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The development of techniques for the analysis of track-chamber pictures at CERN

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REPORT CHS-20

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This paper is the third in a series dealing with the evolution of CERN's scientific policy from 1955 to 1965. The other two discuss the provision of beams and of detectors. This one is concerned with development of data-handling systems to analyse the output from detectors and, more specifically, with the photographic output from track chambers.

Our treatment of this topic is dominated by two questions: What steps were taken at CERN to equip the laboratory with the wherewithal that it needed to handle track chamber photographs? and was it capable of satisfying the demands made of it? Consistent with the logic of this problematic, our overall organizational principle is technique rather than time. There are three main sections. The first two deal with the acquisitions of instruments for handling bubble-chamber photographs, distinguishing between the construction and use of relatively standard measuring machines, and the research and development of a new technology which originated at CERN in the early sixties, the Hough–Powell device. In the third main section we discuss the measures taken from 1961 onwards to acquire facilities for the analysis of spark-chamber data.

The organizational location of the events we are going to describe was the Scientific and Technical Servies (STS) division, renamed Données et Documents (DD) in 1961. Lew Kowarski was Divisional Leader until 1963; Yves Goldschmidt-Clermont his right-hand man. Its main function, as its original name made clear, was to provide a service to the scientists in the organization. Its staff was never very large—about 7% of the CERN total throughout our period—and their 'social weight' was slight—two out of 32 of the CERN senior staff in 1960 were from STS, to be compared with three from Administration and 15 from the Proton Synchrotron group.

It is assumed in what follows that the reader has little or no prior technical knowledge. We begin therefore by introducing some basic concepts, and by explaining the main stages in the bubble-chamber picture-handling chain—stages which were in fact common to the analysis of the output from all forms of track chamber, including cloud and spark chambers.

20.1 The heart of the CERN data-processing system: the Instrument for the Evaluation of Photographs (Iep)

20.1.1 BUBBLE-CHAMBER PICTURE ANALYSIS: SOME BASIC CONCEPTS

A typical bubble chamber photograph comprises a large number and variety of *tracks*. Each track is a somewhat discontinuous string of bubbles generated by nuclear particles as they traverse the superheated liquid in the chamber. Some of these tracks are created by the background (e.g. cosmic radiation) others by the particles from an accelerator directed at the chamber. If the chamber is not immersed in a magnetic field most of the tracks are essentially straight, though their path is modified by scattering with the nuclei of the liquid in the chamber. In the presence of a homogeneous magnetic field the tracks of charged particles are curved into helices, disturbed again by scattering. A minority of tracks display distincitve features—a sudden change of direction or a number of 'rays' diverging from a vertex, for example. These indicate that the incoming particle has undergone an interaction in the chamber, creating products which may go on to react further. These processes are characteristic of an *event*. For present purposes, let us define an event as being made up of one or more vertices from which individual particle tracks emanate.

Six main operations were involved in the handling of track-chamber pictures. They were scanning (selecting the pictures containing the events one wanted from all those produced in the run), measurement (measuring the coordinates of points on the tracks using a digitized projector), geometrical reconstruction (reconstruction of the 'actual' track in three-dimensional space from two or more of the stereoscopic views photographed by the cameras), kinematical analysis (interpreting the measured event using momentum and energy conservation), experimental analysis (e.g. performing statistical analyses on the results), and bookkeeping (maintaining an up-to-date, easily retrievable record of the photographs handled and the results obtained).

In the late fifties–early sixties a reasonably large and sophisticated bubble chamber like the Alvarez group's 15"(38 cm) liquid hydrogen device, produced about a million photographs a year when used on the Bevatron. Of these from 5–10% contained strange-particle events, the kind of event for which the bubble chamber was predominantly used at the time. Manual procedures for measuring and analysing such events had been developed by cloud-chamber physicists but, as Hugh Bradner (LRL) pointed out in 1958, 'the analysis of only the [200] strange particles produced in 24 hours of bubble chamber operation could keep one physicist, using the old analysis techniques, busy for almost a year'. To process the 50–100,000 events being churned out annually by one bubble chamber clearly called for the development of more rapid, automated procedures.[2]

Berkeley took the lead in acquiring sophisticated equipment for handling track-chamber pictures, being the first in the field with big bubble chambers. The problem of handling thousands of events rapidly was attacked by building special equipment to record event coordinates electronically. These were stored on tape or cards and fed to a computer which carried out the subsequent analyses previously also done by hand. An idea of the improvement in the speed and reliability of data handling can be gained from figures quoted by Hugh Bradner in January 1960. He estimated the following average times for the several main operations performed by the Alvarez group:

Scanning - 10 mins/event
Sketching (of the event of interest; subsequently dispensed with)
- 10 mins/event
Measuring - 10 mins/event
Computing 'plus handling' (on an IBM 704)
- 1 min/event
Remeasurement - 2–30 mins/event
Bookkeeping - 6 mins/event[3]

Two points emerge clearly from these figures: that if it was taking as much as a day to analyze manually a cloud-chamber event in 1955, it was taking no more than an hour to analyze a bubble-chamber event at Berkeley in 1960 and, secondly, that as much as half of this time was spent on remeasurement. This had to be done once or even twice for the 15–20% of the events which were rejected on the first run, due, for example, to operator mistakes, mechanism failures in the measuring tables, computer program bugs, poor quality pictures, or simply ambiguous events. The difficulty of removing such flaws completely remained a fundamental obstacle to the fully automatic scanning and measurement of track-chamber photographs.[4]

20.1.2 1956–60: LAYING THE FOUNDATIONS OF A DATA-HANDLING CAPACITY

Towards the end of 1956 Yves Goldschmidt-Clermont, a Belgian physicist who had joined CERN, and Kowarski's group in 1953, along with two other physicists, Guy von Dardel (counters, electronics) and Charles Peyrou (bubble chambers) began seriously to think about the kind of device CERN might need for dealing with the output of track-chambers. Early in 1957 Goldschmidt visited a number of major American installations to refine his ideas. Later that year he and the other physicists, along with F. Iselin, took the opportunity provided by a small European meeting on instrumentation to describe 'the present state of the instrument for evaluation of photographs and future developments' at CERN. Their proposals constituted the backbone of the laboratory's initial 'Iep' programme.[5]

The heart of the measuring system was a standard microscope platform (or stage) carrying a lens, which projected an enlarged image of the event onto a screen. The successive views of the event were brought onto the screen by moving the stage/lens system over the three rolls of bubble-chamber film secured alongside one another in the film-holder. When the image of a point on the track fell on a cross-hair engraved on the screen its coordinates were recorded. This was achieved by measuring the displacement of the stage in the x and y directions from a reference point. The displacement was digitized using a suitable optical system, passed to a binary counter, stored in memory, and then automatically punched on tape. This tape also carried the other information about the event needed at subsequent stages in the data-handling chain, which was punched onto it by the operator using an electric typewriter.

In the simplest version of an Iep one followed the track to be measured by moving the stage manually and used a simple magnifying lens to help centre it on the cross-hair. In more sophisticated versions both of these steps—track centring and track following—were automated to reduce operator error and fatigue, and to improve measurement accuracy. The underlying idea in automatic track centring, achieved at CERN in the late fifties with the so-called sambatron, was to convert the distance between the image of the centre of the track and the cross-hair into an 'error signal'. This was fed to an electronic circuit which automatically displaced the stage until the track was aligned (zero error signal). Automatic track following was but a short step from this: by pressing a suitable 'accelerator' pedal, the operator moved the stage along the track, which was kept centred with a device like the sambatron, and the coordinates were measured 'on the fly'. Rough guidance was provided by a manually operated joystick, which the operator used to steer the automatic system across faded regions of a track, to ensure that it did not set off along a crossing track, and so on.[6]

Along with developing instruments for measuring rectangular coordinates, some members of the STS group, and Ross Macleod in particular, also designed and built a simpler instrument for measuring angles. Called the Baby Iep, these devices were intended for measuring single-vertex, multiprong, star-type events produced in relatively small chambers operated without a magnetic field: i.e. they were limited to rather simple events with straight tracks. The event was measured by projecting its image onto a 'protractor' centred on its vertex. The protractor was fitted with a radial reference line which was manually rotated until it fell along one of the prongs. Thereupon the angle through which it had moved was digitized and punched on tapes by the operator. Two Baby Ieps were built at CERN in the late fifties; they were about four or five times faster, though somewhat less precise, than the standard coordinate digitizing Ieps.[7]

From the start it was generally understood that the bubble-chamber pictures taken at CERN would be distributed between the European laboratory and its member states. This was seen to imply that CERN should not only develop its own measuring equipment, but should take a lead in helping national laboratories build up theirs. As a result a conference 'to catalyse an interchange of information between CERN and various groups working on measuring devices', and 'to enquire what steps could be taken towards initiating a co-operative effort in this field [...]' was held at the laboratory on the 15 and 16 September 1958. It was attended by some 60 visitors, about 50 from western Europe, and seven from the United States. Alvarez's group, who had displayed one of their coordinate-measuring instruments (nicknamed Franckenstein, after its designer J.V. Franck) at the UN atoms-for-peace conference held in Geneva just before, was particularly well represented.[8]

Two main points emerged from this meeting. Firstly, Berkeley's position as world leader was confirmed. The model of Franckenstein displayed in Geneva was similar in conception to CERN's Ieps but was already equipped with automatic track centring and a form of automatic track following, as well as other procedures to reduce operator errors. By contrast, CERN at this time had only completed Iep 1, with manual track centring. The LRL had also taken the lead in developing a new generation of devices, notably Bruce McCormick's spiral scanner or Reaper, as it was called. The Reaper, like the Baby Iep, was a digitizing protractor, but there the resemblance ended. It measured the polar coordinates of curved tracks emanating from a single-vertex star by transforming into pulses the light projected through a number of slits cut in a spinning disc centred on the vertex. These pulses were digitized, and transferred to a computer, which was directed to filter the star-pattern pulses from background pulses, and to store the result on tape. Both devices, the Franckenstein and the Reaper (still in prototype form in the late fifties), were thus far more automated in their operation than anything similar at CERN.[9]

The meeting in September 1958 also confirmed that Kowarski's group was by far the most advanced in Europe. Three institutions, the Ecole Polytechnique, Birmingham University, and Imperial College, were developing instruments similar to CERN's Ieps, but did not envisage more than assisted track centring. Two others, Saclay and the Cavendish, had developed instruments which had no aids like track centring or track following: automation here amounted to punching the coordinates on tape. At the Max Planck Institute in Munich and at Bologna University the entire process was still essentially manual.

