ACCELERATOR PHYSICS

Robert R. Wilson

Cornell University

Ithaca, NY 14853

It is a pleasure, and a privilege, and a honor to be here today to pay homage to Pief on this occasion. Although he is younger than I, Pief has, ever since I met him 40 years ago, been one of my important role models as an experimental physicist, as a teacher, as an administrator, as an internationalist, and as a proponent of nuclear disarmament.

I am really pinch hitting for Gus Voss who was to come and tell you about accelerators today. My topic will be "Big Accelerators and the Individual". Had Gus been here, he would have told you that we have a bright future in building accelerators. There are no immediate limits. The SSC seems to be a "piece of cake." Until one gets up to about 100 TeV and begins to run into problems of synchrotron radiation by the protons, or diminishing cross sections which would require tremendous intensity and a concomitantly large stored energy in the beams, there should not be much of a problem with proton accelerators. Here at SLAC, of course, with the SLC there are new vistas opening up on how to accelerate electrons, and we can foresee that going on perhaps to the TeV region. So the vistas for particle physicists seem to be wide open. I hope that is what Gus Voss would have told you had he been here.

Well, the technical part seems so rosy that I did not want to talk about that. On the other hand, along with other people, I am worried about the sociological problems associated with these vast projects, especially because as the projects

become larger they become fewer. I was horrified when I learned this summer how much money would be invested in each one of the interaction regions at SLC; how many workers—hundreds, even casts of thousands—would be working under essentially one leader, one director and I began to be concerned. Are these workers going to be physicists or will they be glorified technicians? And if they are going to be technicians, will that kind of work attracts the physicists of the future? When you get old, one of the prerogatives is to look back and make invidious comparisons with the way things were and the way they are becoming now. It turns out that I am not so pessimistic as I may sound.

Well, when I was a student at the University of California the situation was quite different in almost every respect except one—that physicists are attracted by the physics. Then we represented a group that was self-selected. We were attracted by scholarship and the mores of scholarship. Perhaps too we were somewhat in flight from the world of business and politics. As people long before had fled to monasteries, maybe we had fled to the universities to do our work. In general, physicists can be characterized by individualism. I think that is built into the scholarly procedure. The Ph.D. thesis is supposed to be a piece of original work in which one individual makes an original contribution, and eventually publishes it under his own name. That was how it was when I was a student.

As an undergraduate I thought up my own project—that was one of the things we could do at the University of California then—built all of the equipment with my own hands, took the data, worked it up, made the theoretical analysis, and then wrote it up and published. It was a tremendous thrill for me to do every thing myself, and that has always been my ideal of how physics should be done. Alas, it has turned out otherwise. I became the director of a large laboratory. How I had fallen from the purity of my youth? I look around at you in this audience and ask how did all of you leave the path of righteousness?

My own fall from grace happened rather gradually. First, I joined the Radiation Laboratory at Berkeley as a graduate student. The Radiation Laboratory was an exciting place but spoiled individual effort to some extent. What we did there was to join together with one individual, Ernest Lawrence, in building an accelerator. But then, we were individualistic about the way it was used. After the war many such laboratories were spawned from one end of the country to the other at universities in which the faculty members gathered together, built an accelerator and then used it in a quite individualistic manner. A professor and his students, groups of six or less, was characteristic.

That went on for twenty, or thirty years into the 1960s when those university laboratories began to disappear and to be replaced by research at the large national laboratories. Those big laboratories were a necessary way of getting on, doing the kind of physics that we wanted to do. It seems that there were dedicated accelerator physicists, very cunning and skillful, who were able to build the accelerators. Then groups of users came in, at first from one university and then later groups from several universities in larger and larger numbers. Then came greater and greater numbers of names on the papers—ten, twenty, thirty—and a kind of an anonymous physics began to set in that I found altogether deplorable.

We can now envisage a period in our future when we will have Tevetron 1 and the SLC as our principal facilities. Roy Schwitters tells me that 150 people from some fifteen or twenty different institutions will be involved at Fermilab and, I

suppose, a similar number at the SLC. I became concerned, as I know Roy is. Most physicists do cherish their individualism as an important part of the work that they do, yet how can this individualism be cultivated in such large groups?

We do have other comparable institutions abroad where physicists can work and it does seem that that is going to get us through the 1980s. Roy pointed out to me that Tevetron 1 and the SLC will account for about half of the present experimental particle physicists keeping busy. There will be many innovative things: people looking for magnetic monopoles, looking for free quarks, or God knows what in the decay of the protons. So we need not worry too much about this decade. There are a lot of healthy things going on here and abroad that will keep all of us busy.

Of course if any one of those off-beat experiments should connect, i.e., if a magnetic monopole were to be found, then everything would change overnight—the technology, the way we build accelerators, the kind of physics we could do.

For the 1990s we see the SSC coming along, colliding linacs, projects in Europe, but fewer and bigger facilities. As I thought about this I had two fantasies.

My first fantasy was what I would call the MBA horror. The MBA horror is the following: that we will not get past Tevetron 1 and the SLC, that the SSC will not be funded in time, and that in the hiatus we will lose the interest of graduate students, that there will not be any graduate students, and that implies that eventually there will not be any particle physicists.

What will happen then? Well, what might happen will be what happens in NASA, as I understand it. There, they give out projects to industry. A scholar may make a proposition for a proposal but then industry builds the equipment, and takes the data. Scholars have very little to do with it, except they do do some very effective work by that method. So I can see that high energy physics might then revert to the same business.

