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Opening Remarks
by Lawrence Berkeley Laboratory Director David A. Shirley

50th
ANNIVERSARY
1931-1981

This year, 1981, marks the 50th anniversary of the Ernest Orlando Lawrence Berkeley Laboratory. The Laboratory was started by Ernest Lawrence in 1931 in an old wooden building on the University of California campus. By late 1932 a research program based on the newly invented cyclotron was well under way, and the Radiation Laboratory, as it was called then, had achieved international recognition. It received further official status in 1936 when the University of California Regents appointed Lawrence "Director of the Radiation Laboratory" and established a separate budget and staff for its research activities. Today the Laboratory is different in many ways from the Radiation Laboratory, but some essential characteristics developed during those early days still shape the direction and spirit of the organization. LBL's major concern is with scientific research at the frontiers of knowledge where the intellectual challenges are the greatest. It uses the multidisciplinary approach, pioneered by Lawrence, to tackle major problems. Where appropriate, it uses large and complex experimental facilities. It is deeply concerned with expanding fundamental scientific knowledge for national technological needs and for solving problems ranging from the energy issues facing our nation to human health and suffering. It has a strong commitment to the training of students and the advanced training of graduates. LBL has produced eight Nobel Laureates and presently has twenty-four members in the National Academy of Sciences.

Luis Alvarez, working with his son, geologist Walter Alvarez, initiated a research project a few years ago that is shedding new light on the great extinction in which half the life forms on earth were destroyed some sixty-five million years ago. Their findings have attracted wide attention and have stimulated spirited debate among geologists, paleontologists, biologists and physicists around the world.

Alvarez, a Nobel Laureate, has been described as an experimental physicist in the classical sense; he is interested in everything under the sun - and beyond. A native of San Francisco, he earned his B.S., M.S. and Ph.D. degrees at the University of Chicago. In the 1930s he participated with Professor Arthur H. Compton in the discovery that cosmic rays are mostly positively charged atomic particles. He joined the Radiation Laboratory of the University of California in Berkeley in 1936. From 1940-45 he was on leave to the Massachusetts Institute of Technology, the University of Chicago and the University of California's Los Alamos Laboratory. Returning to Berkeley after World War II, he resumed his career as a professor of physics and research scientist.

In 1968 Alvarez won the Nobel Prize for the development of the hydrogen bubble chamber and the use of it to discover a large number of previously unknown elementary particles or "resonances." His long list of achievements includes more than 30 patented inventions, for which he was inducted into the Inventors Hall of Fame in 1978. Other awards include the Collier Trophy from the National Aeronautics Association, 1946; the U.S. Medal of Merit, 1947; the Einstein Award, 1961; the National Medal of Science, 1968, and the Albert A. Michelson Award of the Case Institute of Technology, 1965. He is a member of the National Academy of Sciences, the National Academy of Engineering, Sciences and the American Physical Society (President 1969), American Academy of Arts and Sciences and the National Academy of Engineering.

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ASTEROIDS AND DINOSAURS

LUIS W. ALVAREZ

Thank you Dave. It's an unexpected pleasure for me to be here talking to you today. The original program which some of you may have seen had the first speaker as Phil Handler, long time President of the National Academy of Sciences. Unfortunately, Phil is very ill in a hospital in Washington and at the very last minute I was asked to fill in as a sort of pinch hitter. I was on the committee that made the arrangements for this occasion and although none of us on the committee ever said so explicitly, we all agreed that none of us should be on the program; all the speakers should be distinguished invited quests. But at the very last minute, you know, "any old port in a storm," so here I am. You might be surprised that I am talking about a subject like asteroids and dinosaurs which has nothing to with any of the programs at the Laboratory but I think Ernest Lawrence would really approve of this because one of the things that he did that was unusual for a physicist in those days was to bring in people from other disciplines and let them share the resources of the Laboratory. Before the war, before the nuclear reactor came into being, the Laboratory here had a corner, so to speak, on all the artificial radioactivity that existed in the world. We could make more radioactive materials than anybody; I guess more than everybody else combined. Ernest went out and beat the bushes to get people to use this because people didn't understand about tracers and their value; they didn't know what you could do with neutrons. Ernest would go out and grab people at the Faculty Club and explain to them what they could do with these marvelous new things and I'm sure he would have enjoyed seeing what the Laboratory has done by sharing its resources, in this case Frank Asaro and Helen Michel and their neutron activation analysis facility. They've shared that with the geological and paleontological communities. So with that introduction I'll tell you that I'm going to be giving a report of work that's been done by a team of four; by scientific standards these days, that's a very small team, because normally a team in high energy physics has about 50 members.

The first slide shows the title page of our paper that was put out by the Laboratory in 1979 and you can see the list of authors up there. These days papers are known by their first authors. So it's very important to have a name starting with A! It used to be that the most important person was first on the list, but now it's the person who's farthest up in the alphabet. The paper announcing the discovery of the psi meson which won Burt Richter his Nobel Prize, had about 50 names on it. It's universally referred to in the literature as Abrams, et al. Burt's name is never mentioned and so you can imagine the surprise of Frank Asaro to find himself next to the bottom on this list of four authors. Since this is my son Walt's birthday I'll have to apologize for an action that I took part in 41 years ago, by not naming him for Ernest Lawrence or Arthur Compton, but rather for his grandfather, Walter. Otherwise he would be at the top, but it really doesn't make any difference. This is completely a team effort and everyone has done something that nobody else on the team could do and that's the thing that makes it fun.

Next slide. Since I'll be talking about something that I just learned about in the last 3 years, maybe some of you haven't learned about it yet so I'll tell you that you can't tell the players without a program and here are the players and this is the program. These are the geological ages. They're named for the animals in the rock.

The old animals, the Paleozoic, then the middle animals, the Mesozoic, and the new animals, the Cenozoic. I'm going to be talking mostly about the boundary between the Cretaceous and the Tertiary or between the Mesozoic and the Cenozoic, which happened 65 million years ago. I'll talk a little bit about the boundary between the Permian and the Triassic which is 225 million years ago and also a little about a boundary about 37 million years ago between the Eocene and the Oligocene. These ages of course come from the study of the radioactivity in the rocks; that's a contribution that physicists have made to geology. So with that table I'll go on and tell you how I got involved in this odd subject.

Next slide please. My son Walt is a geologist as you've been told. And his field of study for the past several years has been paleomagnetism, which has to do with the reversal of the earth's magnetic field, on an irregular basis something like a million years on the average. And he's been studying this in a valley in Italy, the Bottaccione Gorge near Gubbio, Italy which is in the Appenines. He has been telling me about his studies for a long time and I didn't find them terribly exciting; I mean the earth's magnetic field is not something that excites physicists. It's very important to airplane pilots and not so much these days to marine navigators but it used to be terribly important. But one day he brought in this little sample of rock, which is held together by lucite because otherwise it would crumble. At the very bottom is lucite; that's of no geological importance. Then you go into white limestone; that's the limestone that ends at the top of the Cretaceous Period, the end of the Mesozoic. Then Walt showed me this layer of clay, about a centimeter high, and then he pointed out that it was limestone going up from there. He said the limestone coming up to this clay boundary extends for hundreds of meters in this gorge in Italy. And above the clay layer it again goes for hundreds of meters. He said the extraordinary thing is that right at the boundary is the time that the dinosaurs disappeared. He then gave me a little hand magnifying glass and showed me that in the limestone below the boundary there were lots of foraminiferal shells about a millimeter in diameter and above that there were absolutely none. He said, "that was the great extinction". I looked through the magnifying glass and I think the next slide shows those shells down here at the bottom; you can see these big ones about a millimeter in diameter and up on the top, all of those have disappeared. Well, I had never heard that other things had disappeared besides dinosaurs. I had often heard about the dinosaurs disappearing, but he pointed out to me that about 65 percent of all known species died out at that very moment. When you say a species died out, that means to the very last creature. We don't know of many species that have died out in recent times. I guess the passenger pigeon is one that numbered billions of members and was wiped out by people with shotguns. And the condor is on its last legs; there are only about 25 condors around. So it's a big event for one species to go out in these times. And here out of perhaps half a million species that were on the earth at that time, 65 percent of them were wiped out in an instant, a blink of the eye of geological time. So I said to Walt, "how long did that clay layer take to come down and what made it." And he said, "well, we don't know for sure, but, first of all, limestone is basically 5 percent clay and 95 percent calcium carbonate.

The clay of course is swept down by rivers eroding the continents and it falls down to the bottom of the ocean along with the calcium carbonate that is in the shells of the little animals and they're mixed up together." He continued, "we think that the clay is there because the limestone making mechanism was turned off for reasons that we don't understand, for some period of time, and then it came back on and during the time it was off the clay kept coming down." (As we'll see, that turns out to be wrong; it's easy to demonstrate that it's wrong.) But I said, "I think it would be fun to find out how long it took that clay to come down." I said, "I can't think of any way to do it", but I thought about it for a week or so; I went through the bag of tricks that all physicists have stored away in their heads. And I finally came up with an idea which turned out had been thought of before, although I had never heard about it. That was to use the rain of meteoric dust that's coming down through the atmosphere all the time, as an indicator of the deposition rate. Meteors are coming into the atmosphere at all times. You see them as shooting stars; when they land on the earth they're called meteorites. Hundreds of tons of that dust comes into the earth, lands on the earth every year and it's distinguished from earthly material by having a very large concentration of the rare platinum group elements like platinum and iridium. To me at that time, that was just a piece of handbook knowledge -- that these things were very enriched in platinum and iridium. So I said, "well maybe we can use that as a sort of indicator of the time it took for this layer to deposit." It's as though you went around with a salt shaker with colored salt and you shook it at a constant rate all over the earth and then you looked to see how many of these grains of different kinds of salt there were in the rock and you could tell then how fast the rock was deposited. Let me now tell you just why there's more iridium and platinum in the meteorites and the asteroids than there is on the earth. There used to be that much on the surface of the earth and all throughout the earth but then the earth formed a core; it heated up by its radioactivity and the molten iron sank down to the bottom of the earth; the molten iron scrubbed out, carried away the platinum and iridium and took them down to the basement where they now are and so they are no longer near the surface of the earth where you can find them; that's why platinum costs 5 thousand dollars an ounce; it's very expensive. These elements are called siderophiles which in Greek means iron lovers, so iron likes to alloy with these elements and so they're all down in the basement. The smaller bodies like meteorites and asteroids didn't heat up enough to form a core and so they still have this large amount of these rare elements. So I looked up the characteristics of the platinum group elements in the table of isotopes and found out that iridium was the one that we should look for. It has an enormous cross section for slow neutrons -- about a thousand barns compared to a very small cross section for platinum, so neutron activation analysis seemed to be the way to find these things. In that technique you take a sample of rock, blood if you're a forensic chemist, and put it in a nuclear reactor and then you look at the gamma rays given off by the radioactive materials into which you turn the stable isotopes. A beautiful test was done once to show that Napoleon was poisoned to death. The experimenters got a couple of hairs from his head that had been saved by his friends, and they showed the presence of arsenic. You could see how many grams of arsenic he'd been fed each day and he got very, very sick, but then he'd recover and the British would give him some more arsenic and you could just see this element build up in his hair. That was all done by neutron activation analysis. I immediately suggested to Walt that we talk to Helen Michel and Frank Asaro, who have in my opinion the best neutron activation analysis facility in the world, right up at our laboratory. It's one of those treasures that Ernest Lawrence would have wanted to share with his friends in other disciplines. So we asked Frank and Helen if they would look at the samples that Walt had in his laboratory and check them for iridium. Well it turned out that we found lots of iridium. I'm using the editorial "we"; Frank and Helen found the iridium. There was much too much to be explained by the technique that had brought us into the field. That's the way it so often happens in science, you get started doing something for the wrong reasons and then something interesting develops. But the important thing is to do <u>something</u> and not just sit around and try to come up with the perfect experiment.

The next slide shows Walt and me last summer at the Bottaccione Gorge in Italy. I've been talking about these rocks now for the last two and a half years, but this was the first time I'd actually seen them, and I found that this stuff really was clay. You could scrape it out with a knife and you could make a little pot on a potters wheel and get it fired. It is clay. Of course these strata were laid down on a horizontal plane down at the bottom of the ocean and then they were raised up a few million years ago when the Appenines were formed. That's why they're tilted at this crazy angle.

The next slide is one that I made for physicists, who like to have things simple. They like to have the rocks laid down horizontally and stay that way. I leaned over for this picture and had the camera tilted. So this is the way I think of the rocks. I have my hand there in the place where the clay layer is. Next slide.

This is the information that I had stored away in a corner of my head that said that iridium and osmium, two of the platinum group elements, are down by a factor of almost 10 thousand relative to other elements in the earth's crust. In other words, these are the relative abundances in the earth compared to the meteorites. Next.

These are the kinds of data that Frank and Helen get from their neutron activation work. You see here a gamma-ray spectrum of iridium. You see a lot of other things. If you look on the left you'll see that there's an enormous suppressed zero, the zero is way, way, down in the basement under this room. But nonetheless with the modern technology of germanium detectors you don't have to do any prechemistry the way you used to have to do when everyone used sodium iodide gamma-ray detectors. So you just take the bulk rock, stick it in the reactor and put it by the counter. Actually Frank and Helen did a little prechemistry here, they dissolved away the calcium carbonate in weak acid, but this graph shows you the way the data looked. Next.

Here's what turned out from the measurements. Walt had to go back and get some more samples because he didn't have enough in his Berkeley office, but he had enough to get things started. This is an unusual curve in that the central third is a linear scale; that's about one foot of vertical height -- 30 centimeters. And you can see that the iridium is being plotted this way: 0 abundance here, and 10 parts per billion here. You can see that the curve goes from almost zero up to ten parts per billion and then tails off in just a few centimeters of rock. Now down below, you can see there is a logarithmic scale: 10 centimeters, 10 squared, which is a hundred, a thousand, ten thousand, and a kilometer down here at the bottom. And then up above, you can also see that there is a logarithmic scale. So this is a strange graph, in that you go logarithmically above and below a linear region, but you can see that the background points over here are all about 2/10ths or 3/10ths of a part per billion, above and below the boundary and then there's this enormous spike going up. Well, nobody had ever seen that before. And that was our major new finding. In addition to finding it at the Bottaccione Gorge, which is the top point here, we also found it at other places in the Appenines up to 25 kilometers away. So it just wasn't a local hot spot of iridium where you might have an iridium mine. Since that time, we have found it all over the world; in our original publication, we had seen it in New Zealand and in Denmark, with a much bigger effect in Denmark. It was about five times bigger an increase in Denmark. Since that time, five or six other groups have joined in the search and together we have now found it in twenty-one different places, spaced worldwide and I'll show you a map with those points. This is where we started. The point now was to explain where the iridium came from and what it had to do with killing off the animals. That was the theoretical part of our work. I've just been telling you about the experimental part. Next.

Well, since you've seen the big difference in the amount of iridium compared to other elements in material from outside the earth and material in the earth, due to the separation when the iron core formed, you can appreciate that a certain amount of iridium in the clay layer, if it had come from the earth, would had had to be accompanied by lots and lots of other material. But if you brought the same amount of iridium in from outside the earth, say, from an extraterrestrial source, where the relative abundance of iridium is much higher, then you'd only have to bring in much smaller amounts of everything else. So our first test was to see how much of the other materials -- other elements like chromium and iron and cobalt and so on -- how much you'd have to observe in the clay layer if the iridium had come from the earth, either from the crust or the mantle. And you can see this expected curve, which is the upper jagged line, is quite different than what you do in fact observe, which would tend to indicate that you didn't get the material from the earth, but as the next slide shows, from outside the earth. Because here's the same curve now showing the expected amounts of other elements assuming that the iridium came from outside the earth, from an extraterrestrial source. There's only one point there that's out of line and that's nickel but since various meteorites vary in their nickel ratio by two to one, that's no big deal. We concluded that we had proved that the iridium came from an extraterrestrial source. Then the problem was, what kind of an extraterrestrial source? The first place we looked was at a supernova, because a supernova has been proposed as the source of the great extinctions, 65 million years ago, by a number of physicists and astronomers. We started out believing our iridium probably came from a supernova. We made three tests. The first one was just to note how much iridium we found per square centimeter all over the surface of the earth. Then we multiplied that by the number of square centimeters on the earth's surface. That tells you how much iridium landed on the earth.

You can tell from theories of supernova how close the supernova would have had to be to the earth to bring in that much iridium. It was frighteningly close. It was less than a tenth of a light year. In the past, the typical theories of supernova causes of extinctions have put the supernova at 30 or more light years away. The reasons have to do with the probability of finding one that close. Our closest star right now is four light years and it's not a supernova. So you have to get a star much, much closer and the chance of doing that of course goes as a cube of the distance, so you go from four light years down to a tenth of a light year and take the cube of that and that's how frequently you will find a star that close and then the chance of it becoming a supernova is also very, very small, you know. We haven't seen a supernova in our galaxy since the time of Kepler; they're really rare. The probability that there would have been a supernova going off that close to the earth in the last 100 million years turned out to be about one in a billion. That's okay if you only want to use a supernova once. But we wanted to explain five great extinctions in the last five hundred million years so we could only use it once with such a low probability. That was one test. The second was that a supernova, according to the theories of Willy Fowler, who's in the audience here, is the only place that you can make large amounts of heavy elements like iridium. If you just do normal burning of nuclei in a star you can only get up to things as heavy as iron or nickel because they're the most tightly bound nuclei. And if you want to go beyond that you have to have lots and lots of free neutrons and supernovas are the place to get them.

In order to test the supernova hypothesis, Frank Asaro and Helen Michel did two of the most beautiful experiments I've ever seen. I watched them, sort of from the second row, as they did these experiments in chemistry. They used modern techniques of liquid chromatography to separate out the elements and as I showed in one of the first slides, there was an enormous background of other gamma rays from other materials that made that enormous background, with the suppressed zero. They did chemistry and got rid of everything else. Right down to almost the last atom of almost everything else but iridium in one case, and in the other case, plutonium. And I thought that these were two of the most beautiful experiments I had ever seen. They showed that the isotopic ratio of the iridium isotopes was the same in the iridium seen in the clay layer as it is in what you'd get if you walked down the hall here to the chemistry stockroom and got a bottle of iridium. They measured its isotopic ratio. Now what you get from any supernova should, according to the theory, vary from supernova to supernova --like cookies from different ovens taste a little different because they've been baked differently and have different ingredients. Each supernova will have its own characterisic signature and isotopic ratio. So when the measured ratio in the iridium from the clay layer turned out to be "normal", it looked as though it was not from a single supernova. Then Frank and Helen also looked for plutonium 244. Plutonium 244 is a heavy element, with a lifetime of about 75 million years. So if it had come in with the iridium -- and it should have accompanied it in an easily calculable fraction -- then it should be in that same clay layer and not below and not above. Frank and Helen, whose background is in nuclear chemistry, were able to do the same fantastic job of separating out everything else and looking only at the plutonium. And they showed that plutonium 244 was down by a factor of ten from what it should have been according to the theory. Now that's not a big factor. You could lose that much in the ocean. But recently at Los Alamos this experiment has been repeated and they pushed the level down to one part in five thousand. And also, instead of doing their work with iridium that was deposited down through the ocean where it could have gotten lost by chemical processes, they did it on a sample coming from a continental source where the material fell into a fresh water marsh. So I think the chances are now that the supernova theory is completely dead. Two of my friends who were active in pushing that theory gave up. They sent me a letter saying in effect, "we're throwing in the towel and you guys win." It's one of the nice things about being a scientist that when you come out with some good evidence knocking down one of your friends' theories he writes and tells you, "congratulations, that's great work and we give up." So I think the supernova theory is dead now, although it was very, very much alive 5 years ago.

I'll just quickly run through 4 slides, but, I could take all afternoon telling you about this work of Frank and Helen. Here is the gamma-ray spectrum from a sample of rock in which some plutonium 244 had been "salted". They separated this out and there is the gamma-ray spectrum, all the lines just where they should be. The next one shows a sample of rock that wasn't salted. It's absolutely bare of gamma rays and x-rays. There are no gamma rays there at all, indicating that the plutonium 244 is missing. I should say in passing that my son Walt was delighted when the plutonium wasn't found because he felt that he would never again be able to bring any rocks home from Italy after that because the customs inspector would ask if they contained any plutonium. Plutonium is terribly frightening to almost everyone even though these rocks would have given off only one alpha particle per month, if the expected amount of plutonium had been in the rocks. So they wouldn't have been all that radioactive but it was nice that it wasn't there. Next.

This is just to show you what Frank and Helen can do about getting rid of everything else. Now you notice there's no suppressed zero and there is essentially no background because they systematically got rid of every element. But you see they didn't quite get rid of all the chromium and they left in most of the iridium. But essentially everything is out and now they have peaks due to both of the iridium isotopes and they showed the ratio was the same as in normal iridium. The same thing has also been done on osmium by a group in Holland and it seems now that a supernova is definitely out as the cause of the iridium layer.

So we had to find a source for the iridium in the solar system and to come up with a killing mechanism. It turned out to be not too easy. The obvious place to think of is an asteroid that crosses the earth's orbit. You can see what these objects do by looking at the surface of the moon through a telescope. It's all cratered. You see the same craters on the earth but of course they've been filled in by the processes that make mountains and wash them away. The craters get eroded away, but they're there and the first suggestion was that if the asteroid hit in the ocean, it would make a great tidal wave and that would drown all of the animals. But I couldn't believe that it would drown all the animals in the middle of the Asian continent. I couldn't imagine a tidal wave that would persist over such a great distance. So although it was easy to get the iridium from an asteroid, we couldn't convince ourselves that we could invent an associated killing mechanism. So we tried a lot of other things such as iridium and hydrogen coming from the sun. If you get a flare from the sun you can get hydrogen coming up to the earth. The hydrogen would combine with the oxygen and that would essentially move all the animals up to the top of Mount Everest, killing them by anoxia. It would also bring in the iridium, but the numbers didn't come out quite right. We also couldn't get enough energy in the sun to do that kind of thing. We tried a whole lot of schemes and finally went back to the asteroid, with a different killing mechanism. Here the idea is that the asteroid hits the earth or hits the ocean, which is about the same thing. It vaporizes itself. It has a kinetic energy several times the energy required to vaporize itself, and send much of it into the stratosphere. It also throws a lot of earthly material up into the stratosphere where it blanks out the sunlight and forms a dark cloud that will in a year or so completely surround the earth and that cuts off the light, stops photosynthesis and so kills the plants; they don't grow anymore. That will immedately kill the plants in the ocean -- the phytoplankton that drops down to feed the fish. Thus no more phytoplankton drops down to the bottom of the ocean and so the fish die. The animals die because the plants on which they live stop growing. They die; animals then die from starvation and that was the killing mechanism that seemed to work. Let me carry on and see what the next slides are. Next slide please.

This is one of the first checks that we had that the theory wasn't crazy. According to the idea that Walt told me when he first showed me that rock, the clay that you saw in the boundary layer should have been exactly the same as the clay in the rocks above and below, because it all came from the same place. In his theory, it was washed down from the continents in the rivers and landed on the bottom of the ocean. But if our theory was correct -- that an asteroid blew up a cloud of dust, and that that is where the clay came from -- it was the material that fell out of the stratosphere, then that clay should be different, or could be different because it came from a different place. It came from the place where the asteroid hit. So Frank and Helen made analyses of the clay. To the left is the Cretaceous, the middle bar is the clay layer and the bar to the right is the Tertiary abundance. And you'll see that in these major constituents of the rock, measured in percentage on the left, sometimes there's more iron, there's more magnesium, but there's less sodium and less potassium. So the rock looks different. It did come from a different place, but the rock above and below the boundary is the same. Now for the trace elements, where you measure the abundance in parts per million.

The next slide shows what you see. And there again you see big differences, sometimes it's more, sometimes it's less. Up in the upper right corner, where you show iridium, you can't even find it above and below. These data are taken from Denmark which has a very, very big increase in iridium, about a factor of a hundred (after dissolving away the carbonate), rather than the factor of 30 that we saw in Italy. So that seemed to confirm our idea.