At a discussion on collaboration held at the end of the meeting there was a 'general feeling' that, as a first step towards remedying this situation, 'some sort of standardization of instrumentation' was called for. At a second, smaller meeting held at CERN on 23 February 1959 it emerged that at least France, Germany, and Italy—though probably not Britain—were considerably interested in purchasing a mass-produced device which was not too expensive. A 'standardization and market exploration committee' was set up with Goldschmidt-Clermont as secretary, and it presented its final proposals in December 1959. The committee recommended a device offered by a Paris-based firm, La Société d'Optique et de Mécanique de Haute Précision (SOM). The cheapest version was to cost SF 134,000 (some $35000), and was like Iep 2: the stage was moved manually, and an enlarged oscilloscope display of the track region was used to assist (manual) track centring. By the end of 1961 several of these devices were delivered and in use at CERN and in various European laboratories.[10]

To complete this survey we should like to make two points about the computing facilities available for handling track-chamber picture analysis in our period. Firstly, they were hopelessly inadequate. Towards the end of 1955 the SPC somewhat cautiously approved the purchase of a Ferranti Mercury computer.[11] Originally promised for shipment in February 1957, the computer actually only arrived in the summer of 1958, and passed its acceptance tests in October of that year. What is more the computer was not only late. It rapidly emerged that the machine as purchased was particularly ill-suited for handling the analysis of track-chamber pictures. Because of its slow input/output facilities the Mercury's computing speed was effectively squandered: when running Iep programmes it could spend as much as a quarter of its time punching out results. To get around these difficulties the Iep group rented time on an IBM 704 in Paris, and ordered three magnetic tape decks to improve the input/output situation at the end of 1959.[12]

Our second comment is that the needs of data-handling, which had been of only peripheral concern when the Mercury was purchased, were in the forefront of everyone's minds when the SPC decided on the next computer in October 1959. The month before, at a IUPAP sponsored conference at CERN, Art Rosenfeld (LRL) had described the specialized programmes which had been written at Berkeley to analyse data from Alvarez's hydrogen bubble chamber on an IBM 704. In the previous academic year alone the group had spent 12 man-years on developing these programmes to a level of sophistication way beyond anything that CERN, or indeed anyone else, had yet achieved. The desire to capitalize on these developments by 'joining the club' of American IBM users was a major consideration in the SPC's decision that CERN, too, should have an IBM 704. In the event a somewhat more powerful IBM 709 was delivered early in November 1960, and was put into operation by the end of the month.[13]

20.1.3 THE PERFORMANCE OF THE IEP MEASURING FACTORY

From 1958 onwards the STS division gradually built up its stock of Ieps and scanning tables, the technical staff to operate and to maintain them, and the general-purpose geometric and kinematic programmes to analyze their output. By the summer of 1962 this 'measuring factory' was equipped with 8 Ieps of various degrees of sophistication and about 6 scanning tables. It was staffed by 66 people (including visitors), half of whom were scanning and measuring technicians. There were 9 programmers and 11 physicists, including 6 fellows, assigned to the complex.[14]

At this time the factory processed bubble chamber (and a few cloud chamber) photographs for a variety of experimental groups inside CERN distributed between the Track Chamber (TC), Nuclear Physics (NP), and Nuclear Physics Apparatus (NPA) divisions. DD itself had a group of physicists who teamed up with their colleagues in TC to do experiments. They, along with the measuring factory, constituted the so-called DD (EXP) section of DD. Another section, DD (DEV), was devoted to the maintenance and improvement of the Ieps, and to the development of new techniques for handling track chamber output.

Serious doubts about the performance of the measuring factory were raised at a meeting of the SPC at the end of October 1961. The debate was triggered by the announcement of the discovery of the ω^0 meson by Alvarez's group using the 72" hydrogen bubble chamber at Berkeley. What made the situation particularly galling was that the same resonance appeared in some 'wonderful pictures' (Amaldi) taken at CERN during the preceding months using the Saclay 81cm hydrogen chamber—but that the pictures had not yet been fully analyzed. A wide-ranging discussion of why CERN was late ensued, during which Amaldi 'observed that several Italian scientists who had recently been working at Berkeley considered that Laboratory's scanning effort to be one or two orders of magnitude greater than the European scanning effort', including CERN. He went on to single out the difference in programming effort between LRL and CERN as being especially significant.[15]

How justified were these anxieties? How did the data-handling effort at CERN compare with that of the most advanced and efficient group in the world? Unfortunately we do not have figures for the situation prevailing at the end of 1961, when these criticisms were made. However, those given in Table 20.1 provide one with some idea of the relative degree of developement of the data-handling capacity in Alverez's group and at CERN in 1962/63.

The first striking point that emerges from the table is that, with twice as many digitizing projectors, CERN was processing half as many pictures annually as the Alvarez group. There were three related reasons for this difference.

Firstly, the Ieps lacked adequate track centring and following devices—standard facilities on the Franckensteins. The sambatron, developed at CERN to automate track centring in 1959, and mounted on Iep 2, 3, and 4, had 'introduced measuring errors due to mistakes in its conception and difficulties in its proper use'. This had further implications: on Iep 3 the sambatron signals were used to control automatic track following, but 'due to imperfections of the sambatron and other weak points of the machine, the device was not an overall improvement', and had been put out of service.[16] In their wide-ranging memorandum on the performance of CERN's data-handling 'factory' (on which the figures in Table 20.1 are based), Ross Macleod and L. Montanet claimed that technical improvements of this kind could improve measurement rates by as much as 50%.[17]

Table 20.1

Comparison of bubble-chamber data-handling facilities in LRL and CERN in 1962/3

	LRL mid-1963	CERN mid-1962
No. of digitizing projectors	4	8
No. of events measured/projector/ hour	6 (4–5 in mid-1962)	2
No. of operating hours/week	120	90
No. of events processed annually	70,000[a]	35,000
No. of scanning and measuring technicians	60	33
No. of computing staff	~ 20	9
No. of hours/weeks IBM 7090 time	50 +	8–12[b]

Sources
The data for LRL was obtained from Rosenfeld and Humphrey (1963), except for the information in parentheses which is from Rosenfeld (1962). The CERN data is from Macleod and Montanet—see note 14.

Notes
a. The Berkeley figure is the number of event measurements that '[found] their way into the final published physics'—it thus includes events remeasured *and* events discarded for one reason or another. The CERN figure compensates for remeasurement *only*.
b. This is the estimated demand for the beginning of 1963. It is made up of 40 hours of IBM 709 time and 14 hours of Mercury time. The IBM 709 was 4 to 6 times slower than the IBM 7090, and we assume the Mercury was twice as slow again.

Secondly, there were the remeasurement rates—'unacceptably high' according to Macleod and Montanet. The proportion of remeasurements, they said, varied 'between an average of 3 measurements for each event in the worst case (heavy liquid chamber) and 1.5 measurements for simple events in hydrogen'. The main causes they identified for these high rates were errors of measurement and the too-hasty rejection of input data by the computer programs.

Connected to the first of these causes, and a factor of general concern to Macleod and Montanet, was the efficiency of the operators. Operator mistakes could account for as much as 60% of measurement errors. To overcome this problem the Alvarez group fitted two of their Franckensteins with $20,000 worth of gadgets to facilitate data recording and introduced a strict disciplinary regime in their factory. Non-physicists whose jobs resembled that of a foreman in industry were put in charge: operators were rewarded for efficiency and fired if they did not come up to scratch. At CERN the approach was different, at least partly because the number of technicians was about half that at the LRL (see Table 20.1)—Macleod and Montanet simply called for scientific personnel to play a more active role in the instruction and supervision of operators.[18]

Apart from the above, Macleod and Montanet identified two other areas, peripheral to the factory and its equipment as such, which called for improvement. The first was the programs, which were becoming increasingly obsolete and inappropriate. Those existing in mid-1962 had been written with rather simple hydrogen bubble-chamber events in mind; as the experiments increased in refinement, the events increased in complexity, and 'the corresponding analysis programs [had to] be revised in all their successive steps'. In addition the 'heavy liquid chamber groups [had] particular problems in event computation' which were difficult to solve within the framework of CERN's hydrogen chamber programs, and which might require the adoption and adaptation of the system developed by Howard White's Berkeley propane chamber group. Predictably this led to a call for more staff. It was pointed out that CERN had 9 programmers including one visitor; White had 10 for his propane group alone, and Alvarez 16 for hydrogen-chamber data analysis early in 1962.[19]

Finally, there was the problem of liaison with the experimental groups, particularly in the provision of programming support: this was 'the weak point in [their] data processing arrangements', according to Macleod and Montanet. Here the contrast with Berkeley was particularly striking. In the Alvarez group the whole activity of getting physics results—from planning an experiment, to taking data with the bubble chamber, to analyzing the photographs—was part of the same organizational unit. At CERN data-handling had a separate organizational pigeon-hole, distinct from the divisions explicitly dedicated to doing physics. Hence the experimentalists were dissuaded from taking up programming, while the computer specialists wrote so-called production programs for geometric and kinematic analysis which could not be used as such to analyze the results from a particular experiment. To get over this problem Macleod and Montanet suggested that each experimental group should nominate a physicist who was 'familar with (or prepared to learn about) data processing', and who would work with a programer from the Iep group, to tailor the production programs to the requirements of the specific experiment. A more drastic solution was found in mid-1963. The measuring factory, along with the physicists in DD(EXP) who were interested in doing experiments, were transferred to the TC division; responsibility for the associated computer programs followed at the beginning of 1964.[20]

There is no doubt then that CERN's measuring factory was far from trouble-free in the early 60s, and that those who were anxious about its operation had reason to be. By October 1964 its output was quoted as being 50,000 bubble-chamber events per year. This was apparently the factory's maximum capacity. In April 1964 Hine remarked that it 'did not seem likely that the measuring speed of the present IEPs [could] be greatly increased' and that during the next three or four years it would 'probably be necessary to add a number of SMP-like devices [...]'—a digitizing projector designed by Alvarez in 1961 which we shall meet later—to work alongside the Ieps.[21]

