Ten Years Ago

...some personal reminiscences

Hildred Blewett

called for unprecedented precision in large-scale engineering. Its operation depended on the new principle which had question marks in it even on paper. How much beam, for example, could the machine keep under control when it was accelerated through 'transition' — the energy at which the beam behaviour changes and the accelerator has rapidly to follow suit.

All these problems were in the hands of a brilliant but inexperienced team. They were solved. They were solved on schedule (within six years of the signing of the CERN Convention) and they were solved at a cost (120 million Swiss francs) reasonably close to the estimate worked out in 1955.

The success of the collaboration was perhaps even more impressive. To propose that Belgians, British, Dutch, French, Germans, Greeks, Italians, Scandinavians, Swiss and Yugoslavs could work successfully together on a huge project — demanding the highest technical skill, the production of a million components by manufacturers scattered throughout Europe, very careful planning, and the closest cooperation — was idealistic. But it worked.

In 1969, with CERN a large and experienced organization, it is easy to forget the pioneering spirit of those years. But the building of the PS left its mark on all who were involved. They will still talk intensely about what they did and about their 'fierce pride' in doing it. It is their achievement in particular that we are honouring this month.

Remember the night of November 24th, 1959 ? Of course I do. I was sitting in the Canteen eating supper with John Adams, as we had done many times that Fall. There was not a wide choice of food in those days — spaghetti or ravioli or, occasionally, fried eggs — but our thoughts were not on the meal. We had hardly spoken, our spirits were low, then John lit his pipe and said, *'Well, now that we've finished eating, we might as well walk over and see if anything is happening.'* As we went in the direction of the PS buildings, I asked him, *'Shall we go to the Main Control Room or over to the Central Building ? Chris Schmelzer said that Wolfgang Schnell has that radial phase-control thing working'*. John pulled on his pipe, *'Probably doesn't matter, it may not do much good.'* Our hopes had been dashed fairly often. Then after a few more steps, he added, *'Let's go to the Central Building and see what they're up to.'* It was about quarter to seven.

Trudging along, I thought back over the past weeks, back to September 16th when, during the Accelerator Conference at CERN, Adams had made the electrifying announcement that protons injected into the PS had gone one turn around the magnet ring. Since that time, attempts to put the PS into operation had brought a few triumphant moments but most of the time we had been discouraged, puzzled by the beam's behaviour, frustrated by faulty equipment or, after quick trials of this remedy or that, in despair over the lack of success. The protons just didn't want to be accelerated.

During the summer of 1959, Adams had written to invite me to stay on at CERN after the Conference since it looked as if the PS would be ready for running-in tests and Brookhaven had agreed because information from these trials would be very useful for the start-up of the AGS that would probably occur during the following summer. But I had to go back soon to help on the AGS. Pressure for high-energy protons in the United States was mounting even higher with the imminent production of European ones, so I had already booked passage to sail home. For some time I had been saying to everyone that we

This photo, almost as famous a record of the night of 24 November 1959 as the polaroid photo on the cover, shows the seraphic contentment of Hildred Blewett when full energy had been achieved. She was scheduled to leave Geneva the following day and that night was her last chance to see the PS in. Hildred has become known as the mid-wife of large accelerators having been present at the first operation of the Brookhaven Cosmotron, the CERN PS and the Brookhaven AGS, and close to the first operation of Saturne at Saclay and the ZGS at Argonne. She is now at CERN for three years during which time she hopes to continue her reputation by seeing in the Intersecting Storage Rings.

(Photo J. Sharp)

must get the protons through 'transition' before I left. Now it was November 24th, I must leave Geneva the following day, but the prospects were bleak. Would this beam-night be any different ?

Although the PS had been ready to accept protons from the Linac in September, a great deal of final testing had not been completed and installation and cabling was going on in the ring and the Main Control Room. Consequently, for the first few weeks, beam tests could be scheduled only for Tuesdays and Thursdays from six to ten in the evening; during the final weeks of my stay there was also some time on Friday evenings. During these sessions, our spirits ranged from high to low as the beam behaved somewhat as expected or baffled us completely.

Shortly after the Conference, when the first pickup electrodes were operating, we had happily seen the injected beam spiral in, under the influence of the rising magnetic field (without r.f.), observed betatron oscillations for the first time, and eagerly estimated Q-values from the beating patterns. But the spiralling time was wrong and the Q-values were off, with radial and vertical values different. Why ?