We had several ways of calculating the diameter of the asteroid and they all said that it was about ten kilometers in diameter. You get that from the amount of iridium you see and from the relative amounts of iridium to other things in meteorites, which are chunks of asteroids. That's a trivial calculation that you can do in your head and it says ten kilometers. There was another argument which says if you're going to explain five extinctions spaced out over a period of five hundred million years you need it to happen about every hundred million years, and so you then ask the astronomers who study these things, "how often do asteroids of various diameters hit the earth?" It turns out that the distribution of asteroids goes like one over d squared; that is if you find a certain number of asteroids of a given diameter, d, and you ask about an asteroid ten times that size you'll find one percent as many. So, asteroids of a given size have a given average time between collisions and it turns out that if you ask these gentlemen who have studied such matters for a long time, how big something has to be to hit on the average of every hundred million years, they'll tell you ten kilometers. We had a couple of other ways of estimating the diameter, and they agreed, so we felt fairly confident that we know the asteroid was about ten kilometers in diameter and at that point, since we had found the material -- the iridium spike as we call it -- as far away from Italy as you can get, namely New Zealand, we felt confident enough to publish. Phil Abelson, who is also going to talk today, is editor of Science and we sent the paper to him and he wrote back a very plaintive letter saying "I've already published three or four different theories of the Cretaceous-Tertiary extinction. So what am I going to do with yours; at least n-1 of these theories have got to be wrong." At any rate, he did publish it and that's how I got to be a sort of second class geologist. Next slide.

There's another way of showing that the material came from the solar system -- from a meteorite or an asteroid or a comet, (they're all the same composition, I believe), and that's to look at the ratios of the various rare elements. Over here we see the platinum to iridium ratio and it's divided by the platinum to iridium ratio in meteorites. So if the material that we see in our clay layer did come from a meteorite then the tabulated platinum to iridium ratio would be by definition, 1. You see that there's a square point and a round point; the round point is from the Cretaceous-Tertiary boundary, in Denmark, and the square point is from Spain, and those fit pretty well into that dotted rectangle there which is kind of an average of what you see in meteorites. Now there's also a cross point out there on the right hand side and that's from a terrestrial source. So you might say, "everything looks the same", but it doesn't because if you look at the top of the graph you'll see that there are 97 platinum to iridium ratios from other rocks and the other samples of earthly material are so far away from that rectangular box that they don't even plot on the graph. So there's only one platinum to iridium ratio that looks like what you see in the asteroids, that has been found on the earth. Now the gold to iridium ratio is not as impressive, although both of the points fit in the box. But now there are maybe ten earthly points that get on the graph but there are a hundred and fifty-three taken at random that don't fit on the graph. This is work that was started by Ganapathy. Frank and Helen have done the work which is plotted here. We think this proves that the iridium did come from solar system material, from some big rock that hit the earth. Next.

Now this is almost the present state of the findings by our group and others. The units are nanograms per square centimeter. That's the total amount you see in, for example, a few centimeters going down through the layer. That's the area under the curve that I showed earlier. And you'll see that one of the very smallest numbers is 15 which is in Italy. So we found this new effect in almost the hardest place. Almost all the other numbers are bigger. There's an enormous number out in the Pacific on the right hand side, 520. But the actual fraction of iridium in the rock, in the clay layer there is not different than you see it in other places. The big number is apparently due to some sideward slumping. Geology is very complicated. You know you have to worry about worms that live on the bottom of the ocean and churn things up and spread things out. It's very much more complicated than physics. Physicists would like to say, "let's do a spherical harmonic analysis of this distribution and find out where the asteroid hit," but you have all these confusing things that happen. So you can't do that. There's only one place where there's a zero. That's right up here in the South Atlantic, but there is a good sample right next door, a couple hundred miles away where there is some iridium. These samples are from deep sea cores and we are told that the chances are good that we looked in the wrong place. Somebody probably gave us a sample not from the Cretaceous-Tertiary boundary but just a few centimeters away. Then you don't see anything. So we're going to do that one again. The most interesting point is probably the one called New Mexico, and I'll talk about that in a minute. I'll now show you the "iridium spike" from Haiti. Then I'll finish up with a few new things that some of you haven't heard; I see lots of people in the audience who have heard me talk about this before. Next.

This is our one sad thing. Helen Michel went up to Montana with our friend Dale Russell, who's a paleontologist, and got some samples from a continental site where there were dinosaur bones. Helen stumbled over a dinosaur bone getting these samples. The reason we wanted these was that some people were saying that the iridium didn't come from outside the earth; it just was deposited out of the ocean. So if you can find it in a continental site, where there's no ocean, then that strengthens your case. We got all these very nice peaks here and were suprised to get so many but you know, again these worms stir things up so it's not that bothersome. But then Frank and Helen, being very careful, got another sample of the rock, and repeated all the measurements and the things didn't repeat. In fact, the peaks were in different places. That was very, very frightening. And it finally turned out, after they did some beautiful detective work, that these measurements came from samples that had been prepared by a new lady technician, who had been hired just a month or two earlier. She happened to wear a platinum wedding ring; platinum is always alloyed with iridium in jewelry, and Frank calculated that the normal wear of a ring, which we'll say takes a ring down by 10 percent in a 30 year period -- that the normal wear in a one minute period, during which the technician was preparing the samples would give you close to 100 times our easily detectable amount. This frightened a lot of people in the field because when you talk about parts per billion and tenths of a part per billion, you really don't know what you're saying. They are just words. You don't really have any idea of how delicate this technique is. Our technician lives in Rancho Pales Verdes; now when she comes up here she has to take off her wedding ring. We don't allow her in the building with her wedding ring on. She has since prepared lots of samples, and we haven't had any more of these problems. This problem had been met before in science. People found gold in the ocean but as they looked at larger and larger samples, the percentage of gold kept going down and down because it was just wearing off from a wedding ring into whatever size beaker or vat that they were working with and so the bigger the sample, the smaller the fraction. So we got rid of this problem, and warned everybody in the field and the next slide shows some new data. Next.

Here's some from a different boundary. This is an Eocene-Oligocene boundary at which time a few species went extinct, but the extraordinary thing at the Eocene-Oligocene boundary which is about 37 million years ago, is that there was a tektite field strewn over a good deal of North America. Tektites are little glassy bits of material which are believed to come when a meteorite hits the earth and melts material in the intense heat. The glass particles go up through the atmosphere, and then come back in and you can see aerodynamic shapes frozen into the tektites. Particles in a particular tektite field are all of the same age. Each tektite field has the same age and can be seen over a wide area. The Eocene-Oligocene micro-tektites are called the North American strewn field. These samples were taken from a deep sea drill core in the Caribbean and Frank and Helen found the iridium going up, which you can see by the black points just at the boundary. And so now this is an association of iridium with something hitting the earth. The tektite people had convinced themselves that tektites are made by meteorites hitting the earth, and that some tektite falls and extinctions are simultaneous, and we had convinced ourselves that iridium came from asteroids hitting the earth. Asteroids are just big meteorites and so now all three of these things tie together quite nicely. Next slide.

These are the data that went into that last slide showing the iridium peaks here at the boundary and no iridium above and below. Next.

These are data from the Permian-Triassic boundary. This is an extinction about as large or a little larger than the Cretaceous-Tertiary. It occurred 225 million years ago. We got these samples from China. So far we haven't seen any iridium but we can push the iridium sensitivity down another factor of 10 or 100. I expect it to show up. But you do see, right here at the boundary, where those black lines are, that other elements look different in the boundary than they do above and below, just as they did in the Cretaceous-Tertiary boundary. I think this effect is probably due to a comet which is coming in much faster than an asteroid. That's just a working hypothesis. I can't prove it at the moment. Next slide.

These next two are two of the very exciting ones. These are data from Los Alamos, from a group under Carl Orth. And what they've done is look at the Cretaceous-Tertiary boundary in New Mexico and this is a real continental source and they really did find the iridium. This is going to be published in Science. I hope it will be published very soon. On the left you see iridium going up by a factor of about 300. That's a logarithmic scale here - 10, 100, 1,000, etc. There is an enormous iridium spike, -- about 300 times. And over here you see a signal from a botanical object, namely the pollen grain count normalized to fern spores. In other words, you can see fossilized pollen grains and they come from plants. We find this very exciting because there are a lot of paleontologists -- actually paleobotanists who say, "Those guys out in California are crazy because nothing happened to the plants. They went right through the extinction and you can't see any sign of a wobble." But I think what happened is that they didn't take their samples close enough together. Because when you know where to look and you sample closely you see that the pollen, which is an indicator of how many plants there were at that particular time, drop by a factor of 2 or 3 hundred precisely on schedule and so I think this is really going to shake up the paleontological community when it is published. The final slide is this one blown up. Here you see what happens right near the boundary. And these rock samples are 2 centimeters thick. That's an awfully small sample to take for a geological measurement. And you'll notice that the black dots and the black line, which is the pollen count, drops by a factor of several hundred in one layer, so the steepness of that drop is probably set by the resolving power of the method which is 2 centimeters. When we go back and look on the millimeter scale it may be even steeper. The histogram shows the iridium going up at almost exactly the same time. So that's the state of the project at the moment. Thank you very much.

Bernard M. Oliver retired last spring as vice president of research and development for the Hewlett-Packard Company in Palo Alto, but continues in a consulting capacity as technical advisor. Born in Santa Cruz, California, he was graduated from Stanford with a degree in electrical engineering in 1935, going on to earn his M.S. and Ph.D. degrees from the California Institute of Technology, which conferred its distinguished alumnus award on him in 1972.

His association with Hewlett-Packard goes back to 1952. Prior to that Oliver spent twelve years with Bell Telephone Laboratories, where he worked on the development of automatic tracking radar, television transmission, information theory and efficient coding systems. He has authored numerous technical articles and has obtained 52 patents, many in the field of electronic instrumentation that have had a major influence on technological advancement. He is a past president of the Institute of Electrical and Electronics Engineers and a member of the National Academy of Sciences. Government service includes the Army Science Advisory Panel and the President's Commission on the Patent System.

INNOVATION IN INDUSTRIAL MICROELECTRONICS

BERNARD M. OLIVER

What I'd first like to do this morning is to review some of the progress over the last 20 or 30 years that has occurred in the miniaturization and in the performance improvement of electronic devices. And then on the basis of what we already know about further developments that are about to occur in microelectronics, I would like to offer some predictions of things that we might reasonably expect to see as the '80s unfold and possibly I'll dare to go a little further than the '80s.

I've been involved, as you were told, with electronics for about 45 years now and I don't think there's been a single year in which there weren't many innovations, big or small. Few of these innovations have much effect on the art as they happen. They don't change what we do in that particular year and we sort of accept them and we're aware that they are happening but they don't seem to affect things at the time very much. It's only when you look back over the perspective of several decades that you can see what an enormous cumulative impact all of these innovations have had. So it's with that in mind that I would like to look back to the early days of electronics.

At the end of World War II a substantial amount of miniaturization had already taken place. The development of such things as the proximity fuse had led us to miniaturize vacuum tubes and so the end of World War II saw vacuum tubes reduced from something the size of bottles to something the size of thimbles and we were well on our way.

In addition to that activity, the development of pulse synchronization circuits for television transmission and the development of various pulse circuits in the radar work itself led quickly to the development of digital electronic computers. The first computers built by Aiken at Harvard were enormous devices that filled several rooms. These were great kluges; they had thousands of vacuum tubes in them; they used tens of kilowatts and they were hard to keep going for more than an hour at a time. Today you can hold in your hand a computer that's hundreds of times more powerful than those early computers were; literally, a hand-held calculator does a better job than those rooms full of equipment did in the late '40s.

It was the invention of the transistor in 1948 that made modern computers possible, but particularly the isoplanar junction transistor which was developed a decade later, about '58 to '59. Now, let me explain what that is. Early transistors consisted of a piece of silicon in which regions of n-type material had been doped in from opposite sides and so you had a base connection here and you had an emitter connection here and a collector connection here, all around the transistor. Those are the first types that were on the market in any great extent. The isoplanar transistor had this characteristic. First, you took a piece of, say, n-type material and you did an extensive diffusion to make this whole layer, whole region, p-type material through a mask that defined the area that was exposed to the diffusant. Then that mask was removed and a smaller one was put on and the second diffusion was made of n-type material again and when this mask was removed, you had a contact could be made with the n-type material here, a contact with the p-type material here and contact to the n-type material here. All these contacts are in the same plane, so we have an isoplanar transistor. Now all that you had to do to go to IC's was to make this p-layer be an epitaxial layer on another n-type material and then diffuse n-type barriers between the successive transistors to isolate them. So now you could have many transistors on a single chip and because all the electrodes were on the same plane, they could be connected together by further masking in metal deposition operations. And that's how Silicon Valley began.

You appreciate that the fabrication of that transistor utilizes a process that I described as putting a mask down and then making a deposition and putting a smaller mask down and making a second deposition. These masks are produced by a process that's called photolithography. It's a very old art, actually. The half-tone reproductions in newspapers and magazines are all done with photolithography. They're done with screens that cause the light to make bigger or smaller dots. First you deposit a sheet of material called a resist and then you expose it to light in certain parts and that either hardens it or softens it, depending on whether it's a positive or negative resist. Then you wash away the soft regions and you're left with a mask that protects the material underneath against whatever operation you are going to do. There are various kinds of resists and they protect the material against various kinds of depositions or etchings.

Let me have the lights down and I'll show you the first couple of slides. This is just a repeat of what we were just talking about, the epitaxial isoplanar transistor, and here are a couple of transistors in which the collector connection is here, the emitter connection is here, the base connection is made in here, and then there is a silicon dioxide isolation with a further diffusion of this stop connection between these two n-plus regions here. Here's a plan view of these transistors. You can see that they're all set up for their base emitter collector electrodes to be interconnected. Next slide, please.

This is an attempt to show what has happened to the art of photolithography over the last 10 or 20 years. When we first began to apply photolithography to the manufacture of integrated circuits back in 1960, the smallest features, the detail that was possible, was about 25 microns. This corresponds to about 1000th of an inch and is the kind of detail that is required by high quality magazine printing. In other words, the art had developed to the point where you could make that sort of features in the mask and that was about the lower limit. But further developments in the resists, in the registration machines that were used and in the photography, caused the minimum feature size to drop from the first part of the 1960 decade to about 1/10th as much in 1980. In production quantities we can now have 10x as many devices per unit distance and 20x to 30x if you take special care with small quantities and don't demand high yields. Well, you square that number and you see you can get something between 100 and 1,000 times the number of devices on the chip for a given area that you could at the beginning of the photolithographic era in electronics. At about 1 micron feature size, as you see on that chart, we expect photolithographic technology to kind of level out because we run into a fundamental limitation imposed by the wavelength of light. That says that to go on down to finer detail than about the 1 micron level, we're going to have to abandon light, even UV light, and go to some other medium to cause the exposure in these masks. The trend now is mainly toward electron beam, or E beam, lithography.

An electron beam machine uses (could I have the next slide, please) a precise electron beam that is focused and deflected by a set of focussing coils and deflectors. It traces out a pattern on the target, which is the resist that is to be exposed. You might describe it as a scanning electron microscope that creates the detail rather than observing it. You appreciate that to do this with submicron resolution, the electron beam has to have a very small spot, be submicron in size itself, and it must be deflected with high precision, great, great precision in all directions, and great care must be taken to register one exposure against previous exposures and so on, so there's a lot of technology involved.

The next slide (may I have the next one, please), shows the realization of the column that you saw in cross section in the previous slide. You can see that it is a formidable thing. The next slide shows the assembled machine. The column is now lost from view. All this part has to do with sample handling. Wafer cassettes are put in here, they go in through a vacuum stop, they're shoved in here, they're exposed, they're taken out, and delivered down here at the out box.

(The next slide, please.) Here are some of the control bays of equipment that are used to program and to observe its performance and do self-checks on the system. The next slide is just power supplies. The finer the detail that we're trying to get on the chips, the more tonnage of equipment we seem to need around the chip to do it. An E beam machine can expose successive layers of resist directly on the silica wafer and thus avoid the need for masks and mask making. This has great value for experimental work because instead of having to make masks and wait for them to be produced, you can go ahead and write directly on the wafer, eliminating one whole set of processes. For experimental devices, that's very promising but production will probably still be done using masks. The masks that will probably be used will be those intended for x-ray exposures. In other words, by going to E beam to make the masks and then by using x-rays rather than light to develop the shadows, we avoid the diffraction problems associated with the long wavelength of light. And in the decade of the 80's, as I indicated on the chart there, I do fully expect the art to progress to the point where we are capable of making feature sizes of about 1/10th of a micron. If lithography were the only limitation, we could increase by another factor of 100 the number of devices per chip.

There are many other innovations that have occurred over this period. Projection printing, in which the mask is imaged onto the wafer rather than being put in contact with it, avoids a lot of damage to the mask from irregularities in the surface of the wafer itself. Wafer steppers have appeared. The original process, you know, was to rephotograph the circuit over and over again corresponding to many positions on the wafer with what is called a step-and-repeat camera. And then that mask, with hundreds of circuits on it, hundreds of chips, would be exposed and each wafer then would be cut up into its individual chips. In the wafer stepper method, the final mask is a single mask of a single circuit, a single integrated circuit. It is reduced 10 to 1 in the final operation as the wafer itself is stepped. This eliminates some of the successive fuzzing of the image by having too many steps in series and has resulted in a better reproduction and the ability to go down to the 1 and 2 micron line size.

Another innovation shown on the next slide is plasma etching which is today being used to replace chemical etching. This is a barrel etcher, as it's called, and it's already out of date. Another thing that has been employed is ion implantation. Shown on the next slide is an ion implanting machine. You can see only the control area and the place where you introduce the samples. Ion implantation is simply a molecular beam of ions at a controlled voltage and a controlled current that is used to expose the chip in certain areas where the resist will let it do so, to ions of the dopant that will turn the silicon into p-type or n-type material. Its great advantage is, as compared with diffusion is that it permits very sharp doping profiles. The ions will typically go in a certain distance and then stop so that the doping comes in and then abruptly stops at that point. And so with successive dopings you get clearly defined collector base and emitter regions. It is the method of choice now and has largely replaced diffusion in the manufacture of modern devices. Its disadvantage is that it upsets the crystal structure to poke all these ions in and you have to then go to laser annealing, remelt the crystal in that area, to render it a single crystal material again. We have a lot of tools at our disposal and we have to use all of them.

A later technology is what is known as molecular beam epitaxy. This is the actual depositing of the silicon material required to form, let's say, the base and then the emitter in a bi-polar transistor or to form the gate regions in an MOS transistor, actually depositing it with molecular beams in which the dopant is present in the required proportions. Molecular beam epitaxy is really the synthesis in vacuum of the transistor you want. It results in the sharpest doping profiles of all.

Multilayer resists have also been developed. (Next slide, please.) This slide shows what can be done in the way of etching away a modern resist after exposure. What you see there are lines of resist that are 1/2 micron wide and 2 microns deep with sharp vertical sides on them. The etching process is controlled so that you don't get talus slopes at the bottom of these ridges, and those resist stripes that you see there are crossing an 8/10ths micron high oxide strip, and they go across without any trouble, so it's an example of how well the process control has developed to enable us to fabricate these things on a molecular scale. (Next slide, please.)

I'd like to show now what kind of effect this increase in photolithography capability has had and what the contributions are to the total increase and complexity that has occurred over the years. This slide goes from the same period, from 1960 to 1970 to 1980 and we have some guesses out here in the future. Lithography has improved from the ability to make one device in 1960 to the ability to make about a little over 1,000 times as many devices per unit area in 1980. Then, in addition, the area we have at our disposal has gone up. The chip size has gone up a factor of a little over 10 to 1, maybe something like 20 to 1, in that same period. In addition, people have gotten cleverer at reducing the size and not wasting space on the chip and making the routing of connections much more efficient, so that design and circuit cleverness is another factor that has come in. The total story is that we have gone up from one device in 1960 to something like 500,000 devices in 1980.

Now, for the future, I've assumed that the progress in lithography will continue as we have discussed, that we will go on making still lower defect densities in the chips, and thereby be able to have greater chip areas, and I have assumed that we are not going to get any cleverer in any other way. That ends us up at something like 100-million devices per chip in 1990, a really staggering number of active devices. I never forget that when I graduated from Stanford, Fred Terman assigned us a job of making a five-tube superheterodyne. The idea there was to see how much you could do with only five active devices. People today never consider less than 50,000 or so, so I don't know what they would have done on that test.

Let's see now what the effect of all this has been on integrated circuits to date. That last point on the curve up here, represents a chip announced this year by Hewlett-Packard that contains 450,000 devices. Let's have a look at that chip in the next slide. Well, at this resolution, and looking at the whole chip you obviously don't see 450,000 devices, but you do see certain areas which are described on the next slide. Outlined in white there is a big area over to the right, here, called ROM, or Read Only Memory. That's the memory that the processor consults to find out what to do next in many of its routines. It contains, in other words, all the algorithms needed for all the system instructions and commands. Then there's a thing labeled SEQ, which is a kind of a control logic or sequencing state machine that controls the flow of micro-instructions from the ROM into the PLA. The PLA is the Program Logic Array. It decodes the micro-instruction fields and controls the register stack, which is marked REG, that's the internal registers where numbers are stored on the chip, and the

Arithmetic Logic Unit, or ALU. The thing marked MPB there is a memory processor bus interface that connects this chip with the rest of the machine, and MVX is a self-test multiplexor. When the chip turns on, it assesses its own health and then says, "OK, boss, I'm ready." So these are some of the major areas on the chip.

(Next slide.) Let's have a closer look now at what's on the chip. This slide shows again the whole chip, but there is also a rectangle outlined there which is about 1/40th of the area of the entire chip and on the next slide all you see is that area. Now there's another rectangle on this slide that represents about 1/800th of the entire chip and we'll zoom in on that now. Now you are beginning to see some of the detail. Now let's go the next step to 1/12,000th of a chip and now you are beginning to see quite a lot. The final slide shows 1/200,000th of the chip. This is an electron micrograph and what you're looking at here is some underlying conductors that are beneath the oxide. You don't see the conductors themselves, but they're running across this way, but they cause perturbations in the oxide surface so you see them as lines here, and crossing them here is a second layer of metallization which looks terrible. In fact I can't believe it's a conductor but I'm told it's a very good conductor. It's tungsten, and I guess you see the crystal structure of the deposition but those crystals are in close enough proximity that if they're not in contact, at least the electrons seem to be able to tunnel across, so it does act as a conductor in spite of its looks. These lines are 1 to 2 microns wide.

Let's look now at what this progress has meant in terms of products. The next slide shows what has happened to memory costs over a 12 year period beginning in 1968, at the left here, and carrying on up to 1980. Back in this era, memory was done not with integrated circuits but with 22-mil ferrite cores that were all laced up with wires running through them, very expensive to produce. They had girls in Singapore who did this for a number of years. This curve is the cycle time which is the inverse of the speed. When this comes down the thing gets faster. This curve is the volume in cubic inches per 64 kilobytes, this curve is the watts, and this curve is the cents per byte. We went to 18-mil cores back in about 1971. Then the IC memories came out and from then on they replaced cores completely. First we had, 4k, then 16k, I will say that the slide is out of date because now we're using 64k memories and that has caused another 2 or 3 to 1 reduction in the size and in the price per byte. We see that over these 12 years depicted here, memory speed has increased about 5 times, the size has been decreased by 60 to 1, the power has dropped by 25 to 1 and the cost by 55 to 1, and, as I say, about another 3 or 4 to 1 change is already upon us.

(Next slide) Back in 1968 we introduced the first programmable scientific desktop calculator that had transcendental functions on it. It was called the 9100 and we thought it was a great machine but actually, as we look back on it now with present standards, it had pitifully little memory. It had only a few registers at the user's disposal, it had only something like 200 steps of program and so it was really quite a small machine by today's standards.

