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The Discovery of the Pion in Bristol in 1947*

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The year of the discovery of the pion was 1947, an interesting year for several reasons. That discovery was not the only new thing that happened in Bristol physics in 1947; and of course on a countrywide basis, the discovery of V-particles in Manchester also in 1947, was to my mind just as important. For these and other reasons I shall not restrict myself to the pion discovery, but tell you also of other events taking place at about that time, which form an essential background to the discovery of the pion.

First I want to point out how 1947 turned out to be an important year for particle physics in many ways. It was a sort of watershed. It occurred exactly halfway through the history of particle physics, which began of course exactly 100 years ago with the discovery of the electron by J.J. Thomson in 1897. In the following 50 years — to 1947 — some important progress was made — the discoveries of the positron, proton and neutron, neutrino, pion and muon and V-particles. Had we known it then, the evidence of the heavier flavours of both leptons and quarks had already been detected, (but of course not recognized). In those first 50 years, progress was slow, the particle physics community was very small, detectors were rudimentary, resources were meagre. Practically all published papers were signed by one, two or at most three authors (I recall my shock at seeing the first Bristol paper on pi-mu decay with **four** authors!) Communications with physicists overseas was quite difficult. One had to rely on the post: telephoning was precarious (especially between England and Italy) and very expensive.

The year 1947 was also a watershed in the sense that the pion and muon and V-particle discoveries stimulated an explosion of accelerator building: the subject thereafter moved into top gear, and for the first time, detailed and controlled experiments at accelerators began to take over a field which had so far been dominated by cosmic ray experiments where events were rare and you had to take what Nature gave you. Finally,

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for me personally, 1947 was important as the year in which I published my first paper.

The accompanying Table 1 lists some of the papers in particle physics appearing in 1947 or late 1946. Let us recall that in 1935, Yukawa had proposed a heavy quantum to account for the short-range nature of nuclear forces, with a Compton wavelength equal to this range and a mass of order 1/7 of the proton mass. Two years later, Street and Stevenson, and Anderson and Neddermeyer, detected in cloud chambers the tracks of particles of mass intermediate between electron and proton — and thus called mesotrons. Yukawa had postulated that his quantum would decay giving an electron, and hence account for nucleon beta decay, and decay of the mesotrons was also observed in cloud-chamber experiments. But the problem was that even after numerous crossings of metal plates in cloud chambers, no mesotron had been seen to interact. Indeed, during the early 40s, Japanese physicists had set up a "meson club" to study these questions. Some invented "weak coupling" pseudoscalar meson theories to account for this behaviour, while Sakata and Inoue proposed a 2-meson theory, in which a strongly-interacting Yukawa-type particle decayed to a weakly-interacting daughter mesotron. The English version of the Sakata-Inoue paper did not appear until late 1946. Marshak and Bethe, unaware of it and equally of the Bristol discovery of pi-mu decay in May 1947, re-proposed the two meson hypothesis at the Shelter Island meeting in June 1947 (published in September 1947).

A crucial experiment on mesotrons was finally undertaken in Rome by Conversi and Piccioni, starting in 1943. They used a Rossi-type array (see Fig.1) consisting of two iron blocks magnetized in opposite directions, which had the property of focussing particles of one sign of charge and defocussing those of the opposite sign. A particle stopping in the absorber block would be signalled by a coincidence of the Geiger trays C_A, C_B and C_C and anticoincidence with the bottom tray, A. Any decay of the stopped particle was indicated by a delayed coincidence with counters D. Positive mesotrons stopping in the iron absorber decayed, while negative ones did not. They presumably underwent rapid nuclear capture, as expected for Yukawa particles and predicted by Tomonaga and Araki. I should mention that these experiments were running at the time of the Italian armistice in 1943. (I was at school at that time: I had just built my first radio using a crystal detector, and the news from Italy was the first thing I heard when I tuned in). The armistice was followed by the German army occupying Rome, the university being closed, and Conversi and Piccioni having to move the equipment (and themselves) to a safer place. Then the US Air Force started to bomb the place and they had to move once more (getting, Conversi told me, as close to Vatican City as they could).