Another night, beam from the Linac was on its way but nothing at all could be seen on the scopes in the MCR. After many checks, it was found that a resistor had burned out in the power supply of the electrostatic inflector. Adams, Hine and Lapostolle rushed into the main-ring tunnel, glared at the offending item, hustled to have it replaced. It burned out again — and again — an evening wasted and home discouraged.

Early in October, the programmed part of the r.f. system was ready for trial. Schmelzer and Hans Geibel were in the Central Building and Pierre Germain was peering at scopes in the Main Control Room. Linac said beam was ready and inflector working. Hine was in the MCR, looking at the injected beam, adjusting quadrupoles, changing inflector voltage, rushing from one scope to another. *The beam isn't spiralling properly... wait... all right, go ahead r.f.... Central Building says it's on, programme on. Yes, beam is being captured... it's accelerated... but lost after a few milliseconds. Changes in the r.f. programming... is the beam better... yes, now it goes for 10 milliseconds... no, it's 15... now it's gone again.* But we went home satisfied — some beam had been captured, there had been some acceleration.

More evenings with trials of the r.f. programme followed, mostly unsatisfactory, then a surge of hope spread through the group. Careful checking of the inflector area showed that a temporary observation-flag was sticking into the vacuum chamber at the entry point and, even worse, some of the mechanical structure of the inflector seemed to cut off part of the circulating beam. Mixed with the chagrin at not finding these things earlier was a feeling that here, at last, was the source of the injection troubles. Next night, there was decided improvement in the spiralling, beam was more reliable, but still there was coupling between the vertical and radial betatron oscillations and the Q-values were still wrong. In spite of this, the r.f. programme was tried again, Schmelzer and the r.f. group busy in the Central Building and Hine, Adams, Pierre Germain and the rest of us anxiously looking at the scopes in the MCR. But, although occasionally captured and accelerated for a few milliseconds, the beam again dribbled off and was lost.

The r.f. system had been designed to run with a frequency programme to a few GeV, then to switch over to an automatic system with a phase-lock and with errors in the beam's radial position fed back to the r.f. amplitude for correction. When this automatic system was ready, it was tried with switching-in much earlier than planned and this did succeed in accelerating the beam somewhat longer. But then it was lost, usually in a series of steps and all gone after a few tens of milliseconds. I don't remember if we reached 2 or 3 GeV on an occasional pulse, but certainly no more. The behaviour of the beam remained erratic and unstable. What was wrong?

Measurements of the beam's position on the radial pickup electrodes were hastily plotted by Adams to show that the closed orbit was off in some places, but only by a few centimetres, surely not enough to prevent some beam from going to transition. The rate of rise of the magnetic field was varied to look for eddy-current troubles. Colin Ramm and the Magnet Group rushed around the ring in the daytime, searching for stray fields or remanence effects. Jean Gervaise scanned the survey data for possible errors in magnet positions while Jack Freeman hunted for signs of beam disappearances with radiation monitors. More trials of the r.f., with and without phase-lock, more diagnostic equipment hurriedly inserted, more measurements. But the protons made no progress.

During those Tuesday and Thursday evenings in October and early November, many of the PS builders gathered around the tables in the centre of the Main Control Room. Only the most essential of the control racks were there, the rest were being installed and, while we worked, men in blue coats were busy behind the racks cabling or, from time to time, accidently uncabling. I cannot remember all the people who were there, but *always* there was Hine, usually Adams, Johnsen, Hereward, Schoch, Schmelzer, Pierre Germain. Sometimes Sagnell and Sharp would stop working to take 'candid' photos (often in the way of the *serious* observers). Konopasek would be improving pickup stations, Munday investigating vacuum gauges, disembodied voices coming from Linac, Power House, or Central Building. To try to bring order out of chaos, an Engineer-in-Charge was appointed for each session and among those taking a turn were Bonaudi, Brianti, Fischer, Jacob and Reich.

At one stage, to save (or prevent ?) people from going home to eat and being late for the scheduled 6 p.m. start-up, Hine arranged cold meats, cheese and bread to be sent to the MCR. As I recall, this was not a rousing success.