Over the next 12 years (next slide), several other models were introduced that had greater and greater power and greater and greater memory capacity. Here is the old 9100A here, with about 272 bits of random access memory. 9100B doubled that. Now we have machines that had various options on them, so they are represented by vertical lines. But you see, by the time you get up here you're talking about 100,000 to 400,000, or 500,000 bytes of memory so your capabilities are much more powerful. The purple band shows the increase in cost that has occurred for these machines, and they've gone up in price. But now we come here to a final point: the hp 85. We see that this machine has something on the order of 30,000 bytes and its cost is lower than the 9100 was at the beginning, so there's been an enormous increase in capability with eventual decrease in price. The next slide shows a picture of the 85 which some of you may know. You can see from the picture that it not only can display numbers as the 9100 did, but it can also print them out and display graphics and print them out too.

(Next slide) I'm sure many of you remember the HP35. The next slide shows what has happened over the years to hand held calculators at about the same price. On this slide you see a line which is called "dollars" and that remains almost constant or drops slightly, but the performance of the calculators involved goes up quite a lot, about, I would say, 2 orders of magnitude there for the number of bytes of random access memory and read-only memory so that they not only can remember 100 times as many algorithms but they can handle 100 times as much data and are therefore very much more powerful machines. In short, you can get about 100 times the calculator today for less money or if you are content with the performance of the original 35, you can get that for far less money. I'm really not trying to give you a sales pitch in all of this. In fact I'm doing just the opposite because the only logical conclusion you can draw from what I've said is that no matter what year it is, don't buy a calculator. Wait till next year.

Cheap computing power in the form of a microprocessor is causing a revolution in almost all the gadgets we have anything to do with, ranging from scientific products like spectrum analyzers to cash registers, or from video games to traffic signals. Microprocessors are finding their way into all these things and making them much smarter. Traffic signals respond now to your driving over an inductive loop in the pavement and they adjust themselves depending on the traffic flow and they do a much better job of letting you through than they used to. The ability of instruments to interface with the user has gone up enormously. In the instrument field, as shown on the next slide, the knobs are disappearing. Here is an example of a synthesizer. This machine puts out a precise frequency up to 80 megahertz. You punch in the frequency you want digitally or you move it along with the one remaining knob in an analog fashion, but there are all sorts of system commands that are present and available so it can do fancy things for you with very little fuss on your part.

(Next slide) Here's a modern digital voltmeter and the next slide shows a close-up of its keyboard there, and it looks more like a calculator than it does a voltmeter, so you can introduce multiplying concepts. You can have it do all kinds of things in a very easy way.

(Next slide) Here is a spectrum analyzer. You see it, too, has a keyboard appearance to it and it has a great number of functions, a great many more than the old ones used to and they are made very convenient for you because it has internal smarts, as we say, and can interpret your commands in a rather sophisticated way.

Someone at HP recently remarked that pretty soon all instruments would not only look alike from HP, they will be alike. They'll all consist of an A to D converter computer and a D to A converter. And that's really about all you do need for a large part of the spectrum today because a computer can synthesize wave forms and you can put out signals that used to require signal generators. You can look at the response of systems and digitize it and then compute the serial correlation and do digital filtering and all the operations in a digital form that we used to do electronically with coils and condensers. So the world has changed.

I'm sure that this digital trend will continue as computing power gets even cheaper and faster but there always will be a frequency range, in my opinion, where you will still be using analog practices and this will be the highest decade or so of the spectrum where you still can't digitize rapidly enough. You'll be forced into analog operations there, but I'm happy to say that there has been a lot of innovation in this area too.

The next slide here shows the fastest transistor in the west. Actually, in the whole country, I think. It's the gallium arsenide field effect transistor device that has a 1/2 micron gate that runs the entire width of the picture. You can see the pad, the gate is the grey area here in the center, and the connection coming up, and then there's a gate that runs through that black stripe but the resolving power of the optical microscope, isn't great enough to show it. It takes an electron microscope to do so.

On the next slide you can see what's there. The gate comes up and you can now see the stripe running in both directions and there's a second gate on beyond it and both of those are only 1/2 micron wide. This device has 6 db of amplification up to about 28 gigahertz or so, so it's really a very high speed device. So that's some of the progress that's been done in very high speed single transistors.

The next couple of slides are some examples of hybrid circuits that are constructed on sapphire substrates. These are very common in the microwave world. This happens to be a 4-stage video amplifier that begins at essentially dc and goes all the way up to 2 gigahertz, a wide band video amplifier. The next slide shows a single amplifier that works from L band up to the bottom of X band, a large fraction of the microwave window.

And finally, here is some state-of-the-art lithography on a surface wave acoustic transducer. This is a quartz device. These electrodes that you see, these very fine electrodes, generate very, very short wavelength acoustic waves that propagate down the surface of the quartz like ocean waves and are picked up by electrodes at the other end. These devices can be made to oscillate at 100 to 1,000 megahertz very nicely where quartz crystals are impossible to build because they're too thin. They can also do filtering and convolution and various other operations in this interesting frequency range, so we've extended quartz up a couple of decades by means of this technology. I think we can have the lights and turn off the slides at this point because I don't have any slides of the future.

As I indicated earlier, I do expect about a 100 to 1 increase in capability over this decade in our integrated circuit complexity. What does that mean? Well, it means that we could have 4 chips that could contain the entire contents of Webster's Unabridged Dictionary, for example. That's a lot of data. If you just take it, next time you see it, and look at one page of that and realize there are a couple of thousand pages of it, you'll see what 400 megabits really means. I expect the businessman of 1990 to carry in his briefcase a typewriter-operated computer, a thin device that can compute anything that today's mainframe computers can compute, can store and edit text and telephone it back to his office if he wants. It can be an electronic mailbox. I think that this machine would have enough power to integrate symbolic expressions and return them in symbolic form. In other words, the computing power, I think, would rival or exceed that of some present day main frames and yet it would be quite portable. That's what this increased memory and processor capability will do.

Where the device will probably fall short, this briefcase computer, as it already does, is in the input-output devices that it would have. It won't be able to draw the nice curves that you can do with a bigger machine. You won't have the plotters and printers and disc drives and so on that comprise most of what a computer is today anyway. It's in these input-output devices where the money goes even today. There is where the space and weight are and these are where technological breakthroughs are needed. A lot of present activity and innovation in the computer field is going on in the development of lighter and more powerful input-output devices to match the development that has occurred in the central processor itself.

Today the computer is a ton of peripheral surrounding a milligram of silicon. We have miniaturized the heart of the computer but not the interface with the world and clearly this unbalance needs rectifying, partly by improved sophistication and innovation in the peripherals as

I have indicated. But also, I think it needs correction in the form of better central processors that make use of the cheapness of the silicon chip itself. All computers today use what is called a Von Neumann architecture in which there is a central processor and around it is a memory and there is a control logic that operates on this thing and feeds it all the stuff it needs and it interprets the program. And all these devices dance attendance on it to keep it busy. Now in the beginning, when that central processor was a rack or two full of vacuum tubes and was the most expensive part of the system, that was the logical way to do things. But today, when it's cheap, we ought to replicate it and have various central processors that are expert at doing various things and an executive one that assigns the jobs around on the basis of capability, I think that's coming. We don't have a good formalism yet, or a good structure to control the operation efficiently, but I think it will happen.

Another thing I'd like to mention is an interesting symbiosis that is occurring between LSI and computers. I've been showing you how present computers are possible in their small size because of the large scale integration revolution. I think it's also true that future improvements in LSI, the neo-LSI era, won't be possible without computers, without assistance from computers. I'm referring to design automation. The process by which you simplify the tedious work of getting all these things in the right place on the chip. Very good algorithms have been developed and further ones are being developed to take a simple set of instructions that are a high level description of the circuit that you want, enter it into the computer and have the computer lay the chip out for you. To do a present LSI chip manually takes about 3 man years. If you go to 100 times the complexity as I am predicting, you'd take something like 100 man-centuries, and that's clearly out of the question. So we see design automation as a very essential ingredient of further progress. Work on that is going on. That's part of the present innovation that's happening. We hope to reduce this time to something from the order of man-centuries to a few days or even hours by appropriate computer programs. I wonder if we should call it computer-aided design or man-assisted computer reproduction? There's the old line, you know, that a chicken is an egg's way of reproducing itself. I sometimes feel that way when I'm working with computers.

Another thing I'd like to mention about this 100-fold increase in design complexity and device density is that I think we've reached the end of the line as far as two-dimensional miniaturization is concerned. The limits are going to be set, not by the technology, but by the device physics. Today's devices are around 100 square microns. A lot of independent assessments have shown that, by the time you get down to a little under a square micron, a whole host of problems arise to bite you. There are statistical fluctuations in doping, there is electron tunneling through the layers that you're using, there is punch through in the bases, and a number of things that just lead to poor performance. So if we're really going to go to much higher densities, we're going to have to learn to integrate in three dimensions.

Now, nature has already done that, you see. The conventional figure is that there are about 10-billion active devices in the human brain, and yet a single nerve cell up here is much bigger than our transistors. If you put a single nerve cell down on an IC it will cover about 100 transistors, so we've outdone nature in the two dimensions that we've so far attacked but we haven't begun on the third yet. If we could increase the surface density 100-fold as I've predicted, and if we could learn to deposit 100 sets of such layers, then we might consider somehow in the future arriving at single chips that might have the complexity of the human brain. I don't think that's out of the question at all, because we're already depositing transistors, as I've said, by molecular beam epitaxy and it's only a short step from there to depositing whole layers of transistors this way and ending up with smooth enough surfaces, or surfaces that we can drop a smooth blanket over and repeat the process so that we can pile up layer after layer on the chip. That's a direction that research will take.

It's very hard to imagine the power of a computer so complex as that might permit. Would Arthur Clark's shirt pocket secretary become a reality? I think it might. I think such a computer might accept voice commands and be programmable in natural languages. You might discourse with it rather than type in things on a keyboard, which would certainly make an interesting change. So these are some of the things that I foresee coming out of the large R and D effort that is going on in the electronics industry, Silicon Valley and elsewhere. The industry typically puts something like 5% to 10% of its sales into R and D. This is a large expenditure. It's comparable, or exceeds, I guess, the budget of the National Science Foundation. So a lot of work is going on. We are innovating, contrary to rumor, and I think we'll see some very astounding progress in the next few years.

I'd like to say in conclusion that I am very guilty of what I might call forecaster's syndrome, because all forecasters ever do is to extrapolate existing trends and that's all I've done this morning and that's really all you can do because I think it's absolutely impossible to predict the real innovations, the real substantive changes that occur from time to time. Things like the transistor itself, things like the laser, that completely change our world and I don't think are foreseeable, but I can statistically predict that those things will occur, too, and what they'll be I'll have to leave as a surprise to my audience and to me. These are the things that make the future truly unknowable. These are the things that make science such an eternal and exciting adventure. Thank you.

Philip H. Abelson is editor of Science, the weekly magazine of the American Association for the Advancement of Science, and is a research scientist and past president of the Carnegie Institution of Washington, D.C. A native of Tacoma, Washington, Abelson earned a bachelor's degree in chemistry and master's degree in physics at Washington State University before coming to the University of California in Berkeley to study for his doctorate under Ernest O. Lawrence. He was codiscoverer with Edwin McMillan of element 93, Neptunium in 1940, using the Radiation Laboratory's 60-inch cyclotron. His long association with the Carnegie Institution began in 1939 as a physicist in the Department of Terrestrial Magnetism. He has been associated with Carnegie ever since except for the years of World War II when he served on the staff of the Naval Research Laboratory. Later as director of the Geophysical Laboratory at Carnegie he conducted and directed research on geochemistry, geophysics and experimental petrology. He served as president of the Institution from 1971 to 1978.

He has been editor of Science since 1962. Concurrent career activities include editorship and authorship of numerous scientific and technical journals and books. He has served on advisory committees and as consultant to many governmental organizations, including the Atomic Energy Commission, National Aeronautics and Space Administration, National Institutes of Health, National Council of Governors and the National Academy of Sciences. Honors received include the Scientific Achievement Award of the American Medical Association in 1974, the Kalinga Prize for Popularization of Science, 1972; Joseph Priestly Award, Dickinson College, 1973; and the Carnegie-Mellon University's Mellon Institute Award, 1970.

ENERGY AND ELECTRONICS IN A CHANGING WORLD

PHILIP H. ABELSON

It's great to be back on the scene where I enjoyed a most stimulating period many years ago. I was privileged to be with a dynamic group of colleagues and am pleased that some of them are here today. For example I see Luis Alvarez, Bob Thornton, Wynn Salisbury, Bill Brobeck, Bob Cornog, and Art Snell. They were some of the crew that ran the Cyclotron at that time. We were a limited group who were in at the beginning of an expansion of nuclear physics. It was an exciting time. Almost every day there was something new. We were engaged in research that employed the latest and best of electronics of that particular era. Many of the people later, when the nation needed them in World War II, were familiar with electronics and hence could make great contributions to the country.

There have been, in our professional lifetime, many changes in society and as we look ahead we see further change coming. Today I will talk about developments in energy and electronics. I couple the two because they will be major determinants of the shape of society and because there is interaction between them. Through the use of electronics and computers we are finding it is possible to achieve a far more energy efficient society and a possible reduction, in the demand for energy. At the same time economic problems caused by the high energy costs are forcing advanced countries to seek to develop high-value-added industries that consume little energy while producing valuable products for export to pay for oil. As a result, tough foreign competition is being experienced in the electronics industry and in high technology products.

There are many interactions between energy and electronics, but there are also some contrasts. Time constants for major changes in energy are on the order of 30 years. A complex capital structure and societal attitudes that slow the building of new capital equipment lead to very long times in changing the pattern of energy production and use. On the other hand, as Bernie Oliver was indicating, in the electronics game the time constant is on the order of 2 or 3 years to make a significant change. Furthermore, electronics so far has been fortunate in that societal attitudes have been relatively favorable for development of it. Electronics may some day have greater impact on society, and encounter problems, but thus far societal problems have been minimal. For the future, the key determinant in use of energy will be economics and the time necessary to build energy installations. Let's look at the economics briefly because we can thus get some insight of what is likely to happen in this country and around the world.

Much of the oil that is used today is burned merely to make heat. The cost of oil for such a purpose is between \$6 and \$7 per million Btu's or gigajoules. The cost of mine coal, in strip mines in Wyoming, is 25¢ per gigajoule--of course, companies must make a profit so the cost of coal loaded on the railroad cars in Wyoming is about 50¢. Fifty cents for coal against \$6 to \$7 for oil. If you want to make heat, it's obviously a better bargain to burn coal than it is to burn oil. In texas, lignite costs about 70¢ for a gigajoule. In eastern United States coal is more difficult to mine, and the cost is about 80¢ per gigajoule at the mine and about \$1.25 at the eastern seaports. In South Africa, where the mining conditions are favorable and the pay for miners low, the cost of coal is 40¢ per gigajoule. In Australia, it's about 50¢. On the other hand, German bituminous coal costs about $6 to mine. That is due to the fact that they've worked out much of their most easily minable coal.

It should be evident that there is destined to be a substantial international flow of coal and a substantial conversion from use of oil to coal is already occurring. The people in Germany who are paying high prices for their coal would like to have some of that Wyoming coal for 50¢. The problem is, to a considerable degree, one of cost of transportation. In this matter we are going to see developments in the use of coal slurry pipelines. About half the volume in such pipelines is finely-ground coal and half is water. This slurry can be conveniently pumped in pipelines and then it can be pumped into large tankers. A major future lies in coal slurry tankers of perhaps 300,000 tons capacity for transportation to the place where coal is used with subsequent pumping off the ship. It is quite cheap to unload that way, and then move the slurry to wherever it's going to be consumed. There is of course, a dewatering process just before consumption. A Boeing study, for example, proposes to mine coal in Utah, and to move it via a slurry pipeline to the northern California coast. A pier for unloading it would be located about a mile offshore. It would be shipped to Japan using large tankers, with delivery in Japan of the coal for on the order of $1.25 per M Btu's.

At present, about half of oil produced is used to make heat. In the future it can and will be largely replaced in this application by coal, natural gas, nuclear energy, and renewable energy sources. The premium uses of petroleum today are in transport, whether ground or aerial transport, and as a feedstock in petrochemicals. In these applications major changes are in store. This week there was an announcement by Boeing of the first flight of the B767. That plane has been designed to be aerodynamically efficient. It has special electronics aboard and the net result is that it will consume 40% less energy than the typical plane that is flying today with the same load. In many applications a great improvement in energy efficiency is likely to occur. If the gas guzzlers were off the road today and we were using the current new automobiles, the consumption of gasoline would be down nearly a factor of 2. In Europe there is talk of automobiles on the drawing boards and in development that will get 60 miles per gallon. Some people at the Lawrence Laboratory are exploring a new method of plasma ignition that, when employed, could lead to a one ton automobile having 100 mpg.

Another important use of oil is in making petrochemicals, many of which are used in plastics. Much activity is being devoted to building up those same chemicals from synthesis gas, namely carbon monoxide plus hydrogen. Carbon monoxide plus hydrogen can be derived from coal. In the future many of the chemicals now produced from petroleum will be produced from coal via new synthetic processes. In addition some of the chemicals will be derived from fermentation processes from biomass.

One important source of energy that doesn't get much notice is natural gas. Actually, in terms of energy units, we get something approaching 2/3 as much energy from natural gas as we do from petroleum. Studies I've made, and those of others, lead me to be convinced that there's far more energy in the form of natural gas in the ground available to be tapped than there is in the form of petroleum. We can expect the reserves of conventional petroleum to decline and become more costly. By the year 2000, such conventional oil as remains will be largely located in the Middle East. Much of it in Saudi Arabia.

However, that doesn't mean it's the end of the road as far as petroleum hydrocarbons are concerned because there are very large amounts of such chemicals locked up in tar sand and oil shale. One can anticipate that Canada, with its Athabaska tar sand, will become the Arabian peninsula of the future, about 30 or 40 years from now. This will be, a major, source, along with the Oronoco tar belt, which has comparable amounts. Actually, the possible reserves in Canada considerably outstrip the amounts of petroleum now known around the world. When people say we're going to run out of oil, they're not taking into account what the potentials are. As a further example, the potential in our own Green River oil shale is also comparable to that of conventional oil.

I will speak for a moment about international considerations. We are well off with respect to various energy sources and it's easy for us to be complacent in these matters, but go visit Japan, France or Germany and you will get a different set of impressions. For example, in Japan more than 90% of their energy is imported. France is a little bit better off and Germany better off still. Germany gets about 50% of its energy from coal but it's miserable coal that costs about \$6 a million Btu. Our colleagues in Japan, Germany and France are in poor shape with respect to energy and hence, very sensitive about energy changes. I happened to be in Japan just a week after the beginning of the Iran/Iraq war and people were buttonholing me and asking me "what is the United States going to do when our oil is cut off at the Straits of Hormuz?" They were terribly worried about it because it was a matter of existence for them.

In contrast, the situation in the USSR is quite different. They have oil reserves that are between 2 and 3 times as great as our reserves. They have reserves of natural gas that are a factor of 4 greater than ours. They have about twice as much coal. They are well fixed and can contemplate export, particularly of natural gas, to Western Europe. Several years ago I was in the great distribution center in Austria where the pipeline from USSR comes in. From that distribution center, natural gas goes to Austria, Switzerland, Northern Italy, parts of Southern France, and Southern Germany. Germans are financing an expansion of the pipeline system to carry more Siberian gas to Western Europe. There's a potential for a fairly dangerous situation because of coming dependence of Western Europe on Russian supplies of energy.

Another item of some interest has to do with natural gas in the OPEC countries. People who have flown over the Persian Gulf speak of seeing many flares of natural gas; the place is lit up at night. That gas could be used, for instance, to make fertilizer to help feed an enormous number of people, but it is being flared. Part of the reason it is being flared is, due to worries about the instability of the region. In 1978, DuPont had a plant for making petrochemicals practically completed in Iran. But when the hostages were seized that was the end of that. They just walked away from it. The Japanese were more persistent. They had a petrochemical complex under construction costing $4 billion. Since they didn't have any hostages in any embassies, they hoped that they would be able to somehow complete the construction, but recently the Japanese have walked away from $4 billion worth of investment in Iran.

Energy needs will continue to be an important consideration in diplomacy. We'll see that the various countries will have to solve their problems as best they can. There will be some movement of coal in international trade but the era of cheap oil is over and around the world the various countries will have to fare as best they can in adapting to their local opportunities. A side effect of the high cost of oil is that it made differences in the economies of some countries. For instance, Pierre Aigrain, who was the head of science and technology in the French government before Mitterand, and told me that as of 1973, France had a viable petrochemical industry based on cheap feedstock from the Middle East. When the price of oil quadrupled it, in effect, killed the French heavy chemical industry. So, over the course of a few years, they have shifted their emphasis to pharmaceuticals and other high value added chemicals that don't involve much energy. Germany has had a tremendous chemical industry and has been able to withstand better the high cost of oil, but nevertheless, it isn't the competitor that it once was as a result of the high price. The Germans, too, are looking to other sources of high value added products.

The biggest effect has been on the Japanese. The Japanese know that in order to survive they have to compete. Every boy knows when he starts school that Japan must export and he must learn science and technology so that Japan can survive. The whole society there is geared to be effective, technologically. They have talent for quality control, and for painstaking attention to detail. The kinds of things that Bernie Oliver was talking about is easy for the Japanese. They know how to make tiny things and to do it extremely well. A strong incentive that has come from the oil crisis will tend to make them terribly tough competitors in the time ahead.

I turn now to comments about electronics. All of us who have had experience in science recognize our debt to the makers of scientific instruments. It's been as a result of the use of new instrumentation that we've had the great progress of science during the last 30 years. We get from this personal experience some indication of the great power that electronics is going to have to make changes in societal organization.

I'll present an instance of the drastic increase in measurement capability that I think about often. That has to do with the analysis of crude oil. Crude oil is complex in composition. It contains many hundreds of hydrocarbons, straight chain, branched, rings, and heterocyclics. Right after World War II, big projects were set up to find out what's in crude oil. Many tons of an oil were set aside for the analysis. They put it through stills, vacuum stills, other kinds of stills. Over the course of about 15 years about 200 components were identified in crude oil. Today, one can take a fraction of a gram, put it through gas-liquid chromatography, coupled with mass spectrometry, and get the whole analysis in a day or two. That is an indication of the change in power of the analytic instrument.

Obviously if one can make such determinations the technique can be used in refineries, and a tremendous amount of comparable equipment is so employed in refineries today. Use of electronic equipment has made possible automation of refineries. If you go into a refinery, you will see few people. There will be acres and acres of pipes and towers and all the rest of it and hardly a human to be seen. It's all managed by computers, by sensors that can follow what's going on at all times. And they can do a much better job than humans. The value of different products differs from time to time and you can set those computers to take into consideration value of the different products that you might produce. Use of electronics in the petroleum industry is also paralleled by the use of them in the chemical industry. Chemical industry is generally computer-oriented today and new sensors, new devices are being developed to take full advantage of advances in computing capability.

Another area in which the electronics is being applied is in medicine. A large fraction of the clinical tests performed today are being performed by electronically controlled equipment. This has an important advantage over measurements by humans, because when humans make observations they're often thinking about their date or some other matter. When they transcribe the numbers, they get the numbers down wrong. It's been shown that you can expect something like a 3 to 7% error every time numbers are transcribed. In contrast if the data are produced and stored electronically the numbers are right. Another application has to do with management of hospitals. Lou Branscomb has spoken of a 775-bed hospital that has some 3,000 attendants which in the course of a year develops 3,500,000 pieces of paper that in turn are routed between 8 and 20 people. That's a veritable storm of paper and getting it delivered involves time delays, and the costs entailed are great. By computerizing the system you save mistakes, and speed transfer of information. The patients get discharged a day or two earlier because instead of waiting for the papers to get circulated, the information comes out of the computer. Another set of applications in the medical line has to do with non-invasive tests. There are a good many of them being developed. They include use of computers to process data obtained from x-rays, positron emitters, NMR and ultrasound.

Another area in which many things are happening is in manufacturing. Earlier this year I enjoyed visiting a Weyerhauser sawmill up near Mount St. Helens. There they were bringing in logs from trees that had been blown down in the blast from the mountain. The logs were all kinds of different sizes, they had tapers, they were bent, they had kinks. Earlier, an old grizzled veteran would make a quick estimate of where to cut each log in order to get the most product out of it. This was entirely a matter of judgment which had to be made quickly because you can't stand around conducting a debate, you've got to get production. The operator must set the saws in just a second or two. The computer is better at setting the saws than a human; especially the computer aided by some laser sensors. Furthermore, as in the case of the oil refinery, different products from the logs have different values. The highest price is for lumber but, again, the various sizes of lumber are differently priced. All that information can be put in a computer so that economic return is optimized. In addition the logs can be processed faster than humans could do them.

