A Personal Perspective of High Energy Accelerators

by Gustav-Adolf Voss

At the beginning of this year I retired from the position of Head of the Accelerator Department at "Deutsches Elektronen Synchrotron- DESY", which I had held for the last 22 years. I want to thank the organizers of this conference for the invitation to give a talk on "A Personal Perspective of High Energy Accelerators". This gives me a chance to thank my teachers, my colleagues and friends, whom I have found during the 37 years in which I have been working in the field of accelerators and who have helped to make my life so exciting and wonderful.

My first teachers were Stanley M.Livingston, Ken Robinson and Tom L.Collins. It is widely known, that Livingston had built the first working cyclotron as his Ph.D. thesis under Lawrence . It is also known that Lawrence was inspired with the idea of the cyclotron by reading Rolf Wideroe`s paper on the first linear accelerator, back in 1927. And Rolf Wideroe, although he is 93 years old by now, is still healthy and full of ideas, which he sometimes tries out on me. As you see, accelerator science does not yet have a long history.

Ken Robinson was the legendary genius, who developed most of the theory of electron synchrotrons and storage rings single-handedly and in whose unpublished papers, found after his death, the basics of the free electron laser had already been developed, 10 years ahead of time. Tom Collins was Assistant Director at the Cambridge Electron Accelerator, my first real place of work. He developed the "Collins-Straight-Section" and furthered my technical education.
After 14 years at the CEA I went to the Deutsches Elektronen-Synchrotron DESY from where I have now formally retired.

Among my first friends and colleagues from the CEA times one finds - besides the already mentioned Stan Livingston, Ken Robinson and Tom Collins - such well known names as Karl Strauch, John Rees, Ewan Paterson, Herman Winick and Albert Hoffman.

When I started my professional life in the accelerator field, life was different. The first atomic bomb had been exploded not much more than a decade earlier and the prestige of the nuclear physicist was much higher than it is today. The goals of particle physics were less questioned and there was less competition for public research funds. For accelerator builders, bureaucracy had not yet been invented, at least not in America. Those were the golden days.

Accelerator science and technology were in their infancy. Strong focusing had just been invented by Courant, Livingston and Snyder and independently by Christophilos, but there was hardly anyone who understood tolerance problems, nonlinear resonances, dynamic apertures and other aspects of theoretical machine physics and, at the same time, equally well the engineering aspects of those machines, their costs and their potential technical pitfalls. If there is not one single person, who has at least some rudimentary understanding of all these aspects, it is very difficult to find the right compromises in building accelerators. So, with hindsight, some of these early machines look rather awkward: Huge tunnel cross sections, fantastic magnet support systems which were sometimes so complicated, that it became a real problem to turn on these machines, big and expensive air conditioning systems for the ring tunnel, which would not tolerate much power dissipation in the ring tunnel and thereby made other installations expensive and complicated. Radiation safety was another area, where lack of understanding and technical perspective produced technical abominations.
It was Bob Wilson, I believe, who was the first all-round accelerator builder, to set a new style. Bob was a penny pincher when it came to building accelerators, surpassed perhaps only by Wolfgang Paul of Bonn University in Germany. Both of these two outstanding physicists were and still are my heroes. Paul was most unhappy when he saw money squandered "as if it was play money in a Monopoly game", and to Wilson it was a challenge to see what he could get away with technically in his wish to save money. Project leaders like Wilson and Paul (Paul built the first strong focusing synchrotron in Europe) had to have a good understanding of the risks they were taking when they cut all the trimmings of accelerators down to the bone. They put the other people on their projects at ease by assuming the full responsibility for all major decisions themselves .

What happens, if you do not have competent project leaders with full authority and power to make the decisions on how the money is spent? The first result is usually a cost overrun: People working on the project don´t want to be blamed if the machine does not work when completed. So they insist on extra safety margins in those parts they are responsible for. And that means more money spent. Then, the schedule also begins to slip, because the number of changes increases, changes which are a result of insecurity, trying to play it safe. And so it goes on. And this sorry state of affairs is not helped by imposing organization experts or military brass. Also, large numbers of reviews and vast herds of reviewers can no longer save the situation. This little piece of wisdom should be obvious to everyone, particularly to those in power to make major decisions.