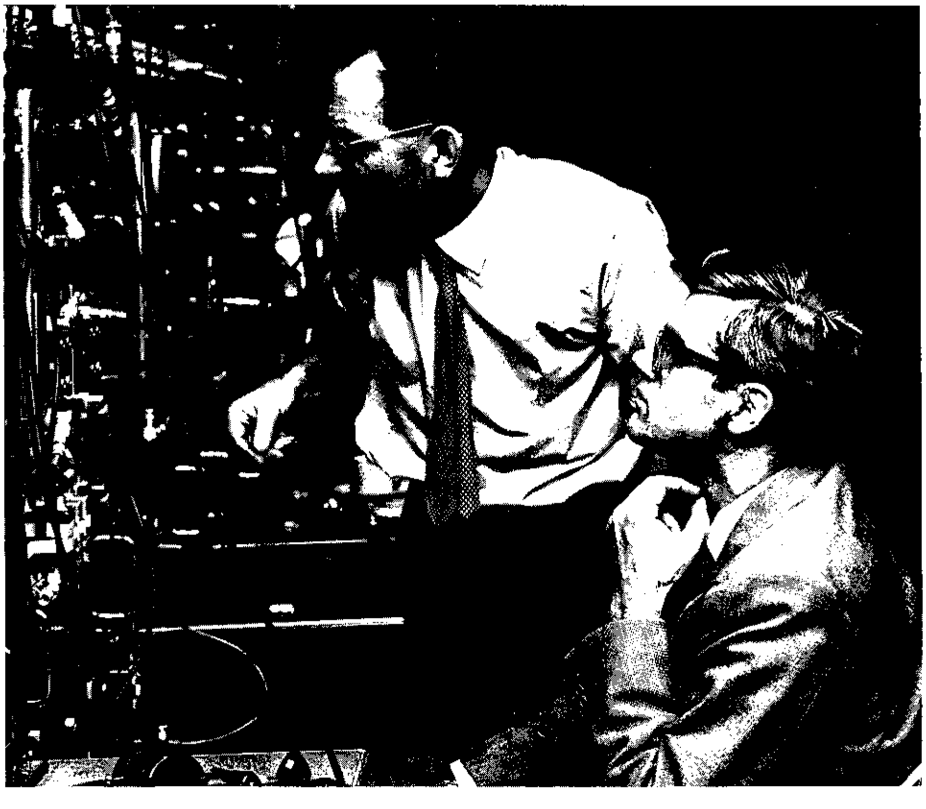
There were periods of frantic activity. *Beam is on... let's try this... with r.f.... without r.f.... look at the scope... measure this... photograph that...* But there were also long periods of waiting. *Inflector is off again... the high-voltage set feeding the Linac is sparking over... instruments not ready... something disconnected.* We sat at the tables and waited and waited. One night, just as beam came on, all the lights went out — trouble at the CERN

Commissioning days

Pierre Lapostolle (left) and Mervyn Hine do not give the impression of having been highly delighted with the way things were going.

Below : Mervyn Hine and Kjell Johnsen also looking a bit short on confidence in what they were doing.

(Photos B. Sagnell)

