

Viewpoint

Gustav-Adolf Voss - with everybody following their pet idea, it is hard to see how an international collaboration can be formed.

Retiring after a distinguished career, including 14 years at the Cambridge (Massachusetts) Electron Accelerator and 22 years as Head of the Accelerator Department at the DESY Laboratory in Hamburg (March/April, page 6), Gustav-Adolf Voss was invited to give a 'Personal Perspective of High Energy Accelerators' at the Particle Accelerator Conference in Dallas, Texas, in May, of which we publish an abbreviated version.

At the beginning of this year I retired as Head of the Accelerator Department at the Deutsches Elektronen Synchrotron, DESY, in Hamburg, a position I had held for 22 years. An invitation to talk gives me a chance to thank my teachers, colleagues and friends from the 37 years I have been working in the accelerator field.

My first teachers were Stanley M. Livingston, Ken Robinson and Tom L. Collins. It is widely known that Livingston built the first working cyclotron as his PhD thesis under Ernest Lawrence. It is also known that Lawrence was inspired with the cyclotron idea by reading Rolf Wideroe's pioneer linear accelerator paper in 1927. Rolf Wideroe, although 93 now, is still full of ideas, which he sometimes tries out on me. Accelerator science does not have a long history.

Ken Robinson was the legendary genius who developed most of the theory of electron synchrotrons and storage rings single-handed and in whose unpublished papers, found after his death, the basics of the free electron laser had been developed, a full 10 years ahead of time. Tom Collins was Assistant Director at the Cambridge Electron Accelerator (CEA), my first real workplace. He developed the 'Collins Straight

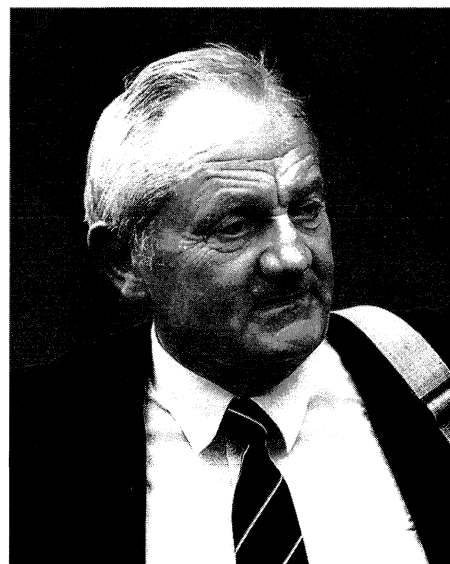
Section' and furthered my technical education.

Among my CEA colleagues and friends were also Karl Strauch, John Rees, Ewan Paterson, Herman Winick and Albert Hofmann.

When I started 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 today. The goals of particle physics were less questioned and there was less competition for research funds. For accelerator builders, bureaucracy had not yet arrived, at least in the US.

Accelerator science and technology were in their infancy. Strong focusing had just been invented by Courant, Livingston and Snyder and independently by Christofilos, 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, appreciated the engineering aspects of those machines, their costs and potential technical pitfalls. With no single person with at least a rudimentary understanding of all these aspects, it is difficult to make the right compromises in accelerator building. Now, some of these early machines look awkward.

It was Bob Wilson, I believe, who, as the first all-round accelerator builder, set a new style. He was a penny-pincher, surpassed perhaps only by Wolfgang Paul of Bonn (who built the first strong focusing synchrotron in Europe). Both these outstanding physicists are still my heroes. Paul was very unhappy when he saw money squandered 'as if it were play money in a game of Monopoly', and for Wilson it was a challenge to cut



technical corners and save money.

Project leaders like Wilson and Paul had to have a good understanding of the risks they were taking when they trimmed their machines to the bone, and put others at ease by taking full responsibility.

What happens when there are no project leaders with full authority to decide how the money is spent? The first result is usually a cost overrun. People do not want to be blamed if the machine does not work, so insist on generous safety margins, which cost money. Then the project starts to slip as these changes proliferate. This sorry state of affairs is not helped by imposing organization experts or military brass from outside. Frequent reviews and many reviewers cannot save the situation.