Whatever the imperfections of the Ieps in our period, the fact remains that we have found only one occasion — the SPC meeting in October 1961 — on which their clients, the experimental physicists, openly questioned their performance. On the contrary, Bernard Gregory who was the chairman of the Track Chamber Committe from 1961 to 1963, repeatedly implied that there was no cause for concern. In October 1961 he refused to accept Amaldi's interpretation of why Berkeley had beaten CERN to the discovery of the ω^0. 'The delay in producing results from the bubble chamber pictures', said Gregory, 'was basically due to the fact that these pictures had been taken later at CERN than at Berkeley'. In April 1963 he again expressed his confidence in the factory: 'only Berkeley' he said, 'had better facilities than CERN for the analysis of bubble chamber pictures, the East Coast laboratories being about similar to CERN'. In short, even if CERN's measuring factory was outperformed by LRL's — as Amaldi and Gregory rightly pointed out — it did what the physicists asked of it.[22]

The crucial factor facilitating the match between supply and demand was the policy of distributing bubble-chamber pictures taken at CERN throughout Europe. Generally speaking it was said that no more than about a third or a quarter of the total would be analyzed in Geneva. In fact, if we exclude the neutrino experiment — which made relatively light demands on the factory because of the very low event rate — CERN had to deal with about one million pictures taken with 'its' bubble chambers from 1960 to 1964, so about 14% of the total number of pictures, and 100,000 events. This was just about the load the factory could carry.[23] Of course one must add that physicists naturally tailored their experiments to the data-handling equipment that was available to them. For example, if they did not have access to a McCormick Spiral Reader they did not do 'many-measure' as opposed to 'easy-measure' experiments. Seen from *this* point of view the measuring factory was never seriously overloaded because no one demanded more of it than they thought it could give.

20.2 A new technology for handling bubble-chamber pictures: the Hough–Powell device (HPD)

By the beginning of the sixties the first generation of advanced measuring devices for bubble-chamber pictures — devices of Franckenstein-Iep type — had reached maturity, the number of groups interested in processing film taken around the big accelerators had begun to increase rapidly, and the 'mass' production of a 'standard' digitizing projector had become commercially attractive in both Europe and the United States. At the same time the development of the next generation of picture scanning and measuring instruments to serve both bubble and spark chamber physicists gathered momentum.

CERN played an important role in the launching of one of these new instruments, the so-called Hough–Powell device (HPD). In this section we shall discuss the adoption and development of this instrument by the laboratory as a tool for analyzing bubble-chamber pictures. It is divided into three main parts. The first discusses the rationale for building instruments more sophisticated than the digitized projectors available at the end of the 50s. The second and third focus more directly on the HPD itself, treating successively the hardware for the device and then the demands it made on the programmers.

20.2.1 THE DAWNING OF THE AGE OF THE MEGAEVENT

The most important single stimulus to the development of new picture-handling systems was the conviction that, by the mid-sixties, devices capable of *measuring* a million events a year would be needed — to be compared to 50–100,000 events annually aimed for in the largest laboratories at the start of the decade. This anticipated order of magnitude increase in demand was primarily due to changes in the mode of usage of the bubble chamber, and to a jump in the ratio of scanned to measured events.

Around 1960/61 there was, to quote Ross Macleod, a 'widely held philosophy that a bubble chamber [was] a device to be run for a few days per year [...]'. This philosophy, he went on to stress, was crumbling with the advent of very big chambers like Alvarez's 72″ detector. Both the physics which could be done with the large chamber, and the huge financial investment in it, demanded that it be used as much as possible. This meant continuous operation for long periods, photographs taken 24 hours a day, and an expansion of the chamber with every beam pulse. Macleod calculated that if CERN ran its 2m chamber continuously for three shifts for one year at 66% efficiency, with 10 pulses per minute, it would produce about a million events annually to be measured.[24]

Several rather different factors worked towards increasing the measure/scan ratio. The imperative to make full use of costly accelerator time meant that physicists had to be prepared to run their chamber parasitically in a beam which was not optimized for it, and so to measure many more pictures to get good statistics. The large interaction lengths in the bigger chambers meant more, and more complex, measurable events per picture. And the rise to prominence of dispersion theory meant a growing interest in measuring single-vertex scattering events of which there could be as many as one or two per photograph.[25]

It was generally recognized that to deal with this situation a three-pronged attack was necessary. Firstly, there was a need to develop McCormick's Spiral Reader — which could deal with the single-vertex events but whose use was otherwise limited. Secondly, there was the need to multiply the number of Franckenstein/Iep-type digitizing projectors, setting up a measuring factory. Cost, and the problem of supervizing a staff of much more than 50 scanning and measuring technicians, limited the capacity of such a factory to about 100,000 measured events per year. Finally there was the need to develop more automated scanning and measuring devices, possible involving some form of pattern recognition, which could reduce human intervention to the minimum. This was the problem: the HPD was one possible technical solution.

20.2.2 THE CHANGING FORTUNES OF THE COLLABORATIVE HPD DEVELOPMENT PROJECT

Around September 1959 Paul Hough, a TV specialist based at the University of Michigan, visited CERN for a year. While there he teamed up with Brian Powell, a physicist from Imperial College, London. Within a few months they had developed the idea and some of the hardware for a new kind of measuring machine which, they said, could easily be transformed into an automatic scanner as well.

The HPD used the absorption of a spot of light as it crossed a bubble image on a film to locate the tracks on a photograph. Its key innovation was the method of coordinate measurement, which Hough described as follows in February 1960:

> A spot of light 10–15μ in diameter is made to travel in the y-direction over the negative. The spot travels for 2 ½ms, there is 2 ½ms of dark [i.e. the spot is stationary], and then the spot retraces its original path. At the same time a precision table is advancing the film in the x-direction at such a rate that successive spot paths are separated by 15μ. The 2000 paths required to cover an x-displacement of 30 mm [as required by 35 mm film] are traced in 10 sec.

The position of the spot and of the table were digitized and the coordinates fed (originally via tape) to a computer. Early versions of the HPD used a perforated spinning disc to generate the spot of light; the transition from a mechanical to an electronically-produced spot only waited on refinements in cathode-ray tube technology.[26]

The HPD as thus described recorded the coordinates of *all* the bubbles in the negative. Some means had to be found of selecting only those of interest to the physicist. Several ways of doing so were tried at CERN in 1960 before the idea of *rough digitizing* was hit upon. This involved manually measuring a few key coordinates of the track of interest at a digitized scanning table. These were fed to a computer which fitted a parabolic zone some 200–400μ wide through the points. These 'roads' served as a 'gate' through which the 'filtered' points passed into the geometric and kinematic stages of the analysis.[27]

Hough was quick to appreciate the importance of the new device, both to bubble-chamber physicists and in more applied areas of research (he pointed out to IBM that it could read a page of a book in ten seconds, could be used to analyze photographs in missile work, and so on). In anticipation of his return home in the autumn, he began to look 'for a way to take advantage in the United States of our development work at CERN'. It occurred to him that a joint venture between CERN and an American laboratory would be one way to speed up the development of the measuring instrument and of mutual benefit to himself and the others. In July he proposed the idea to Ralph Shutt (BNL); in September he raised it with the bubble-chamber groups at Berkeley.[28]

A major conference on instrumentation at Berkeley from 12 to 15 September 1960 provided all those concerned with an opportunity to explore the merits of the idea. Discussions at Brookhaven ten days later confirmed, in the words of Ronald Rau, that the 'the atmosphere towards building HP as a tripartite enterprise [i.e. BNL, CERN, and LRL] [was] better than HP by itself'. A possible framework for technical cooperation was mooted: on the assumption that CERN would manufacture a working prototype of the HPD anyway 'Berkeley and Brookhaven (on their technical levels) expressed the desire that this manufacture should be undertaken *at CERN* not as a purely CERN enterprise, but as a cooperative project, and that the prototype should be built *in triplicate'*. The American laboratories would offer financial and technical support in return. Kowarski was asked to come up with an official proposal from CERN, and it was agreed to hold another technical meeting, if possible, in November.[29]

In a memo drafted for the management on his return to Geneva, Kowarski stressed the advantages to CERN of this arrangement. Explaining why a massive increase in demand for measuring machines was imminent he concluded that, in the short term, the HPD was the best way to transcend the limitations of the Franckenstein-Iep system. As things stood at present, CERN held a leading position in the development of the device: 'no other, even remotely similar gadget [existed] in any other laboratory'. This lead would count for nothing if CERN tried to compete 'against a resolute American effort which [would] be deployed very soon'. The proposed cooperative venture, on the other hand, would safeguard the lead 'for some time ahead', and could also solve some of CERN's financial and technical difficulties connected with the developement of the HPD—in particular she could tap Berkeley and Brookhaven's 'electronic and programming thinking as applied to bubble data', in which they were considerably ahead of Europe. Kowarski suggested that about ten people could be diverted from Iep activities for this purpose.[30]

These proposals were received lukewarmly by the CERN management: it was not yet clear to them 'what kind and what size of [data reduction] effort should be undertaken at CERN, taking into account its budgetary latitude and the foreseeable processing requirements at CERN and in Europe'. In the course of discussions at various levels during October and early November 1960 it was decided

- that CERN should build an HPD: 'it would be wise to maintain a minimum viable effort on the development side of data processing', and the device 'might prove very useful' for analysing spark-chamber data;
- that CERN should *not* build two additional mechanical scanners for Brookhaven and Berkeley: it was feared that the laboratory 'would bear an undue share of the burden in this connection and that the construction of this equipment would seriously strain its already overtaxed man-power and construction facilities';

- that roughly six to eight of the staff allocated to the measuring factory in 1961 should be diverted into development of the HPD: help from 'European laboratories who have not their own IEP', and 'a few visitors from abroad' would be welcome;
- finally, that the 'necessary contact' be maintained with developments in America; 'as you see', wrote Kowarski to Alan Thorndike (BNL), 'nothing as yet like a definite "joint project"—but the door remains open'.[31]