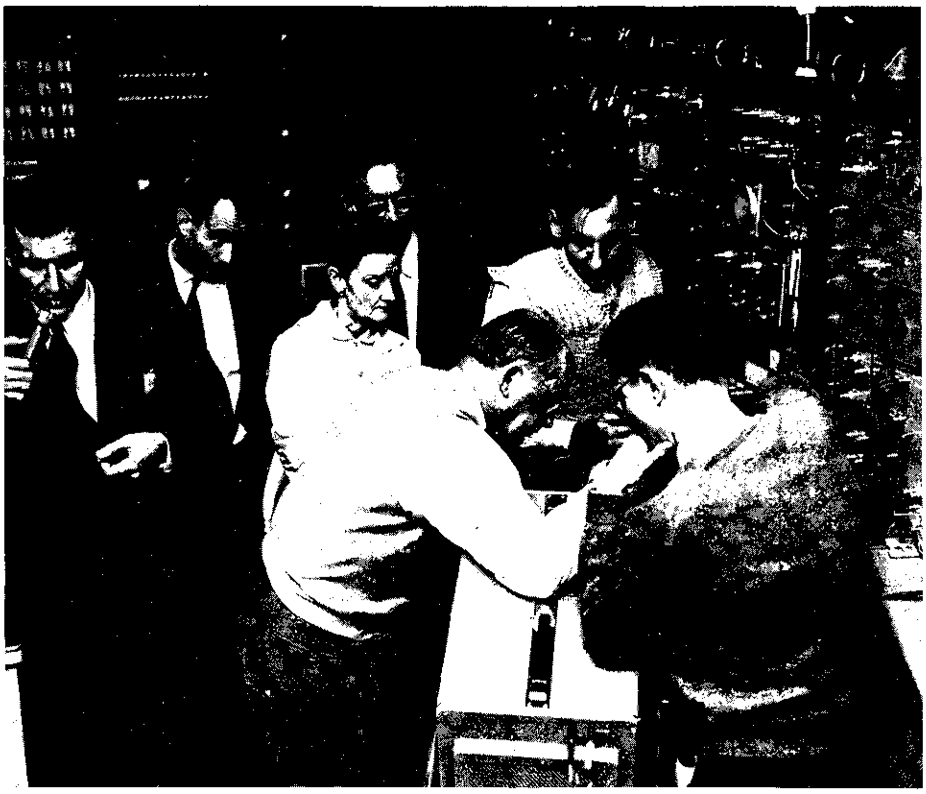


main power house — and we groped our way out in darkness, Adams striking matches all the way.

During the days, many activities were going on : tests on the magnets and pole-face windings, Linac running, tests on r.f. components, tests in the Power House, interminable cabling. Throughout the buildings, there were sets of call lights which flashed in various patterns. One pattern was repeated so often that I finally asked whose it was. It was for Kees Zilverschoon, whom we seldom saw. He was in charge of all installation and coordination and was always off in tunnels, chasing workmen, following missing items, running from place to place.

I had a desk in Mervyn Hine's office where in the mornings, particularly after beam-nights, one after another would come in — Johnsen, Hereward, Schoch, Schmelzer, sometimes Adams, many others — and the talk would start. *Are the closed-orbit deviations causing serious trouble ? Is the Linac emittance all right ? What about the missing bunches, caused by the poor performance of the inflector ?* Every Monday morning, in the PS Conference Room, there was a meeting of the 'Running-in Committee', starting at 9 a.m. sharp and lasting until well after 1 p.m. or even 2 p.m. Discussions and arguments — on and on.

But sometimes it was quiet, when Hine and the others were off preparing for the evening runs. Then, I might go down the hall to a nearby office where Ed Regenstreif was compiling the full story of the PS and we would talk of the time back in 1951 when, at the request of the early CERN Council, he had come to the United States to look at the construction of the Brookhaven Cosmotron and the Berkeley Bevatron. Regenstreif had earned the eternal gratitude of the Cosmotron builders (then an unknown and inexperienced group) for his praise of our design that resulted in the CERN Council's decision, at first, to build a scaled-up Cosmotron of 10 GeV. It had been to reduce the costs of this machine that Stan Livingston, in a spirit of international goodwill, had first considered possibilities for size reduction by reversing blocks in a Cosmotron-like magnet. This was in the summer of 1952, at Brookhaven after the Cosmotron had



Expressions still worried as some of the commissioning team cluster around an oscilloscope. Left to right : John Adams, Hans Geibel, Hildred Blewett, Lloyd Smith, Chris Schmelzer, Wolfgang Schnell, Pierre Germain.

(Photo B. Sagnell)

successfully operated. We had been looking forward to a visit by Dahl, Goward and Wideröe to discuss the plans for the proposed European high-energy accelerator. Further work by Livingston, Courant and Snyder had led to the alternating-gradient design principles in time for presentation to our European visitors.