How refreshing and exhilerating on the other hand is the situation, where you have very little money but the urgent will to do new and exciting physics with a new machine. Where competent people "steal, rob and cheat" to scratch the resources together to get their project built in the fastest way imaginable, at a bargain price and in record time. Some of our most productive electron positron storage rings have been built that way.
By claiming that some of our most productive machines have been built in situations where very little money was available, I do not want to give the impression that I have a contempt for wealth. Wealth makes great opportunities. Tunnels which have been built in the olden days with 5 times the necessary cross

section make it now possible to install a second or even a third machine. Laboratories used to inflated budgets can now build new machines with very little new money. Stepping into such a situation, making use of all that wealth, can make you an instant hero. It is the change of style which makes those golden opportunities.
Wealth can manifest itself in buildings, in money but also in staff. After all, many laboratories today spend much more money on salaries than on electricity bills and new equipment. In some places it seems to be difficult to focus a significant portion of the staff on the actual problems at hand. Personnel management is something very few of us have studied. Some people in responsible positions do it right instinctively, some succeed by the sheer power of their enthusiasm and power of conviction, but many do it wrong or not at all. If it were possible to focus only half of the staff on the real important things at hand, in most places there would be no staff shortage, rather the opposite. Some smaller exceptional labs seem to be doing fine, but others need change. Several stories come to my mind to illustrate how much the output of a lab depends on spirit and motivation: The rapid cycling Princeton Penn proton synchrotron had a staff of more than 300 when they were told one day to close down the place. In the year after this announcement they gradually reduced the staff to 25, while at the same time running a full proton program and at the same time also developing the techniques for heavy ion acceleration. That was the most productive year they ever had in their whole history.

In some places what seems to be needed is a change in the style of management .
Style of course is also subject to the external forces, with which we have to live. We are no longer allowed to do business the way it could be done 40 years ago. Innumerable reports, review committees and other checks apparently have to accompany many major projects. There are laudable exceptions and I myself have enjoyed such blissful conditions during the last two decades of my professional life. But in other not so happy situations things are quite bad: This reminds me of large interlock chains which people sometimes arrange in order to improve equipment safety. After all, this is given by the product of the probability of the failure of all the components of the system, and if there are only enough interlocks in series you must be safe. This of course is only true if each single interlock is tested with the same rigor and sincerity which you want to have applied to the system. Coming back to reviews and reports, each of them, to be meaningful, should have a depth, a competence and a sincerity fully matched to that of the project leader whose head is always on the block. I believe, this is rarely the case.

Things certainly were different 25 years ago. Professionally, the most formative years for me were those between 1967 and 1972, when the very small group of people: Bob Averill, Albert Hofmann, Roy Little, Harry Mieras, Ewan Paterson, Ken Robinson, Karl Strauch, Herman Winick and myself embarked on the CEA-By-pass project. Since money for a multi-GeV electron-positron storage ring was not available, we had the idea of using the old 6 GeV electron synchrotron, modifying it and using it for storage of counter rotating electron and positron beams. This way we hoped to be the first to do colliding beam experiments in the multi-GeV region. And all that had to be done with almost no extra funds.

Nothing came easy and to everybody involved these were the most grueling but also the most rewarding years. The CEA had an alternating gradient magnet focusing system and synchrotron radiation led to a horizontal anti-damping of betatron oscillations. Special damping magnets, invented by Robinson, the world's first wiggler magnets, were necessary for stable beam storage. Damping magnets, actually, are considerably more sophisticated than ordinary wiggler magnets, because they need a very strong gradient field at the location of the beam.
The 100 MeV positron linac had much too small an energy to inject and accumulate positrons in the synchrotron. A special multicycle operation became necessary, where the synchrotron cycled between the injection energy of 100 MeV and a peak energy of 3 GeV. Synchrotron radiation at the high energy part of each cycle damped injection oscillations sufficiently to allow beam accumulation.
A special by-pass to the synchrotron had to be invented, to create enough space for a meaningful detector with the world`s first low beta interaction region for high luminosity. Nothing of all this worked from the beginning and it was an uphill battle all along. In the course of this work we did a number of things which were great fun: We decided that with certain precautions it would be perfectly safe to be in the tunnel with a stored 2 GeV 1 mA beam. This can make a huge difference to the set-up and adjustment of a machine. The very small aperture beam pipes of the by-pass could be adjusted simply by watching beam life times, a total of 48 distributed sextupole magnets could be adjusted simply by watching Q-changes, when they were shorted by hand. Vertical beam height at the interaction point could be minimized by taking polaroid pictures of the photon -beam from a carbon fiber at the interaction point and by adjusting the rotational tilt of some of the critical magnets. Synchrotron radiation monitors are much easier to install and align if you can see the light, adjust the mirrors and the lenses for optimum focusing. Before we did all this, of course, we had carefully assessed the situation and determined that what we were doing was absolutely safe, albeit not very conventional. My teacher Tom Collins always maintained that the safest thing in the world is a person who really understands all aspects of what he is doing and acts accordingly in a responsible, conservative way. The emphasis is on understanding all aspects and acting accordingly. No tiger could be trained to do that. As an aside: Studying the personel roster of a lab with a challenging project a few weeks ago, I found 2 accelerator physicists, and 10 full time radiation safety officers!