When you say business, you mean that it reverts to the Harvard MBA's. The first thing they would do, of course, would be to move the project to North Korea where there may be more competitive tunnel builders and more competitive labor. Having done that, they will probably sell the whole thing to some international conglomerate who will mine it for the superconductor, etc., and that could well be the end of the physics. Well, that was my MBA horror: hardly an optimistic thing for the future of particle physics.

Another fantasy, quite different, is what I might call data bank democracy. Let me describe this and then see what attractions it might have. In this fantasy, the words of Art Roberts' song were resounding in my ears "take away your billion dollars, we'll be physicists again." In this one, the SCC would be built in a reasonable time and the various detectors built by a cast of thousands. But in particular one of those detectors is going to be THE Great Detector and it is going to measure every particle and its energy and direction in 4π.

Each one of these events will, as it comes in, be put into a data bank in some gigantic computer such as my colleague Kenneth Wilson will have built by that time. As soon as these events are entered and as they build up in numbers, then any physicist can phone to the data bank and extract what it is that he will. And just depending on his ability, whether he has the PC or whether he has a tremendous computer and has joined with other people, why he can do with that data what he wants to.

This has tremendous advantages. There need not be a program committee. There need not be any committees. The people will just call up and get whatever data that they want. They can use their intuition to work the data as they want. We will have physics again. It is an attractive thought. But it is not so clear that that is a realistic thought. There are some difficulties.

The first difficulty that I can see is that when someone calls up, he will find that the line is busy. The next difficulty that I can see is the reason it is busy is because those teenagers that always get into any data bank. They will be calling up and getting OUR data and working it up without us! So all of that good physics will be coming out from those kids, and that will be an embarrassment for our professional egos.

I can see other difficulties such as there will be some colleague who is going to get a really BIG computer, and then there will be technological imbalance. Probably the data bank democracy that I am discussing will not be stable because that colleague will do all the physics and will just ride roughshod over everybody. And he will be the one who keeps the phone busy as a part of his gigantic computer.

Well, I do not think that is likely to be entirely the case because that kind of person is not going to know what kind of questions to ask. His intuition will not guide him to the kind of questions and the kind of cuts that will determine how that physics is accessed.

To some extent there already is a data bank democracy existing in the various projects where there are large colliding beam detectors. To some extent, it is pretty much a standard thing that the members of the group who know about the detectors, who know about the calibrations, and who know about the data that is available, do form—although there may be from twenty to one hundred people in the total group—individual collaborations of a few physicists looking into their data bank whenever they feel like it and following their noses, not just following what some strong leader tells them to do. So, I think to some extent, the great data bank democracy does exist. But it exists in terms of particular detectors at particular facilities and the people who have associated themselves closely with that detector. That is pretty much the way that it is at Cornell and that it has been at other places.

Also, there are other facilities we have experience with such as the bubble chambers at Berkeley or Brookhaven. They represent data banks where physicists can get into them easily and look up and do physics even though the data has long since been milked for most of its value. And when I heard Carlo Rubbia talking about the evidence for the top meson this summer at Snowmass, it seems to me that he was talking about a data bank that was accumulated in search of the W and the Z intermediate particle. Then that same data bank was gone through again in looking for evidence for the top meson.

There are experts here and the only reason that I am talking about this subject is that I hope to provoke a bit of discussion countering the idiotic things that I am saying. I have already mentioned one of the most difficult things with the data bank is the detector itself. For one thing one has to know the calibrations of all of the counters and that takes an expert, probably on the spot, who has done all of this intercalibration and has the confidence to know what can and cannot be trusted.

The other thing is that any project of that kind is going to be an evolving project in which the data—and the detector itself—will change back and forth depending on the people who are responsible for building it and the way that the physics seems to be going. They will increase the definition in one place or another of the detector; they will change things as required to look at what is being lost down the pipe. In any case, the detector is not likely to be one immutable detector corresponding to one vast data bank, but rather it will be an evolving detector.

Those are some difficulties, but actually I think that it might still be a way of the future. I still think that there is something wrong about the anonymous kind of physics we do when the papers come out with twenty to fifty to one hundred names, and probably more in the future. Five pages of names and one page of physics is what we might anticipate. That is the kind of procedure that will not encourage the best people to go into our field.

Accelerator physicists for some time have adopted an anonymous role, and they do not put their names on all of the experimental papers that come out of the accelerator that they have spent much of their lives on. I think that they get their satisfaction from building a good accelerator which they can feel, quietly, is the first half of any experiment that gets done. And in that sense that is something to emulate. The people working on detectors probably will have to feel that their names should not be on each part of the experiment, and that it would be best if somehow, we could revert back to a few names.

Now in doing that I also feel that there do have to be first-class physicists working on these things with their egos as exposed as ever. So there is no question to me that when the preparatory work is done, i.e., the accelerator and the detector, and when you are at the data-receiving end of the cornucopia than that is where the physicists should be. It seems right that Carlo Rubbia's name comes up so frequently when we are talking about the recent results from CERN because he did play a role in every part of that adventure that was just overwhelming, but as a general physicist, not a specialist.

I would imagine that for any kind of development on any detector, one would have a period when the people who have done the hard and imaginative work would have a priority on its application, but then later on that data might revert to a data bank for the use of everyone.

Well, I am not terribly concerned about all this. As I have said, the physicists and the production of physics seem to be quite constant in time. The physicists have shown that they do whatever it is to be done to get the data out, to get their experiments finished and done. Whether it is becoming politicians or becoming laboratory directors or becoming plumbers, God knows that one does what must be done to get at good physics. Furthermore, physics is just as vigorous today as it ever has been. It is clear that the difficulties we seem to be having now will be eventually overcome. Yet, the sociology of physics is changing. Perhaps we should just leave it to Pief, who as in the past as a great physics leader, will advise us wisely how to solve these future problems—if indeed they exist.