At the same time, across the Atlantic, Berkeley and Brookhaven were having doubts of their own. In mid-October Art Rosenfeld informed Kowarski that the Alvarez group had reluctantly decided not to participate in the construction of an HPD, but to bend all their efforts to the Reaper instead: they had some technical qualms about the instrument, and felt that a working version of the Reaper could be developed more rapidly, and with firmer cost estimates. He confirmed, however, that Howard White's propane-chamber group had decided 'to ask permission from the laboratory management to go ahead with the cooperation'. At Brookhaven it was 'far from clear' what was to be done. Should BNL not let others do the development work, and buy a commercially-produced device later? What of the organizational problems of cooperative international development? Would it not be preferable to link up with MIT, where Irwin Pless, in consultation with Paul Hough, was thinking of putting an electronically-generated spot under the control of a computer (the so-called PEPR, Precision Encoding and Pattern Recognition, project)? ... As for the envisaged technical meeting in November, Thorndike proposed that it be held at BNL on 21 and 22 if a reasonable number of people were interested.[32]

The question of whether to develop the HPD was soon complicated even further. On 8 November Alvarez published an internal memo at Berkeley describing another new kind of instrument for digitizing bubble-chamber photographs. The device, called the Scanning and Measuring Projector (SMP), combined together the two operations referred to in its name. It differed from the Hough–Powell in that they were performed almost simultaneously by a scanning technician, and not by a computer. For the scanning operation, the SMP capitalized on the human's superior ability to pattern recognize; for measuring it was fitted with a new kind of electro-optical device which compensated for the limitations in an operator's ability to measure tracks precisely.[33]

A small informal 'Flying Spot Digitizer Conference' duly took place as proposed. The atmosphere on the eve of the proceedings was strained: both Berkeley groups were present, and Howard White and Wilson Powell had prepared a paper outlining the advantages and disadvantages of the SMP and HPD. Their preference for the latter was clear. The programme was changed to allow a full exposition of the SMP by its inventor, White came out explicitly in favour of the HPD and Brookhaven said nothing. By December the situation had been clarified: Ralph Shutt confirmed BNL's interest in an interlaboratory collaboration, and Berkeley's Director, Edwin McMillan encouraged the propane chamber group to go ahead with the HPD while Alvarez continued with his SMP.[34]

It was understood that the collaboration between the three laboratories would remain at a purely technical level, its organization left primarily in the hands of Powell (CERN), Hough (BNL), and White (LRL). A division of labour reflecting the strengths of each group was agreed upon — hardware at CERN, electronics at Brookhaven, programming at Berkeley. In April/May an interlaboratory team, including representatives from the Rutherford laboratory (UK), gathered in Geneva to test a prototype HPD coupled to an IBM 709. By the end of May the use of the instrument as 'an input device for measurement and numerical storage of photographic data in the memory of an on-line computer' had been demonstrated to the satisfaction of all present. At a meeting of the interested parties on the 16 and 17 May it was recognized that 'future developments [would] have to diverge to a certain extent [...]'. Each laboratory would produce its own working prototype tailored somewhat to local needs, and an exchange of personnel would be encouraged particularly in the area of programming.[35]

Soon after returning home White wrote to Kowarski that people at Berkeley were 'profoundly impressed by the results obtained at CERN in the spring. Despite vigorous opposition from Alvarez — indeed 'the war between Alvarez and McMillan [was] of gigantic proportion' — by mid-summer the laboratory was 'really rolling on Hough-Powell'. Brookhaven also sped ahead, helped on by Berkeley's offer to build the optical and mechanical system for them. Early in September Hough reported that at BNL qualified people had been assigned to every problem, that a 15% overall budget cut had been survived 'without ruin to the development', that the Applied Mathematical division was making important contributions in programming, and so on. At CERN, on the other hand, the HPD just ticked along, 'without having much to suffer from budget difficulties [...], but without any acceleration either'. By the end of September it was clear to Kowarski and Powell that CERN could no longer contribute much to the now all-American collaboration, though 'we sure could use some help'.[36]

This turn of events was only to be expected. CERN actually had rather little to offer its American partners: the mechanical and optical hardware for the measuring tables could easily be built in a workshop like Berkeley's already experienced in the construction of such devices. The European laboratory was further marginalized when Brookhaven, following Berkeley, decided to acquire an IBM 7090 early in 1961, a computer six times faster than CERN's IBM 709. This difference in speed considerably simplified the electronics interfacing the flying-spot device with the one-line computer. It also made for a much more efficient use of the computer by the HPD, which was reflected in the structure of the programs being written under White's leadership to handle the digitized output. Add to this the geographical and cultural proximity of the two laboratories, and it is easy to see why they drew together.

To this it must be added that if the American laboratories pushed ahead so quickly it was because their physicists became increasingly convinced of the merits of the HPD, were prepared to dedicate themselves to it, and were prepared to fight for it, if necessary, against those who opposed them. There was no comparable *growth of conviction* at CERN. The research on the HPD was triggered by the arrival of a visitor, and pushed ahead with what help and support the members of Kowarski's group could give. Once Hough returned home the momentum was kept up by the hopes and demands of the collaborative project, and by the gradual diversion of a few staff intended for the measuring factory. Brian Powell was of course determined to see the device work — but it did not matter to him whether, say, Berkeley or CERN developed it first.[37] In other words neither the physicists, nor its inventor, were prepared to launch a sustained campaign for the HPD in CERN. When the management considered the scheme it was already *there,* an ongoing project, the only existing development effort in the field of data-handling in the laboratory. They did not choose to support it; they simply decided not to cut it back.

The difference in resources dedicated to the development of the HPD in the summer 1961 is a useful indication of the difference in attitude at this time on either side of the Atlantic. At CERN between five and ten staff in Kowarski's division were allocated to the project in 1961, and about $100,000 were allowed for in the 1962 budget for all 'post-Franckenstein' initiatives — HPD, cathode-ray tubes, spark-data processing etc. At Berkeley, by contrast, up to 25 people (including visitors) were attending regular work-in-progress meetings on the HPD in September 1961 and, despite the 15% budget cut at Brookhaven in 1961, the laboratory allocated $170,000 to the development of HPD alone.[38]

20.2.3 'FILTER CAN NEVER WORK PROPERLY'

At the end of 1961 it was generally believed that it was only a matter of time before the HPD would go into production, at least at BNL and LRL — Howard White estimated that it would be ready to do physics there by about mid-May 1962. He was still rather optimistic at a meeting in July 1962 — 'volume measurement of data was expected to begin in the autumn' at Berkeley. A month later Macleod and Montanet 'hoped that the HPD [at CERN] [would] be operating sufficiently well for small scale production to begin at the cadence of about 1 hour per day at the beginning of 1963'. All of these estimates proved to be unrealistic. In January 1963 Lew Kowarski reported that over the past year it had become clear that there had been a 'major miscalculation' at all three laboratories, none of which yet had an HPD ready for daily use in physics. The basic hardware, Kowarski pointed out, was essentially ready; it was the software, the programming, that was causing the holdup.[39]

The main source of the problem lay in the FILTER program, which was Brookhaven's contribution to the joint programming effort being led by White. The task of FILTER was to select these coordinates belonging to the track of interest from the 2000 or so which fell into the zone or 'road' in which the track lay. In practice it was proving exceedingly difficult to devise a program which could reliably distinguish coordinates belonging to the event from those associated with crossing tracks, parallel tracks, background noise, and so on, all of which were also fed to FILTER ('gated') if they fell in the road.[40]

Each of the three laboratories reacted differently to this problem. Berkeley took a pragmatic approach. Despite the limitations of the device they decided to go ahead in July 1963 and use the HPD for the large-scale analysis of pictures from the 72″ chamber, 'even at the cost of allowing occasional invalid points to be output from regions of confusion'. 'This was fine for early 72″ runs in which the frequency of nearly parallel and overlaying tracks was low [...]', explained White. However, about a year later the group switched the HPD to measuring events in their 25″ chamber filled both with hydrogen and deuterium. For this purpose they decided to reverse priorities: to reject the points coming from regions of possible confusion, even if it meant losing some good events. Hine, who visited Berkeley in the summer of 1964, and saw the HPD at work on the 25″ chamber pictures, remarked that although this system was failing to process about 10% of the events, even after one remeasurement, White did 'not seem to worry': rather than trying to delay the automatic system he measured the outstanding recalcitrant events on the Franckensteins. By virtue of this policy White's group had measured 175,000 bubble chamber events with the HPD by August 1965.[41]

CERN, for its part, took 'a much more difficult or at least a more extreme approach' than Berkeley or Brookhaven (to be discussed shortly). Eschewing pragmatic, short-term considerations, a group around Gerry Moorhead in DD decided to rewrite GATE and FILTER completely. They were taking this step, explained Moorhead at a conference at the Collège de France in August 1963, because they felt that 'FILTER [could] never work properly unless it [was] modified so as to converge towards a track following system', i.e. a system in which the previous history of a track was used to predict its subsequent trajectory. As a result in April 1964, to quote Hine, 'HPD at CERN was still on the point of starting to analyse bubble chamber film'. By June 1965 it was in production, and had measured its first 12,000 out of 17,000 antiproton interactions.[42]

Why the difference in approach between CERN and LRL? Differences in personality apart, one crucial reason was that there was no great urgency in Geneva to develop a new device for measuring bubble-chamber pictures. As we have seen the physicists felt that Ieps were adequate for their purposes. This in turn gave the computer specialists at CERN that much more opportunity to take a long-term approach to the problem of FILTER, and to indulge their perfectionism to the full. For a pragmatist, and physicist, like White, the aim was to get something that could go into production as soon as possible, and to do useful physics with it. For a computer enthusiast like Moorhead, the point was to get something that worked 'properly', even if it took a long time to do it.

This divergence between two professional 'rationalities' is nicely illustrated by the case of Brookhaven. Here too 'the HPD hardware and most of the software [had] been developed by members of the BNL Bubble Chamber Group'. Confronted by the track failures of FILTER, this group adopted a pragmatic approach. A cathode-ray tube was installed to monitor visually the performance of FILTER as it travelled down the road selecting event coordinates. This helped the operator 'to force good filter output', and it helped the programers debug the system. By the summer of 1964 BNL had begun production on this basis. Up to this point the priorities of the physicists had dominated. The computer specialists were not happy, however. Even as these performance criteria were being met, Hough told Hine that he thought it was 'perhaps a mistake to have concentrated on physics production so soon, before the accuracy and consistency had been properly established'. In January 1965, to quote Hough 'one accepted defeat at Brookhaven in efforts to make the first FILTER program do the job'. Hough felt that he had been rushed; that the rejection rate, as good as it was, was not good enough. His perfectionism demanded that a fresh attack be made on the problem. By August 1965 a new program was 'mostly coded' and 'about half debugged'.[43]

By virtue of the circumstances we have just described the HPD did not go into production on bubble-chamber pictures at CERN until 1965—five years after Paul Hough had arrived at the laboratory and teamed up with Brian Powell. Concretely this meant that far and away the bulk of the pictures analysed at the laboratory were processed by the Iep measuring factory.

