\bar{p}p$ collider program at 1 TeV on 1 TeV. This is clearly an ambitious program involving significant R&D, and it will require vigorous effort by the staff and users of Fermilab for effective achievement. It is one of the most exciting efforts currently underway in the U.S. High Energy Physics program. It is important that the authorization of the two Tevatron projects proceed in a timely fashion in order that the Fermilab management can implement an optimal program utilizing the Energy Saver and Tevatron facilities. It is recognized that surprises may be in store due to the lack of operating experience with major large superconducting magnet and cryogenic systems in accelerator applications. These new challenges will have to be met with vigor and additional R&D support may be required. Schedule risk remains high.

(2) The Fermilab Energy Saver

This project is underway at Fermilab and is scheduled for completion in 1982. After experiencing a number of difficulties in the R&D program for the development of production-quality magnets,the Fermilab personnel believe they will soon be ready to proceed with full production of the 800 cryostatically-encased superconducting dipoles. They have produced some 200 production-quality coil structures which they believe are satisfactory. Recently, a problem occurred with the assembly of these coil structures in the cryostats, which they now believe has been solved so that full-scale production on one shift per day can begin soon. In order to meet the schedule, two-shift production must begin in the fall of 1980.

The Subpanel feels that considerable progress has been made in overcoming the problems of constructing superconducting magnets, but it is concerned that more problems may be encountered which will require additional R&D efforts for their solution. Such problems could result in a delay in completion of the project.

Fermilab has developed a number of critical quality assurance and magnet measurement programs to ensure a satisfactory design. This has been a difficult pioneering effort in an area of critical importance in the development of future accelerators and in the development of other large superconducting magnet and cryogenic systems. Fermilab has also tested strings of magnets and cryostats in a variety of configurations to ensure that their design for magnet protection, controls, cryogenic and other systems will perform adequately. They are in the process of installing a string of accelerator-type superconducting magnets to bring the present 400 GeV proton beam into the Meson Laboratory. This system, operating continuously with a particle beam in an environment similar to an accelerator, will be another important systems test. Other elements of the Energy Saver, i.e., the refrigeration system, and the rf system, are under construction, and testing and commissioning of major subsystems are occurring.

Operation in the Energy Saver mode is scheduled to begin in 1982 with energies up to 500 GeV, resulting in significant savings in power compared to the present fixed target operation.

(3) 1 TeV Fixed Target Program

The Tevatron I-Tevatron II program will allow the superconducting accelerator to reach proton energies near 1000 GeV with adequate repetition rate and will provide extracted 1 TeV proton beams and experimental area facilities for a 1 TeV fixed target research program.

This facility will provide the highest energy secondary particle beams in the world for many years. The opportunities afforded for experimental investigation of important fundamental issues in the study of the basic constituents of matter are rich and unique.

The research and development work leading to this design has been extensive. A Summer Study was held in 1976, followed in the last year by workshops treating the individual experimental areas. There are many technical papers available on particular studies. At every stage of the work leading to this design report, experimenters and users from the entire national and international particle physics community have participated and contributed.

The 1 TeV fixed target program, Tevatron II, includes:

(a) The construction of a slow-extraction system for 1 TeV protons.

(b) Improvement of the external beams switchyard to 1 TeV and the provision for 1 TeV targeting in the present external experimental area.

(c) The construction of facilities for new secondary-beam enclosures and support facilities in each external experimental area designed to take full advantage of the fixed target physics opportunities of the Tevatron. The total number of secondary beams will not be increased because the new beams will replace existing ones, but the energies and intensities of the secondary beams will be significantly increased.

This project will build on the experience achieved with the Energy Saver program in accelerator, extraction, and proton beam targeting systems.

(4) $\bar{p}p$ Collider

Fermilab plans to use the 1 TeV superconducting ring for beam storage of counter-rotating antiproton ($\bar{p}$) and proton (p) beams. Colliding beam experiments can then be performed in a special straight section, where beams are focused to very small cross section to enhance the interaction rate (low beta section).

The maximum reaction energy of 2000 GeV between colliding particles and the expected high interaction rate expressed by the luminosity $L = 10^{30}\ cm^{-2}s^{-1}$ will make this $\bar{p}p$ collider a truly unique facility in the world for many years to come.

