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EARLY CLOUD CHAMBER EXPERIMENTS AT THE PIC-DU-MIDI

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Résumé. - On décrit l'histoire primitive du travail avec les chambres à nuages sur les particules étranges. On s'attache surtout au 'groupe Manchester' qui a défriché le sujet au Pic-du-Midi, France. L'étude se termine avec quelques mots préliminaires sur la valeur des études historiques du développement des sujets principaux de la physique.

Abstract. - The early history of cloud chamber work on strange particles is described. Particular attention is given to the pioneering work of the Manchester group at the Pic-du-Midi in France. The paper concludes with some preliminary remarks on the value of historical studies of the development of main areas of physics.

George Rochester has already described in some detail the Manchester experiments on the properties of penetrating showers of cosmic rays which led to the discovery of V-particles in 1947. Although the apparatus was operated until the summer of 1949, no further decays of V-particles were observed. One decay was seen in 1946 and one in 1947 but none in 1948 or the first half of 1949. With hindsight we now know that the apparatus was not well arranged. For example, the producing layer for the penetrating showers was some distance above the small cloud chamber.

In 1948 Rochester started using photographic emulsions for cosmic-ray experiments at Manchester, whilst I continued to operate the small cloud chamber in the Blackett magnet. Keith Barker joined me and we extended the studies of the general properties of penetrating showers. Interesting events in a small chamber (30 cm in diameter and 9 cm deep) were comparatively rare at sea-level. We urgently needed access to a mountain observatory where the rate of showers would be enhanced by a factor of at least ten. A possible place was the cosmic-ray laboratory on the Jungfraujoch in Switzerland but unfortunately the maximum individual load that could be transported to the actual laboratory was only about 1 tonne. The Blackett magnet could not be broken down into such small pieces so, from the perspective of 1947/1948, it seemed that Manchester would need a specially designed magnet and chamber for mountain work.

Sometime in 1947 it was decided to build a new rectangular chamber (55 cm square and initially 9 cm deep) and a 15-tonne electro-magnet which could be broken down into small pieces. J.A. Newth was in charge of this project and Metropolitan-Vickers Ltd, who made the original Blackett magnet in the mid 1930's undertook to construct the new magnet. Both Blackett and J. G. Wilson advised on the new project, altogether it was an ambitious programme and consequently it took some time to complete. The new magnet was installed at the...
Jungfraujoch during the summer of 1950 but the chamber was not completed until February, 1951. Blackett originally hoped that this new Manchester chamber would have confirmed the 1947 Rochester & Butler discovery. Although the new chamber made important contributions, particularly on the lifetimes of $V^-$ particles, and observed an early example of associated production, it was completed almost two years too late for the role of confirming the $V$-particle discovery.

Rochester has already explained at this conference that we knew in 1949 that similar cloud chamber work to our own was in progress in the USA, both at sea-level and at high altitudes. Carl Anderson at the California Institute of Technology had long been a rival of Blackett's and his group had a small portable magnet and cloud chamber which they were using at a variety of locations, at sea-level, at a mountain station and in a large aeroplane.

Before any firm news about $V$-particles reached us from the California Institute of Technology, G. P. Occhialini suggested to Blackett that it might just be possible to install and operate the old Blackett magnet at the astronomical observatory on the Pic-du-Midi in the French Pyrenees at a height of 2867 m. Occhialini and Cosyns from Brussels knew this observatory well and particularly its Director, J. Rösch. Early in 1949 they set about convincing Blackett that it would be worthwhile to move the magnet to the mountain during the summer of 1949. This was seen in Manchester as an attractive possibility which might anticipate the operation of the new Jungfraujoch chamber by at least a year.

It happened that Rochester's emulsion group was at the end of 1948 following up a suggestion of Blackett and Occhialini to see whether particle tracks in emulsion could be bent by strong magnetic fields, and if so to evaluate the possible usefulness in cosmic-ray research. Some preliminary experiments had been carried out by Rochester and Page in a Blackett-type magnet being built at Metropolitan-Vickers Works in Manchester for Ceylon, and though the results were encouraging an exposure was desperately needed at a mountain laboratory. Undoubtedly Occhialini wanted Blackett to allow his magnet to go to the Pic-du-Midi for this reason, as well as hopefully to forestall the efforts being made in the USA to confirm the discovery of $V$-particles.