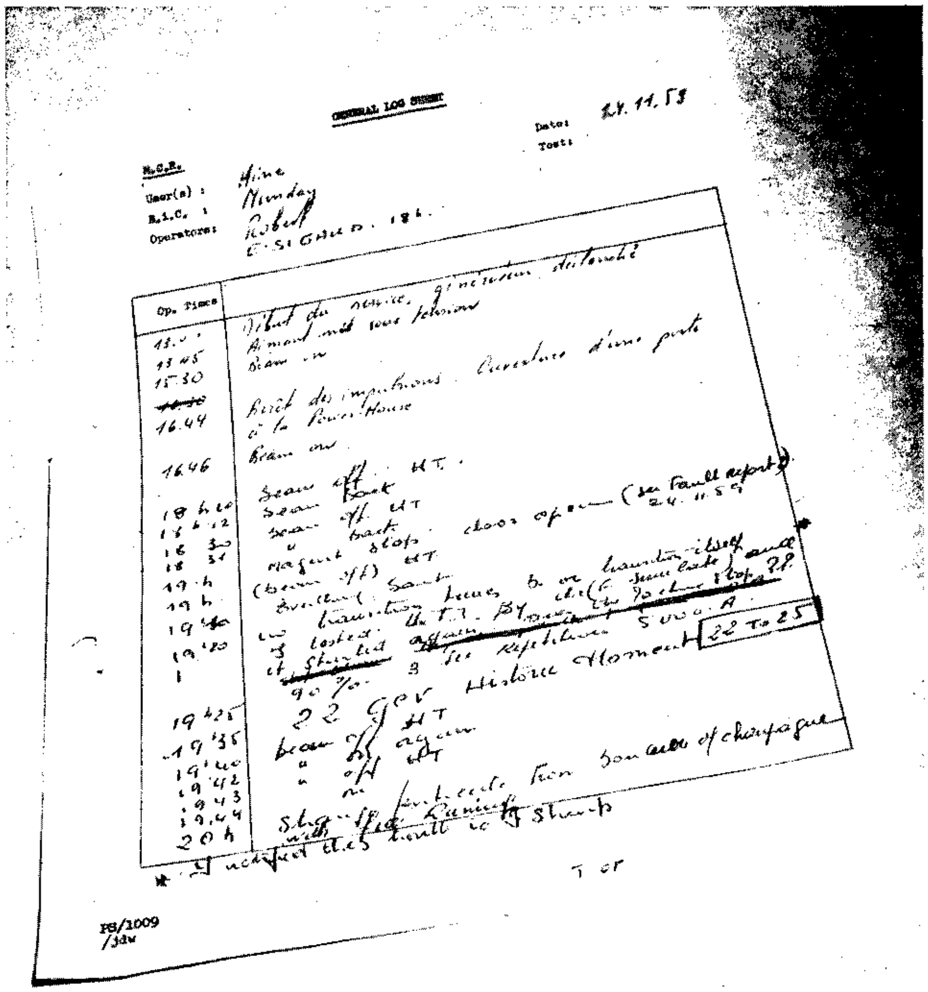
Often, in the daytime, I went over to the Linac Building, to look at the 50 MeV beam, to watch measurements being made by Pierre Lapostolle, Peter Standley, Colin Taylor, Bryan Montague and others. Over there another American visitor was frequently watching or helping — Lloyd Smith, at CERN a year on leave from Berkeley. Later on, he was to be so impressed by the performance of the CERN PS that he would go back to Berkeley with a set of parameters for a similar machine of 100 GeV, infect his coworkers with his enthusiasm and begin serious design. Afterwards, this would be changed to a design for 200 GeV that would find support from the US high-energy-physics community, culminating in the large accelerator at Batavia.

Occasionally, on a Sunday, I would go along the lake to visit my good friends, Kjell and Aase Johnsen, and we would recall the days in 1953 when the first designs for the PS were being worked out by groups in various places (Harwell, Paris, Heidelberg, Bergen, etc.) all under the leadership of Odd Dahl in Bergen. John Blewett and I had spent some months in Bergen in the summer of 1953 and, during that time, Johnsen had been working on the behaviour of the beam at transition energy (where there is no phase stability). His calculations had given us the first confidence that beam could be accelerated through this dangerous region. Or, the Johnsens and I would go back to November 1952, when a small group (including Kjell and myself) had come to Geneva and been taken out near the French border to see the fields where some day a large Laboratory might be built. This trip was after a meeting in Paris where the main topic of discussion had been the recent work of Adams, Hine, and Lawson on the serious effects of magnet inhomogeneities and their discovery of frightening resonances. Or, we would come back again to 1953, to early October, when the PS Design Group had come to Geneva (a risky business with no governmental commitments and finances on a month-to-month basis) to start work at the Physics Institute of the University of Geneva. There we had all worked hard to prepare for a Conference, held later that October, where papers on the design of a 30 GeV machine (later reduced to 25 GeV by the Council) had been presented to an audience of accelerator experts, Council members and their advisors. The Design Group had been nervous — it could not be proved that this novel type of accelerator would work, so many hazards, such inexperience, the future of European high-energy physics at stake. I had been nervous, too. I had worked up some cost figures, based on Cosmotron costs, and the totals were *much* in excess of what had been anticipated. Would the Council accept it ? But these reminiscences with the Johnsens would be broken off by the sudden arrival of Hine, bringing us back to 1959, to the problems of the PS. He had a new theory. What did we think ? Should we try this next week ?