During a trip to Japan, I visited the Toyota engine plant. It includes literally acres of engine blocks on conveyor belts. They move about from one machining station to another. You had to look pretty hard to find a human in the plant. It was all automated and, as you know, the quality control there has been good. I also visited the Matsushita color TV assembly line, and there a large fraction of the components are put in place automatically. The automation is effective. When we got to the end of the line, I accosted my Japanese host and said "I've heard all this talk about quality control. Where were your quality control stations to monitor the quality?" "Oh," he said, "we had some until about 4 months ago, but we found out that they never found anything wrong."

There are in progress in the United States and elsewhere further developments in manufacturing. I refer to computer-aided design and computer-aided manufacturing. These developments will have large effects on the composition of the labor force and on employment. Another application that is going to touch all of us is computers in automobiles. I'm told that by 1984, all of the automobiles manufactured by Detroit will have at least one computer in them. This will have the essential functions of controlling the ignition and emissions. It will use information obtained from about seven different sensors. For instance, how far is the throttle depressed?; what is the temperature of the motor?; what is the atmospheric pressure? All the components necessary are measured to give the information required to determine at what instant the spark should be discharged to get optimum power.

While I was in Japan I visited IBM Japan. IBM has been there for many years and they have a very active research and development group there. One of the things I saw was their computer system for design of buildings. At a typical terminal there is a keyboard and a display screen. In the computer memory are standard structural components that can be taken out of computer memory and displayed. You can sit there, and before your eyes, erect a building. The Japanese are very sensitive as to how a particular man-made object is going to fit into the environment. You can have on your display terminal what the background scene is. You can see what kind of landscaping you need to have to make the building look good. And so, in a very short time the whole building can be designed. Since the design was made using a computer, the computer has a record of what kind of components were used. One can obtain a print-out of the bill of goods of the materials needed to build the building. Computer graphics were in color so that one could see what a room would look like. One can put various objects in the rooms and decide what color the rug should have.

I've given a number of different examples of how the computer will be important. I will now mention one of the areas in which there will be a very great societal impact. Today three-quarters of the labor force is employed in service functions. Two-thirds of those three-quarters are engaged in handling information and that means that most of them are pushing around pieces of paper. From 1960 to 1970 there was a growth in productivity in manufacturing of about 80%, but at the same time the productivity in the service sector increased only 4%. There was comparatively little capital investment in the service sector. A typical office obtained an electric typewriter instead of the mechanical one, but the way things were done was just about the same at the end of the decade as it was at the beginning. Studies have shown that one of the major ways of beating inflation is through increased productivity. One of the reasons we've had the inflation that we've experienced is because our productivity has not gone up, especially the productivity in the service sector. Here is the big, main chance, to increase productivity.

Companies like IBM, Xerox, are acutely aware of the opportunity and they are seeking to design the office of the future. A visit to Xerox in the Palo Alto Center can provide a prototype of what they think the office of the future might be. Each scientist there has his own computer terminal. He has available to him all manner of programs, which are very friendly. He can, for example, design a complex circuit, bring the components to the screen, place them in position, and the wiring is automatic. The design information goes into the computer memory, later to be used in the manufacture of the item.

Another development is an enormous proliferation of data bases, many of them involving text. For instance, the text of the Economist is now on computer and can be searched using key words. Great use of search of legal data bases is made by the legal profession. Much economic data is now in computers along with models of the economy. One of the interesting stories, perhaps apocryphal, about such data bases was that several years ago Giscard d'Estaing asked his economists what would happen if he made a certain change in the French laws that would affect the economy. They said, "We don't know but the people at MIT have a model of the French economy and they could tell us what would be the effect of this change." When the president of the Republic heard that, he was angry and said, "From now on we're going to develop the capability in France to make our own predictions." That sort of incident has led to a development in Europe of a European data network and a good deal of effort there to become independent of the United States in the matter of data bases.

There will be a great impact of the computer in the home. As one example, video disks store 54,000 color frames on a single disk. They will provide a broad variety of home entertainment. Ultimately they will serve educational functions.

I've give samples of applications that indicate a broad and increasing role for electronics and computers. In an attempt to get a better picture of the future, I've consulted experts in major industrial laboratories who should be in position to estimate future potential developments. They all predict great changes and great societal impact but they are reluctant to speculate in detail. In part, that may be due to proprietary reasons. Were a company to have a reasonably clear picture of the future, that company would have an enormous advantage over its competitors. I know that various companies are trying to visualize what's going to happen and it could mean a lot of money to them. In the absence of predictions by others I have made my own set in a special issue of Science to be published 12 February 1982.

The following are some of my own perceptions:

1) The revolution will continue throughout this century. Technological progress in this area will likely move toward (i) ever denser packing of solid-state devices, approaching 1 million circuits per chip, (ii) widespread use in telecommunications of both satellites and fiber optics, (iii) merging of data-processing and telecommunications technologies and systems, and (iv) large stand-alone machines and systems giving way to multinode networks and distributed processing.

2) Applications of computers will increasingly permeate a wide spectrum of human activities. Costs will continue to decrease relative to those of other goods and services.

3) There will be a change in the nature of the employment of a large fraction of the work force. Many routine tasks will be automated. Highly intelligent and educated people will find themselves in even greater demand. There will be more unemployment of those less well endowed or adequately trained.

4) In the management sector there will be more decentralization of office facilities. Electronic communication with video terminals will lessen the need for personal encounters and for much of the travel that now takes place.

5) A proliferation of sources of information, means of communication, and entertainment of many kinds will have profound effects on home life. The home will become a more attractive place to live in and to use as a base for many kinds of intellectual and other activities.

6) The revolution will affect those aspects of the economy that compete for the consumer's disposable income. Many consumers will find that they obtain more satisfaction from money spent on electronic products than, for example, on automobiles.

7) While bringing many benefits to society, the revolution will also bring problems, tension, and disbenefits. Changes in employment carry with them trauma for those displaced. To the ancient tensions between rich and poor will be added tensions between those with high intellectual capacity and the less gifted, and between the well educated and the untrained. Problems will develop with fixation on entertainment and possible misdirection of computer systems for antisocial purposes.

In conclusion, while world attention has been largely focused on energy problems, a relatively unnoticed revolution has been taking place in applications of electronics. This revolution is destined to have great long-term consequences. It is quite different in nature from the earlier industrial revolution. That revolution was based on profligate use of energy largely in the form of fossil fuels. Much of its technology was crude with only a modest scientific or theoretical base. In contrast, the electronics revolution is one of the greatest intellectual achievements of mankind. Its development has been the product of the most advanced sciences, technology and management. In many applications, electronics requires little energy. The industrial revolution dependent on energy will be slowed and limited by cost and scarcity of energy. The electronics revolution, fueled by intellectual achievement, is destined for long, continued growth as its knowledge base inevitable increases and provides increasing potentials for change.

Steven Weinberg, winner of the 1979 Nobel Prize in Physics, is the Higgins Professor of Physics at Harvard University and a senior scientist at the Smithsonian Astrophysical Laboratory. During the 1980-81 academic year he was visiting professor of physics at the University of Texas and will be returning to a permanent faculty position there next year. The Nobel Prize was awarded to Weinberg, Sheldon Glashow, also of Harvard, and Abdus Salam of the International Center for Theoretical Physics in Trieste and the Imperial College, London, for their contributions to the theory unifying the weak and electromagnetic forces, two of the four fundamental forces of nature.

A native of New York, N.Y., Weinberg earned his bachelor's degree at Cornell University, studied for a year at the Copenhagen Institute for Theoretical Physics, and then at Princeton University, where he received his Ph.D. in 1957. He taught at Columbia University for two years before coming to Berkeley to be a research associate at the Lawrence Radiation Laboratory and to teach at UC, where he attained the rank of professor in 1964. In 1969 he left Berkeley for the Massachusetts Institute of Technology. He joined the Harvard faculty in 1973.

He is the author of "The First Three Minutes," a prize-winning book for the layman on the origin of the universe, and also has written a scholarly work, "Gravitation and Cosmology: Principles and Applications of the General Theory of Relativity."

He is a member of the National Academy of Science and recently became one of a select few foreign scientists to be elected to the Royal Society of London. Other awards include the Dannie Heineman Prize for Mathematical Physics, the Oppenheimer Prize and the Elliott Cresson Medal of the Franklin Institute.

THE ULTIMATE STRUCTURE OF MATTER

STEVEN WEINBERG

I want to make it clear from the outset that the rather grandiose title of this talk, "The Ultimate Nature of Matter" was not chosen by me. It was chosen by the organizing committee. However I did leave it although I had a chance to change it. Partly because that way I could blame it on the organizing committee. Also in fact it is precisely what I'm going to talk about. To be a little bit more specific, I want to talk about an old question in physics - what are the fundamental entities of which we regard our universe as being composed -- particles or fields?

I don't mean this in the sense of how we should look at our existing theories. It really isn't terribly important given a theory whether you describe it in words having to do with particles or words having to do with fields. The important thing is whether it works. The question I'm asking is in what direction we will have to look in the future for more satisfactory theories of matter. In other words, my question is not philosophic but strategic. It is a question that quantum physicists have had to struggle with a great deal and it's appropriate to talk about it here because many contributions to our enlightenment on this issue have come from here. I'm going to describe something of the history of this old question and then tell you where I think it stands now in today's physics.

In its modern form, the question of a universe of particles or universe of fields is roughly about as old as the "rad lab." In the first decades of the 20th century the question didn't arise, or at least not in its modern form. Physicists lived comfortably with a kind of dualism. There were undoubted particles like the electron of J.J. Thompson, and the atomic nucleus, discovered by Rutherford. And then there were fields. There was the electromagnetic field, and the gravitational field. True, it was understood and worked out during the period from Einstein in 1905 to Dirac in 1927 that light has a particle nature, that electromagnetic waves can in some sense be thought of as consisting of particles, called, (by the Berkeley chemist, G. N. Lewis) photons. Also, there was an effort early in the century by Abraham and Poincaré and others to understand the electron as a bundle of energy of the electromagnetic field.

But no one at that time, I believe, dreamed of turning it around and thinking that such material particles as the electron or the proton might have anything to do with an electron field or a proton field. And this was not changed, despite what is sometimes said, by the advent of the quantum mechanics of the 1920's. In that quantum mechanics, as it developed 1925-1926, the description of nature was changed from a description in terms of the trajectories of particles -- where a particle is at any one moment and how fast it's going -- and fields -- what are the values of the electric and magnetic fields at any one moment and any one position - to a description in terms of wave functions, probability amplitudes, quantities that give you the probability of finding a certain configuration. But these probability amplitudes were still regarded as depending on the positions of particles <u>and</u> the values of fields. For example, the position of every electron in the universe and the values of the electric and magnetic fields at every point in space and time were taken as the arguments, the independent variables, the things on which the wave function depended.

Dirac attempted in 1928 to formulate a relativistic quantum mechanics; he attempted to take this theory of particles and fields, this dualistic theory, and make it consistent with the principles of special relativity. It was strikingly successful as applied to electrons and electric and magnetic fields, but with the benefit of hindsight we can now say that it could not be extended to the rest of physics, and in particular not to the weak and strong nuclear forces. The fact that it was not generally successful has often been lost sight of because the mathematical machinery invented for by Dirac for this purpose has become part of the standard stock in trade of all theorists that followed him. But in fact Dirac's effort to make a relativistic quantum mechanics of particles and fields was not the way of future physics.

Then in 1929, for the first time, it became possible to have a unified view of the constituents of the universe. I refer to the work of Heisenberg and Pauli in a pair of articles written in 1929, one of them published in 1930. In these articles, Heisenberg and Pauli constructed what we have come to call quantum field theory. The name tells exactly what it is. In this theory, the fundamental constituents of matter are taken to be fields. There is an electro-magnetic field, that's no surprise. There's also an electron field, there is a proton field, there is a field for every fundamental particle. The particles emerge when quantum mechanics is applied to these fields, but the particles themselves are mere epiphenomena, just bundles of energy of the field. The energy of the fields are concentrated in little knots and the knots go zipping around and that's what we call particles. But the underlying reality is the field.

The quantum field theory of Heisenberg and Pauli led to an idea of what is meant by an elementary particle. An elementary particle is a bundle of energy of one of the fundamental fields that inhabit the universe. Everyone thought the electron, for instance, was an elementary particle, so they assumed that the fundamental field theory would have to involve something called the electron field. Likewise the photon was regarded as a fundamental particle, so the fundamental field theory would be regarded as also containing an electromagnetic field of which the photon was the quantum or bundle of energy. Other particles, like say the nucleus of the iron atom or this blackboard eraser were regarded as composites. And the fact that they were composites was given a precise meaning in the sense that the basic field equations, which govern the system of fields that makes up the universe, do not contain a field for the iron nucleus or a field for this eraser. These are composed of more elementary particles whose fields do appear in the fundamental equations.

Quantum field theory scored an immediate triumph in 1933 when Fermi used it to derive a theory of the kind of radioactivity known as beta decay, which is the prototype of a whole class of elementary particle interactions which have become to be known as the weak interactions. Beta decay is a process in which a nucleus changes from one element to another element, emitting in the process a negative or a positive electron and a neutrino or an anti neutrino. This was a process that involved the creation of new particles that could not have existed inside the nucleus. And it was a process that could not possibly be understood within the framework of the old particle quantum mechanics either in its original form or in the relativistic version of Dirac.

I don't know why Fermi's achievement did not by itself convince physicists of the need for a quantum field theory. Perhaps part of the reason, perhaps the whole reason, for a hesitancy about quantum field theory after its initial development by Heisenberg and Pauli, was the fact that it immediately ran into a terrible mathematical trouble. In the first few months after the second Heisenberg/Pauli paper was written in 1929, the new quantum field theory was found to be plagued by a terrible inconsistency. One of the first to encounter this problem was a young professor of physics new on the faculty at Berkeley, J. Robert Oppenheimer.

Oppenheimer set out, using the quantum field theory of Heisenberg and Pauli, to calculate the contribution to the energy of atomic states, (the various levels that an atom can be found in) from its interaction with the quantum field of the electron and the proton. And Oppenheimer found, to his surprise and chagrin, that the answer was infinite. The answer was even infinite if you subtracted the energy of two different levels, so it wasn't just that the whole zero point of energy was being shifted by an infinite amount, which wouldn't be observable. The energy difference between two atomic energy levels, the quantity which is directly measured when you observe the frequency of light given off by an atom, came out to be infinite in Oppenheimer's calculation.

It was regarded as a disaster. Waller, in Sweden, discovered the same thing, although he was considering free electrons rather than electrons in atoms, and he told the result to Pauli and Pauli did not believe it because it seemed to mean the end for the quantum field theory that he and Heisenberg had just developed. Then other infinities were discovered. Theoretical physicists would set out to calculate some perfectly sensible thing like the energy difference between two states of a hydrogen atom, and the answer they get would be a perfectly nice number, and then they would carry the calculation to the next order of approximation, and in the next order of approximation they would get an infinite answer. The infinite answer takes the form of an infinite sum over all the ways that momentum and energy can flow between the electron and the radiation field. Other infinities were found in other physical processes. (In fact, more infinities were found than there actually were; there were errors made in some of these calculations which did much to confuse the issue.) There soon developed a general feeling of pessimism about the whole future of the field view of nature.

Many physicists retreated to a position that while the field concept might work as an approximation, there was something basically wrong with it, and if you carried your experiments to, say, one order of magnitude higher energy than the energies which were then accessible to the kind of accelerator that Lawrence was working with here, that then you would find that the field concept just did not work. (That's always a very common sentiment among physicists, that if you carry experiments to one higher order of magnitude in energy than the energies that are now accessible, that then you'll find that existing ideas don't work. And it's sometimes true.) In particular, Oppenheimer was very much impressed by the fact that in the cosmic ray experiments which were being done at that time, there were discrepancies between the field theory of electricity and magnetism that had been worked out by Heisenberg and Pauli and what was observed for cosmic ray showers. At the time no one realized that this was due to the production of particles called mesons, of whose existence physicists were then unaware.

Oppenheimer interpreted the cosmic ray data indicating a breakdown of quantum field theory itself.

Because of the problem of the infinities, there began by the middle 1930's a return to a view of nature as particles rather than fields. It started with John Wheeler in 1937 and then Heisenberg in 1944. Heisenberg took the point of view, following an ancient and honorable tradition in physics, that the laws of physics should not only make predictions solely about observables, but should not in the formulation of the laws refer to anything but observables. That is, every ingredient in physical law must be something that can be directly observed; physics has no business talking about things that are in principal outside the range of experiment. This satisfied a deep urge in physicists. This philosophic doctrine, which I believe is sometimes called logical positivism, is a recurrent theme in the physics of our century. It was for instance very useful to Einstein in his work on relativity. I think physicists often have a feeling that when their theories don't work it's because they've been naughty and introduced unobservable quantities, and if they would only purify themselves and return to things that are observable, then everything will work out.

For Wheeler, and then Heisenberg after him, the observables were the probabilities, or to be more technical, the probability amplitudes, for various collisions among particles. These give the probability, for instance that if you start with two particles coming toward each other at such and such an energy and angle, then you'll end up with three particles going out at such and such energies and angles. All of these probability amplitudes were united by Wheeler and Heisenberg into a quantity called the S matrix, S for Streung or scattering. This matrix, an infinite array of complex numbers, would give you all the probabilities for all conceivable collision processes among particles. And the idea was that these were the only things in physics that were ever going to be observed. You would never ever look into a collision and see the local field theoretic processes that had been described by Heisenberg and Pauli so you shouldn't think about them, you should just think about the S matrix and make a theory in which all the laws were formulated in terms of the S matrix.

The issue was now squarely joined. On one side was a field theory of nature in which the underlying reality is a world of quantum fields and in which particles are merely bundles of energy in the fields. In this view, the laws have to be formulated in terms of the equations that govern the fields -- equations like Maxwell's equations that govern the electromagnetic field. In opposition to this was a particle or S matrix theory, in which the underlying reality is a world of various kinds of particles, and in which when the field idea is useful at all (as everyone knows it is useful in dealing with electromagnetism), the fields are to be regarded as just some kind of collective state of huge numbers of particles, of the sort called a coherent state, but these fields are merely a convenient mathematical abstraction for describing large numbers of particles. In this view the laws of nature have to be formulated not in terms of field equations but in terms of axioms that describe the S matrix, the array of all the probabilities for all different collision processes.

From the mid 1930's on, the mood of physics was going to swing back and forth several times, from S matrix theory back to quantum field theory, then back again to S matrix theory, then back again to quantum field theory. I want here to raise the question whether or not the time is approaching when we will have another swing back to something like S matrix theory.

But before I go into this history of these swings of opinion and my comments about where we're headed now, I think I must admit that I have been guilty of an historical oversimplification. Although we may now look back and see a clear opposition between a particle or S matrix view and a quantum field theory, nothing in life is ever that clear. It certainly wasn't that clear in the 1930's. One of the great confusions in this story is that for certain problems (though by no means for all problems), Dirac's relativistic quantum mechanics, the dualistic quantum mechanics of particles, of electrons and the electromagnetic field, was equivalent to the quantum field theory of Heisenberg and Pauli. Physicists often referred to them as if they were interchangeable. Weiskopf recently has written in some reminiscences that the paper he and Pauli wrote in 1934 was designed specifically to demonstrate the need for a thoroughgoing field theory. The issue hinged on the nature of anti-matter. Dirac's view was that in addition to the electrons that we see normally, there is an infinite number of electrons with energies lower than the zero energy of empty space, the so-called negative energy electrons. Every once in a while there's a hole in the sea of negative energy electrons and that hole we see in the laboratory as an electron but of opposite charge (because a hole in the sea of negatively charged electrons would appear as a positively charged particle). This "anti-electron" or positron was then discovered in 1932. Pauli and Weiskopf showed that this view of the nature of anti-particles which was built into Dirac's relativistic quantum mechanics was inadequate; they did this by showing that other sorts of particles also had anti-particles, particles that could not possibly form a sea of negative energy particles. These are the particles that physicists call bosons. They cannot form a stable sea of negative energy particles; if you put a boson into a negative energy state it will just sink forever to lower and lower energies. We now understand that Pauli and Weiskopf were right and that in fact every non-neutral particle has an anti-particle and these anti-particles are in no sense to be thought of as a hole in a sea of negative energy particles. (Among the non-neutral bosons are the $W^{\pm}$ particles which are changed in nuclear beta decay.) The fact that every particle has an anti-particle I suppose became settled in most people's minds when the anti-proton was discovered here in Berkeley in the 1950's. However, even now the hole theory still appears in textbooks. I suppose it's an example of physicists not taking the trouble to rewrite their history.

Now back to the mainline of my talk. I want to talk now of the swings of opinion between quantum field theory and S matrix theory. The first revival of quantum field theory came in the late 1940's through the work of a number of people, Schwinger, Feynman, Tomanaga, Dyson, and others. It was found that the infinities that had been discovered by Oppenheimer and Waller and others in the 1930's were in fact due to a simple misinterpretation of the theory. (That is, to what we now say with the benefit of hindsight was a simple misinterpretation. Nothing is simple when it actually happens.) The misinterpretation was the identification of the quantities e and m which appear in the field equations with the electric charge and mass of the electron as they're actually measured in the laboratory. It became clear that when we measure, say, the mass of the electron in the laboratory, we're not measuring the quantity m which appears in the field equations but in fact we're measuring that mass plus the effects of a huge number of so-called radiative processes in which the electron emits and then re-absorbs photons many times. And all these processes are always going on all the time. You cannot ever measure the mass of the electron apart from these processes. The Department of Energy may turn off accelerators but it can't turn off these processes. As a result, no one has ever seen a bare electron, an electron without its cloud of photons. And once you realize that the quantities appearing in your equations are not the ones that are measured and you reinterpret your equations to express them in terms of the measured quantities, all the infinities simply cancel. I'm making it sound really quite simple, although none of it was easy to see.

Quantum field theory now worked magnificently. A few years ago, just to give an example, I looked up the numbers for the comparison of theory and experiment for what's called the magnetic moment of the electron. (That's just its strength as a magnet, in the units of what the electron's magnetic moment was in Dirac's theory.) The experimental value was 1.00115965241 and the theoretical value is 1.00115965234. The discrepancy is in the tenth decimal place and is easily accounted for by both experimental and calculational uncertainties.

There is an interesting historical aside which I suspect not too many people know because probably not many people have read Oppenheimer's 1930 paper. (Incidentally, that paper appeared in a journal which had not until then been the scene of important publications in fundamental theoretical physics, the Physical Review.)

One of the triumphs of the revival of quantum field theory in the late 1940's was Bethe's calculation of what's called the Lamb shift, which is just the splitting in energy (due to emission and adsorption of photons) of two otherwise equal energy levels of the hydrogen atom. He calculated it and it agreed with the experimental value that had just been determined by Willis Lamb. If you look back at Oppenheimer's paper you will find that he has everything there for the calculation of the Lamb shift. All he needed to do was to give the crank one more quarter turn and he would have had Bethe's formula for the Lamb shift, and could have calculated the numerical value. Really, all he had to do was put in the numbers and when he had the infinity, just throw it away, and he would have gotten the right answer, or an answer to as good an approximation as Bethe's. He didn't do it because he didn't have confidence in quantum field theory. What happened in the late 1940's was precisely a restoration of confidence in quantum field theory.

But the confidence didn't last very long. More problems were found and there was another revival of S matrix theory in the 1950's and early 1960's. One of the problems was that although these infinities beautifully cancelled in the theory of electricity and magnetism and electrons, which is known as quantum electrodynamics, the infinities would not cancel in that way in the theory that Fermi had developed to describe the radioactive process of beta decay. (The generic name for the force that produces these processes, as I said before, is the weak interaction because it has an intrinsically weak strength, which makes processes like beta decay go very slowly.) For the weak interactions (of which many more were known at that time), this lovely trick of cancelling infinities just didn't work. That was, of course, an old story but it was hoped that the new idea of absorbing the infinities into a redefinition or, in other words, a renormalization of the electron's mass and charge would also work when you applied the idea to the weak as well as the electromagnetic interactions. But it just didn't work.