After the war, the experiment continued, with Conversi and Piccioni joined by Pancini. They changed the absorber to carbon. The object of this was to record nuclear γ-rays which would follow nuclear capture of the mesotron and nuclear excitation and disruption; so they needed an absorber of a light element, which would not absorb the gammas. They changed to carbon: imagine their astonishment on finding that negatives stopping in carbon **all** decayed!

At the other end of Europe, in England, a by-product of the war and the nuclear programme was the setting up, in 1946, of a panel by the Ministry of Supply to oversee the development of special photographic emulsions to record nuclear particles. The chairman was Joseph Rotblat (winner of the 1995 Nobel Peace Prize) and the eight or nine of us on the panel included Cecil Powell, Otto Frisch, George Rochester and Berriman and Waller, the chemists from Kodak and Ilford. Under constant prodding and goading, by mid 1946 Ilford had produced a series of emulsions with four times the normal silver halide/gelatine ratio, which would record tracks of charged particles of ionisation down to about six times the minimum value. The series were called A, B, C... in order of increasing "grain" (= microcrystal) size, and B1, B2... in order of increasing sensitivity.

I was in the fortunate position at Imperial College, where I was a graduate student, that my supervisor, Sir George Thomson, was a Nobel Prizewinner and had been chairman of the famous Maud Committee in 1940, which had pronounced that a ^{235}U fission weapon would be possible. So he had a lot of clout, got on to the Air Ministry and persuaded them to arrange that flights of the RAF Photographic Reconnaissance Unit at Benson, near Oxford, should carry some of these emulsions for me (a total of six $3'' \times 1''$ 50 micron thick B1 emulsions). In November 1946 I got these back, processed them and found about 20 nuclear disintegrations, one of which was produced by an incoming charged particle (see Fig.2). From scattering and ionization variation

I estimated the mass to be 100-300m_e. The secondary protons from the interactions were of low energy (4 or 5 MeV) which meant that (taking account of Coulomb barrier effects) this had to be capture in a light nucleus (C, N or O) of the gelatine, not Ag or Br or I of the halide.

I had heard about (but not seen) the Conversi result, that negative mesotrons stopping in carbon at sea-level in Rome seemed always to decay; while my negative particle, stopping in a light nucleus at $35000'$ underwent nuclear capture. I realised there was a big difference here, but I had absolutely no idea what it all meant. Two weeks later, Occhialini and Powell in Bristol published six similar events.

The big breakthrough, however, was the publication, in May 1947, of two events in C2 emulsion exposed at the Pic du Midi, now called pi-mu decays, by Lattes, Occhialini, Muirhead and Powell — see Figs.3 and 4. In Fig.3, a parent particle comes to rest and decays into a second particle which leaves the emulsion surface just before coming to rest. The true secondary range could be quite well estimated. In Fig.4, the event is complete; the secondary comes to rest after a range of $600\mu m$. The estimated range in the first event and the observed range in the second event were almost exactly the same — evidence then for a simple 2-body decay. The two-meson hypothesis had finally been discovered by experiment.

In April 1997, at the conference dinner of an IOP meeting in Cambridge, someone asked me "what about the third pi-mu decay?" I had thought this was a 50 year old secret, but I must have talked about it in an unguarded moment. Fig.5 shows the event: it is a terrible picture of a complete pi-mu decay in B2 emulsion, exposed for me near Chamonix by Leprince-Ringuet. I found it in July 1947, and put it in my thesis. I did not publish it. In retrospect, it would of course have been independent confirmation of the Bristol events, from a different laboratory and with different emulsions. But in those days, the atmosphere was very different from today: we didn't just rush into publication and I remember clearly having been deeply impressed by the Bristol work, and thinking that confirmation was not really necessary! I believe I did telephone Powell about it, but that was all.

In any case, the final proof of pi-mu two body decay had to await until September 1947, after several dozen C2 plates had been exposed by Lattes from Bristol on Mt.

Chacaltaya in Bolivia. Bristol found 10 more complete decays. Fig.6 shows a histogram of the muon range distribution which, taking account of range straggling, clearly proves the two-body nature of the decay.