Many of these things were in my thoughts as Adams and I approached the Central Building. I was depressed at having to leave the next day, with the protons still balking. I had wanted so much to see this machine operate successfully before I left. All through the years, I had been so involved with CERN and its PS that I had felt a glow of pride with each milestone passed during construction. More than ever, over these past weeks, I had felt that it was partly my machine too. John interrupted my thoughts with *'Well, Hildred, we haven't done much during your stay. It's hardly been worthwhile, you haven't learned...'.* I broke in, *'Wolfgang thinks this radial phase-control will really work, he's very optimistic, and maybe...'.* But I knew that no one else had great hopes for any improvement. Even Schmelzer had thought it was hardly worth the effort, but Schnell had gone ahead over the last couple of weeks wiring it up for a quick test. Just a few days before, I had been down in the basement lab, listening to his enthusiasm. The idea was to use the radial-position signal from the beam to control the r.f. phase instead of the amplitude. With this system, the sign of the phase had to be reversed at transition and, in his haste, Schnell had built this part into a Nescafé tin, the only thing of the right size. But could Nescafé tins help ? There had been so many disappointments. Perhaps something really fundamental was wrong. I felt a certain last desperate hope, but I wasn't optimistic.

Adams opened the door of the Central Building. For a moment the lights blinded us, then we saw Schmelzer, Geibel and Rosset — they were smiling. Schnell walked towards us and, without a word, pulled us over to the scope. We looked... there was a broad green trace... *What's the timing... why, why the beam is out to transition energy ?* I said it out loud — *TRANSITION !*

Just then a voice came from the Main Control Room. It was Hine, sounding a bit sharp (he was running himself ragged, as usual, and more frustrated than anyone), *'Have you people some programme for tonight, what are you planning to do ? I want to...'.* Schnell interrupted, *'Have you looked at the beam ? Go and look at the scope'.* A long silence... then, very quietly, *The page from the PS log book, 24.11.59. At 19 h. 35, it reads '22 GeV Historic Moment'. By 20 h. 00 the entries had become rather incoherent.*



Below : Congratulations Italian Style. Gilberto Bernardini embraces John Adams in the Main Control Room.

(Photo J. Sharp)

Hereward's voice, *'Are you going to try to go through transition tonight ?'* But Schnell was already behind the racks with his Nescafé tin, Geibel was out in front checking that the wires went to the right places, not the usual wrong ones. Quickly, quickly, it was ready. But the timing had to be set right. *Set it at the calculated value... look at the scope... yes, there's a little beam through... turn the timing knob* (Schnell says I yelled this at him, I don't remember) *...timing changed, little by little... the green band gets longer... no losses. Is it... look again... we're through... YES, WE'RE THROUGH TRANSITION !*

How far ? What's the energy ? Something below 10 GeV because the magnet cycle is set for lower fields and a one-second repetition rate for testing. Hurried call to Georgijevic in the Power House. *Change the magnet cycle to full field.* Beam off while we wait. The long minutes drag by. Will the beam come on again ? This is just the time for that dratted inflector to go off again, or the high-voltage set to arc over. *Hurry up, Power House !* I remember Schnell murmuring, *'I promised you we'd get through transition'*. But we were all rather awed by it. No one spoke — Schmelzer lit a cigar, Adams relit his pipe, we waited.