The second problem was the apparent hopelessness of calculations involving strong nuclear forces. (These are the forces that hold the nucleus of the atom together. It's the strength of these forces that makes the nucleus so much smaller than the atom and that gives rise to such enormous energies when you disrupt the nucleus, typically a million times larger than the energies that are released when you disrupt an atom in an ordinary chemical reaction.) In quantum electrodynamics one has a lovely situation in which the next to lowest order approximation to any calculated quantity is about 137th the lowest order (in rough magnitude). The next to next-to-lowest order is another factor of 137 smaller, so if you want values to a certain accuracy you just take a certain number of terms in your perturbation expansion and you get pretty good results. In the strong interactions, the quantity which in quantum electrodynamics is 1/137, the number that defines how strong the force is, is more like 1, and so the first term is of order 1, the second term is of order 1 square, the third term is of order 1 cube, and you just don't make any money that way.

Finally, during this period from the late 50's to the early 60's, there was a profusion of new particles being discovered, very largely here at Berkeley at the Bevatron and very largely through the capabilities opened up by the bubble chamber. These particles seemed every bit as elementary as the proton and the neutron which are the constituents of the nuclei of ordinary atoms. In fact some of them even form families with the proton and neutron. And others look just as good. And there were so many of them. And physicists even got to the point where they had to carry around a booklet, which many of you have seen which is published now here in Berkeley, listing all the particles. It was clear that anything you need a telephone directory for can't be elementary. Remember, this idea of elementarity was tied to the idea of a field theory. That is, the elementary particles were those which were associated with the fundamental fields described by the field equations which were seen as the basic laws of nature, but it didn't look like there were any particles that were any more elementary than any others.

Here at Berkeley, Chew and Mandelstamm and Stapp and others, set about reviving S matrix theories, but now with a much more specific and mathematically powerful set of axioms. I hope the nonphysicists in the audience will forgive me, but I'll just name these by the code words, the buzz words that became common among physicists: unitarity, analyticity, Lorentz invariance and clustering. (I won't tell you what any one of them are.) But these were the basic properties which it was argued, a theory would have to have to be physically sensible at all. And the hope was that if you demanded these properties, they would provide so many equations relating different elements of the S matrix that the whole theory would become uniquely defined and you could actually solve the equations and come up with numbers for physical quantities. And on top of this you would have the happy feeling in the back of your mind that you were doing what a scientist should do: You were dealing at every point with physical observables and you were not getting involved with the mythical quantities, the quantum fields, that Heisenberg and Pauli had used in 1929.

Now S matrix theory as developed by Chew and Mandelstam and the others, did not in fact, prove in any sense a failure. It did not lead to results that we now turn our backs on. It was however to a certain extent bypassed by the main stream of the history of physics in the following years. This first revival of S matrix theory was in the 1950's and the early 1960's. But it was followed by a second revival of quantum field theory (the first one was the 1940's), extending from the late 1960's to the present.

The reasons for the second revival of quantum field theory as I say had nothing to do with any failure of S matrix theory, but successes in other directions. First of all, theories of the weak interactions and of the strong interactions were developed during this period which were just as good from the point of view of infinities as the older theory of quantum electrodynamics. When I say just as good, I mean again, if you just were careful to properly identify what are the physical quantities that you're talking about, what's the physical electric charge, what's the physical mass of the electron, what's the physical quantity analogous to electric charge in beta decay, then all the infinities would cancel. Theories were developed for both the weak and strong interactions in which that was true. In fact, it was not just accidentally true; these theories were built on an analogy with the theory of electromagnetism. In the case of the weak interactions it was more than an analogy; there was actually a unification with electromagnetism, so that increasingly physicists no longer refer to weak and electromagnetic interactions, but just call them electroweak-interactions. The theory of strong interactions was also constructed in close analogy with electromagnetism. In fact if you look at a page of equations, until you're told exactly how many values the varying parameters take and what the indices mean, etc., the equations look the same. They're very similar theories and the infinities cancel in essentially the same way. There are reasons why it took so long to develop these theories, having to do with gauge symmetries and broken symmetries and things like that that I will not have time to go into here.

Another reason for the second revival of quantum field theory was the fact that it was found that the strong interactions, although very strong at distances typical of the size of the particles in the nucleus, get progressively weaker as we go to very, very short distances or equivalently very, very high energies. This has meant that it is now possible to use our theory of the strong interactions to do actual calculations in the same way that we use quantum electrodynamics to calculate what happens inside an atom. The calculations tell us what happens to elementary particles at very high energy, much higher than is typically found in the nuclei in atoms. And the successes of these calculations in comparison with experiment are sufficient to convince us (or will be in a short time) that this theory is correct. I can't claim that it's entirely verified experimentally but there's not much question that it's correct.

Oddly though, the old questions of how to calculate the nuclear forces that hold the nucleus of the atom together, or the kind of thing that was worried about at Berkeley when I was here in the late

50's, like what is the cross section for scattering a pion on a nucleon at 700 MeV, cannot be answered today any more than they could have been 20 years ago. This is a process I think that often happens in physics. You don't solve all the problems that concern one generation of physicists; instead, the next generation finds there are more urgent problems. The only important thing in the end is not to solve every problem, but to solve enough problems so that you know you have the right theory. And that is what we're in the process of doing with the theory of the strong forces.

The third thing that happened during the second revival of quantum field theory was the realization that there are particles that seem pretty elementary after all. There's the good old electron, and it comes with a family of siblings, the muon, the tauon, and so on, and related particles called neutrinos. No one has ever found any structure inside them, and there aren't an enormous number of them, just the electron, mu, tau and their corresponding neutrinos. In addition, in place of the proton and the neutron and other particles that were being discovered in Berkeley in the 1950's and early 60's, we now have a much smaller set. The proton and the neutron and all those other particles discovered here seem to be composites, made up of these quarks. In addition, we have one other class of elementary particles, there is the photon, the good old quantum of light and it too has siblings, particles called gluons and other particles too heavy to have been produced yet called W and Z particles. (The Z particle is due to be discovered in Geneva pretty soon now.) All these particles are regarded as elementary, in the sense that they are manifestations of the quantum fields that appear in the underlying field equations, and they are what they are because of these field equations.

In 1975 I gave a talk about all this at Harvard and I don't remember anything at all about what I said in the talk. But I remember the title I used for the talk, and it gives a pretty good idea of what I think had been happening in the preceding decade. The title was "The Renaissance of Quantum Field Theory". That might seem the end of the story. These have been exciting times. Quantum field theory is riding very high and one might be forgiven for a certain amount of complacency with it. But perhaps we will see another swing away from quantum field theory. Perhaps that swing will be back in the direction of something like S Matrix theory, back to a view of particles as fundamental. There are several reasons that I can point to for this. (By the way, in case you didn't notice I'm now finished with the historical part and up to 1981.) One of the reasons is the continued failure to make a mathematically satisfactory quantum field theory of gravity. The problem again is these damn infinities that Oppenheimer and Waller discovered here in 1930. There's no quantum theory of gravity which is free of these infinities and we don't have any good idea of how to make one. From all indications the existing quantum field theory, at least of gravity, and perhaps quantum field theory in general, needs some kind of modification at an energy at or below the very high energy of 10^{19} proton masses. (That's a one with 19 zeros.) Something has to happen new in physics because our existing theories simply break down at these energies.

Another hint of a new energy scale in physics comes from the fact that I've mentioned already, that the strong interaction strength decreases as you go to high energy. If the strength of the strong interactions decreases as you go to high energy, then perhaps it's merely the accident that we are doing experiments at relatively low energy that makes the strong interactions look so much stronger than the other interactions. Perhaps the strong and electro-weak interactions really all have the same strength at a fundamental level.

The decrease in the strength of the strong interactions is only logarithmic with energy, so the energy at which the strong and electroweak forces become comparable have to be enormously high; in a very wide variety of theories, it is found that that energy is about 10^{15} proton masses. So here again we're led to contemplate enormously high scales of energy.

These hints suggest that there is a fundamental scale of energies in physics far beyond anything that is accessible or will ever be accessible to our accelerators until someone finds some way of putting a macroscopic amount of energy, like the energy in an automobile tankful of gasoline, on one elementary particle. Perhaps the theory of the new ultra-high energy scale in physics will not be a quantum field theory at all. We don't know. We can't do experiments at these energies.

But if it's not going to be a quantum field theory, the question naturally arises, What are these beautiful theories that we've delighted so much in developing? If the underlying truth is not a quantum field theory, then how come the quantum field theories that we have developed, quantum electrodynamics and then the generalization of quantum electrodynamics to include the weak interactions and the theory known as quantum chromodynamics that describes the strong interactions, why do these beautiful field theories work so well? What are they if they're not fundamental? The answer may be given by reference to two theorems. These are what I believe Wightman calls "folk theorems," that is, things that have never been proved but are well known to be true.

Let me quote to you two folk theorems. I quote them partly because I think they can probably be formulated in precise terms and proved, and though I haven't done it, they are true. The first folk theorem is that if we write down the most general quantum field theory (for the physicists here, I'll say the most general Lagrangian) including all possible terms in the theory, that satisfy the appropriate symmetries, and we calculate processes to any given order of perturbation theory, that is to any order of approximation, then what we get, provided we are talking about the most general possible quantum field theory and not some specific theory, is simply the most general possible S matrix element which to that degree of approximation satisfies the S matrix axioms of Chew, Mandelstam, and Stapp, et al. Another briefer way of saying that is that field theory is without content. Quantum field theory, divorced from specific theories, but just the general idea of quantum field theory, is without content, it's just the best way of implementing the axioms of S matrix theory.

In fact quantum field theory has been used in precisely this way ever since 1967 in studying the interactions of low energy pi mesons, particles which no one today regards as elementary anymore and yet which are described by a quantum field theory which is used to calculate their various reactions. The reason that it works is precisely because all that quantum field theory does for you is to reproduce the most general S matrix consistent with the assymetries that you're assuming and consistent with the axioms of S matrix theory.

But if that's all quantum field theory is, if quantum field theory is just a clever mathematical trick for implementing the axioms of S matrix theory, then why are the detailed working theories that we've developed, quantum electrodynamics and quantum chromodynamics and quantum electroweak dynamics and so on, why are these theories so beautiful? You expect to find beauty in physics if you deal with physics at a fundamental level. If you're dealing with something that's just a lot of mathematical trickery, then why should it look so simple? Why, for example, are the field equations of quantum electrodynamics, or Maxwell's equations, for that matter, so simple?

The answer to that question may be found in a second folk theorem. The second folk theorem says, in theories with a natural energy scale, (and I'm thinking here of energies like 10^{19} proton masses or 10^{15} proton masses or whatever) if studied at much lower energies than the natural energy scale will always be found to be described to a good approximation by an effective field theory which is as simple as possible. (In technical language, the effective Lagrangian is dominated by terms with the fewest fields and/or derivatives.) Where possible, the interactions in this effective field theory will be so simple that they allow the cancellation of infinities to go through as they did in quantum electrodynamics. I suggest that this is why quantum electrodynamics and quantum flavor dynamics are as simple as they are. Where this is not possible, where the symmetries simply don't allow interactions that are that simple, then the physics will be dominated by interactions which do not allow the cancellation of infinities to go through in a simple way, and which are also very weak, being suppressed by powers of the natural mass scale, whether it's 10^{15} or 10^{19} proton masses or whatever.

We see at least one example of such very weak forces in the world we study today: gravitation. The fact that gravitation is outside the scope of quantum field theory is just because the symmetries that it has to satisfy are too stringent to allow field equations which are simple enough for this cancellation of infinities to occur.

Another possible class of extremely weak interactions which has been much discussed lately and which we may discover in the laboratory are the interactions which could lead to the decay of the proton or to the mass of the neutrino. Elaborate experiments are underway now looking for these things and they may be found.

Of course, if you're waiting for a proton to decay, you have to have an awful lot of protons. They live, after all, much longer than the age of the universe. You have to have a huge number, like say all the protons in 10,000 tons of water and wait for one or two flashes a month of light which indicates that a proton has decayed. So these experiments are done deep underground to get away from cosmic rays which would otherwise give too many spurious signals. (The thing that is holding up the two biggest experiments on proton decay, you may be interested to learn, is that these experiments are being done, in a salt mine in Ohio and in a silver mine in Utah, and at the present moment the price of salt and silver are both so high that the companies are furiously mining, and they will not allow the physicists to get into the elevators and take their equipment down into the mines. You thought you had problems with accelerators.)

If our quantum field theories of which we're so proud are just the debris of some really fundamental theory which describes all of physics including gravity, it may be that the really fundamental theory will have nothing to do with fields; it may not look like a quantum field theory at all. I think we have to leave this as an open possibility and maybe, in fact, that it will be something like an S matrix theory.

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He has held various offices in the American Board of Nuclear Medicine, the American College of Radiology, the American College of Nuclear Physicians, Society of Nuclear Medicine, Association of University Radiologists and other professional organizations. Recent honors include: Distinguished Scientist Award, Western Regional Chapter of the Society of Nuclear Medicine; Eugene P. Pendergrass Lecturer, University of Pennsylvania; Ernst Jung Prize for Medicine, Hamburg; David M. Gould Memorial Lecturer, Johns Hopkins University School of Medicine; John Kershman Memorial Lecturer, Eastern Association of Electroencephalographers; Herman L. Blumgard, M.D., Pioneer Award, New England Chapter, Society of Nuclear Medicine.

FROM SCIENCE LABORATORY TO HOSPITAL: NEW IMAGING INSTRUMENTS

DAVID E. KUHL

A revolution in noninvasive imaging is now underway in medicine. This is especially true in the imaging of structure and function of the brain. These advances, past and present, come from many laboratories, including Lawrence Berkeley. The implications of this revolution are not only for enhancing the diagnosis of human cerebral disorders, but also for improving our understanding of how the brain works.

In the past century, the great neurologists learned that brain lesions seen at autopsy could be associated with the signs and symptoms of their patients and, in brilliant ways, developed clinical neurological examinations from which they could infer the nature and location of intracerebral disease in the living patient. Although marvelously integrated, the brain also functions in many ways as a collection of individual organs and what happens to one specific part of the brain can characteristically influence the patient's symptoms and signs. However, the soft tissue of the brain protected in its boney box is not so easily examined as the abdomen by palpation, the lungs by auscultation, or the bones by simple radiography. Thus, the subsequent introduction of neuroradiological techniques such as air contrast pneumoencephalography, contrast arteriography, and, more recently, x-ray computed tomography, have had great influence on the diagnosis of intracerebral disease, especially that of mass lesions such as tumors, hematomas, and abscesses. Brain disease lacking such focal alterations in brain structure have remained more difficult to localize by imaging methods.

At the same time as clinical diagnosis of focal brain lesions improved, neuroscientists advanced understanding of brain organization and function. It was learned that as each part of the normal brain performs the work of receiving and transmitting electrical information, the required energy is provided by proportionate increases and decreases in local metabolism which in turn cause proportionate increases and decreases in local blood flow required to provide an appropriate supply of metabolic substrates. Neurochemical studies yielded new insights into how messages are transmitted from one part of the brain to another. But a major restriction in neuroscience has persisted. Studies depending on destructive testing remain limited to animal models of disease or to autopsy specimens from patients.

One approach to extending neurochemistry research to the living human brain is by exploiting the radioactive tracer method. In theory, extremely small amounts of appropriately chosen chemicals might be labeled with radioactivity, injected intravenously, and their involvement in cerebral processes measured by external detectors. In practice, this has been difficult to accomplish because the radiations coming from each small part of the brain must be gathered and analyzed separately from those of their neighbors, and the significance of these data must then be understood in terms of real physiological processes.

One step in this quest was realized when Cassen at UCLA introduced the rectilinear radionuclide scanner and it was subsequently applied for noninvasive detection of brain tumors in the early 1950s. Simple compounds labeled with radioisotopes of iodine, mercury, or, later, technetium were injected intravenously in patients suspected of having brain tumors. Because of the different permeability of blood vessels in tumor as compared to normal brain, tumor radioactivity could be detected as an enhanced zone in pictures made by systematically moving a collimated scintillation detected in a rectilinear raster over the brain and recording brain radioactivity by means of a flashing light on film. This procedure was made even more efficient with the introduction of the scintillation camera by Anger here at Berkeley. Brain radioactivity could now be imaged as a picture projected on a large thin crystal of sodiumiodide where individual light flashes were sorted out by an array of photo multipler tubes. The combination of scintillation camera in conjunction with technetium-99m became the mainstay of clinical nuclear medicine and remains one of the most important diagnostic methods in hospital use today for management of a wide variety of diseases.

With ordinary rectilinear scanner or scintillation camera imaging, radioactivity from different individual parts of the brain were superimposed and obscured; accurate separation of their individual activities depended on the introduction of reconstruction tomography. By the end of the 1950s at Pennsylvania, we became interested in displaying brain radioactivity as a cross-section or tomograph. This was accomplished by systematically scanning the edges of a particular plane in the brain and back-projecting these data to produce a reconstructed image of the brain radioactivity in cross-section. This approach had considerable advantage in separating images of brain structures one from another, and later, with algebraic computer reconstruction, we finally were able to produce images that were nearly exact quantitative reconstructions of actual brain radioactivity concentrations. This was necessary if the radionuclide scanning method was to serve as a substitute for the autoradiograph used in animal research. Later, it could be seen that the solution of transverse section reconstruction from data projections was the same as applied to astronomy, x-ray crystallography, x-ray computed tomography, and radionuclide emission computed tomography.

To accomplish a new way of imaging local brain physiology, quantitative autoradiography of animals was to be imitated. At NIH, investigators were exploring local blood flow and metabolism of individual brain structures using long-lived carbon-14 labeled iodoantipyrine and deoxyglucose. Valid models were available which permitted determination of blood flow and glucose utilization in absolute units after animals had been injected with radioactive tracers, the brain sliced and placed on film, and brain activity determined from film blackening. Similar studies would be possible in man when tomographic scanners became more efficient and radiopharmaceutials were introduced appropriate for human use. Unfortunately, commonly used radiopharmaceutials such as technetium-99m and iodine-123 were not easily adaptable to the labeling of biologically relevant indicators.

An important advance now depended on the cyclotron invented by Lawrence at Berkeley many years before. From hospital based cyclotron laboratories established first at Hammersmith, Washington University, and Massachusetts General Hospital, came the realization that biologically relevant compounds labeled with cyclotron-produced short-lived radionuclides such as oxygen-15 (2 minute half life) nitrogen-13 (10 minute half life), carbon-11 (20 minute half life), or fluorine-18 (2 hour half life) without changing biological specificity. Since all of these radionuclides are positron emitters, that is, they emit positive electrons which produce annihilation radiation, coincidence counting methods could improve distribution imaging. Rapidly exploiting reconstruction tomography, Phelps, Ter-Pogossian, Brownell, Budinger and their co-workers developed sufficient instruments for positron emission computed tomography which could now be applied to a potentially large group of organic compounds labeled with short-lived emitters produced in small hospital cyclotrons. The Lawrence Berkeley Laboratory has played an important role in this development, first with the introduction of the cyclotron itself by Dr. Lawrence, and now with innovative contributions in positron emission computed from Dr. Budinger's group. This same experimental approach is now being used in perhaps a dozen laboratories throughout the world for determination of local cerebral blood flow, blood volume, oxygen and glucose utilization, and protein synthesis.

At UCLA, a major portion of our recent efforts have concerned positron emission computed tomography using ^{18}F-fluorodeoxyglucose (FDG) for mapping local cerebral glucose utilization. Our positron tomograph was designed by Phelps and Hoffman and has a spatial resolution of 1.7 cm. We first applied the FDG method using single-photon emission computed tomography at Pennsylvania in collaboration with Reivich and with Wolf at Brookhaven. The method is based on the ^{14}C-DG quantitative audoradiographic model of Sokoloff. With knowledge of certain constants, an operational equation permits calculation of local cerebral metabolic rate for glucose, if one measures the time course for intravenously injected FDG and quantifies local brain activity by tomography. The result is a cross-sectional map where the intensity of each part of the picture is proportional to glucose metabolism at the time of the study. The model depends on the special chemical characteristics of FDG which is taken up by each part of the brain in a manner very similar to ordinary glucose, but which proceeds no further than glucose-6-phosphate in metabolism. Glucose utilization maps are of scientific and clinical significance because each part of the brain depends primarily on glucose for its energy needs and these needs are directly proportional to local function.

You will note in the slides that the FDG scan is a pattern of blackening proportional to glucose utilization, resembling somewhat the cross-section anatomy of the brain in that the surface, or cortex, has higher activity than the more central portions of the picture, just as the color of the grey matter of the cortex is darker than the underlying white matter. Subcortical grey matter structures, such as the caudate nucleus and thalamus can be seen, as well as the more superficial frontal, temporal, parietal, occipital, and primary visual cortex regions of increased metabolism. The FDG scans appear blurred compared to photographs of a sliced brain because the resolution of the method used here is only 1.7 cm.

Our multidisciplinary group at UCLA is actively applying the FDG scan in clinical research to learn more about brain function in health and disease. My slides illustrate examples of what we have learned.

To understand the significance of the FDG scan when the brain is abnormal, we first must understand better results to be expected in normal human subjects. With advancing age, we can detect a progressive reduction in overall cerebral glucose utilization; on the average, the 80 year old brain metabolizes glucose about 20% less than the 20 year old brain. However, this same difference can be found among perfectly normal persons at any age. In England, positron tomography studies using oxygen-15 have shown no decrease in cerebral oxygen utilization with advancing age. This suggests that either glucose is used in alternate cerebral pathway's preferentially in the young, or that increased use of substrates alternative to glucose may be used in the aged. The anatomic distribution of glucose metabolism is much the same in younger and older persons, except for a relatively greater decline in the frontal cerebral cortex with advancing age. This could result from selective deterioration of neuronal components in the frontal cortex of elderly individuals. It could also be a result of an age-dependent difference in the cognitive responses of each individual during the study. For example, Mazziotta has found that the normal cerebral metabolic map as determined by the FDG scan depends strongly on the condition of sensory input and even cognitive processing during the study. In right handed individuals, there is left-right metabolic symmetry when the eyes and ears are open to ambient light and sound, but with eyes and ears plugged, there is right-sided relative hypometabolism. Other metabolic asymmetries can be demonstrated using auditory stimuli, depending on the content of the stimulus and the analysis strategy of the subject. Phelps has shown progressive increases in metabolic activity of the visual cortex with progressive increases in the complexity of the visual scene. These results give promise that tests such as these may help explain how the brain works and eventually be applied to patients with a variety of behavior and sensory alterations.

We now ask ourselves the question: "If the FDG scan is altered by normal brain functions, will these alterations be characteristic when brain function is abnormal?"

These illustrations show some of the results when patients have undergone x-ray computed tomography and the FDG scan after stroke. As you can see, portions of the brain which have been destroyed are clearly seen as defects in both the structural scan and the functional scan. However, we were surprised to find metabolic defects in broad zones of cerebral cortex and in deeper structures in the brain that appeared perfectly normal on the CT scan. This paradox probably represents a distant interruption of neuronal input to local regions of the brain where function and metabolism are thus reduced, but blood flow and structure remain intact. Metabolic patterns such as these may be useful in understanding better the neurological deficits which remain after stroke and also may help guide therapeutic measures to reduce the sequelae.

In collaboration with Engel, we have found the FDG scan is a very valuable asset in the study of patients with epilepsy. In partial epilepsy, a small part of the brain can unexpectedly become overactive, produce abnormal electrical discharges and cause behavioral seizures. In some epileptic patients, medication no longer controls these seizures and consideration is given to surgical removal of the abnormal part of the brain. Electroencephalography, or measurement of brain electrical activity, is helpful in localizing the site of seizure onset. The slides I am showing you illustrate progress we are making in applying the FDG scan to this problem. In the interictal state, or when the patient has no seizures, the part of the brain responsible for seizure onset is reliably demonstrated as a hypometabolic zone. During seizure activity, the previously relatively quiescent brain becomes extremely active and this localized zone appears intensely hypermetabolic on the FDG scan. In another form of epilepsy, petit mal seizures increase brain metabolism uniformly and in the absence of seizures the FDG scan appears normal. Studies such as these should aid in better defining the physiological status of patients with epilepsy.