A challenge on the other hand is a situation with little money but the chance to do new and exciting physics with a new machine. Highly competent machine physicists can cobble together the resources to build their project as quickly as possible, at a bargain price and in record time. Some of our most productive electron rings have been

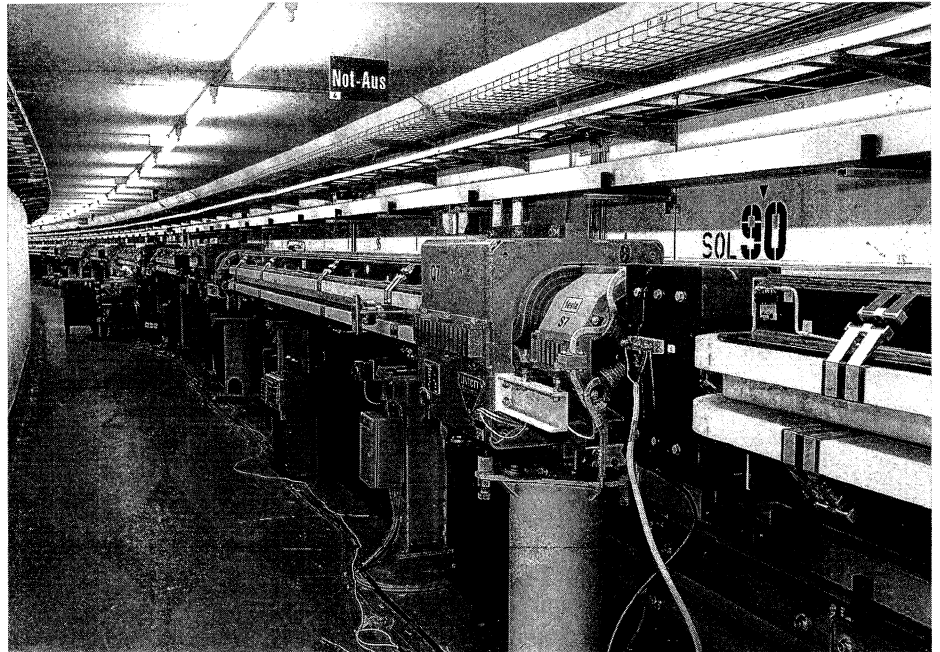
Under Voss' guidance, the PETRA electron-positron collider at the DESY Laboratory, Hamburg, was turned on just 2 years and 8 months after authorization.

built this way.

But I do not want to give the impression that I have a contempt for wealth. Wealth provides opportunity - tunnels which were originally built with five times the cross-section then needed can now accommodate a second or even a third machine. Laboratories used to ample budgets can now build new machines cheaply. Stepping into such a situation and making use of these opportunities can make you a hero overnight.

Wealth can be in buildings and money and also in staff. Many Laboratories spend more money on salaries than on power and new equipment. Manpower has to be focused on the problems at hand. The rapid-cycling Princeton-Penn synchrotron had a staff of 300 and one day were told to close down. One year later they had reduced the staff level to 25, while still running a full proton programme and at the same time developing techniques for heavy ion acceleration!

After 14 years at the CEA I moved to DESY. For me, and I hope for the DESY staff, the PETRA 20 GeV electron-positron storage ring was an exhilarating experience. Scheduled for a four-year construction period, the staff became so concerned with the schedule that everyone worked ahead to leave ample time for unforeseen problems. There were not many of these, so the schedule could be advanced. PETRA was turned on just 2 years and 8 months after authorization. Staying with the imposed 100 million DM budget had become such an overriding issue that only 80% of this amount had been committed when the first beams were stored, leaving a comfortable reserve for subsequent improvements. Wilson's rule seemed to be true also in this



case - construction time and project costs go hand in hand.

Over the past 30 years, accelerators have become more efficient and more economical. Comparing inflation-adjusted prices of the first GeV machines with those of today, we have gained a factor of at least ten in GeV per dollar. This is due not so much to new technologies like superconducting magnets and radiofrequency cavities, but more to a better understanding of what is really necessary in accelerator building.

Where do we go from here? How can we build tomorrow's accelerators with ten times the energy at a cost which the taxpayer can afford?

One school believes this needs new ideas to produce ultra-high accelerating gradients. Their dream is a tabletop multi-GeV accelerator. But it is cost, more than size, which has to be minimized. Many people investigating ultra-high accelerating gradients overlook that luminosity, as well as energy, has to be increased. At very

high collision energies, extremely high luminosities have to be achieved to provide the physics conditions. And this translates, for a variety of reasons, into very high beam power, and in turn the overall power efficiency of the machine becomes important. None at all of the new accelerating ideas (and I have contributed to this effort) shows much promise in this respect.

So we are left with the traditional approaches - superconducting storage rings for proton-proton colliders and more or less conventional linear accelerators for electron-positron colliders. Building a machine for ten times the collision energy at a reasonable cost can only be achieved by an extreme economy of standard technology, together with an enveloping international collaboration, and some good fortune. The LHC has just passed this hurdle, and I trust that it will manage to sidestep the missing magnet proviso under which it was approved.