Blackett was not easily convinced, no doubt because he had in mind the Newth cloud chamber being tested for the Jungfraujoch and my group was obtaining interesting results on penetrating showers at Manchester. Letters went to and fro between Blackett, Occhialini and Dilworth, and Rösch and in the end Blackett agreed to visit the Pic-du-Midi on 25th June, 1949. Unfortunately other commitments prevented this visit at the last moment, but I went, together with a senior colleague, H. J. J. Braddick. We were very impressed with the potentialities of the Pic-du-Midi laboratory. Rösch soon convinced us that it would indeed be feasible to haul the magnet parts from the end of the mountain road to the laboratory itself before winter set in. We learnt that the existing power supply was inadequate for our purpose but work was in hand to install a new power cable. The difficulty was to gauge when this work would be completed. In June it seemed that a great deal of work remained to be done before the winter.

On the way back to Manchester, Braddick and I visited Leprince Ringuet at the Ecole-Polytechnique in Paris. At that time he was doing cosmic-ray experiments on the Aiguille-du-Midi, near Mont Blanc. This station was very much higher than the Pic-du-Midi but an
old-fashioned magnet like ours could not be taken to the laboratory on the Aiguille. Leprince Ringuet tended to advise us against the Pic-du-Midi because it was not very high and the facilities were poor, particularly the electric power supply. Access to the Pic-du-Midi was difficult in winter— one had to ascend on skis and supplies had to be carried.

In the end, notwithstanding all the difficulties, Braddick and I advised Blackett to sanction the move to the Pic-du-Midi largely because we were convinced that Rösch wanted us. If we set the move in motion we believed that this would enable Rösch to speed up the work on the electric cable and to plan for improved access during the winter. Rösch was a young, ambitious man and the Manchester project would undoubtedly help him.

Fortunately much of the correspondence between Manchester and the Pic-du-Midi during the summer of 1949 has been preserved among Blackett’s papers. On 4th July, 1949 he wrote to Rösch agreeing to the move but leaving the timing open; perhaps we would operate the magnet and cloud chamber at Manchester during the winter of 1949/50 and so delay the move until the summer of 1950. Blackett also felt that the proposed Manchester group at the Pic-du-Midi should be joined by a French physicist. He asked Pierre Auger (then at UNESCO) to find a young French physicist for the new programme at the Pic. He also pressed the authorities in Paris to make sure that all the funds needed for the electric cable were available. We soon realised it would take some time to ship the magnet from Liverpool to Bordeaux so the final decision was taken on 9th August when Blackett wrote to Rösch saying he agreed to dismantle it right away. At that time the news from the Pic on all the preparatory work was good. Subsequently Blackett paid his first visit to the Pic-du-Midi in mid-September when he was joined by Occhialini and Cosyns. The magnet had reached Bagnères-de-Bigorre, the base station for the Pic-du-Midi, by 4th October and in a letter to Occhialini dated 31st October, Blackett announced that the gamble had come off, the magnet was at the top. The haul to the top was not made without incident. At the last moment the French engineers found that the magnet base plate was heavier than they expected!

The magnet was energised by 24th November and so it was possible to use it right away for the emulsion experiments proposed by the Manchester and Brussels’ groups.

Since access to the Pic-du-Midi observatory during the winter was by ski and equipment could not be moved in easily, Blackett wondered if this difficulty could be overcome by using a helicopter. He consulted experts in the aircraft industry in Britain but before the idea had developed very far, Rösch wrote (March, 1950) to say that a goods and passenger téléphérique would be built, hopefully by October, 1951.

During the early days of our work at the Pic a French physicist, J. Daudin was very helpful but he did not actually join the Manchester group. In November, 1949 Auger suggested that a young doctoral student of his, André Cachon, should do so. We were very pleased to have him as a colleague.

Late in November, 1949 Blackett received a remarkable letter from Carl Anderson. The first three paragraphs of this letter asked Blackett to allow a young physicist from Cal. Tech. to work at Manchester, hopefully on a Fulbright scholarship. In the fourth paragraph Anderson mentioned, almost casually, that his group had obtained about 30 cases of the Manchester V-shaped tracks associated
with penetrating showers. He remarked that "so far as we can see now, their (Rochester & Butler's) interpretation of these events as caused by new unstable particles, seems to be borne out in our experiments." Unfortunately the Cal. Tech. group was experiencing difficulties measuring the events in their small chamber but they promised to publish their work - a paper duly appeared in 1950. This was splendid news for us but, sadly, we had failed to confirm our own discovery. It turned out, however, that the tremendous efforts of RBsch and many others to install and energise the Blackett magnet at the Pic, were not in vain.

The chamber and electronic equipment was sent up in the spring of 1950. Unfortunately one of our young colleagues, Cyril Green, died suddenly while climbing the Pic on skis. Despite this sad set-back the work proceeded and the chamber started operating regularly during the summer. Between July, 1950 and March, 1951 about 10,000 photographs of showers were taken. Sixty-three V-shaped tracks were found, 51 of which were due to neutral decays.