20.3 Spark chamber data processing

20.3.1 1961: KEEPING KOWARSKI IN CHECK

A spark chamber is a detector which exploits the fact that, when an ionizing particle crosses the gas-filled gap between two electrodes, one of which is at a substantially higher potential than the other, light (and sound) are emitted. The spark was typically about 1 mm wide, and in early versions of the device its location with respect to the true track of the ionizing particle was estimated to be about ±0.5 mm. The instrument was first used at an accelerator by Bruce Cork (Berkeley) and James Cronin (Princeton) in 1960. It was introduced to a wider public via a symposium held at Argonne on 7 February 1961 under the chairmanship of Arthur Roberts, by which time its use in high-energy physics was growing at an explosive rate.[44]

For the enthusiasts, one of the most attractive features of spark chambers—as opposed to bubble chambers—was that they were 'extremely easy to build, [were] inexpensive, and there [seemed] to be no difficulty in making them go'. This meant—and the reader will make the necessary allowances for the speakers' excitement here—that you could 'build a device for a particular experiment and give it away, store it in the basement, or forget it and build another one for the next experiment'. As a result there was a 'great incentive to build and rebuild detectors of all sorts of shapes and sizes'.[45]

From the point of view of data-handling the spark chamber also differed in some important ways from the bubble chamber. For one thing, it was capable of an even greater output of photographs. The device had a very fast recovery time—of the order of 5-10msec—so that used with the 100msec pulse produced every three seconds by the CERN PS (in 1961) one could take 10–20,000 photographs *per hour.*[46]

On the other hand, it was also true that the useful information in spark chamber photographs was 'far less dilute' (to use Roberts' happy phrase) than in bubble-chamber pictures. They contained almost no background. Spark positions were partially digitized since they traversed clearly-defined gaps. Desired events were often 'readily and obviously distinguishable' from undesired ones. And their relative abundance could be controlled by using the spark chamber as a 'seeing counter' recombining it with triggers set to discriminate against unwanted events.[47]

Prima facie the huge output of spark chambers made them strong candidates for automatic data-handling systems. The low information content of the pictures suggested that such systems would be technically far simpler than those needed for bubble chambers. However, by the same token, it was clear that if one was concerned with, say, rare events, manual systems would be quite adequate. To give Roberts' example: 100,000 spark-chamber photographs had been scanned and 4,000 measured at Berkeley at the rate of 400 per hour in six man-weeks by untrained technicians—'divinity students in fact'.[48]

At CERN serious thinking about the development of a data-handling system for spark chambers got under way in some circles early in 1961. It was stimulated by several factors: discussions about the design of the CERN neutrino spark chamber which continued throughout the year, a visit from Arthur Roberts from Argonne, and an extended visit by Herbert Gelernter, a physicist from the IBM Research Centre. Roberts planned to bring a team to CERN early in 1962 to do an experiment on the PS with a spark chamber in a magnetic field. Gelernter was developing a prototype of his 'vidicon'—a system which dispensed with film, and used a television-camera tube to record a spark-chamber event and store it as digitized data on magnetic tape.[49]

Around end April/early May 1961, Roberts had a couple of meetings with CERN staff interested in the 'experimental potentialities of spark chambers as compared with other instruments and detectors [...]'. On 8 May he wrote a widely-quoted, informal 'Note on the application of automatic scanning and data reduction systems to spark chamber experiments'. One of the main aims of this note was to point out the limitations of manual systems for analyzing spark-chamber data. In particular he identified certain experiments which would be 'difficult or impossible' to perform unless one had an automatic data-analysis system at hand. He identified two cases: those in which the required event was not easily distinguished from similar events, either visually or using triggers, and those in which the number of desired events was very large. He also alluded to the usefulness of two techniques for automatic analysis: Gelernter's vidicon and, at a more sophisticated level, the use of an on-line computer which digitized data directly and which kept up in real time with the experiment. The advantage of this approach, Roberts stressed, was that one could combine the 'procedure and philosophy' of a counter experiment with the advantages of a visual detector: parameters could be continually modified in the light of the results, 'so that again the experimenter [was] in command of the experiment rather than vice versa'.[50]

On 26 June 1961 Lew Kowarski produced a comprehensive paper for the SPC outlining the 'progress and prospects' for new methods of handling visual data. The section dealing with sparks was heavily influenced by Roberts' arguments for automation. Kowarksi adumbrated three such systems for spark-chamber data processing: Irwin Pless's PEPR, a computer-controlled, electronically-generated flying spot, which we have met before, a pattern-recognition project which Jerry Russell, an electronics specialist from Berkeley hoped to start up at CERN, and Herbert Gelernter's vidicon. Having described these initiatives, Kowarski went on to ask that DD be allocated the resources to develop them. The division needed at least ten more people 'mostly electronic and programming staff, whose work could be made relevant to all of the lines suggested above [...]' in 1961/62.[51]

It will come as no surprise to learn that Kowarski had considerable difficulty in interesting the physicists at CERN in his new venture. Even a more modest project which he put to a group of experimentalists from the NP division on 15 November was received with scepticism.[52] The reason was simple: the physicists saw no need for the kind of equipment Kowarski had in mind. The event rates of the two major experiments envisaged at the end of 1961 — the neutrino experiment and the Roberts experiment — were low, and manual techniques were adequate for dealing with their output. What is more any unexpected surge in demand could be met with automated equipment already developed for bubble-chamber picture analysis: Goldschmidt pointed out at the meeting on 15 November that DD could make an Iep and one or two scanning tables available to spark-chamber groups. As for the future, there was always the possibility of adapting the HPD to sparks.

Taking our analysis a little further, we can see that Kowarski and the physicists had different priorities. Kowarski's main preoccupation was to ensure that CERN kept up with leading developments in the field of data-handling equipment. To this end he constantly surveyed the state of the art, identified general trends, and suggested particular approaches or techniques which, he felt, might be of interest for CERN. For their part, the physicists' main preoccupation was to do physics. For a man like Fidecaro, there was no point in CERN devoting resources to a data-handling device which would only be useful three or four years hence when there were not enough bending magnets available here and now to do experiments around the proton synchrotron. To Kowarski, such people were 'on the whole, interested in the kind of physics they [knew]', and unwilling to think beyond its confines. To physicists, like Goldschmidt, Kowarski was a man of vision but a man who was interested in data-analysis equipment for its own sake.[53]

This determination to keep doing physics weighed particularly heavily on the minds of physicists in the STS/DD division, and was one of the main reasons why someone like Goldschmidt was transferred to TC division in mid-1963. He had made the point to Kowarski as early as 1959, even as the first Iep reached a degree of development which made it a useful experimental tool. The activities of Iep physicists, he insisted, should 'usually not be confined to technical development'; their career as *physicists* demanded that they have 'some activities in experimental physics of a kind more directly connected to nuclear physics'. It only remains to add that it was *visitors*—people like Hough (Ann Arbor) Gelernter (IBM) and Russell (LRL)—people who were not active experimental physicists, who took the initiative in developing new techniques at CERN.[54]

20.3.2 1962/65: DEVELOPING NEW DEVICES FOR SPARKS

Despite the opposition, by the end of 1961 Kowarski had received tangible, if modest, support from the Directorate and from Gilberto Bernardini for a new instrument for measuring spark photographs. In the light thereof he drew up the specifications for a device in a memorandum which he circulated widely on 10 April 1962.[55]

Kowarski began by stressing that as things stood CERN had nothing better than Ieps to measure spark pictures, and rehearsed the arguments we now know for developing an automatic system. 'Automation means digitization by means of a flying spot plus programming', he went on; for the present he would concentrate on the electronic and optical aspects and simply 'bear in mind Robert's gloomy request of "at least one programmer" per physicist working with a fast-producing spark chamber'.

The new device should satisfy several criteria, Kowarski pointed out. While representing 'substantial progress' over the Ieps, it should not be so ambitious as to take 'several years of preparatory studies' and should be flexible enough to 'suit as many spark chamber experimentalists as possible, in all their diversity of chamber sizes, shapes, etc'. To meet these conditions Kowarski proposed that CERN construct what amounted to a faster, but less accurate HPD than that being developed for bubbles. The spot was to be created by a commercially-produced cathode-ray tube (CRT). A small team of three duly set to work on building such a device. Nicknamed Luciole, it was initially conceived of as a prototype developed for 'qualitative rather than quantative reasons (flexibility, compatibility with developments achieved elsewhere, new methods of storage and computation, etc.)'.[56]

Kowarski's note was received with a 'general though not intense feeling of disagreement' by Mervyn Hine. His main objection was that Kowarski's proposal was too modest. It appeared 'to be really only a CRT version of the existing HPD', and would never outperform it in just those respects which were needed on a spark-chamber system. As an alternative Hine proposed that in the short term the existing HPDs be used 'as a stop gap, and also as a test bed especially for programming techniques'. At the same time a working party should be set up 'to review evidence and make a proposal' for the development of a pattern-recognition system to be ready 'within the next three years'. In his draft four year programme presented to the Council in June Hine identified this new system as 'the most urgent item in the data analysis development programme'.[57]

Hine's attitude calls for some comment. After all, throughout 1961 he had been among those in the Directorate who were unenthusiastic about new ventures in the field of spark-chamber data handling. Now we find him criticizing Kowarski for not going far enough—even as the latter was pruning back his ambitions to bring them in line with physicist's needs. Hine's main arguments in his memo written in reply to Kowarski's proposal were two.

Firstly, he felt there was likely to be an enormous growth in the use of the spark chamber—its simplicity of construction (particularly the type with visual output), and its fast rate of data accumulation made it very attractive to physicists. 'To take advantage of this potential for development', wrote Hine, CERN had to 'foresee an equally deep and fast improvement on the analysis side'.