We have also used the FDG scan in the study of dementia. In Alzheimer's Disease, a biochemical abnormality is considered to be present in the cerebral cortex. The effect of this can be seen in these FDG scans of severely demented patients with Alzheimer's Disease. The overall metabolism of the brain is decreased, but hypometabolism is particularly severe in the frontal and pareital temporal association cortex of the brain.

In contrast, we have also studied patients with Huntington's Disease where the primary damage to the brain is known to be in the caudate nucleus and putamen, subcortical structures deep wtihin the brain. Huntington's Disease is an inherited disorder characterized by the appearance of progressively worsening dementia and abnormal body movements at the age of 30 to 40 years when the subject may have reproduced without knowing of his affliction. At autopsy, the deep brain nuclei associated with body movements, the caudate and putamen, are found shrunken. In patients with advanced Huntington's Disease, we see the effect of this shrinkage in both the x-ray CT scan and the FDG scan. More important, however, in patients with very early Huntington's Disease, we still find profound hypometabolism in the Caudate, even though that structure does not appear shrunken on x-ray CT scanning.

We are now exploring if this hypometabolic defect precedes the appearance of symptoms. These findings are further evidence that biochemical abnormalities are early events in the life history of a disease and precede in time those structural abnormalities on which so much of diagnosis depends today. Technological innovation is important to the progress of the new medical imaging. The FDG images I have shown you today are not nearly as clear and sharp in appearance as those now possible with new positron tomographs which have much better spatial resolution. At the same time, however, attention must be paid to providing as much as possible of these new kinds of information to hospital patients without the present investment of resources required by the positron tomograph-cyclotron combination. There is increasing hope now that single-photon emission computed tomography using more ordinary radionuclides such as technetium-99m or iodine-123, may be a more simple and inexpensive avenue for mapping local cerebral blood flow. As an example, I present these images of our recent work with single-photon emission computed tomography using iodine-123 labeled isopropyl iodoamphetamine. The cross-section images of local cerebral blood flow produced by this means are very similar to FDG scans produced by positron tomography. A pattern may be emerging in which positron emission computed tomography continues to play an important role in sophisticated clinical research and single-photon emission computed tomography is employed to carry the benefits of this research to hospital patients.

I have concentrated today on new advances in imaging brain function with radionuclides. Medical imaging of structure continues a rapid and remarkable evolution. For example, Tobias, Fabrikant and their co-workers here at Lawrence Berkeley are producing high contrast images of extremely small tumors by employing a flux of carbon or neon nuclei rather than x-rays. An elegant recording method employing a sandwich of plastic sheets etched to reveal differences in particle range yields much greater contrast than previously achieved by conventional methods.

In concluding, I show here a series of remarkable pictures of brain structures obtained by nuclear magnetic resonance which depends on other principles, uses no ionizing radiation, and has been predicted as an eventual replacement for x-ray CT in day-to-day study of neurologic disorders.

We have come then to a situation where wonderful benefits from physics and chemistry laboratories have given us the ability to visualize the gross structure of the brain of a living patient with a facility that not too many years ago was only possible at the autopsy table. Now, we are entering a period of facile examination of how each part of the brain functions in its biochemical activities, and this has never before been available to us.

These are exciting times in medical sciences.

John Bertram Adams, British scientist, on December 31, 1980, concluded a five year term as Executive Director General of the European Organization for Nuclear Research (CERN) in Geneva, Switzerland. In the United Kingdom New Year Honours List it was announced that he was to receive the accolade of Knight Bachelor, becoming Sir John Adams.

Born in Kingston, Surrey, he was educated at Eltham College and became associated with the Radar Research Establishment, Malvern, 1940-45, and the Atomic Energy Research Establishment, Harwell, 1945-53 where he helped to build the 180 MeV syncrocyclotron. Joining CERN at its beginning, he headed the team which built the 28 GeV proton synchrotron and later served as Director General before returning to England. From 1960-67 he was director of Culham Nuclear Fusion Laboratory, United Kingdom Atomic Energy Authority, and during that period also served as controller from the Ministry of Technology and as board member for research on the UK Atomic Energy Authority. In 1969 he returned to CERN, to head the team building the 450 GeV proton synchrotron serving until 1975 as Director General of Lab II and from 1976 until this year as Executive Director General.

His honors include the Roentgen Prize, University of Giessen, 1960; Duddell Medal, Physical Society, 1961; Leverhulme Medal, Royal Society, 1972; Faraday Medal, I.E.E., 1977; and the Royal Society Medal, 1977. The latest in a long list of honorary doctorates was presented by the University of Milan to Sir John Adams as "originator and devoted constructor of powerful accelerators which have made fundamental discoveries in elementary particle physics possible, in the framework of collaborations of which he has always been a strong advocate."

THE EVOLUTION OF A BIG SCIENCE

JOHN B. ADAMS

I feel it a great honour to be asked to give an address at the 50th Anniversary Symposium of the Lawrence Berkeley Laboratory. Back in the 1940's, Ernest Lawrence and the physicists and engineers of Berkeley were the heroes of the young men in Europe who aspired to build nuclear particle accelerators. At that time, I was one of these young men and now, 40 years later, I find myself back again at Berkeley joining with you in celebrating the foundation of this great laboratory and being given the opportunity of paying tribute to the accelerator pioneers of the old Radiation Laboratory.

I have been asked to speak about the impact of high energy particle physics on research institutions and particularly to discuss CERN, the European Organization for Nuclear Research, as an example of a science shaping an institution and its relationships with nations, universities and individuals. This is rather a broad subject, more suitable for a social historian than an accelerator builder and I would not like you to mistake what I am going to say as a scholarly history of what I believe to be one of the most exciting and dramatic periods in scientific research. I very much hope that such a history is written one day but that is a job for professional historians who are more detached from the events that happened and can take a more balanced view. This account contains only the personal views of someone who, as a young man, first learned about particle accelerators from the physicists and engineers of Berkeley, who had the good fortune to help build some of them in Europe, and by chance became involved in the institutional changes that these giant machines caused in high energy particle physics.

A real history will need many inputs of this kind by many people from all the regions of the world which have taken part in this great intellectual adventure.

Let me start by recalling very briefly the changes that have occurred during the last 50 years in the way this research is carried out. When it started as a recognizable new subject back in the early 1930's, it was an academic inquiry like any other research carried out in the physics departments of the universities, and it grew out of the studies in atomic physics. As the research probed deeper and deeper into the structure of the atomic nucleus the need arose for intense and well defined beams of particles of higher and higher energies. Specialized machines were developed to provide these particle beams and as their energy increased so did their size and cost until the time came when the machines outgrew their natural habitat, which was the universities, and national and international laboratories were created in which to house them. The research physicists, however, remained attached to the universities where they continued to teach, but they had to travel further and further afield to carry out their research, sometimes to other countries. The size of the experimental equipment - the particle detectors and data analysis systems - also grew with the size of the accelerators and so did the size of the groups carrying out single experiments.

All these changes in the way the research is carried out have profoundly affected the institutional structure of the research system and the life style of its participants. It has even given rise to a new term in the language - "Big Science" - used to describe the large scale operations involved and in recent years this style of research has been frequently criticized. I find it rather significant that the talks today started with dinosaurs and end with the big accelerators.

Nevertheless, despite all the changes of scale and style and the problems these have brought, the research remains a fundamental enquiry into the basic constituents of matter and the laws governing their interactions - an enquiry which started with the ancient Greeks and has persisted throughout recorded history up to the present day despite many setbacks compared with which the present economic crisis is a relatively minor incident.

The driving force behind all the changes that have occurred in this field of research is, as I just have said, the need for beams of particles of higher and higher energies. At the beginning, experimenters used radioactive substances to provide the particles they needed, for example the alpha particles emitted by radium and thorium sources, but these give only low energy particles. At the other end of the spectrum Nature provides very high energies in the cosmic rays but they are ill-defined in time, energy and direction and of feeble intensity. Two events happened at about the same time, just at the beginning of the 1930's, which promised an alternative to these natural sources. The first was the experiment of Cockcroft and Walton using a voltage multiplier to accelerate protons with which they disintegrated the nucleus of the lithium atom. The second was the invention by Ernest Lawrence of the first cyclic accelerator. The voltage multiplier proved a dead end as far as developing accelerators to higher energies was concerned but it showed the potential of accelerators as particle sources. The cyclic machines turned out to be eminently developable and through a series of remarkable inventions have enabled particle energies to be increased by about eight orders of magnitude in 50 years during which time the size of the accelerators has grown from devices that could be built on a table top to machines which measure many kilometers in circumference.

Although the history of the development of cyclic particle accelerators is very well known, especially in this laboratory, it is so vital to my subject that at least I must recall the main events. What happened was that one type of machine succeeded another and as each type reached a limiting energy, sometimes for fundamental reasons but more often because extending its energy would have led to prohibitive costs, a new idea was put forward which overcame these limitations and allowed higher energy machines to be built. The remarkable thing was that these new ideas arrived at just the opportune moment so that the research proceeded rather smoothly from one energy range to the next. When the cyclotrons of Lawrence and Livingston were reaching their energy limit due to relativistic effects causing the particles to drop out of phase with their accelerating voltage, McMillan and Veksler invented phase stability. The cyclotron became the synchrocyclotron and the energy limit was extended from about 20 MeV to nearly 1 GeV. When the huge magnets of the synchrocyclotrons looked like becoming an economic limitation, annular magnets were adopted and the accelerating voltage frequency was tracked with the rising magnetic field to keep the particles circulating at constant radius as their energy increased. This new type of machine, called the synchrotron, enabled the energy limit to be pushed up by another order of magnitude to 10 GeV. When the size of the magnets of synchrotrons was reaching an economic limit due to their weak beam focusing properties, a much stronger focusing system was invented by Courant, Livingston and Snyder, which greatly reduced their cross-section dimensions and their cost, and this enabled the 30 GeV machines of Brookhaven and CERN to be built. By separating the functions of bending and focusing in the magnet system, further economies were made and machines of 450 GeV top energy were built at Fermilab and at CERN. By this time the machines had reached 6 or 7 km in circumference.

And in parallel with the development of these fixed target machines in which particles are accelerated and then extracted from the machine to hit others at rest in a target, which has the disadvantge that most of the primary beam energy is wasted in propelling the collision products along the direction of the incoming beam and only a fraction is useful in the collisions, another type of machine was developed in which two intense particle beams could be brought into head-on collision so that all their combined energy ended up in the particle collisions. A machine of this type, in which two beams of protons are collided together, the ISR machine, has been operating at CERN for several years and another, called ISABELLE, to give much higher collision energies, is now under construction at Brookhaven. Colliding beam machines have also been developed for electron-positron collisions and the two highest energy machines of this type are the PETRA machine at DESY, Hamburg, and the PEP machine at the Stanford Linear Accelerator Laboratory. An even bigger one, called LEP, about 27 km in circumference, is now awaiting approval at CERN. Recently, the possibility of using fixed-target proton accelerators as proton-antiproton colliders has become feasible thanks to another invention called beam cooling for which two methods were put forward a few years ago, the electron beam cooling system of Budker at Novosibirsk and the stochastic cooling system of Van der Meer at CERN. The big proton machine at CERN has now been modified as a collider and in July this year the first proton-antiproton collisions were observed at energies of over 500 GeV. To reach such high collision energies with a fixed target machine would require a primary beam energy of about 150,000 GeV and a machine measuring 1000 km in circumference. Fermilab is planning to modify their big machine in the same way and also to double its proton energy by adding a ring of superconducting magnets. It is interesting to observe that nowadays one can accelerate and store beams of antiprotons, a particle which was discovered many years ago using the Bevatron, one of the first of the giant accelerators. These colliding beam machines, although giving the highest energy collisions, do not replace the fixed target machines but are complementary to them since the fixed target machines give many orders of magnitude higher collisions rates and for many experiments this is the more important parameter. With this in mind, the Soviet Union is constructing a 3000 GeV fixed target proton accelerator, called UNK, which will be 20 km in circumference. Fifty years ago, Lawrence and Livingston struggled to reach 1 million electron volts with their cyclotrons - now a machine is under construction which will give 3 million million electron volts.

I hope this very brief review shows that the development of particle accelerators has more than kept up with the needs of the research over five decades of time and eight decades of energy - a truly remarkable achievement without which the research would never have progressed in the way it has. The other necessary ingredient was, of course, money with which to build these machines and to operate their laboratories and it is to that subject I will now pass.

It is a source of wonder to the general public and of some consternation to other sciences that so much money has been invested by governments over the years in the construction of the giant accelerators and in operating their laboratories. High energy particle physics, say the critics, is not only a "Big Science" but also a "Big Spender".

To a certain extent, this impression is due to the concentration of the large machines and the expenditures in central laboratories. For example, in Europe, there is only one CERN laboratory and apart from DESY, the German National Laboratory, no other laboratory is now operating a front-line machine for this research. All the high energy particle physicists in Europe, which number over 2000, now depend on these two laboratories and mainly on CERN. Although the budgets of the central laboratories are large so is the number of physicists which depend on them for their research and the capital investment and operating costs per physicist are not as high as people sometimes imagine or fear. Another result of this concentration, which has also happened in the U.S., is that the choice of a new machine, which occurs very infrequently these days - every ten years or so - concerns a very large physics community and its construction is a matter of government decision which involves other scientists who sit on committees dealing with the allocation of national science budgets. The expenditures on this research have thus become a very public business and are highly visible at government level.

Since the late 1960's, the fraction of their wealth which countries invest in this research has not increased and in some cases it has substantially decreased. For example, in the U.S., it has fallen by about a factor of two since 1968, which I believe accounts for the present difficulties in operating the remaining three central laboratories in the U.S. However, there was a rapid growth of this research after the Second World War up to the mid 1960's when its budgets increased faster than the national economies which needs some explanation and to find it I think one has to go back to events which happened before and during that war.

In 1932, Chadwick discovered the neutron and a new force of Nature - the nuclear force - entered the world of physics, which up to then had known only the electromagnetic and gravitational forces. Just before the war, nuclear fission was discovered and the possibility of using the nuclear force, which is much stronger than the other two, became evident to physicists in the countries on both sides of that conflict.

The first application of these discoveries was the nuclear fission bomb. It may be, as Robert Oppenheimer said, that the nuclear physicists knew sin for the first time, but sinners or saviours, those who took part in these developments, which eventually included most of the nuclear physicists of the prewar generation in America and in Europe, became powerful figures on the world scene and very important people in the eyes of governments. When they returned to their research after the war, some of them to universities, others to head the Atomic Energy Agencies which were set up at that time in several countries, they used their influence with governments and the knowledge they had gained of large scale scientific projects for the benefit of high energy particle physics. It was no accident that many of the post-war accelerators were built under the umbrella of nuclear energy developments and to this day the funding of high energy particle physics by Atomic Energy Agencies or their successors still persists in many countries.

It can be argued that the development of this research, which took place after the war, would have happened anyway and that the Radiation Laboratory at Berkeley had already shown the direction in which it would go. Indeed Ernest Lawrence should be credited with more than the invention of the cyclotron - he also invented the big science laboratory where physicists, engineers and technicians could work together symbiotically to design and construct ever larger and more complicated particle accelerators and to develop their technologies - something which was unknown in Europe before the war. But even if the course of the development of high energy particle physics was inevitable and would have happened independent of the war the rate it developed afterwards was, I think, due to the war-time events, which greatly increased the importance and influence of its participants and gave them access to government funds in place of private donations, the seeking of which occupied so much of Lawrence's time and efforts before the war. It was noticeable that, as the pre-war generation of physicists gradually disappeared from the scene during the sixties and early seventies, the rapid expansion of the budgets for high energy particle physics first slowed down and then began to decrease, but this may not have been the main reason for this decline since it also coincided with the end of post-war economic boom in the West and with a general decrease in science budgets. That the research was able to continue to maintain its momentum since the end of the sixties has been due to building fewer though larger machines and to concentrating them in fewer and fewer central laboratories.

The enormous increase in the size of the particle accelerators and their concentration in central laboratories have certainly changed the way in which the research is carried out and has posed many problems to its participants. When the machines outgrew their university environment and national and international laboratories were created in which to house them, one result was that the place where experiments were carried out became separated from the place where students were taught physics. The same people - academic staff - continued to carry out both functions and indeed they are inseparable if scientific research is to prosper in the long term. But teaching students in one place and carrying out experiments in another has placed a great strain on the experimental physicists. It takes a particularly robust personality to teach academic courses at, say Harvard University and carry the heavy responsibility of a major experiment at, say, Fermilab or CERN, especially when the experiment lasts several years. The obvious solution of leaving one's colleagues to do the teaching whilst the experiment is underway is hardly acceptable for such long periods of time, and in any case, one experiment is usually followed by another without any break. The other solution of building up a resident research staff at the central laboratories not only would divide research from teaching but also would isolate the central laboratories from the national university systems with the danger that they would lose the support of the universities and ultimately of the governments. There seems no alternative therefore but that the experimenters continue to commute between their universities and the central laboratories but it is a hard life and not to everyone's taste.

Another problem raised by this concentration was who would control the central laboratories and determine the research programmes of their machines. The solution generally adopted was to set up Experiments Committees with the chairmen and the majority of the members coming from the universities. The role of these committees was to receive proposals for experiments and to select the best. This was a far cry from the days when a group of university physicists led by an all powerful professor decided for themselves what experiments they would do. Of course, scientists have always been judged by their peers when they publish their experimental results, and indeed their reputations depend on it, but it is rare to find in other fields of research that scientists are prejudged by their peers before they can even do an experiment. One wonders how some of the great scientists of the past, say Rutherford or Fermi, would have reacted to this system. One also wonders whether crucial experiments have been overlooked by this system, perhaps due to a leader not being skilled enough, or too shy, to defend his experiment before such formidable committees; or even if some experiments never get proposed at all because they would seem too risky for such public scrutiny. I know of no such cases, but surely there must be some shy and inarticulate physicists left in the world - perhaps they work in other fields of research. It is noticeable, however, that experimental groups take the precaution these days of getting strong theoretical support for their experimental proposals, which has the result that experiments are more and more designed to test current theories or to distinguish between them. In this context one is reminded of a remark Rutherford once made when Arthur Tyndall asked him: "How is physics these days?" - "There is only one thing to say about physics", Rutherford replied, "the theorists are on their hindlegs and it is up to us to get them down again".

Experiments, as I have already said, have grown in size at the same time as the particle accelerators and so have the number of physicists in each experimental group so that it is quite common these days that a group consists of 20 or 50 physicists from many universities and from several countries and that the first page of a publication is taken up with the list of authors and their affiliations. The role of the individual physicist in such large groups is clearly very different from what it was in the past. Also the experiments take longer and longer to design, build and carry out so that from first conception to final publication many years elapse and the opportunity of carrying out several major experiments in a working lifetime has diminished considerably. Perhaps this is why one now finds physicists involved in more than one experiment at a time.

As a result of the business of the experimenters and the size and complexity of the machines the field has split up into specializations - into theoreticians, experimenters and machine builders, and it is rare to find anyone these days who contributes to more than one specialization.

Nevertheless, despite all these changes in the lifestyle of its participants, the research still continues very vigorously indeed. But however exciting it is to the present participants, its long term vitality depends on it continuing to attract some of the most intelligent of the young physicists as they emerge from the universities year by year and hence on whether the fundamental nature of this research and its exciting discoveries outweigh in their minds whatever disadvantages they see in its present style and its large scale operations.

I come now to the changes that have occurred in the institutional structures which have enabled this research to continue. In general, the end product, that is to say the present structures, are rather similar in the three regions of the world in which this research is most actively pursued, but each region arrived at its present structure by a different route and it is the routes which, I think, are the most interesting. I will be very brief about what happened in the U.S., more lengthy about what happened in Europe and brief again about the Soviet Union. This is not due to chauvinism but because the changes in Europe were more difficult to make simply because national frontiers had to be overcome between countries which had recently been fighting each other.

From the point of view of someone living in Europe, the institutional changes which took place in the U.S. seemed to go very smoothly indeed and to follow logically the developments at Berkeley in the 1930's and the war time nuclear energy developments. After the war, Berkeley returned to high energy particle physics and the 184" cyclotron was completed as a synchrocyclotron. The Brookhaven National Laboratory was set up and its 3 GeV Cosmotron was the first machine to exceed 1 GeV energy, to be followed two years later by the 6 GeV Berkeley Bevatron. Elsewhere, during the 1960's several new machines were built in the U.S., at Cornell, Princeton, Harvard-MIT, Argonne and at Stanford. Fermilab was created in the late 1960's and its machine came in operation in the early 1970's. The laboratories were usually operated by associations of universities with the funding coming mainly from the Atomic Energy Commission and a part from the National Science Foundation. In recent years, some of these laboratories have diversified their research activities into other fields and several machines have been closed down. Nowadays, the three big central U.S. laboratories are Brookhaven, SLAC and Fermilab.

But all this is familiar history to you so let me pass on to what happened in Europe where the situation was far different after the Second World War. Whereas the U.S. had survived that war with little material damage and its institutions and its economy intact, it is hardly an exaggeration to say that Europe was in ruins economically and, in many countries, materially as well. Furthermore, due to the Nazi regime in Germany and the Fascist regime in Italy, it had lost a large number of its best nuclear physicists who emigrated to America in the 1930's, and more followed as the countries in Europe were overrun in the early war years. This exodus continued even after the war as physicists, despairing of continuing their research in Europe, sought and found positions in the U.S. and the research tools they lacked in Europe.

Those who stayed behind in Europe and those who returned from the war-time projects in America found themselves in a particularly frustrating position. European physics had enjoyed a very productive period for two decades before the war - a golden period, rarely experienced before in the world of science - when the physics of the atom was unravelled and a flying start made in exploring its nucleus. After the war there seemed no way of getting back to the forefront of this research again or even of restarting the research in many of the European countries. Even worse, the particle accelerators which had been built just before the war were very small machines, and nothing like the Berkeley laboratory had been created in Europe.

But despite all these difficulties some new machines were constructed in Europe during the immediate post-war years. A synchrocyclotron was built at the Harwell Atomic Energy Laboratory and came into operation in 1949 but it was too small to reach the meson production threshold. Birmingham University built a 1 GeV proton synchrotron which operated for the first time in 1953, a year after the 3 GeV Cosmotron, but not even so influential a person as Marcus Oliphant could assemble sufficient staff and resources to make it a real success. The Saclay Atomic Energy Laboratory in France built another proton synchrotron later on, similar to the Cosmotron but it came into operation four years later. A machine the size of the Bevatron seemed beyond the reach of any institute in Europe.

By 1950, it was already clear that no single country in Europe was willing to pay the price for getting back again to the front line of this research and it might have declined irretrievably had it not been for a quite different group of people, not scientists at all but statesmen, who were looking for some way of demonstrating that the European countries could achieve far more together than they could ever do separately. It was the coming together of these two groups of people, the nuclear scientists anxious about the future of their research and the statesmen interested in joint European action, that saved nuclear particle physics in Europe, and the reason the two groups came together was again due to the war-time nuclear developments.

Just after the war, the United Nations Organization set up a Commission on the control of nuclear energy the members of which were government officials and diplomats. They in turn called in a number of eminent nuclear scientists as experts, many of whom were the nuclear physicists who had worked on the war-time developments and were by then in leading positions in the Atomic Energy Agencies. After the official meetings were over for the day, these physicists chatted with the officials about the future of scientific research in Europe and from these conversations emerged the idea of inter-governmental action. Hans Kramers from Holland, Pierre Auger from France, and Robert Oppenheimer from America were amongst the first contributors and they were shortly joined by Francis Perrin from the French Atomic Energy Commission, Isidor Rabi from Columbia University and Eduardo Amaldi from Italy. Unfortunately, I have no time to follow the meanderings of this idea around Europe and the U.S. during the ensuing years. Nearly all the pre-war generation of nuclear physicists got involved in it at one time or another, including many Americans, so it is hardly surprising that nuclear particle physics was chosen as the first joint European action. It also had the merit that no commercial application was foreseen for the research and no military purpose - two features which made it more acceptable to governments, as an initial venture. A site was finally chosen for the laboratory near Geneva, not without considerable difficulty, and the final formalities to set up the CERN laboratory were completed in 1954. This was the year when the 6 GeV Bevatron first came into operation and the highest energy machine operating in Europe at that time was the 1 GeV proton synchrotron at Birmingham University, which gives some measure of the gap that existed.