The By-pass project was an almost impossible mission and I secretely made the vow, that if I ever should get out of it without loosing face, I would never again get myself involved in another major project. Well, we measured the first multi-hadron production cross sections, results which nobody believed at that time because the cross sections were so unexpectedly large. But, it turned out, they were right! And I went to DESY and - against my better instincts - got involved with PETRA and - together with Bjoern Wiik - with HERA. Particularly the 20 GeV e+ - e- storage ring PETRA became an exhilerating experience for the DESY staff and for me.

Scheduled for a 4 year construction period, the staff became so concerned with keeping the self imposed schedule, that each of the people responsible for a particular component worked ahead to leave time for unforseen problems. Thus, the schedule could be revised twice and moved forward. The machine was actually turned on 2 years and 8 months after authorization. Also, staying within the authorized budget of 100 MDM had become such an overriding issue, that only 80% of this amount had been committed at the time of first successful beam storage, leaving a comfortable cushion for later improvement work. No inflationary increases needed to be claimed. For some obscure reason R.R. Wilson´s rule seemed to be true also in this case: Construction time and project costs seem to go hand in hand.

Accelerators have become more efficient and much more economical over the last 30 years. I once had to give a talk on the evolution of new technologies and costs in the accelerator field and discovered, that we have gained at least a factor of ten in GeV/ M$, when we compare inflation adjusted prices of the first multi-GeV machines with machines we build today. And it is not so much the new technologies like superconducting magnets or superconducting rf which cause these savings but it is mostly the better understanding of what is and what is not really necessary in accelerator building.

The big question is now, how will we go on? How will we build tomorrow´s accelerators with an order of magnitude higher energy at a cost which society is willing to pay?
There is one school which believes, that what we need now are new ideas. Many people of this school aim at ultra high accelerating gradients: A table top multi GeV accelerator is their dream. Thinking about such machines can be great fun and very entertaining. Of course, it should not be the size of an accelerator but its costs which must be minimized. What most of the people studying ultra high gradient ideas overlook is, that not only energy but also luminosity has to be increased. We are talking about colliders at very large center of mass energies, which require very high luminosities to produce a meaningful counting rate. There are many good reasons why this then translates into very large beam power. The aspect which then becomes increasingly important is the over-all power efficiency of these machines. None of all the new accelerating ideas (and I myself have contributed to this effort) shows much promise in this respect.
So we are probably left with the old principles: Superconducting proton storage rings for proton-proton collisions and more or less conventional linear colliders for electron positron collisions. How then can we possibly build a machine with ten times the center of mass energy at a cost society is willing to pay? I believe that the only answer is an extreme economizing of standard technology, plus a large all-encompassing international collaboration, plus lots of luck. The LHC has just passed that hurdle. I only hope, that ways may be found to avoid the missing magnet concept under which it was approved.
The problem of getting the next linear collider for electron positron collisions approved looks more formidable. Despite a large number of international workshops I have not seen, over the last 6 years, much convergence of ideas and concepts. With everybody just following his own pet idea, it is hard to see how a big international collaboration can be formed and find approval from the respective governments. Part of the problem may be, that linear collider studies have not yet reached the degree of maturity where a proposal can be written. We may have to wait until detailed technical lay-outs and in particular prices are better known and we must hope that a merging of ideas and designs will then occur. If there is not one single government willing to start such a project but if instead international concensus is required from the beginning, such convergence must be reached. The basis, I believe, must be proven technology and economy. As long as these two aspects cannot be substantiated by hard facts and numbers, one cannot write a proposal, not to mention any hope of approval.
I hope very much, that an international consensus can be found soon. I love our science and I am fascinated by it, even if the gap between our machine science and technology and the latest theoretical ideas like superstrings seems to widen all the time and the energies to check a particular "final" theory seem to be dishearteningly out of reach. There are enough open questions within the energy range we can cover in the next decade.And as long as the field finds so many bright and motivated young people as evidenced by this conference, we need not worry!