Secondly, Hine argued that to cope with the output of pictures from spark chambers, automatic scanning (i.e. involving pattern recognition) was needed. He specifically rejected two other ways of isolating an event of interest: the use of 'roads' to pick it out from the unwanted information on the photographs, as was done for the HPD, or the selective triggering of the detector. 'It looks to me wrong in principle', wrote Hine, 'to make the logic distinguishing the unwanted event work in real time on the experimental floor'. This approach, he went on, was typical in classical counter experiments, and was 'very wasteful of machine time, which [was] more expensive than experimenters' time'.[58]

These two arguments go some way towards explaining Hine's behaviour, but they do not tell us why he chose to use them early in 1962, and not sometime in 1961. We suspect that they emerged *when they did* because of the particular task that Hine was concerned with at the time. As member of Directorate responsible for forward planning, Hine was called upon to do a considerable amount of work during the first half of 1962 for the Bannier working party which was formulating a procedure for controlling CERN's budget over a four-year period. One of his major conclusions (and one strongly endorsed by the SPC), was that to date technical development — and particularly the 'development of radically new and advanced apparatus or techniques' — had been poorly supported in CERN. 'The build-up of this work in the future', wrote Hine, 'should go, if possible, twice as fast in the next few years as that of the rest of the programme'. It was in this context, a context in which strategic concerns were at least as important as scientific ones, that Hine stated that CERN '[could] not afford to ignore' a pattern recognition system for spark pictures.[59]

To return to our narrative. Hine's proposal that a working party be set up was duly acted on, though its terms of reference did not restrict it to looking at pattern recognition systems. Its first meeting took place on 17 October 1962; a second was held on 5 November, and at least one more followed that. The working party stimulated the production of a large number and variety of internal reports documenting the present status and future needs of CERN's data-handling capacity for sparks. Four points of particular interest emerged from these exchanges:

- that the existing hardware (Ieps and HPD) was adequate for the immediate needs. A group led by D.O. Caldwell who had just taken over half-a-million photographs reported on 4 January 1963 that the HPD was 'essentially ready' to treat their spark chamber pictures. The form of their desired events was well determined by specific counter triggers and the physics, and the resulting 'simplicity of the track and event recognition in [their] pictures' made 'the remaining programming for HPD relatively easy [...]';
- that Luciole should be developed with a view to taking over from the HPD during the first half of 1964. The HPD was 'a relatively too powerful device' for sparks and although it could be fruitfully used in the short term, it would be wise to replace it later with Luciole, 'allowing a more rapid processing of spark chamber pictures';
- that there was considerable interest in starting work immediately on a 'programmed spot' device. This differed from Luciole in that it involved a step towards pattern recognition. The electronically generated flying spot was placed under computer control, and the computer was programmed to scan only those portions of interest to the physicist on the film. By the end of 1962 a very successful version had been developed by Martin Deutsch at M.I.T. and both the Caldwell group and I. Pizer suggested that CERN do the same, the latter calling for 'vigorous action in this direction';
- that the development of the electronics and other hardware for any of these instruments for the automatic analysis of spark-chamber pictures was not likely to be a bottleneck: the HPD's was essentially ready, Luciole's was not unduly complicated, and all the hardware for Deutsch's system could be bought off the shelf. That granted it emerged that 'the main hold-up in the analysis' of pictures

by 1965 [might] well be caused by the programming and the computing'. A faster computer than the IBM 7090, which was to replace CERN's IBM 709 in September 1963, was essential; the training of people to write programs was imperative. These conclusions held whether or not the emphasis were to shift to filmless techniques—an experiment using a sonic chamber in which the sound made by the spark was used to record its position directly was running at CERN at the end of 1962—or whether the popularity of using small computers on-line with experiments should grow—they would have to be connected either directly or indirectly (via magnetic tape) to a big 'main frame' computer anyway.[60]

This last concern, the concern to expand rapidly CERN's computing power, was to dominate thinking on data handling at the laboratory during 1963. A European Committee on its future computing needs was set up following discussions in the Finance Committee in February. Its task was to advise on the next computer CERN should acquire. In October it recommended that CERN purchase a CDC 6600 equipped with a time-sharing facility so that it could be used for on-line experimentation. At the same time work on the HPD and Luciole went ahead, though not quite as rapidly as had been hoped at the end of 1962. By October 1963 programs for using the bubble-chamber HPD to measure 200,000 pictures taken in a spark-chamber experiment were 'nearing completion'. They had been 'completely and automatically scanned, measured and analysed at the rate of 1000 per hour' six months later. Luciole ran its first test in April 1964, and by April 1965 had 'processed some 100,000 events successfully with a maximum speed of over 2,000 events per hour'. What of the programmed spot? This project was shelved, despite Hine's interest in pattern recognition devices, and despite the recommendations made by the Caldwell group and Pizer that it be developed, a recommendation reaffirmed in April 1963 by an NP working party.[61]

The demise of the programmed spot in CERN was more or less inevitable granted the kind of device it was. As developed by Deutsch it differed from an instrument like the HPD or Luciole in that whereas the latter recorded *all* the information on the film for subsequent processing by the computer, the programmed spot was *selective*. 'The "intelligence" of the computer', as Kowarski put it, 'was applied to the film itself, and not, as in HPD, vidicon, and other systems, to a set of figures stored in a core or tape memory'. This selection was made in the light of physics considerations, which thus impinged directly on the functioning of the hardware itself. Indeed its development called for the constant interaction between the physicist and the instrument during the scanning and measuring process—so much so that Kowarski jokingly described it as having 'Deutsch-on-line' with its small computer.[62]

It follows that the programmed spot was not a device that could be designed and built by engineers, technicians, and computer specialists and 'handed over' to its users. The close involvement of physicists willing to devote a considerable effort to instrumentation was necessary at each stage of its development. However, as we have seen no CERN physicist was prepared to give much attention to the development of data-handling techniques in the early sixties: the 'sacrifice' was too great. Hence no programmed spot.

One final question. Were Luciole and the HPD capable of handling together the output of spark-chamber photographs at CERN in our period? The limited documentation we have suggests that the answer to this question depended on who, or what, one was. In the eyes of those who developed the equipment, it was sufficient. Thus at the end of 1964 Hine suggested that the expected speeds of the HPD and Luciole seemed to be 'enough to keep pace with the CERN output in the next 2–3 years'. This output, he thought, would be limited to about one to three million pictures annually by the cost of film and the change to filmless techniques running on-line with computers. Against this we have the view of (at least) one (visiting) physicist, who attacked Hine for his complacency early in 1965. This user claimed that the digitizing tables used in NP to back up the automatic devices were 'primitive and inadequate even for the experiments done 3 years [before]' and that CERN should replace them with something better—as well as initiating 'a program for programmed-spot digitizing of spark chamber film [...]'.[63]

20.3.3 CONCLUDING REMARKS

To conclude this study we want to shift our analysis to a somewhat different key, and spend a little time discussing the relationship between data-handling in bubble and spark chamber physics as it has emerged from our work. More specifically, we want to evaluate critically some interesting ideas on this put forward by Peter Galison. Galison's major study on this issue has not been published, and we only have several preliminary written fragments to go on. These, however, are sufficient for our rather limited purposes.

Galison claims that in the history of modern physics, particularly in the postwar period, one finds two kinds of experimentalists, each possessing their own 'style of experimentation', and each being members of a more or less homogenous 'experimental culture'. One group—in the *image* tradition—is anchored around *visual* detectors, and its members progress 'naturally' from cloud chambers and emulsions to bubble chambers. The other—immersed in the *logic* tradition—has *electronic* detectors as its focus: its members are to be found working with counters, and then spark chambers and wire chambers. The image group 'have certain scanning and analysis techniques in common'; the logic tradition demands 'familiarity with electronic switching, counting and logic circuits'. Galison argues that these two cultures developed independently of each other until the 1980s, when they 'merged' around a new generation of 'digitized-visual instruments'.[64]

Galison's idea that different groups of people developed and used bubble and spark chambers is certainly borne out by our study. It is confirmed by a quick look at the proceedings of instrumentation conferences like those at CERN and Purdue in 1962 and 1965. These groups certainly had different technical skills, made different demands on the accelerator, and organized their work in different ways, for example. However, his proposal that one can usefully distinguish between them by characterizing one tradition as favouring visual ways of presenting data and the other not is obviously not substantiated. As we have seen, in the first generation of spark chambers developed in the early 60s the output was predominantly recorded on film, and scanned and measured with the equipment already in use for bubbles. Nor should it be thought that this was merely some temporary expedient resorted to by the members of a 'non-visual' tradition as a stop gap while wholly electronic techniques were being developed. At least in the mid-60s, when filmless chambers were beginning to make an impact, some spark physicists were convinced that there would always be a place for photographic methods. They were particularly suited to the study of complex processes. They allowed the separate analysis by physicists or scanners of the raw spark information. And they were simpler to use: 'the complexity of simultaneously operating electronics which [were] involved in an experimental run with the accelerator' was reduced.[65]

This then is the first point we want to make: visual techniques were just as much a part of the experimental practice of spark- as of bubble-chamber physicists. And we can go further: visual techniques in fact had an important role in *the whole 'electronic' tradition* identified by Galison. In the late 50s counter physicists at CERN regularly took pictures (which could take up 'a few kilometres' of film) of oscilloscope displays of the events they were analyzing, and scanned (at rates like 10,000 pictures/week) and measured them (using the Ieps). In the mid-60s physicists using filmless spark chambers had constant recourse to visual representation. Almost every one of them, said Hine in 1964, 'had some form of display of his data, a quick display in a form which [looked] like a pattern of sparks in a chamber, perhaps transformed a little, but a display which [enabled] them to use their visual imagination to detect whether the system [was] working properly and whether the events [were] the kind of events they [were] expecting or no'.[66]

Visual methods of recording data were thus a permanent feature of both the traditions that Galison has identified, and it is meaningless to try to differentiate them around a rigid image/logic axis. It follows that any suggestion that these two traditions 'merged' in the 80s 'when electronic detectors became capable of producing [well-resolved] computer constructed images' is equally flawed. Images related to the processes occurring in electronic detectors were common to this 'experimental culture' from its inception. The use of computers to build such images did not entail the fusion of two hitherto distinct traditions of handling data. It was simply a new and sophisticated technique developed by physicists who were constantly searching for ways of transforming information into a visual form.