The most difficult part of the project is the collection of intense current of antiprotons in 12 bunches with 10^{10} antiprotons in each of them. Antiprotons are produced by bombarding a special target with 80 GeV protons. Because the production efficiency is very low (3×10^{-6}), antiproton accumulation is a slow (5 hours) process. Antiprotons created in the target with energies of about 4.5 GeV are injected into a special precooler ring, where particle momentum spread is reduced by so-called "stochastic cooling." This makes it possible to decelerate the antiprotons to an energy of 200 MeV and inject them into a second special ring (the "cooler ring") where they are accumulated and their oscillations and momentum are further reduced by interaction with an intense electron beam ("electron cooling"). When the required total number of antiprotons (12×10^{10}) has been accumulated, they are transferred to the main proton synchrotron, accelerated to 150 GeV, extracted from the main ring and injected, in the form of 12 bunches, into the superconducting Energy Saver ring. After an equal number of proton bunches (each with 10^{11} protons) has been injected in the opposite direction in the Energy Saver ring, protons and antiprotons are slowly accelerated to the maximum energy of 1000 GeV.

Because of the long accumulation time for antiprotons, the reliability of the accelerator system, especially that of the "cooler ring," is of major concern. There are also new techniques involved, which have been tested to some extent but which need further investigation. The most critical components of this colliding beam scheme are being studied extensively at present. The production target for the $\bar{p}$ has to withstand a beam burst of 1×10^{13} protons at 80 GeV with a burst length of 1.6 microseconds and a beam spot of approximately 0.4 mm diameter. A number of studies have been conducted to address this problem and an appropriate R&D program is planned. Alternatives such as sweeping the beam over the target are being considered.

A version of a 200 MeV electron cooling ring exists at Fermilab including an electron gun for the electron cooling. First electron cooling experiments will be done in the near future. In the same ring longitudinal and transverse cooling experiments with stochastic cooling have already been carried out successfully.

In the main ring, beam storage studies and rf manipulation ex riments with the goal of bunching a large fraction of the beam at 80 GeV into 1/13 of the circumference are underway.

In 1979 a Conceptual Design Report of the $\bar{p}$-source was published and submitted to DOE for FY 1981 approval.

Currently Fermilab plans to build two experimental areas for $p\bar{p}$ collisions. Fermilab, together with ANL, LBL, the University of Wisconsin, and the Institute for Nuclear Physics at Novosibirsk, USSR, has formed a collaboration supporting the R&D and construction efforts for the $p\bar{p}$ project.

Such an advanced accelerator project needs commensurate R&D efforts. To date, Fermilab is heavily engaged in the Energy Saver R&D and construction. As a result, the $p\bar{p}$ Collider R&D has slipped. It is hoped that the Collider can soon receive the necessary attention.

In comparison to the CERN $p\bar{p}$ scheme, the Fermilab design has a number of advantages. Due to the larger momentum of the colliding beams (2 x 1000 GeV compared to 2 x 270 GeV) and the smaller emittances, the design luminosity approximately 10^{30} cm^{-2} sec^{-1} can be obtained with one-fifth of the antiproton current. Hence the accumulation time is expected to be 5 hours rather than 24 hours as required at CERN.

There are a number of potential improvements such as increased solid angle in the $\bar{p}$ acceptance, increased target luminosity, reuse of the antiprotons, etc, which could increase the number of $\bar{p}$ in the same collection time and hence increase the luminosity or reduce the collection time and ease operational problems.

The uniqueness of this project and its rich research potential, particularly the very high energy achievable, make it most desirable to proceed expeditiously.

<u>BNL ISABELLE</u>

ISABELLE is an intersecting ring accelerator and storage ring facility designed to collide opposing beams of protons at center-of-mass energies between 60 and 800 GeV. The design luminosity at the "standard" crossing points is 2 x 10^{32} cm^{-2} sec^{-1}. By provision of special focusing elements at the crossing points, the design indicates that a peak luminosity of 10^{33} cm^{-2} sec^{-1} might be achieved.

The accelerator itself consists of two independent rings of superconducting magnets of 50 kilogauss peak field, arranged roughly on a circle of 3834 meter circumference in such a way that the opposing beams can be brought into collision at 6 places around the ring.

The status of the project is as follows:

(1) Civil Construction

ISABELLE construction was authorized in FY 1978. A substantial portion of the conventional (civil) construction is now underway. One-third of the tunnel is almost complete. Work is underway on most of the rest of the tunnel.

Two of the interaction areas are now under construction. The civil construction is on schedule at cost. The tunnel is scheduled to be finished in mid-1981 with the rest of the civil construction to be completed in early 1983. Experience to date indicates that this schedule is realistic.