The problem for building the next

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Living in large experiments

ECFA report

linear electron-positron collider looks more formidable. Despite a large number of international workshops, I have not seen much convergence of ideas over the past six years. With everybody following their pet idea, it is hard to see how an international collaboration can be formed and approval obtained.

Part of the problem might be that linear collider studies have not yet matured to the degree where a proposal can be written. We may have to wait until detailed technical specifications and in particular prices become more concrete before ideas and designs can merge. Such convergence has to happen before international consensus can be reached. Its basis, I believe, must be proven technology and economics. As long as these two aspects cannot be substantiated by hard facts and numbers, one cannot write a proposal, let alone get it approved.

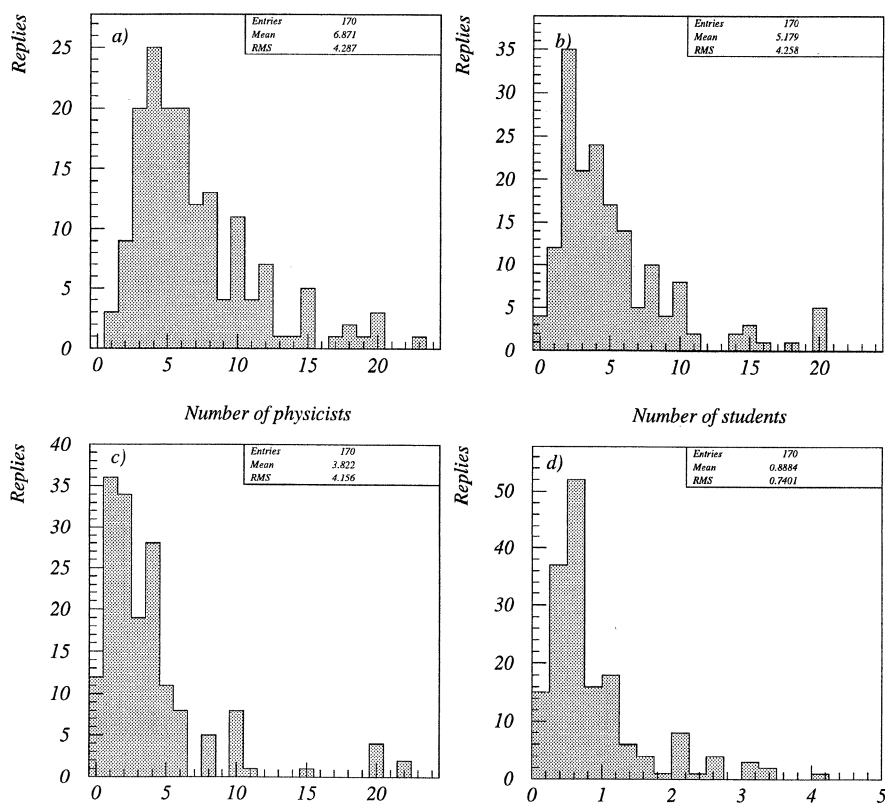
I very much hope that such an international consensus can soon be found. I remain fascinated by this science, even if the gap between machine physics and the latest theoretical ideas like superstrings seems to diverge and the energies needed to verify a 'final' theory seem unattainable. There are important open questions to answer within the energy range within reach in the next decade. As long as the field continues to attract bright and motivated young people, then we have no cause for concern.

The European Committee for Future Accelerators (ECFA) plays an important role in shaping the physics programme in Europe, especially around major facilities which attract increasingly large numbers of users. How happy are physicists working in these scientific Towers of Babel? Apart from making physics discoveries, what are their requirements? Following a suggestion of CERN Director General Chris Llewellyn Smith, to provide an updated answer to these questions ECFA Chairman Günter Flügge initiated a survey on the sociology of large experiments. It fell to Bjarne Stugu of Bergen, as the youngest member of ECFA, to lead an ECFA subcommittee to carry it out.

A similar ECFA survey, carried out back in 1979/80, was influential in preparing infrastructure and thinking for the LEP experimental programme.

In the past 20 years, high energy physics collaborations have grown from typically 20 persons at fixed target experiments to around 500 for current major detectors at large colliding beam facilities.

The high complexity and cost of the two general-purpose experiments for the LHC require even larger collaborations, approaching 1500. With these two experiments alone - ATLAS and CMS - absorbing a considerable fraction of the entire high energy physics community, it is



How big is a physics research group? A survey on the sociology of large experiments carried out by the European Committee for Future Accelerators (ECFA) gives a detailed answer - a) is the number of physicists, b) students, c) technicians, and d) students per physicist.