Finally, the call came through — magnet on again, pulsing to top field. *Call the Linac for beam. Beam on, it's injected, inflector holding, beam spiralling. R.f. on, all set as before with the blessed phase-control and the Nescafé tin. Change timing on the scopes, watch them and hold your breath. One second (time for acceleration) is a long time. The green band of beam starts across the scope... steadily, no losses... to transition... through it... on, on how far will it go... on, on IT'S ALL THE WAY ! ! Can it be ? There it goes again, all the way as before... and again... and again. Beautiful, smooth, constant, no-loss green band... Look again at the timing... all the way... it must be 25 GeV !* I'm told I screamed, the first sound, but all I remember is laughing and crying and everyone there shouting at once, pumping each other's hands, clapping each other on the back while I was hugging them all. And the beam went on, pulse after pulse.

Slowly, we came back to earth. John Adams was first. Looking very calm, he

John Adams announces the news to the CERN staff in the Main Auditorium the following morning. In his hands the famous vodka bottle and the polaroid photograph it was to carry to Dubna.

went to the phone to ring up the Director General, C.J. Bakker, to tell him the news but Bakker didn't seem to grasp it right away (could it be that John was just a little incoherent?) Schmelzer was beaming, for once even his cigar forgotten, cold on the ashtray. Schnell looked supremely happy, he was the hero of the hour. Gradually, I collected my wits enough to write out a telegram to Brookhaven that Geibel dashed off to send immediately. We went over to the Main Control Room and found Hine calling round to locate some sort of counter for checking the energy. Johnsen was saying, heatedly, *'Did someone change the timing on this scope ? I just turned away from it for a moment and here is the beam going out...'* How could it be 25 GeV without poleface windings on ? But all the scopes showed the same smooth green trace, one second long — it really *was* 25 GeV. Even more unbelievable, the signal on the pickup electrodes gave an intensity of about 10^{10} protons per pulse. No, that can't possibly be right, we're lucky if it's 10^{9}. Check and recheck... look at the calibrations... yes, that number *is* right, 10^{10}.

The rest of that evening has been described many times. People came flooding in, I don't know who told them the news. Polaroid pictures of the scope traces were passed around for signatures on the back, cherished souvenirs. Bottles appeared, by magic, including the famous bottle of vodka given to Adams by Nikitin. Bakker arrived with a bottle of gin under his arm. Bernardini bounded in, hugged Adams and Hine, launched into a description of what he wanted to do as a first experiment, then lapsed into pure Italian. Miss Steel and the secretaries were there, smiling happily — they had had to put up with our complaints and bad humours. I remember Colin Ramm muttering, *'Where do we go from here ? What about two or three hundred GeV ?'* (He was ahead of the times.) I left shortly before midnight to pack my suitcases.

Early next morning (at 2 a.m. New York time) I had a phone call from John Blewett offering congratulations from Brookhaven and asking questions. My telegram had come as a bombshell and the word had spread rapidly across the United States. What had brought success ? I told him about the phase-control system and, since it was similar to the one being built for the AGS, it was a relief to know that this was just what the protons liked.

Then out to the Lab for final goodbyes, over to the Auditorium to hear Adams tell the story to all CERN, my PS friends grinning proudly but no one happier than I. Then, Hine was driving me to the station and many of these good friends there to see me off with, *'Well, we did it for you !'* General euphoria in the air. Just one complaint — Kjell Johnsen, wistfully, *'You know, we've been cheated. It all happened too fast. I thought we would be happy to reach transition, then, after a while another good feeling at 10 GeV, a week or so later maybe 15 GeV and, perhaps after a month or more, 25 GeV. Now it's all happened at once, there is only one moment of triumph. We've been cheated'.*

But the train is moving and there is Pierre Germain waving from across the tracks. The train passes the CERN site. There is the great PS ring and the glorious memories of the previous evening. Now to hurry and get protons around the AGS.

1959-1969 Ten years in the life of a machine

Operation and development of the PS have brought a dramatic improvement in its capabilities. This article describes the years of getting to know the machine and of developing its potential, picking out some major landmarks on the way. The article was put together with the help of P.H. Standley (PS Division Head), C.S. Taylor (Linac), G. Plass, W. Richter (Main Ring), M. Georgijevic (Power Supply), J.H.B. Madsen (Operation), and G.L. Munday (Experimental Areas).