(2) Standard Accelerator Components

Design and model work is now underway on the accelerator components other than the magnet and refrigeration system, which are discussed below. Some of these items are injection and ejection equipment, rf stacking and accelerating systems, vacuum system, magnet power supplies, and instrumentation and control systems.

(3) Refrigeration System

Extensive work on the refrigeration system has been underway for some time. Heat loads for most major components have been measured (transfer lines excepted) and found to be within design specification. Considerable R&D on some of the critical components such as special heat exchangers, oil removal system, etc., has been carried out. The design for the main cold box for the system is underway. The system is designed to rely on turbine expanders. Small, gas-bearing expanders in test refrigerators have given considerable trouble but, from other experience at BNL, the ISABELLE group feels that the larger turbines for the main refrigerator, which use oil bearings, will be more reliable. This has yet to be demonstrated. No large-scale tests of the cryogenic system have been carried out under realistic accelerator operating conditions.

(4) Superconducting Magnets

The current ISABELLE design calls for 732 dipoles of 5 Tesla (T) field strength and 348 quadrupoles of $60 Tm^{-1}$ gradient. The manufacture of the dipole magnets has encountered unexpected difficulty. The magnets produced so far do not reach the design maximum fields or field quality. In response, BNL has instituted an R&D program to analyze the magnet design with the aim of correcting design deficiencies. A major element in this program is the set-up of manufacturing facilities at BNL for the fabrication of full-scale magnets. By experimental study of magnets built with tightly controlled design variations, supplemented by theoretical analysis, the magnet development group hopes to uncover those features of the current design which result in substandard performance. The tooling available could be capable of manufacturing four sets of dipole coils per month. The plan is to produce at least two sets per month at BNL and ultimately to have most of the magnets produced by industry. This will require careful quality control on the production magnets. In addition to the field strength and training problems, the Laboratory recognizes that the problems of magnetic field quality and quench propagation must also be resolved.

The quadrupole magnets built so far at BNL are reported to be successful in terms of overall performance. Pilot production of quadrupoles at the BNL site is to begin shortly. Fifteen quadrupoles will be manufactured and tested thoroughly before proceeding further with production.

In an attempt to bring more experimental and analytical effort to bear on the magnet problems the organizational structure of the ISABELLE construction staff has been changed and the number of personnel in the magnet program has been increased during the past 2 years almost fourfold, up to 130. Outside help has been sought and some outside experimental or analytical work is being done at LBL and the MIT Magnet Lab. Additionally, the BNL management has set up an internal mechanism for exploring alternative magnet designs, should the problems with the current design prove intractable.

At this time, the BNL management hopes that pilot production of dipoles can begin in mid-1981. Current information from the R&D program is insufficient to assess the likelihood that this goal can be met. If it is not, the ISABELLE project will encounter significant delays.

For achievement of overall project goals, the BNL management may need to enhance technical leadership in a number of areas. In addition, a broader participation in ISABELLE by other BNL staff, particularly from the Physics Department and the AGS, and, perhaps, broader use of personnel from other Laboratories and industry may be necessary. BNL should continue to strengthen the practice of periodic in-depth technical review, involving outside expertise.

Although significant steps have been taken and are being taken by BNL management to solve ISABELLE problems, the very nature of the hardware involved and the lack of experience anywhere with systems of similar technological complexity increase the likelihood of further schedule slippage and the need for additional R&D. In meeting this very difficult challenge, technical leadership will be crucial.

PROPOSALS AND IDEAS FOR NEW FACILITIES

SLAC Linear Collider

(1) Description of the Project

SLAC has proposed to build a novel linear collider device (the Stanford Linear Collider or "SLC"), which would provide electron-positron collisions with an interaction energy (center-of-mass) of 100 GeV and a luminosity of $10^{30}\ cm^{-2}\ sec^{-1}$. Based in large part on the SLAC linac, the SLC would provide colliding 50 GeV beams rapidly and at moderate cost.

The proponents point out that the SLC could be the first of a new class of electron-positron colliding beam facilities. It now seems relatively certain that circular colliding beam storage rings for electrons would not be feasible at center of mass energies above 300 GeV or so. On the other hand, there are no known fundamental technical limits to the energy which might be reached with linear collider devices until one reaches center-of-mass energies much beyond 600 GeV. The costs of a linear collider will be less than those of a circular machine at some beam energy in the few hundred GeV region. Nevertheless, the costs and power consumption of a linear machine are also likely to be very high. One of the goals of the SLC project is to demonstrate the feasibility of the linear collider concept, and to begin exploration of ways to reduce the expected costs and power.