Appendix 20.1

List of conferences

Major sessions on data-handling were held at the following international conferences:

LRL, 12–14/9/60, *International Conference on Instrumentation for High-Energy Physics, Proceedings* (New York: Interscience, 1961)

CERN, 16–18/7/62, *Instrumentation for High-Energy Physics, Proceedings, Nuclear Instruments and Methods,* **20** (1963), 1–520.

Purdue, 12–14/5/65, *Instrumentation for High-Energy Physics, IEEE Transactions on Nuclear Science,* NS–**12** (1965), Number 4.

In addition to this there were a number of more informal meetings restricted to specialists interested in specific aspects of data analysis. The most important of these for our purposes were:

CERN, 15–16/9/58, *International Meeting on Instruments for the Evaluation of Photographs, Summary of Proceedings,* (Geneva: CERN 58-24, 1958).

Brookhaven, 21–22/11/60, *Flying Spot Digitizer Conference.* We have only the Agenda for this with Kowarski's annotations and the paper presented by Wilfred Powell and Howard White comparing the HPD with the SMP (LK22482).

Berkeley, 29–30/1/62, *Hough–Powell conference.* We have only the list of participants (numbering over 50), in (LK 22384).

CERN, 19/7/62, *Informal Meeting on Track Data Processing* (Geneva: CERN 62-37, 1962).

Collège de France, Paris 21–23/8/63, *Programming for HPD and other Flying Spot Devices* (Geneva: CERN 63-34, 1963).

CERN, 3–6/3/64, *Informal Meeting on Film-Less Spark Chamber Techniques and Associated Computer Use, Proceedings* (Geneva: CERN 64-30, 1964).

Bologna, 7–9/10/64, *Programming for Flying Spot Devices* (Geneva: CERN 65-11, 1965).

Notes

1. Apart from conference proceedings and the official minutes of CERN committees, the most important source of material for this chapter was four boxes of documents in the Kowarski papers in the CERN archives, numbered (LK 22482) (LK 22483), and (LK 22484), for the Iep and the HPD, and (LK 22487) for spark-chamber data processing.

   For some helpful review articles on the field written at a variety of levels see Goldschmidt (1958), (1959), and (1964), Kowarski (1960), MacLeod (1962), Rosenfeld and Humphrey (1963), and Soop (1967).

   There is not much secondary literature dealing with the data-handling process in our period—see however, Galison (1985a) and Swatez (1970) for the Alvarez group's approach in particular.
2. For the information in this paragraph see Bradner & Glaser (1958)—the quotation is at p. 421—and, for example, Bradner (1960a) and Rosenfeld (1959).
3. Bradner (1960a).
4. For a list of the sources of error in the first generation of measuring systems see Bradner (1960b).
5. For Goldschmidt's trip report see Goldschmidt (1957). The outline of CERN's initial Iep programme is in Goldschmidt et al. (1957) and von Dardel et al. (1957).
6. The actual evolution of CERN's Iep building programme in the late fifties is described in documents CERN/SPC/54, 25/3/58, CERN/SPC/55, 14/2/58, CERN/SPC/78, 17/11/58, and CERN/SPC/90, 31/3/59. See also *Memorandum*, Goldschmidt-Clermont to Peyrou and Bassi, 30/4/59 (LK 22483).
7. The device is described in Macleod (1960).
8. CERN's policy vis-à-vis Europe as a whole is outlined in discussions held at the eighth and eleventh meetings of the SPC—CERN/SPC/63 Draft, 2/4/58, and CERN/SPC/84, 2/12/58. For the conference see Iep Meeting (1958).
9. For further information on the Berkeley equipment see, for example, Bradner & Solmitz (1958) for the Franckenstein, and McCormick & Innes (1960) for the Spiral Reader.
10. The proceedings of the smaller meeting were published as Iep Conf. (1959). For a general idea of the kind of measuring instrument which would be of interest to European laboratories see the *Note on a Proposed Collaboration on Picture-Analyzing Devices [...]*, prepared by the STS Division and dated 16/1/59. The specifications for the standardized instrument for evaluating photographs were circulated on 26/2/59, and the *Final Report of Standardization Committee* recommending the design proposed by SOM is dated 10/12/59—these three documents are all on (LK 22483). A comprehensive survey of equipment available for handling pictures in Europe is given in *Summary of the Answers to the Questionnaire on Bubble Chamber Picture Analysis Facilities in Europe*, 9/6/60 (LK 22483), and in Goldschmidt (1960). For the European situation in mid-1962, see paper CERN/TC/COM 62/5/Rev, alternately DD/IEP/61/45, 20/2/62 (LK 22483).
11. Kowarski's paper proposing that CERN purchase a computer was CERN/SPC/13, 11/11/55. Unsure of whether such a move was wise, the SPC appointed a sub-committee to look into the matter—see minutes of its third meeting, CERN/SPC/14, 7/12/55. The sub-committee met on 22/11/55. For their report see Annex of letter, Richemond to Finance Committee, 8/12/55 (SC 22840).
12. Difficulties with the Input/Output system are described by Kowarski in CERN/SPC/92, 8/5/59, and in CERN Annual Report, 1958, 61.
13. The impetus towards obtaining an IBM was provided by Rosenfeld (1959). For the discussion in CERN see CERN/SPC/99, 25/9/59, and CERN/SPC/101, 19/11/59.
14. See memo Macleod and Montanet to Goldschmidt-Clermont, *DD (Exp) Estimates for 1963*, 24/8/62 (LK 22483).
15. The minutes of the meeting are CERN/SPC/149/Draft, 27/11/61.

16. The sambatron was described by Goldschmidt-Clermont in paper 2.3 at the meeting held at CERN in September 1958—see note 8. For comments on the performance of the Ieps, see the memorandum from Durupthy and Mascas to Goldschmidt-Clermont, *IEP Reliability*, 28/7/60, Macleod and Montanet (note 14), from which the last quotation is taken, and a note by R. Budde and D. Wiskott entitled *Possible Improvements in the Iep Factory to Increase the Measuring Output*, 5/4/63, from which the other two quotations were taken. All documents are on (LK 22483).
17. cf. note 14.
18. The figure for operator errors is from Bradner (1960b), Table 1. The improvements to the Franckensteins (from the operators point of view) are described by Rosenfeld (1962). Swatez (1970) describes the organization of the Alvarez-group factory in some detail. See also Galison (1985), 347.
19. The information in this paragraph is from Macleod and Montanet, *op. cit.* note 14, and the memo from Goldschmidt-Clermont to the Director-General, *Programming Staff for Track Chamber Data Reduction*, ref. DD/EXP/YGC/DA, 26/2/62 (DIRADM 20330). Macleod (1962) includes a survey of the computer programs available at the time at CERN.
20. The extent of the problem is spelt out in Macleod and Montanet, *op. cit.*, note 14. For a description of the organization of the Alvarez group see Galison (1985) and Swatez (1970). See also letter Macleod to Kowarski, 16/6/60 (LK 22482).
21. The performance in October 1964 is mentioned by Hine in his memorandum to the members of the Data Handling Policy Group, *Notes and Suggestions for CERN Data Handling and Computing Policy 1965–1967*, (DHPG 12), 6/10/64 (DIRADM 20324). He made the remark about the limit in capacity in CERN/SPC/182/Add., 20/4/64, to correct an impression given in a widely-circulated memo DIR/AP/136, 2/4/64 (MGNH 22062) that with 'minor improvements' to the CERN factory its output could be pushed from 50,000 to 100,000 events per year.
22. Gregory made his remarks at the 21st and 26th meetings of the SPC—minutes CERN/SPC/149/Draft, 27/11/61 and CERN/SPC/170, 16/5/63, respectively.
23. Comprehensive statistics on the output of the bubble chambers used at CERN in our period are provided in CERN/SPC/213, 28/11/65. Detail on the distribution of pictures by country is also in CERN/SPC/194, 10/2/65.
24. The figures in this paragraph are from a memo by Macleod, *Notes on Iepping Prospects 1961–63*, 19/9/60 (LK 22482).
25. Some of the arguments are spelt out by Kowarski in a note to Weisskopf, *Some Thoughts on "Data Processing and the Optimum Use of an Accelerator"*, 14/10/61 (LK 22482). For the importance of dispersion theory see Bradner (1960). The need to make many small-angle scattering measurements was stressed by Adair at an informal meeting on *Information Retrieval Problems in High Energy Physics* held in Chicago from 22–23/4/60. We have Macleod's summary of the papers given—see (LK 22482).
26. The classic papers on the HPD are Hough and Powell (1960a, b). The quotation in this paragraph is taken from letter Hough to Shutt, 16/2/60 (LK 22482).
27. Roads were first established by coating the interesting parts of the film with red dye (letter Hough to Shutt, 16/2/60 (LK 22482)). Later it was decided to draw the roads on a transparent strip beside the picture but rigidly attached to it, and to measure their coordinates with an additional flying spot (letters Hough to Shutt, 4/60, and to Gosselin, 12/5/60, both on (LK 22482)). In August the idea was to mask out the unwanted areas of the bubble-chamber image by placing on top of it another film on which the roads appeared as transparent zones in an opaque field—Hough & Powell (1960b). Rough digitizing is described in Hough & Powell (1960a), a paper given in mid-September 1960.
28. See letters Hough to Shutt, 16/2/60 (from which the quotation is taken), 4/60 and 18/7/60, letter Hough to Gosselin (IBM), 12/5/60, and the proposal to develop *709 Matched IEP-Y* from Hough to Kowarski and others at CERN, and Rosenfeld and others at Berkeley, 12/9/60—all on (LK 22482).
29. The conference was Instrumentation Conf. (1960). Kowarski reported on the meeting at Brookhaven in his letter to Hough, 23/9/60 (LK 22482); his private notes of the meeting, which include the remark attributed to Rau are on the same file. The framework for the cooperative project was spelt out in two undated

memos written by Kowarski soon afterwards and entitled *New Developments in Data Processing for Track Chambers* and *Note on the Proposed Collaboration CERN-U.S.A. on Data Processing* from which the quotation is taken—both on (LK 22482).