Parameters of the 28 GeV Proton Synchrotron

	1959	1969
Linac		
Type of ion source	*r.f.*	*duoplasmatron*
Pre-accelerator (Cockcroft Walton)	*500 kV*	*500 kV*
Number of Linac cavities	*3*	*3*
Final energy	*50 MeV*	*50 MeV*
Pulse length	*10 μs*	*20 μs*
Intensity of 50 MeV beam	*about 3 mA*	*about 110 mA*
Peak Linac intensity	*4.8 mA*	*140 mA*
Main Ring		
Diameter of ring	*200 m*	*200 m*
Magnetic field	*147 G to 14 kG*	*147 G to 14 kG*
Number of magnets in ring	*100*	*100*
Weight of magnet	*3 400 tons*	*3 400 tons*
Vacuum chamber circumference	*628 m*	*628 m*
Vacuum chamber cross-section	*14.5 × 7 cm²*	*14.5 × 7 cm²*
Number and type of pumps	*70 oil diffusion*	*60 oil diffusion 24 sputter ion*
Vacuum pressure	*about 3 × 10^{-6} torr*	*below 2 × 10^{-6} torr*
Number of r.f. units	*16*	*14*
Frequency range	*2.9 to 9.55 MHz*	*2.9 to 9.55 MHz*
Peak voltage of r.f. system	*133 kV*	*143 kV*
Energy gain per turn	*54 keV (at 12 kG/s)*	*75 keV (at 16.7 kG/s)*
Number of protons accelerated per pulse	*about 3.10^{10}*	*about 2.10^{12}*
Power Supply		
Type of power supply	*Three phase motor alternator Twelve phase static convertor*	*Three phase motor alternator Two twelve phase static convertors*
Rotation speed	*3 000 rev/min*	*1 000 rev/min*
Weight of flywheel and rotor	*28 tons*	*90 tons*
Magnet losses	*1.6 MW*	*2.8 MW*
Pulse repetition time for a 20 ms flat top	*3 s (25 GeV/c) 5 s (28 GeV/c)*	*1.7 s (25 GeV/c) 1.9 s (28 GeV/c)*
Operation	*(Early 1960)*	
Number of internal targets	*1*	*3*
Number of ejected beams	*None*	*2 fast, 1 slow*
Number of external targets	*None*	*4*
Number of experiments fed per pulse	*2 to 3*	*7 to 8 (plus 3 to 4 testing/parasiting)*
Experimental Facilities	*(Early 1960)*	
Experimental floor area	*about 2 500 m² (North and South Halls partially available)*	*9 000 m² (North, South, East and South-East Halls, plus two bubble chambers)*
Bubble chambers	*30 cm hydrogen (in March 1960)*	*81 cm and 2 m hydrogen 1.2 m heavy liquid*
Number of beam transport elements	*3*	*about 250*
Maximum power consumption in Halls	*about 0.5 MW*	*21 MW*

When the PS was designed many of the techniques and the associated apparatus which are now in everyday use on the machine had not been dreamed up or were then just not technologically feasible. No-one could predict either the growing demands of particle physics or the growing expertise in the use of the machine which has led to continuous development of its versatility. It is amusing to remember the feeling of slight unease which mingled with the triumph among the machine builders when the synchrotron first came into action. The machine was built and most of the design staff expected to return home — this was the simple assumption on which most of them had come to Geneva.

But ten years of experience have taught that an instrument of this kind will probably never be 'ready' — ready in the sense of a machine that is switched on and off and runs without change. During these years many modifications have been introduced and new systems have been added — to increase its capabilities as a source of high energy particles, to ease operation, to improve control of the many parameters, and to reduce damage due to irradiation of the synchrotron components. Even more radical changes will be forthcoming within the next three or four years as part of the PS Improvement Programme.

Linac

In 1960, when the linear accelerator began to provide a beam for the PS on a regular basis, the task of the Linac Group seemed to be fairly clear and straightforward. The staff consisted of the physicists, engineers and technicians who had actually built the machine (the theoreticians and designers having moved to other jobs) and the priorities were only too obvious to the practically-minded, the reliability of component parts being measured directly in terms of lost sleep and lost week-ends. The beam behaviour seemed reasonably clear as well. Early measurements suggested that the energy spread, and the beam diameter and divergence, were about what one expected. The problem, mainly, was to increase the intensity, since it appeared that the PS could use more protons if the Linac could provide them.