In the operation of SLC, two microscopic, high-intensity particle bunches -- one of electrons and one of positrons -- are accelerated to an energy of 50 GeV in the Stanford two-mile accelerator after which the two bunches are separated, and transported in separate, reversed-double-bends (like "question marks") so as to be brought into a head-on collision. At the collision point -- where the high energy interactions are studied -- the bunches are compressed by focusing magnets to diameters of a few micrometers in order to achieve the desired interaction rate. Bunch pairs arrive at the collision point at the basic pulse rate of the linear accelerator (180 per second), and after the collision the electrons and positrons are disposed of in a dump.

The SLC requires major modifications and additions to the existing SLAC facilities.

(a) The linear accelerator must be modified to obtain higher electric fields, and thus to reach a peak electron energy of 50 GeV (to be compared with the present value of 33 GeV).

(b) Special sources of both electrons and positrons must be developed and built to provide the two microscopic beam bunches. Each bunch must have a duration of a few picoseconds and a current of a few kiloamperes, providing 5×10^{10} particles (to be compared with the current maximum performance of about 2×10^{9} electrons in a single bunch).

(c) The instrumentation and control system for the steering of the beam along its axis, and for the focusing of the particles along the full length of the accelerator must be upgraded in a major way to permit acceleration of the two particle bunches down the length of the accelerator, without an unacceptable increase in their lateral sizes.

(d) The reverse-bend magnetic channels (which bring the bunches into collision) and the tunnels to house them must be designed and constructed.

(e) An experimental hall and experimental physics apparatus must be provided at the intersection point.

Stanford has proposed to build the SLC in a period of about 2-1/2 years beginning in October 1981 at a cost of $63M (FY 1980).

(2) Appraisal of the Project

The Stanford Linear Collider is a highly imaginative and innovative project which will involve a large number of diverse and novel accelerator developments -- from the realization of the very high-intensity sources of electrons and positrons to the achievement of the microscopic beam sizes at the interaction point. The Subpanel did not find any fundamental difficulties with the concept and proposed design during the short time the design report has been available. It believes that many of the required technical developments can be made in a straightforward manner. Some developments will require a high level of intellectual and technical effort to reach the required performance.

One major concern would be the capacity to accelerate a high intensity bunch along the full length of the accelerator with no significant increase in the effective dimensions of the bunch. Another concern is that relatively minor technical imperfections in each of the several components of the SLC would so reduce the overall efficiency in the acceleration and transport of the electrons and positrons that the SLC would fail to reach the design luminosity -- which some feel is perhaps already marginal. On the other hand, the SLC proponents have pointed out several areas in the system in which their evaluations are conservative and where additional factors of two or three in luminosity may be achieved after further study and development.

In summary, the Subpanel views the SLC as a fascinating and challenging new departure in accelerator technology which might in the near term provide, at relatively low cost, electron-positron collisions at an energy of 100 GeV, and which might well demonstrate an avenue which could possibly reach energies of the order of 1 TeV in the distant future.

The Subpanel has found no basic technical flaws in the SLC proposal and is impressed by the large amount of new knowledge about significant extensions of current techniques that should and would be gained if the project were to proceed. The Subpanel sees important opportunities for making advances in many aspects of accelerator science, and the intriguing chance of exploring methods which might lead the way to the study of electron-positron collision at very high energies.

The Subpanel believes that it is premature to attempt a detailed comparison of the SLC project with the colliding beam storage ring being contemplated by Cornell.

Cornell e^+e^- Storage Ring

Cornell has produced a preliminary design of an electron-positron colliding beam storage ring which would provide a maximum energy of 50 GeV per beam and a luminosity of $3 \times 10^{31} cm^{-2} sec^{-1}$ in each of the 4 interaction regions. The project would achieve significant economies of power and size by the use of superconducting rf cavities and would extend the U.S. energy range of electron storage rings significantly beyond the PEP energy (18 GeV per beam), and into the region where the Z^0 particle should be produced and where a very exciting field of physics will no doubt be found.