So it was in this way that the barriers of the national frontiers and national enmities were broken down for the first time in Europe after the war, not for reasons of economic prosperity or military defense as one might have thought but by a fundamental research science. But even though the barriers were down, it still remained to build a laboratory and particle accelerators which hopefully would one day bring Europe back again into this research on an equal footing with America.

Needless to say the newly created CERN faced many problems, some real and some imaginary. In the first place, not all the nuclear physicists in Europe were happy with the idea of setting up an international laboratory. They feared it would take what little money they had away from their universities to be lavished by the governments on their new creation. Fortunately, it was the start of the economic boom in the West when science budgets increased even faster than the economic growth rates and budget increases of 10 to 15% per year were not uncommon. In fact, the boom went on so long that some scientists began to believe that these growth rates were almost a natural law. The result was that CERN did not take money away from the national programmes but more money became available and new national laboratories were created in parallel with CERN, such as the Rutherford and Daresbury laboratories in England, the Frascati laboratory in Italy and the DESY laboratory in Germany where new machines were constructed complementary to those at CERN.

Then there was the problem of recruiting competent staff and the fear that no one of any value would ever leave his own country to work at such a risky place as CERN, and even if they did, that they would never work together or even understand each other, due to the many languages in Europe. In fact, very good people did join CERN but most of them were young and inexperienced. Of the team of people which built the first big machine at CERN less than half a dozen had any previous experience in accelerator building but fortunately they were helped enormously by the Cosmotron team at Brookhaven who, by then, were building the AGS machine. As for working together, the critics underestimated the desire of young people at that time to forget the sad past and to try to construct a new Europe. The language problem resolved itself quite simply when people discovered that an inability to express oneself forcibly in another language without appearing ridiculous lowered the normal tensions in a laboratory. In any case, the problem is not to understand what someone is saying but to forgive him for saying it and that it is easier if one does not understand very well what he is saying.

The first real challenge came with the choice of the first big accelerator at CERN. Right from the beginning, the ambitious idea was to build a machine whose energy would be second to none in the world and the first machine designed was a scaled-up Cosmotron for about 10 GeV energy, considerably higher than that of the Bevatron. But on a visit to Brookhaven in 1952, it was found that a new idea was being put forward by Courant, Livingston and Snyder which would enable a much higher energy machine to be built for the same cost - this was the alternating gradient focusing idea - but there was no guarantee that such a machine would work since nobody had ever built one, not even a model. It says a lot for the courage of the project leader at CERN in those days, Odd Dahl, and of the Council Delegates, that this idea was nevertheless adopted so that when the machine first operated in the autumn of 1959, its energy was indeed second to none in the world, although that glory did not last very long since its sister machine, the AGS at Brookhaven, came into operation in 1960 with a slightly higher energy. Nevertheless, the fact that the machine worked well and was built on schedule and within the budget foreseen established CERN as a serious laboratory on the world scene, and many of the fears and doubts about international laboratories subsided.

The next problem was to start the research programmes and this turned out to be more difficult than foreseen. The founding scientists of CERN, men like Cockcroft, Perrin, Heisenberg, Bohr and Amaldi held the view that what was lacking in Europe was not bright young physicists but the experimental equipment for their research. Like Churchill during the war, they believed that, given the tools, they and their students would do the job. But they underestimated the effect on the European university system of all the physicists who had left Europe before, during and after the war and the fact that European physicists had missed several generations of accelerators - machines like the Cosmotron and the Bevatron. So although in 1960 Europe had arrived with one of the two highest energy machines in the world, it lacked experience in using such a machine and it took quite a few years to recover this lost ground.

Even how the physicists from the different countries would use the CERN machines was not obvious at the beginning. The first idea was that "national truck teams", that is to say groups of university physicists from each country, would arrive with their equipment to carry out experiments at CERN, but that did not work out successfully. Very soon mixed teams were formed from many universities and several countries, brought together by their common interest in an experiment and the contributions that they could make to it, and this has proved the best system and the most efficient. Just as in America, committees of university physicists were set up to select the experiments that were to be carried out using the CERN accelerators. Looking back, I think that the series of decisions that were taken at CERN in 1960 on the way its machines would be used and its scientific programmes established, constituted the second major challenge since the methods adopted were a complete break with the past in Europe, when the universities had their own accelerators and all-powerful professors could decide their own programmes, and this break happened very quickly indeed.

The third major decision came in two parts but that was not the original intention. In 1964, a proposal was made by CERN to build two new machines, a large proton-proton collider, called the ISR, and a super proton synchrotron, called the SPS, ten times the energy of the first big machine at CERN. For reasons of cost and because the ISR would give the higher energy proton collisions, it was approved first. This again was a very courageous decision since nobody had ever built such a collider before and there were many doubts about whether it would work successfully. It was thought at the time that the second machine, the SPS, would be approved soon afterwards but, in fact, its approval was delayed for six years and its construction started only in 1971 just when the ISR first came into operation. Meanwhile in the U.S., the construction of a machine of similar energy, 400 GeV, was started at Fermilab in 1968 under the inspired direction of Robert Wilson. The history of these two projects is worth retelling for the light it throws on the institutional structures involved.

By 1965, the economic boom in Europe was beginning to slow down and the budgets for science were no longer increasing as much each year as they had in the past. It was, therefore, too much to expect that the European countries would agree to construct two new big machines at the same time. In addition, it was proposed that the SPS machine should be built at some other place in Europe, since it was considered that the land around the Geneva site was not flat enough for a machine measuring 7 km in circumference. However, the idea of creating another CERN laboratory and operating two of them together under the foreseeable economic conditions in Europe was very worrying to many people and the possibility of having to close down the Geneva site when the other became operational was hardly attractive to countries which had spent so much time and money creating it. Nevertheless, the CERN Member States spent six years proposing attractive locations for the new laboratory and organizing splendid visits to their sites, but they could not decide which to choose. In the end, a proposal was made to build the machine next to the Geneva site in a tunnel deep underground to avoid the surface contours and at half the cost of the original project by using the first big CERN machine as the injector of the new one and all the infrastructure of the existing laboratory.

In the U.S., on the other hand, there were apparently not the same anxieties about future economic trends and possibly because there was one Federal Government and not twelve sovereign states, a decision was reached more quickly, but it went in a different direction. Instead of building the machine at one or other of the existing laboratories, Berkeley or Brookhaven, a new site was chosen and a third laboratory, Fermilab, was created.

The delay in reaching the decision posed another serious problem at CERN since once again Europe was slipping backwards. It was only solved by leaving to Fermilab the first generation experiments with their new machine and concentrating all the efforts at CERN on a series of massive second generation experiments.

There have been other major decisions taken at CERN more recently, such as that to go ahead with modifying the SPS machine to make it into a proton-antiproton collider, which was one of the fastest ever taken, but I think I have said enough about Europe to show how the institutional changes occurred and how they worked out in practice and I would now like to make a few remarks about what happened in the Soviet Union.

At the end of the Second World War, the Soviet Union was certainly in no better shape than Europe, so it came as a surprise to learn from Vladimir Veksler, in a talk he gave at the first "Atoms for Peace Conference" in Geneva in 1955, that there was a large synchrocyclotron, comparable to the Berkeley 184" machine, operating in a laboratory at Dubna, near Moscow and a proton synchrotron nearing completion in the same laboratory whose design energy was 10 GeV, considerably more than that of the Bevatron. It was fortunate indeed that CERN had not gone ahead with the scaled up Cosmotron otherwise it might have ended up with only the third highest energy machine in the world, behind the Americans and equal to the Russians. The Dubna laboratory ultimately became an international laboratory by including the Eastern European States and some others, and another Soviet laboratory was created at Serpukov on the other side of Moscow where a 70 GeV alternating gradient focusing machine came into operation several years after the 30 GeV Brookhaven and CERN machines. It is at this laboratory that the 3000 GeV proton machine is now being constructed. With the announcement by Veksler of the Russian machines at Dubna, a third partner suddenly entered this field of research and from 1956 onwards, collaboration with the Soviet scientists grew steadily and they became regular participants at all the international conferences. Later on, joint projects were undertaken, for example, the construction by CERN of the extraction system for the Serpukov machine, and joint European-Soviet experiments first at Serpukov, when it had the world's highest energy machine, and later at CERN, when its 400 GeV machine came into operation.

As I promised at the beginning, I will end with some remarks about the future of high energy particle physics - or rather a possible future, since, as Niels Bohr once remarked, prediction is always difficult, especially when it concerns the future.

In the first place, of course, any possible future must depend on the research continuing to make important new discoveries and significant progress towards its goal and that sufficient numbers of the best physicists continue to devote their lives to it. In the second place, it must be possible to develop the particle accelerators to give higher energies without them becoming monstrous in size and prohibitively expensive, and thirdly, governments must be willing to allocate enough money to build new machines and to operate the few remaining central laboratories in the world at an efficient level.

As far as the research is concerned the progress made in recent years is truly remarkable, as we have heard from Steven Weinberg this afternoon, but whether it is still attracting enough of the bright young experimenters these days it is difficult to say since one of the main problems facing them in recent years has been finding stable posts in the universities where they can continue their research. It is rather in the development of particle accelerating machines and colliders and in the financing of the experimental research that the real problems lie, now and in the future.

Experimental high energy particle physics has depended for the last 50 years on the continuous development of accelerators and colliders to provide higher and higher energies. As I have already said, this development has been achieved by a series of remarkable inventions which have not only increased the particle energies by 8 orders of magnitude but also reduced the cost per GeV also by many orders of magnitude. Nevertheless, the size of the machines has increased enormously and so has their cost and also the operating expenditures of the central laboratories in which they are located.

We live now at a time of economic recession in which many governments are seeking to reverse this trend by cutting back their own expenditures of which those of fundamental scientific research form a modest but significant part.

The question arises therefore whether high energy particle physics with its dependence on the large accelerators and colliders can continue to make the rapid progress it has made during the last 50 years or whether it will slow down to a pace where theoretical hypothesis may in the future not be adequately tested by experiments and the health of this research deteriorate as a consequence.

To be sure some experimental tests of the current unification theories can be carried out without accelerators, for example, the experiments to measure the proton lifetime now being constructed, and there are always the cosmic rays to fall back upon whose top energies far exceed those of any machines likely to be built in the foreseeable future. But so far it has been the accelerators and the colliders that have provided the principle means of the experimental research and it is to their future development that one naturally turns. Clearly, when these machines have grown to be 20 to 30 km in circumference, there is every reason to be worried.

Looking back at the history of the development of these machines one sees that the inventions which enabled their energy to be increased so spectacularly mainly concerned the design of the machines. The development of the technology used in their construction advanced more slowly. For example, the technology used in constructing their magnet systems had depended until quite recent times on magnetic steels and copper or aluminium conductors. Coil insulation slowly changed from paper tape and oil to glass tape and epoxy resin. The big change in magnet technology, the use of superconducting magnets, is only now being incorporated in the new machines such as Tevatron, Isabelle and UNK. This relatively slow progress in construction technology is due to the very long time it takes - 10 years or more - to develop a new technology to the stage at which it can be used with confidence in a giant machine. The machine design changes on the other hand can be introduced rather quickly. For example, the changeover from constant gradient focusing to alternating gradient focusing was adopted in less than a year.

One also sees that there are fundamental limits to the technologies used in machine construction, for example, the maximum volts per metre that can be sustained across R.F. accelerating cavities and the maximum magnetic fields that superconducting magnets can give due to the ultimate strength of materials to withstand the enormous forces set up as field strengths exceed 10 Tesla. Certainly, new ideas to overcome these limitations have been proposed but so far these do not seem to be useful for very high energy machines.

One condition for the future progress of this research, at least so long as it depends on accelerators and colliders, is that ways are found in the coming years to build much higher energy machines at much less cost.

The other condition is that governments are prepared to finance the construction of new machines from time to time and to operate the central laboratories at a level sufficient to allow the physics communities who use them to carry out experiment research at a rate which maintains their interest in the research. The problem here is how to interest governments in fundamental research in general and this research in particular. As I explained earlier, it was the combined interest of the statesmen and the scientists which led to the creation of CERN. The scientists would never have succeeded on their own.

Clearly the conjunction of circumstances which took place after the Second World War and led to the rapid expansion of this research is unlikely to repeat itself - at least I sincerely hope not. Surely not even the most desperate physicist would wish for another war just to increase his budget.

The eminent scientists who established such powerful links with governments during that War and used their influence thereafter in favour of scientific research have nearly all disappeared from the scene and their successors have not the same close relationships with governments nor the same influence - at least not in Europe. Furthermore, the novelty of joint actions in Europe has worn off as other more complicated ventures have been launched and got underway. And although the general public, especially young people, seem more interested in science than ever before this enthusiasm has not yet been translated by governments into more money for research.

Clearly we must look for new ways to interest governments in this research and in scientific research in general or adapt some of the previous methods to the present circumstances. Unfortunately, I have no really new ideas to present to you today on this subject but, as my concluding remarks, I would like to suggest that wider international collaboration between the world regions participating in high energy particle physics may offer a solution.

It can hardly have escaped anyone's notice listening to this address that a certain healthy competition between the partners has been a powerful motivation for pressing ahead with successive accelerator constructions in the different regions of the world during the last 30 or more years. Each region has tried to keep its equipment up to date so that its physicists could contribute at the forefront of this field of research. But in parallel with this competition there has been a steadily growing collaboration between the regions, so that at laboratories like CERN and Fermilab, it is quite common to find scientists from the U.S., Europe and the Soviet Union, and from many other countries, working together in joint experimental teams using equipment built in the different regions. Budget limitations have accelerated the inter-dependence of the regions in recent years since it is no longer possible to afford all the particle accelerators and colliders which are needed for this research in each region of the world. The new machines, Tevatron, Isabelle, UNK and LEP are all considered to be necessary for the research and will offer complementary experimental facilities but Tevatron and Isabelle will be in the U.S., UNK in Russia and LEP in Europe - at least they will be if they all go ahead. Depending on the experiments they want to do, particle physicists in the three regions will need access to one or other of these machines and the way they could get this access has already been worked out and agreed, at least at the level of the operating laboratories concerned. Whether it will work out in practice will depend not only on the continuing goodwill of the laboratories and of the research scientists but also on the political situation as it develops in the coming years, but if it works out well it could point the way to a further evolution of this research.

I realize that all this may seem hopelessly optimistic in the present circumstances so let me recall again the situation in Europe at the end of the Second World War. Then, it was countries recently at war with each other which set up CERN and it took quite a lot of optimism in those days to believe that an international laboratory could be set up at all, let alone succeed in bringing Europe back again to the forefront of the research.

Indeed, since Berkeley was set up in the aftermath of a major economic depression and CERN after a World War, it might almost be concluded that such great ventures need desperate situations for their birth. So, if the physicists are convinced that wider international collaboration is essential for the future of their research and if the governments of the regions of the world want to show that they can do something together and are looking for a subject which does not have commercial or military complications, then joint action on an inter-regional basis may well succeed. It may even release more money for this research. Indeed, unless the Western economies boom again and more money becomes available for scientific research, or new ideas make it possible to build higher energy machines at much less cost, it may be the only way of providing the accelerators and colliders which this research will need in the future.

50th ANNIVERSARY BANQUET

DAVID A. SHIRLEY, Master of Ceremonies:

Best wishes and congratulations on the Laboratory's 50th Anniversary. I'd like to introduce the people at the head table and I'm going to do this in the following format. I will introduce some people and I will skip others who are going to be speakers. All of the people at the head tables are directors, former directors, wives of former directors or present directors, officials, former associates, and so forth. Starting from your left, my right, Gladys Sessler. I'm going to skip the next person because he's going to be speaking. Jane Wilson, wife of a speaker. Andy Sessler, who started this tradition with the 45th Anniversary dinner here. Elsie McMillan you all know well. Jim Liese, head of the High Energy and Nuclear Physics, DOE. Then we have another speaker. And on my left, your right, we will have another speaker at the very end. Then we have Grace Fretter. Ed McMillan. Selma Lofgren. Then we have a speaker. Then my wife, Virginia. And another speaker. I'd like to acknowledge all the members of the 50th Anniversary committee. They have worked for well over a year to put together an interesting and exciting series of events which have started late last week and will continue today, tonight, and into Family Day tomorrow. There are many members of the 50th Anniversary committee. I'm not going to introduce them all. Their names are written at the bottom of the menu. I would, however, like to introduce three of them who have worked hard and diligently very recently. Louise Millard, recording secretary. Ted Kirksey, executive secretary. And I guess I have to introduce this man now even though he's going to be a speaker, Ed Lofgren, who has been the chairman of the committee and has done a great job.

Very well. I'd like to move on, then, with our speakers. We have selected a small number of people to make brief remarks based on their association with the Laboratory and we are then going to go on to a main keynote address. The first person you know well. She needs no introduction to this audience. She's been one of us for just about the whole 50 years that we're celebrating. Molly Blumer's marriage to Ernest Lawrence took place on May 14, 1932, six months after the date that we've chosen to commemorate as the Laboratory's 50th anniversary. But Molly actually preceded the cyclotron in Ernest's life and affection. Their courtship began at Yale when Ernest was a graduate student and she was a schoolgirl. During the seven years between their first meeting and their marriage, Molly found time to graduate from Vassar and to do graduate work in bacteriology at Harvard under Hans Zinzer. When she arrived at Berkeley as Ernest's bride she continued her work as assistant in the University's bacteriology laboratory. Though Molly later gave up her scientific career to concentrate on the raising of the Lawrences' children, her feeling for science and her experience, however brief, as a working scientist, was one of the many ties that bound her to Ernest and continues to bind her to us. Ladies and gentlemen.......... **Molly Lawrence.**

MOLLY LAWRENCE:

Thank you. Friends and fellow members of the Laboratory family and distinguished guests. Five years ago you will recall there was a certain amount of jocular speculation about the reason for such an elaborate 45th anniversary celebration when the really important milestone, the 50th, was such a short way down the road. Andy Sessler, in his opening remarks at the banquet, put into words what I think a lot of us may have been thinking when he admitted that there might be some concern that we wouldn't make it to the 50th. At the time I wasn't sure whether Andy's intimations of mortality referred to us as individuals or to the Lab as an institution or maybe even to planet earth as a habitat to humankind. Obviously, that last would have encompassed the other two. In any case, I thought Andy spoke rather tongue in cheek and the audience obviously took his remarks in that spirit because we all had a good laugh. So we didn't believe we'd had it.

And here we are tonight in the same room and approximately the same number to prove we were right. I can say again as I have said before, it's wonderful to see so many old timers here tonight. However, there is a sad note on an occasion like this because there were a few very special people who shared in the festivities with us last time and who have since gone on ahead on that last journey. Three of them were oldtimers, very much valued members of the early team who later left Berkeley to spread the gospel of cyclotron here and abroad in the land, you might say. Milton White and Larry Marshall were among the old boys photographed on the steps of Wheeler Hall in 1976. Their absence leaves two very wide gaps in this year's picture. Henry Newsom did not join us in Berkeley but I have no doubt that his good wishes and his greetings came to us by telepathy from North Carolina. And then, of course, there was that very special person, Don Cooksey. Don had been Ernest's friend long before the cyclotron was dreamed of and he was appointed the first assistant director of the Laboratory when it received its official recognition. Don was the kind of person we all instinctively turned to for sound advice and practical help when we had any kind of problem and we also looked to him for accurate information about Laboratory events and Laboratory people. And he had that marvelous file of pictures to illustrate the history. In fact, I believe Herbert Childs could simply not have written his biography of Ernest without Don's help. Last time, he shared a table right down here in front with members of my family, young people who had called him Uncle Don from the time they learned to talk. And finally, Raymond and Irene Birge. While they were not members of the immediate family, so to speak, they, too, were like those courtesy aunts and uncles so many of our own families enjoyed when we were growing up. Such good friends and wise counsellors and loyal supporters, that their presence was not merely expected but demanded on every important family occasion. On this very important occasion of our golden anniversary I know we miss all these old friends very much and we're thinking of them now, as we always shall, with a great deal of affection and respect.

But to return to the 45th for a moment. When my esteemed brother-in-law, Ed McMillan, persuaded me to make the first after dinner speech of my life I hoped it would also be the last. In fact, I closed my remarks with a "Please don't ask me to make another speech. I've already told you everything I know." It was true then, and it's still true. I really don't have any more amusing anecdotes to share with you so this time I'll have to go off on a little different tack. And of course, in any case my plea has been honored. Technically, at least. I am not making a speech. I'm making a few remarks. Not long ago I happened to turn on the television set just as a talk show host was quoting those disheartening lines of Whittier's. "For of all sad words of tongue or pen, the saddest are these: It might have been." And I thought, what peculiar creatures we are that we spend so much more time lamenting what might have been than we do rejoicing about what might not have been. And that train of thought led to a whole series of "what if's". What if Rolf Wideroe had not published an article on the acceleration of potassium ions in 1928? What if Ernest had not come across it in the library one day and managed to understand the general principles even though he couldn't read German very well? What if Nils Edelson had not been persuaded to build the first Berkeley accelerator, that messy little glob of glass and sealing wax? You know, the one you've all seen pictures of that was said to have been mounted on a kitchen chair. It's funny how the incongruity of that kitchen chair still seems to tickle the fancy of the Fourth Estate. What if Stan Livingston had not undertaken the task of building larger accelerators and come up with some very ingenious solutions to some of the knotty problems that arose? What if that wonderfully inspired, dedicated, hard-working, long-suffering bunch of young people had not gravitated to Berkeley to work night and day, Sundays and holidays, for their demanding maestro? What if Robert Gordon Sproul had been an old fuddy duddy of a University president instead of a young, dynamic one? One who was willing to gamble on the far out ideas of his young physics professor and to back him with as much support as the University could muster in the midst of a great depression, fortunately including a condemned building? And finally, what if Raymond Birge had not been a most remarkable Physics Department chairman? A scientist so lacking in professional jealousy that he could watch with equanimity and even encourage the rapid growth of that upstart Radiation Laboratory. After all, it was supposed to be an adjunct of his physics department. But it soon became, as Birge himself once said, the tail that wagged the dog. What if any of these substantial elements in the success of the Laboratory had been lacking? What if the right people had not had the right ideas at the right time, the right degree of enthusiasm and persistence, at the right time and in the right place? Surely the Radiation Laboratory would not have been founded in 1931 at Berkeley and we wouldn't be here tonight celebrating this golden anniversary of those auspicious circumstances. But they did, and it was, so here we are.

DAVID A. SHIRLEY:

In our archives is an intriguing photograph of about two dozen of the bright young men who worked with Ernest Lawrence in the early days. They're shown seated, kneeling and standing around the magnet of the 60-inch accelerator. I'm sure you've all seen that photograph. In the back row standing next to Bill Brobeck is Bob Wilson, who earned his Bachelor's and Ph.D. degrees while working with Ernest Lawrence. Herbert Child's wonderful book about Ernest Lawrence, "An American Genius," tells this story: "It was a rare night when no one could be found at the Laboratory. For several, the night hours were almost as habitual as those of the day. Stan Van Voorhees, Philip Abelson and Bob Wilson were among those who worked most weekends, too. Wilson found his work so interesting that he once worked too many hours, refusing to admit even to himself that he was ill, until he collapsed." The book goes on to tell how he was scolded by Lawrence for not taking care of himself and when he was rushed to the hospital, it was found that he had been suffering from a ruptured appendix for several days. After recovery, the book relates, he was as industrious as before.