During 1950 there was correspondence between Anderson and Blackett about nomenclature. How should the new particles be described? Some people favoured using Greek letters following the general agreement on $\bar{T}$ and $\bar{p}^-$ mesons. In June 1950 Anderson wrote to Blackett asking him to name the new particles but remarking that he was strongly against the use of Greek letters. Blackett replied that he would consult Niels Bohr and, after doing so, he told Anderson that he favoured the V-particle notation which had grown up at Manchester. Anderson replied at once agreeing to Blackett's suggestion.

Undoubtedly the V-events recorded at the Pic-du-Midi during the 1950/51 period were the best available anywhere and their analysis, which was done at Manchester, led to important new discoveries. For the first time protons and negative mesons (probably $\bar{T}$- mesons, since one $\bar{T} \rightarrow \bar{\mu}^-$ decay was seen), were identified among the secondary particles of $V^0$ decays.

Although at this time the evidence was not conclusive, we had circumstantial evidence that the $V^0$ particles decayed into only two secondary particles, both charged. There was strong evidence that the $V^0$ particles were of two kinds, called $V_1^0$ and $V_2^0$. Tentatively we suggested that the decay schemes were:

$$V_1^0 \rightarrow p^+ + \bar{\pi}^-$$
$$V_2^0 \rightarrow \bar{\mu}^+ + \mu^-$$

By the summer of 1951 good measurements were made on 26 $V_1^0$ tracks. It was not possible to classify each event as a $V_1^0$ or a $V_2^0$ straightaway because in many cases, both secondary tracks were at minimum ionisation. The Manchester group were the first to use kinematic methods to provide a separation procedure. In this analysis use was made of the general properties of two-body decays, particularly following the work of Blaton and with the help of Podolanki at Manchester. Using, for the first time, this quite sophisticated analysis procedure, it seemed that the 26 $V^0$ events could be classified as 15 $V_1^0$ and 11 $V_2^0$. The mass of the $V_1^0$ was about 2250 $m_e$, while that of the $V_2^0$ was about 950 $m_e$.

Several convincing examples of well measured $V^0$ decays of both types were observed during 1951 by Thompson et al. By the summer of 1952 200 $V^0$ decays were obtained at the Pic-du-Midi. Although the two simple two-body decay schemes were still not fully established,
the Pic-du-Midi evidence remained consistent with them. To settle the question it was clear that more accurate measurements were needed on the secondary tracks of the decays. This work was largely done later on by other groups with much larger chambers than ours. We endeavoured to improve our technique at the Pic, but the modest pole face size of the old Blackett magnet was the limitation.

The charged V-particles were less frequent than the $V^0$ and were more difficult to analyse. The evidence available early-on strongly suggested that they were the same as the $K$-mesons found in photographic emulsions. Undoubtedly the cloud chambers were most suitable for studying neutral decays thus complementing the emulsion technique which was paramount for charged decays. We did have one outstanding picture taken at the Pic-du-Midi on which a $V^-$ appeared itself to produce a $V^-$ track. This event was called a cascade decay and was the first of its kind to be published. In due time this discovery was confirmed by other groups and the $\Xi^-$ hyperon was established.

The Cal. Tech group were not entirely convinced that the Manchester analysis of $V^0$ events in terms of two body decays was the complete story. They had some indications of decays involving 3 secondaries. Some events, however, showed the $V^0$ track and the origin of the $V^0$ particle in a nuclear interaction. In each of these cases the secondary tracks were coplanar with the appropriate origin, as required for a 2 body decay. Cloud chambers containing many thin metal plates were used by some groups and found to be invaluable for linking $V^0$ tracks with their origins in the plates. Ultimately these experiments and others led to the confirmation of the original Manchester interpretation.

The first major review of all the V-particle work took place appropriately at the 1953 IUPAP Cosmic-Ray Conference held at Bagnères-de-Bigorre, near to the Pic-du-Midi. This was a particularly splendid occasion and the conference report is a valuable historical document because it records the achievements and views of the many different groups of experimenters at a particular instant of time.