30. This paragraph is based on the memos quoted in note 29. The request for ten staff is made in a memo Kowarski to Weisskopf, 19/10/60 (LK 22482).
31. For discussions at the level of the CERN Directorate see the records of the decisions taken at its meetings on 10/10/60 (CERN/DIR/2, 14/10/60), 17/10/60 (CERN/DIR/3, 20/10/60), 24/10/60 (CERN/DIR/4, 27/10/60), and 2/11/60 (CERN/DIR/6, 5/11/60) all in (DG 20862). For the discussions in the SPC see the minutes of its meeting held on 4–5/11/60 (CERN/SPC/123/Draft, 17/2/61), and Kowarski's paper prepared for it, *Techniques of Data Handling for Track Chambers* (CERN/SPC/120, 26/10/60). Quotations in this paragraph are from these documents and from letter Kowarski to Thorndike, 28/10/60 (LK 22482).
32. Letters Rosenfeld to Kowarski, 11/10/60 (LK 22482), and 19/10/60 (LK 22484), and Thorndike to Kowarski, 25/10/60 (LK 22482).
33. We do not have a copy of Alvarez's original memo. For descriptions of the SMP see, for example, Galison (1985a), 343, Humphrey & Ross (1964), and Snyder et al. (1964).
34. The agenda of the meeting, adjusted at the last minute to accomodate Alvarez's description of the SMP, as well as (Wilson) Powell and White's paper comparing the HPD and the SMP are on (LK 22482). For events at the meeting and subsequently, see letters Powell to Goldschmidt-Clermont, 30/11/60, and to Kowarski, 1/12/60, and letter Hough to Goldschmidt-Clermont, 10/12/60—all on (LK 22482).
35. For a survey of these developments see Kowarski's oral report to the SPC, CERN/SPC/133/Draft, 7/7/61, and his paper for it, *New Methods for Handling Visual Data: Progress and Prospects*, CERN/SPC/132, 26/6/61. The official report of the meeting held in mid-May is Benot et al. (1961).
36. The information is derived, successively from the following sources: letter White to Kowarski, 16/6/61, letter Russell to Kowarski, 27/8/61, letter Russell to Gelernter, 27/6/61, engineering note on *Flying Spot Digitizer Development Status* by Russell, 12/7/61, letter Hough to Kowarski, 5/9/61 (LK 22482), letter Kowarski to Russell, 22/9/61, and letter Kowarski to Hough 4/10/61 (LK 22482). Unless otherwise stated all on (LK 22484).
37. Letter Powell to Kowarski, 9/12/60 (LK 22484).
38. For CERN, see letter Kowarski to Hough, 4/11/60 (LK 22482). For LRL, see typically the informal minutes of *FSD Meeting No. 2*, 19/9/61 (LK 22484), chaired by White and attended by 19 people. For BNL, see letter Hough to Kowarski, 5/9/61 (LK 22482).
39. Letter White to Kowarski, 14/11/61 (LK 22484), White et al. (1962), memo Macleod and Montanet, *op. cit.*, note 14, and Kowarski's presentation to the ECFA meeting on 17–18/1/63, FA/EC/2, 65–67 (DG 20783). See also Franck et al. (1962).
40. For a general idea of the problems being encountered in programming for the HPD see the papers presented at FSD Conf. (1963) and FSD Conf. (1964).
41. For developments at LRL see letter Russell to Kowarski 26/2/63 (LK 22384), and White (1965), from whom the quotations are taken. Hine's trip report, which was widely circulated, is dated 9/9/64 (DG 20598).
42. Moorhead made this remark in Moorhead & Krischer (1963). The observation at the start of the paragraph was made by Daniel Tycko in discussion—FSD Conf. (1963), 131. For a caustic assessment by Kowarski of the way CERN was tackling its HPD programming problem see his closing remarks to this conference (at p. 238). Hine's comment is in his *CERN Programme 1964–1968: Data Handling and Computing Equipment*, DIR/AP/136, 2/4/64 (MGNH 22062). The production figures are those given by Macleod in his semiannual report for June 1965, CERN/SPC/214, 3/12/65.
43. For the quotations on the situation at Brookhaven see Abrams et al. (1964), and Hough (1963). The state of play in the summer of 1964 and Hough's feelings about it are derived from Hine's trip report—see note 41. For developments in 1965 see Hough & Powell (1965).
44. The symposium, from which much of the information in this paragraph was derived, is Spark Chamber Symp. (1961).

45. The first two remarks were made by Brue Cork at the Spark Chamber Symposium. The last is by Hine in a memo to Kowarski, *Your memo on spark chamber data analysis*, 10/5/62 (LK 22487).
46. The figures are given in Gelernter (1961).
47. See Roberts (1963), 31 for the quotation.
48. Koester (1961), for example, discusses the specific conditions under which the need for automatic scanning may arise, as does Roberts in his *Note on the Application of Automatic Scanning and Data Reduction Systems to Spark Chamber Experiments*, 8/5/61 (LK 22487), from which the quotation is taken.
49. We have the record of one informal meeting held during this period: *Report on the Second Meeting on Spark Chambers with Professor Roberts*, 5/5/61 (LK 22487). For the vidicon see Gelernter (1961), and for the CERN neutrino spark chamber see Faissner et al. (1962).
50. This paragraph is based on the *Report [...]* quoted in note 49 and the *Note [...]* quoted in note 48.
51. The paper, *New Methods for Handling of Visual Data: Progress and Prospects*, is CERN/SPC/132, 26/6/61. For information on PEPR see Pless (1962) and Pless et al. (1964).
52. For the immediate reactions of the senior staff to Kowarski's initial project, see the meeting of the SPC held on 21/7/61—its minutes are CERN/SPC/140/Draft, 27/9/61. His revised project is described in CERN/DIR/87, 8/11/61, and the Directorate's reaction in CERN/DIR/89/Rev., 24/11/61 (DG 20862). The report of the informal meeting with physicists is *Disscussion on Development, Scanning and Measurement Facilities for Spark Chamber Photographs*, 15/11/61 (LK 22487).
53. For Fidecaro's reaction, see *Discussion [...]*, previous note. For Kowarski, see his concluding remarks to FSD Conf. (1963), 240. Goldschmidt expressed the view of his division leader attributed to him in a conversation with the author.
54. See Goldschmidt's memo *Suggestions for Iep Collaboration*, 2/3/59 (LK 22483).
55. The memo is L. Kowarski, *Guiding Lines for a Spark Data Processing Project*, DD/DEV/62/6, 10/4/62 (LK 22487).
56. The planning and development of Luciole is described in Anders et al. (1962), (1963). Its place in the CERN data-handling programme is described by Kowarski and Hine in *The Future of Data Handling Activities at CERN*, CERN/SPC/161, 31/8/62.
57. Hine's immediate reaction is in his memo to Kowarski re *Your memo on spark chamber data analysis*, 10/5/62 (MGNH 22001). His claim that a pattern-recognition system was urgent was made in CERN/443, 31/5/62.
58. This argument carried no weight for the physicists, of course. For them, the machine was there to *do physics*. Doing (strange particle) physics with bubble chambers meant taking many more pictures than had useful events; doing physics with spark chambers meant having the opportunity to control the operating conditions in real time, and reducing the ratio of unwanted to wanted events in the output. These were two different ways of using the machine and it was meaningless to suggest that the one wasted machine time and the other did not.
59. For information in this paragraph see Hine's memo *"Grown-Up" CERN*, DIR/AP/102, 6/3/62 (LK 22384), CERN/443, 31/5/62, from which the quotations are taken, and the minutes of the 25th meeting of the SPC, CERN/SPC/165, 1/10/62.
60. The minutes of two of the meetings of the *Working Party on Spark Chamber Data Analysis* are documents DIR/AP/110, 18/10/62, for the first meeting held on 17/10/62, and DIR/AP/112, 12/11/62, for the second held on 5/11/62 (LK 22487).The most important of the reports were D.O. Caldwell et al., *Analysis of Spark Chamber Pictures*, NP/Memo/427, 4/1/63 (LK 22488), and G. Fidecaro et al. *Programming Requirements for Spark Chamber Data Analysis*, DD/EXP/62/34, 23/11/62, D. Harting, *Some Figures Concerning the Data Analysis of a Typical Spark Chamber Experiment*, 31/10/62, L. Kowarski, *Methods of Spark Data Processing in USA*, DD/DEV/62/17, 30/10/62, I. Pizer, *Automatic Spark Chamber Photo Analysis*, 5/12/62, H.I. Pizer et al., *Résumé on Flying Spot Devices for the Evaluation of Spark Chamber Photographs*, DD/DEV/62/18, 21/11/62, A.E. Taylor, *Sonic Spark Chambers*, 31/10/62, all on (LK 22487).

61. For the European Committee see *Report of the European Committee on the Future Computing Needs of CERN*, CERN/516, 25/10/63, and the *Appendices to the Report [...]*, CERN/516 Appendices, 25/10/63.The performance figure for the HPD is in *CERN Programme 1964–1968: Data Handling and Computing Equipment*, DIR/AP/136, 2/4/64 (MGNH 22062), and for Luciole in B. Stumpe, *Summary on Present Performance of the Cathode-Ray Tube [...]*, CERN/DD/DA/65/5, 2/4/65 (MGNH 22002).
62. For Deutsch's descriptions of the programmed spot see, for example, Deutsch (1962), (1965). The quotation from Kowarski is in his *Methods of Spark Data Processing in USA*, DD/DEV/62/17, 30/10/62 (LK 22487).
63. Hine's view that CERN's facilities were adequate for the near future is in his *Planing the Next Generation of Film Measuring Instruments*, DIR/AP/144, 11/2/65 (MGNH 22002). His estimate of the expected output of pictures is in his memo to members of the Data Handling Policy Group, *Notes and Suggestions for [...] Policy 1965–1967*, DHPG/12, 5/10/64 (DIRADM 20324). The criticism was made by L.W. Jones, in a memo to Hine et al., also entitled *Planning the next generation [...]*, 23/2/65 (MGNH 22002). For the kind of measuring table developed for the Argonne group, for example, see de Shong (1962)
64. Galison has spelt out these ideas in Galison (1985a), which deals specifically with the bubble-chamber tradition, and more generally in Galison (1985b), and (1987), 425–427.
65. The advantages of photographing spark-chamber output were stressed by Jones in his memo to Hine cited in note 63.
66. The figures quoted for the photography of oscilloscope traces in counter experiments were given by Fidecaro at the meeting on spark chambers held on 15/11/61 — see *Disscussion on [...]* cited in note 52. Hine's comments are from his concluding remarks to Spark Chamber Meet. (1964), 372.

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