In a high energy storage ring, synchrotron radiation causes energy loss of the circulating electrons and necessitates continuous beam acceleration with powerful radiofrequency systems. Power consumption of these systems becomes one of the dominating aspects of these machines. The optimization of construction and operating costs depends critically on the magnitude of the required radiofrequency power. Superconducting rf cavities offer very large savings in operating costs and allow the optimized storage ring to be built with substantially smaller radius, thereby roughly halving also the capital cost. The Cornell Newman Laboratory of Nuclear Studies is leading the world in the development of superconducting cavities for circular accelerators. The maximum accelerating field of 3 MV/m for the proposed collider is substantially below the maximum field of 4.5 MV/m reached already in their test cavities. The proposed R&D effort in the next 2 years should allow the envisaged accelerating cavities to be tested in the CESR storage ring. Questions of parasitic mode damping, synchrotron radiation effects on the cavities and the economy of a large technical system could then be answered with confidence and might then lead to a formal proposal. This approach seems to be prudent and sensible. Since the proposed storage ring, with the exception of the radiofrequency system, is standard and well understood, such a machine -- after the R&D effort -- could be built with little technical risk. It would be unique in the U.S. and should -- because of its smaller radius -- produce larger luminosity than the proposed 86 GeV x 86 GeV storage ring at CERN in the region below 50 GeV x 50 GeV. The development of large superconducting accelerating systems would have a great impact on all new circular electron and even proton accelerators and may lead to large power savings, and it could have other applications.

ep Colliders

Interest in electron-proton colliding beams in North America has been expressed by a Columbia University group and independently by a group composed of several Canadian institutions. Both groups have considered a 10 GeV electron storage ring tangent to the Fermilab Tevatron ring, providing center-of-mass energies up to 200 GeV.

Since no detailed design of this facility has been presented, only a few general remarks are included here. The combination of high luminosity ($>10^{31} cm^{-2} s^{-1}$) and polarized electron beams required by ep physics introduces severe problems of beam dynamics that should be carefully studied. Other problems requiring investigation are the beam-beam interaction, the stability of high current bunched beams, the depolarizing effects, and the comparison of various possible spin rotation schemes. Experimental information on some of these problems will become available during the next 2 years both in Europe and the U.S.

The possibility of using ISABELLE instead of the Tevatron should be seriously considered. There are potential advantages which compensate for the lower energy of ISABELLE, such as the longer ISABELLE straight section, the exclusive use of one interaction area, and the fact that ISABELLE does not have to supply beams for a fixed target program. All these facts and the overall impact of the ep program on the host laboratory should be evaluated in any future ep proposal.

Higher Energy Proton Accelerators and $\bar{p}p$ Colliders

Studies of multi-TeV proton accelerator/colliders are being pursued at Fermilab. Specifically, the studies are focused on a 5 TeV proton synchrotron/collider, 5 km in diameter and using 8 to 10 T superconducting magnets. A considerable amount of R&D work will be required before such high field magnets suitable for accelerators can become a reality.

SECTION V

UNIVERSITY PROGRAMS AND FACILITY UTILIZATION

Roughly 1,100 experimentalists and approximately one-half that many theoreticians are currently active in U.S. High Energy Physics. The vitality of this community depends primarily on the existence of accelerator facilities which allow one to probe interesting new phenomena, and on effective utilization of those facilities. We discuss here the efficiency of the present program, in terms of both accelerator utilization and manpower.

Universities

The High Energy Physics program depends upon the involvement of physicists at the major educational institutions of the United States, and benefits from the efficiency of experimentation which is possible at large central Laboratories. The diversity of initiatives, competition among ideas, and high quality of talent that results from the participation of University groups lends breadth and originality to the program and is fundamental to it. Most important, the participation of students and University scientists in High Energy Physics is essential to the future of the field, and contributes to the vitality of the University community and the education received by students of all the physical sciences.

There are, however, increasing problems which confront the University groups. It is essential that the University researchers be able to spend a significant amount of time at their home institutions so that they may interact effectively with students, both graduate and undergraduate, and with other faculty. Hence, while the running of an experiment and data-taking must occur at the Laboratories, the design, building, and testing of equipment, and the extensive data analysis required to yield physics results is often most profitably performed at the Universities. This approach has become increasingly difficult, however, for several reasons. First, the computational facilities of the Universities have not kept pace with evolving needs, forcing many groups to reside at the Laboratories long after data-taking is complete as they perform the analysis at Laboratory facilities. Several groups have managed much of their analysis at home institutions very efficiently, using small, but powerful DOE- or NSF-supported computers. The policy of distributed computing, where it is efficient and at a level that is in balance with the major facilities necessary at the Laboratories, should be encouraged. Secondly, the technical complexities of each experiment have grown to such an extent that the size of a typical team of physicists from a University and the time required for the pursuit of an experimental program have steadily grown. Roughly one-third of University research support is used for faculty salary supplements and another one-third for non-faculty personnel, principally post-doctoral fellows. The remaining support available for technical development has dwindled as indirect costs charged by the Universities and the costs of travel have soared.