It is not my purpose here to review all the early work on the V-particles, both neutral and charged. Undoubtedly the early observations at the Pic-du-Midi by the Manchester group established a major new activity. We continued working at the Pic for nine years. During this time the Ecole-Polytechnique group installed a very large magnet cloud chamber with a second multiplate chamber below. In due time our small magnet chamber was inevitably referred to as Josephine! Towards the end of our stay at the Pic-du-Midi we worked on high-energy showers in collaboration with the Ecole-Polytechnique group. By this time almost all the initiative on a strange particle physics was centred at the accelerator laboratories. The Cosmotron generated a useful beam of protons at the beginning of 1953 and the Bevatron was working at the end of 1954. The diffusion cloud chamber was used very effectively with the Cosmotron by Shutt's team; their first great success was the discovery of associated production of strange particles. The bubble chamber was invented by Glaser in 1952 and Hildebrand successfully operated a very small hydrogen chamber in 1953. By 1954 Alvarez had launched his magnificent programme for building and exploiting hydrogen bubble chambers at the Bevatron. Sadly, cloud chamber work at mountain observatories was doomed by about the mid 1950's but emulsion work with cosmic rays continued throughout the decade.
During the 1950's many scientific papers were published on the properties of the new particles. Conferences proved to be important, particularly for reviews of broad areas of work as well as occasions for the exchange of ideas. My first major international meeting was in July, 1951 at Bohr Institute at Copenhagen. It was a nuclear physics meeting, but Cecil Powell and I reported on the strange particles. I realised then that there was in practice a wide gulf between nuclear and cosmic ray physicists. The synchrocyclotrons were producing pion beams but I don't think that in 1951 nuclear physicists envisaged the great advances that would soon be made with the Cosmotron and the Bevatron. In 1952 I made a lecture tour in the USA when it was a particular pleasure to visit Carl Anderson's group at California Tech. and to see the MIT mountain station at Echo Lake in Colorado.

The excellent practice of publishing major review articles developed in the 1950's. For example, a series of volumes called 'Progress in Cosmic Ray Physics' (North Holland), edited by J. G. Wilson, was introduced in 1952. I wrote a review on both neutral and charged V-particles for the first volume; data available in late 1951 were included. Perhaps the best and most comprehensive review of cloud chamber work, on neutral V-particles was written by R. W. Thompson from the standpoint of early 1955 for the third volume which appeared in 1956. Effectively he was able to cover all the cosmic ray work; thereafter the initiative was entirely taken over by the accelerator physicists. Thompson's group at Indiana University in the USA was outstandingly successful in making accurate measurements of $V^0$ decays using a large magnet and cloud chamber at sea level. They developed and refined the kinematic methods first used by the Manchester group with the Pic-du-Midi data.

Undoubtedly these reviews are of great value to the historian. The achievements at the particular time are usually clearly set out and controversies, if any, should appear as well. I wonder if this is all the history we need as physicists? The reviews inevitably do not draw out very far the roles of personalities; rather, they faithfully summarise the literature and include the latest unpublished results. Thus each aims to be a still photograph of the development of a subject at a particular date. Inevitably some provide a sharp photograph but others, for various reasons, may only reveal a dim and fuzzy picture. There seems to me to be left a very distinct role for the historian of science. Let us look very briefly at some of the more important aspects of this role.

The historian needs to understand the achievements of physics at a particular time, but then he must probe to discover how these came about. He must look at the relative contributions of various physicists to see what motivated them and how they worked, as individuals or as groups. In my view this type of study is worth doing as a work of scholarship. It is difficult because detailed records of scientific experiments are rarely kept for many years and when they are kept, they are rarely indexed and made available to scholars. It is important for the historians to start this work while people who were active participants in the science remain alive.

In thinking about my short contribution to this conference some over-riding conclusions seem evident. The discovery of the strange particles, both mesons and hyperons from 1947 until the late 1950's is a fascinating story which was undoubtedly controlled by a small number of leading physicists. Many of them are here at this conference but sadly some are no longer alive and some are unable to attend the conference. I particularly think of Patrick Blackett,
Cecil Powell, Bernard Gregory, Carl Anderson, Louis Alvarez and Ralph Shutt, all of whom I knew well, but of course there were other outstanding leaders in the 1950's. It is interesting to speculate whether physics today depends to the same extent on the leadership of a relatively small number of physicists - I suspect not.

Another characteristic of the 1950's was the growth of large and effective groups at institutions, many of which have survived over 30 years although their personalities may have changed completely. My Manchester group still survives. We moved to Imperial College in London in 1953. When cosmic-ray work was abandoned in the late 1950's, bubble chamber work, using CERN and the Rutherford Laboratory, was started up. I left physics research in 1970 but the group continues under the admirable leadership of Ian Butterworth. No doubt some of the 1950 cosmic ray groups have disappeared but I suspect that a detailed historical study would show that many have survived in one way or another.

The third main feature of the 1950's which I believe is important was the growth of collaboration between groups based in several universities and in several countries. Undoubtedly this wide collaboration began within the European emulsion programme; it developed in a small but valuable way at the Pic-du-Midi and later was developed further at CERN and similar laboratories throughout the world.

I believe the history of modern science is worth pursuing for its own sake, as a work of scholarship. I doubt if we shall learn anything which may enable us to forecast the future, but one never knows!

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