Bob Wilson's career, since leaving Berkeley, has taken him to Princeton where he worked in early measurements of the neutron absorbing properties of uranium 235, to Los Alamos, to Harvard where he helped design a cyclotron, to Cornell, and eventually to the National Accelerator Laboratory to become director of what today is known as the Fermi National Accelerator Laboratory. Three years ago he became Director Emeritus of Fermi Lab and now is a professor at Columbia. Bob Wilson's contributions to Fermi Lab will live on. Not only because of the research achievements of that great laboratory under his direction, but also because Bob Wilson is an artist of renown. He not only designed the laboratory buildings, which are noted for their distinctive beauty, but he also created three major pieces of sculpture which adorn the laboratory grounds. **Bob Wilson.**

BOB WILSON:

Thank you. These celebrations, homecomings, are particularly precious times for old geezers like me. You get to meet your old friends from the old Radiation Laboratory days and then make a lot of new friends. I'm looking forward to the next one very much. There's another reason, though, that I find these occasions very rewarding. Because it's a time to renew the Berkeley ambience, the memories of Ernest Lawrence, and of the inspiration that he provided for so many of us. I'm particularly thinking of that as one of his students. But we all have our role models and of course Ernest Lawrence was my particular role model as my professor. Other ones, Robert Oppenheimer, Enrico Fermi, Niels Bohr, Harry Smythe, people older than I am, of another generation, and they also were my role models. But it was Ernest Lawrence in particular that, whenever I was having a hard time at Fermi Lab I would always invoke the spirit of Ernest Lawrence and wonder, well, what would Ernest do under these circumstances? and then I would do my best to emulate him trying to solve the problem.

I think, if anything very good came of that place it was because of that relationship, that I'd have an inspiration and being touched by Ernest Lawrence as his student here. There's a lot of that in Stan Livingston's talk yesterday. Well, why should I be bringing up these role models? It's because we are in a time again of a crisis of sorts. Not the kind of crisis that we used to go through with wars, depressions, as we were reminded today, but a moderate crisis where we have seen our funds cut back, cut back again, as we were told today, perhaps by a factor of 2 from the late 60's. It might be a good time, in the spirit that I went through at Fermi Lab, to ask what would Ernest do if he were here today? Well, of course I don't know what Ernest would do but I would like to imagine a few things. In the first place, I just couldn't think that Ernest would stand around wringing his hands. That would be very atypical. The next thing is I think he would think, well, there's a lot of money and resources that we do have, and he would pause and he would think about what he wanted to do, or he thought ought to be done, take a long view and he would start to do it with the resources and the funds at hand. Now it's just as when he gave a thousand dollars, all of a thousand dollars, to Stan Livingston to make a cyclotron. But we also heard that he wasn't expecting to make a lot of other cyclotrons for a thousand dollars. He was in the east desperately raising money for more, having decided what he wanted to do, for more cyclotrons of a larger kind before that was even finished. So I think it's appropriate at this time that that spirit is somehow continued.

There is a committee of wise men, headed by George Trilling, who will take that long view of physics to decide what it is in order of our priorities of what should be done. I think that it's appropriate that it should be an old "Rad Labber" that's leading them. The next thing, though, that I am sure that Lawrence would do is, not only would he be making innovations himself and inspiring young people such as Stan to be making innovations also, but he would also be participating in the fundraising experience, and with no holds barred and every direction that he could. And that I think is something that we also should emulate. Doing our best to see that the funds are adequate would be maintaining the ingredient of our culture, the ingredient of the advances of technology that are needed so badly by this country. Well, I think I've shocked my British friends, many of whom are here, by being serious. I don't know any jokes, but I do have a house poet at the end of the table who has written a poem for me and I'll end up by reading the poem, in honor of the occasion. "Then cheer, dear lab of radiation/ You well deserve this celebration/ Of accelerators, acceleration/ Consider this my equation/ A humble beginning/ A grand inspiration/ Of particles stemming/ Equals pride of the nation/ Of 50 years winning." Thank you.

DAVID A. SHIRLEY:

Our next remarks were to be given by John Lawrence who, unfortunately, was not able to participate tonight because he is in Europe and unable to get back for the occasion, but who was active on the 50th Anniversary committee. However, he wrote a letter which Ed Lofgren is going to read for us.

ED LOFGREN:

You know that one of the important characteristics of our Laboratory is its interdisciplinary nature. This was fostered from the very beginning by Ernest Lawrence and I believe you all know that. One of his earliest moves in that direction was to bring his brother John from Yale University to the Radiation Lab in, I believe, the year 1935 to investigate the medical possibilities of cyclotron radiation. John has had a virtually continuous career at the Laboratory and the University since then. He founded a medical program in the Laboratory, he founded Donner Lab, he founded the Department of Medical Physics in the University and, with his collaborators, pioneered a major section of medical physics. He is continuing his present position as that of Regent, so he is still very important to the Laboratory. He has continued his very active interest in medicine and is at the moment attending meetings in Rome, Paris and London. We should be so busy. This man really keeps going. When he realized that he couldn't make it back here quite as rapidly as Steve Weinberg could, he sent a letter to me and asked me to read it. I'll now read the letter.

"People have frequently asked me about early experiences. I can mention an early experiment on the physiological effects of neutrons produced from a cyclotron target. We placed a rat ("we" is Paul Ebersol and John Lawrence) in a small container located near the cyclotron target. Ernest was at the controls of the 37-inch cyclotron. At the end of the exposure, Paul and I crawled between the coil tanks and, to our consternation, found that the rat was dead. This sobering result gave us a healthy respect for radiation exposure. A little bit later, Paul and I discovered that the rat had actually died from suffocation. We, however, thought it best to suppress this information, which we did, and it promulgated a healthy respect for radiation in the Laboratory and this marked the beginnings of an outstanding radiation safety record in the Radiation Laboratory. Another incident I remember occurred one day when I walked by the cyclotron magnet with a pair of pliers in my white coat pocket. It flew into the d-stand insulators and smashed them, laying up the cyclotron for several days. This did not make me popular because the cyclotron, then and now, was in great demand."

There was a similar story about Bob Wilson which involved a gasket being sucked into the vacuum tank and resulted in his temporary expulsion from the Laboratory, but he came back in greater style than ever. That was an interjection. Let me go on with the letter.

"Recently, I was asked about my feelings when we lost Ernest. They are better expressed than I could express them in a letter from Dr. John Northrup, a friend of Ernest and mine. He is a Nobel prize winner, would like to have been here tonight, but couldn't make it. He said as follows, 'I have just learned of Ernest's death and I hasten to send you my sympathy and affection. I know how much you loved him and how empty the world must seem to you now that he is gone. I miss him also, for he was surely one of the greatest, as well as one of the most charming men I have known. No one can take his place and no one will ever do more for his country'."

That's the end of the letter from John Northrup. Then John Lawrence goes on.

"These are my feelings about Ernest, beautifully expressed by a great scientist and a great friend. If Ernest were here tonight, he would say that this Laboratory's continuing accomplishments are due to his many outstanding students and associates, many of whom are here this evening."

Thank you.

DAVID A. SHIRLEY:

Thank you very much, Ed. Our next speaker will bring us a greeting from the University of California. Bill Fretter's association with the University of California spans some 4 decades, going back to the days when he was an undergraduate student on the Berkeley campus and then a graduate student working for his doctorate in physics. For 35 years he taught physics at Berkeley, at the same time proving himself an able administrator by serving in such posts as Dean of Letters and Sciences, Chairman of the Academic Council, and Chairman of the Assembly of the Academic Senate. He has more than 70 publications in physics to his credit, not the least important of which may be the textbook entitled "Physics for Liberal Arts Students." His co-author was another physics professor who has gone on to administrative duties, University of California President David Saxon. In 1978, President Saxon appointed Bill Vice President of the University and made him responsible for long-range planning for the nine-campus University system. He also holds responsibility for overseeing the health, science and hospital activities of the University and for overseeing the operations of the Department of Energy laboratories, including, of course, Lawrence Berkeley Laboratory. Bill.

WILLIAM FRETTER:

Thank you, Dave. On behalf of President Saxon, who is away at Oxford on a visiting fellowship, it is my privilege tonight to represent the University of California at tonight's celebration. I am honored to do so and delighted to join you.

One of the things we are celebrating this evening is the genius of Ernest O. Lawrence, that enormously gifted man who accomplished more in one lifetime than most people could accomplish in three. It was Lawrence who first recognized the value of interdisciplinary effort in research, a whole new way of practicing science that has made possible undreamed of advances in our knowledge of the physical world. In addition to physicists, he brought in chemists and engineers to work on his first cyclotron and that tradition has continued and broadened to encompass such areas as biology and medicine and geology and geophysics, to name a few. His influence on the Laboratory he founded has been so pervasive and so profound that it's tempting to characterize LBL as proof of the saying that every institution is the length and shadow of one man.

But we also celebrate, besides the achievement of individual genius, a partnership that is in many ways unusual and in some ways unique. Close working relationships between research laboratories and universities are not all that common. A close working relationship between a great university and a great laboratory is a rare event, indeed. Yet that is exactly what has happened in the case of the Lawrence Berkeley Laboratory and the University of California to the lasting benefit of both. I can't think of a similar relationship that comes close to being as active, as vigorous, as mutually stimulating. You can see vivid evidence of this in the fact that so many faculty and students choose to carry out research at LBL. More than 150 faculty and more than 550 graduate students just this year. They are unusually fortunate in that LBL's superb shop, its fine backup facilities, its great computers and its interdisciplinary character offer opportunities for research that simply don't exist at most universities. At the same time, LBL gains from the human diversity and intellectual vitality that are typical of the university environment.

Together, LBL and the University are getting ready for a future that bristles with problems. Among them, how to maintain this extraordinary Laboratory, with its special relationship to the Berkeley campus, in the face of large budget cuts now proposed by the Federal Government. As a great national resource in scientific research of many kinds, LBL has a potential for major discoveries, for more major discoveries and technological development whose economic implications could dwarf the amounts involved in the proposed budget cuts. Short-sighted budget policies have hobbled basic research and, in particular, the kind of University-LBL collaborative research that has been so successful in the past can only lead to further declines in the productivity of American society relative to others more aware of the importance of scientific research and development.

Whatever the obstacles, we must somehow continue this uniquely successful partnership in the service of society. My hope and my expectation is that the University and the Lawrence Berkeley Laboratory will find the next 50 years as mutually rewarding, as amazingly productive, as the half century of accomplishment we celebrate tonight. Thank you.

DAVID A. SHIRLEY:

Thank you, Bill. We now move on to our keynote speaker, Dr. George Keyworth.

It is indeed an honor for Lawrence Berkeley Laboratory to have Dr. George Keyworth with us this evening. It has been only a matter of weeks since Dr. Keyworth left Los Alamos National Laboratory to go to Washington as science advisor to the President of the United States and Director of the Office of Science and Technology. I know that he's traveled a lot because he told me today that he went through one whole week on the road and realized he'd only had 5 nights in that week. The University of California laboratories may well take pride in the fact that President Reagan has chosen one of their own for this important position. Dr. Keyworth joined Los Alamos in 1968, shortly after receiving his Ph.D. His research interests have included nuclear structure, polarization, fission, laser fusion, and neutron physics. When he was called to Washington, he was serving as head of Los Alamos Physics Division. Those in Washington who have observed Dr. Keyworth in his appearances before Congressional Committees and elsewhere, have used such words as "confident," "realistic," and "forthright" to describe him. I'm sure that Dr. Keyworth will find us a very attentive audience as he addresses himself to the topic that is utmost in our minds these days, "National Science Policy and the Role of the National Laboratories."

DR. GEORGE KEYWORTH:

Thank you, Dave. And I thank you all for inviting me to join you on this very happy occasion. Needless to say, I am always pleased to be among scientific colleagues, especially those from a sister national laboratory. To be with them celebrating such an auspicious event as a 50th anniversary of an institution as great as LBL has a very special meaning for me. Before beginning my own remarks, it is an honor for me to read to you the following message from the President of the United States. It begins.............

"I am delighted to extend my warm congratulations to the Lawrence Berkeley Laboratory on its golden anniversary. This is a significant milestone in the history of this outstanding institution. Lawrence Berkeley Laboratory is among America's premier scientific organizations. It started in 1931 as the Radiation Laboratory built by Ernest O. Lawrence and his co-workers. This facility has led the way in making scientific breakthroughs. The Laboratory's history is a distinguished and a proud one. Just as LBL has been a pioneer in expanding our frontiers of knowledge for the past 50 years, so we look forward to even greater accomplishments in the years ahead. These will be years when scientific advances will be crucial to our nation's future and to the well being of people around the globe. I know all Americans join me in honoring the achievements of all those associated with the Laboratory's work. You have my hopes and wishes for every success in the future. Sincerely, Ronald Reagan."

I will offer today the original of that letter for LBL's memorabilia. I believe that message expresses confidence that LBL can look forward to a future as proud and productive as its past. But the

President also speaks in terms of the challenge ahead and the critical needs of the nation. I would like to touch briefly on these things in my comments.

This great national laboratory has made many contributions during its 50 years, a period during which our country was challenged in many ways, and prevailed. Now we face a host of new problems, demanding new ideas, new skills, as well as new strengths. We should ask what policies we might adopt to assure us that the national laboratories remain a source of ideas and expertise.

But let me begin by putting this in a somewhat broader framework -- National Science Policy and its relationship to the needs and capabilities of the nation today. I think we must admit at the outset that we have never had a well defined national science policy. Over the years there have been many discussions of such a policy's putative direction. These discussions were never very conclusive. Often they degenerated into an attempt to establish, for example, a certain percentage of GNP for research and development as a substitute for a real policy. Of course, during some limited period such as that immediately following Sputnik, we did pursue effort that momentarily may have given the impression of having such a science policy. But the post-Sputnik era was a reaction to a scientific threat and much of the support of science and technology that resulted was tied to a rapidly growing space program and to other programs involving large research facilities and costly demonstration projects. Also, we must recall that was an era of remarkable general economic growth in this country. The events and the results of that era gave many the impression that such growth was the normal course of events.

But we soon learned differently. Today we are confronted by realities that force us again to think of the need for a National Science Policy, but one which is more compelling and consistent with those realities. The climate today is perhaps unique in our history for two reasons. First, as a result of our collective commitment to bringing the focus of our economy back to an emphasis on fiscal responsibility and increased productivity, one must scrutinize every element of the federal budget, with no exception. Secondly, following the modern era of technology that dates from the Second World War, the nation is now entering, just as the national laboratories are, a new era of maturity. It is an era of maturity that requires, among other things, that we ask ourselves what are the most effective investments that we as a nation can make in science and technology and what is the role of government in these investments? These are the key questions for any national science policy to address. But in a time of severe fiscal constraint, they assume dominant significance.

In recognition of these facts, early in my tenure I addressed what I consider the most fundamental element of a national science policy. That is, the assessment of priorities and the establishment of mechanisms by which they can be set and followed. I soon discovered that this concept was susceptible to broad interpretation, to put it lightly. When one talks about priorities in science support, there is a tendency on the part of many people and many scientists to assume that one intends to divide all of science into perhaps one hundred areas and to assign them priorities, one to a hundred. The problem is further confounded by the conviction that any attempt to predict the importance of a field of scientific research ignores the serendipitous nature of discovery, and the impossibility of predicting the precise future benefits of a discovery.

I think these fears are based on a misinterpretation of the type of prioritizing we need. Rather than a one to a hundred listing, the type of priority heading I have in mind calls for identifying that perhaps ten, perhaps twenty percent of research upon which to put increased emphasis, and an equivalent ten to perhaps twenty percent where the effectiveness has not been great over the last ten years or more. In a sense, we make similar evaluations in everything we do in life where we have limited resources and must make hard choices. It seems to me that in setting our priorities in the support of science and technology, we have two bases upon which to define those priorities. Excellence and pertinence. The principle criterion for the fundamental pursuit of knowledge must be excellence. Excellence in the investigators and excellence in the subject. An additional criterion for the support of areas in research directed toward technology advances is pertinence. And this means pertinence to the recognizable economic and societal needs of the nation.

Admittedly, all these considerations imply value judgments and value judgments are difficult and fallible, even when they involve a consensus of experts. However, if, as I said before, one concentrates on emphasizing the most promising avenues and deemphasizing the least promising, the probability of a large error in judgment is diminished. My central premise, then, is simply that we cannot continue to distribute our limited support of research and development without applying stringent and fundamental criteria. This is not just a painful exercise that we in the science and engineering communities have to endure as part of the President's economic recovery plan. It is also good for the future health of American science. As Lord Rutherford once said, "We haven't got the money, so we've got to think." I believe that in some respect this philosophy applies today and it may have many benefits. We cannot substitute thinking alone for money but we must evaluate more carefully what we do with our resources.

Let me take this matter a step further. In discussing the idea of pertinence in a recent speech, I stated that the objective was to define areas where America wishes to achieve or maintain preeminence. I then extended this by stating, in effect, that for a number of good reasons we could not expect to be preeminent in all fields. As perhaps some of you may have seen, this statement of mine has been interpreted to say "The President's science advisor says U.S. can't be number one" and other such headlines. I think perhaps this is a good time and place to clarify this, since we are celebrating the anniversary of an institution that has brought this country so much esteem, but also an institution that recognizes the true internationality of science and technology. The idea that we can't be preeminent across the spectrum of science and technology is not a statement of our flagging economy.

The fact is, that after World War II America was alone in developing and defining the trends of technology. The rest of the world, with much help from us, we should remember, followed. And in particular, Japan and Western Europe have achieved, if not technical parity, technological competitiveness. This is healthy for the world and for the world's stability. There are certain areas of science and technology that are more pertinent to other countries than to us. But there are also areas where the U.S. is, and shall remain, a leader. I firmly believe, for example, that space is one of these. I think that this shuttle introduced a new capability that will greatly extend U.S. predominance in space. In adapting to the very new capabilities that the shuttle supplies us in near-earth orbit, it does not mean that America's space program is being compromised. To the contrary. It means that an exciting new era is being ushered in where emphasis may go, for example, in astronomy and astrophysics, with a space telescope, rather than the planetary physics emphasis of the last 15 years.

There are many other examples across the spectrum of science and technology that indicate our preeminence and competitiveness. They exist in the new biological advances, in chemistry, in computers, in aviation, in materials, in agriculture and in many other fields. But it is time for the scientific community as well as for all Americans to recognize the real state of the world today. The new competitiveness. The new realities of the times. And particularly, the realities of limited resources. Of having to bear, with other sectors of the society, some temporary sacrifices necessary to economic recovery and to a more productive future. That doesn't mean that we can't and shouldn't strive for preeminence. Of course we should. It simply means that we should concentrate our efforts.

In this difficult process the scientific community should bear primary responsibility for establishing its own priorities. Something generally that we have not been adept at doing in the past. One exception is the disciplinary area with which you here at LBL are quite familiar, being a pioneer in high energy physics. I would compliment the high energy physics community for having demonstrated the ability to examine, realistically and responsibly, the status and direction of the future for high energy physics in this country. I think it has taken as its objective the preservation of the strongest possible high energy physics program to assure American preeminence in this promising and creative area of science while operating in concert with a strong European effort at CERN.

This latter point brings up the important matter of recognizing both the economic and scientific benefits of cooperation, particularly in so-called big science, and the benefit of scientific exchanges in general. I consider it a major thrust of my tenure at the White House to put international cooperation in science in an appropriate perspective. The scientific community itself has long realized that science is conducted in the international arena, that one's passport is virtually irrelevant to one's credential. Governments must also recognize the international nature of science. It is logical for American scientists to do research on foreign facilities just as it has long been routine for scientists from all over the world to conduct research on U.S. facilities. Necessarily, we will maintain our leadership role. This implies that we will maintain overall leadership in facilities, and we intend to do this. However, a spirit of cooperation rather than competition is consistent with a modern perspective of international science.

Now, having set the context with these rather general sentiments about national science policy, let me relate some of what I've said more directly to the situation of the national laboratories. The national labs must play a major role in influencing the direction of American science and therefore of science policy. The national labs represent a substantial fraction of our basic research capabilities and our R and D capabilities. You possess an enormous wealth of talent. Part of this task that is before you, an important part, is a reexamination of the role that you should play in American science and technology. LBL comes to this task with a unique background and a decided advantage. You are unique not only in that you were our first national lab, but that you developed on the Berkeley campus primarily as a basic research laboratory. And you remain totally committed to world leadership in basic research, as your director, Dave Shirley, has so well reminded me. Now the missions of the national labs, even including LBL, have changed, diversified, or expanded considerably. Of course, we should recognize that much of this is due to the changing missions of the supporting federal agencies. These have broadened, too, as we had first an AEC, then an ERDA, and then a DOE.

Now we have to look very carefully at the missions and functions of the labs themselves, particularly in the process of giving sharp focus to national science policy. In doing this, we are faced with problems. Problems that we should confront together. Two of the most important of these involve a declining quality of life in academia and the related problem of the nation's education and manpower needs across science and technology. In the new administration, we're focusing on these problems. We are examining ways to keep an adequate number of our best young scientists and engineers in advanced research and on our faculties. This is not to deprive industry of the new blood they need. Rather it is to assure a future flow of the best trained talent to their labs and plants. I think that the national laboratories can play a role in meeting this objective. A wealth of talent exists in the labs to be used in a number of effective ways. You can serve as new training ground. You can work more closely with industry and academia to create new programs to meet the manpower requirements.

This administration cannot continue in its budgeting exercise without taking an unprecedented look at the national laboratories and the nation's return on its investment, as it were, from these institutions. I think that because of my particular function in the White House, it will necessarily fall upon me to do this and we are beginning this initiative now. It is one I most certainly intend to carry out in concert with members of the entire scientific community, including the national labs, academia, as well as industry, because I feel that the three-way perspective is essential.

If all this sounds somewhat forboding to you, I'd like to interject an optimistic note here. The scientific community has historically, and particularly in recent years, had a tendency to overreact to the slightest government examination. And therefore, I would encourage you not to be looking for disaster around the corner. It simply won't come. One reason is that the national labs have earned considerable respect, not only in the scientific community but in the sponsoring agencies and among the legislators. Most importantly, we are absolutely convinced that the national laboratory system comprises a resource of enormous value. One whose health is of paramount importance.

Let me conclude with a few specific words about LBL. Here I can be most optimistic, and not just because it's your 50th birthday. As I mentioned before, LBL is unique - in its primary missions of basic research, combined with its location on the campus of one of the world's most esteemed academic and scientific institutions. I think that uniqueness insures it not only a special role, but opportunities for a special future. Now it is the responsibility of the University of California and this great laboratory to make sure that that role is fully recognized and those opportunities fully grasped. We expect you to participate in the process of creating a new, mature era of American science and technology in partnership with industry and academia.

We now have to look ahead to the next 50 years. Those years may be filled with challenge and with change. But they will also be filled with an array of scientific and technological advances that may pale the remarkable advances of the past half century. Those past years and your work here at LBL have laid much of the groundwork of knowledge in fundamental sciences, in physics, chemistry, and biology, that will produce the most profound changes in our society as we enter the next century. We must work hard to be masters of that change. To guide it, to shape it, and to give it direction. If not, we will be swept along by forces not of our control and by others who have seized the opportunities and taken the initiative. I am confident that this will not happen. I look to your strengths, skills, understanding, and creativity to help assure that it does not. I know that you won't fail us and we won't fail you.

As the President has said, we are all proud of the Lawrence Berkeley Laboratory for its historic accomplishments. Now we look to the future and to your future with new expectations and hope. We are certain you will help us meet them. Congratulations to all of you on this 50th anniversary and our best wishes for the next 50 years.

DAVID A. SHIRLEY:

Thank you very much for the kind words, George. We do look forward to the next 50 years and to playing a role in helping to shape the science policy for the United States.

Well, this brings us to the end of our banquet celebrating our 50th anniversary. Not to the end of the celebration because we will be having a Family Day tomorrow in the Laboratory from 12:00 until 5:00, from noon until 5 p.m., and we look forward to visits by as many of you as can make it up to the Laboratory. We have a lot of very interesting sights for you to see. I would like to finish by first of all raising a glass to the last 50 years, our first 50 years, and to the next 50 years. And secondly, by reminding you that you may keep this glass as a memento of this occasion. Thank you, and good night.