The Bristol people were able to show that, after taking account of geometrical efficiencies (the fact that the emulsions were only 50μm thick and the muon range was 600μm), the true number of π^+ mesons and the number of negatives giving nuclear capture 'stars' were very comparable: thus the latter particles could be ascribed to π^-. Events were also found where π^+ and π^-, produced in nuclear disintegrations, came to rest in the same emulsion layer and underwent decay and nuclear capture respectively.

In 1948/49, both Kodak and Ilford were able to produce much more sensitive emulsions, NT4 and G5 respectively, which were sensitive to minimum ionizing particles. Fig.7 shows four examples of complete $\pi \rightarrow \mu \rightarrow e$ decays. Since 1949, there have been no further developments of emulsion technology, and today's emulsions (made in Japan) are similar to those of 48 years ago.

The assumption that the first pi-mu events were really decays was not taken lightly. A Bristol solid-state physicist, Charles Frank, looked into the question of whether such events could represent capture of a **negative** meson by an atom or molecule, which then catalyzed a nuclear reaction with release of energy and ejection of the original meson with a few MeV kinetic energy. Frank concluded that this could not occur in the emulsion, but **was** possible in a hydrogen-deuterium mixture. We now know this process as muon-induced fusion. A negative muon comes to rest in the hydrogen forming a μH_2 molecule (replacing one of the electrons because the muon binding energy is 200 times larger) and eventually finds an HD molecule to which it transfers (again, a reduced mass effect). Because the Bohr radius of the muon is only 10^{-11}cm, proton and deuteron can come close enough to fuse: $p + d \rightarrow {}^3\mathrm{He} + 5.5$ MeV. The muon is ejected and can repeat the process, which was re-discovered experimentally a decade later by Alvarez at Berkeley (see Fig.8). With the right HD mixtures, temperature and pressure, one muon can catalyze some 200 fusions. Unfortunately, this is just not enough — it takes on average some 8 GeV of energy to create a negative muon. The problem is that the "sticking probability" of the muon to ^{3}He (about 0.5%) is just a little too big. But for this one wrong constant, muon-induced light element fusion could have been a viable source of power, and one would have had no Chernobyls, no problems with radioactive waste from fission reactors — and the standing of particle physicists on the world stage would have been a lot higher.

The final paper I want to mention from 1947 is that describing the neutral and charged V-particle events, by Rochester and Butler at Manchester — see Fig.9. The first event (which incidentally, occurred on my 21st birthday) was probably what is now called $K_s^0 \rightarrow \pi^+ + \pi^-$, the second probably $K_\mu^+ 2 \rightarrow \mu^+ + \nu_\mu$. After these two events, no other example was reported for two years; then confirmation trickled in from MIT, CalTech, Ecole Polytechnique. Personally, I believe that Rochester and Butler never received the acclaim due to them for this discovery. I recall that in 1952, a conference was held in Copenhagen to discuss particle and nuclear physics and, in particular, international collaboration in the field. At this time, the choice of laboratory for CERN had not been made. Niels Bohr of course wanted it to be in Copenhagen, while Auger and Rabi preferred Geneva. (For obvious reasons, had we known then what we know today about Swiss banks during and after World War II, the decision may well have gone in favour of Copenhagen). George Rochester and I did the overnight sea crossing from Newcastle to Esbjerg. George and I went into the ship's bar and after several drinks, I persuaded him to sign a piece of paper to the effect that if he and Butler got the Nobel Prize, they would give me 10%. Unfortunately, like most of my schemes for making money, this has come to nothing. In any case, I've lost the piece of paper.

I should make some concluding remarks to indicate the atmosphere surrounding research in high energy physics 50 years ago. People did some very way-out experiments, simply for the hell of it, and because nobody really knew where or how the next big step would come. The early measurement of the charged pion lifetime provides an example of the ingenious approaches used. Table 2 shows some results. First, there was an experiment by Reg Richardson at LBL, measuring the number of pions surviving after successive spirals in a magnetic field. The pions were made at the 184″ cyclotron, using an alpha-particle beam on a target (protons were no good, as they did not have enough energy: but the Fermi motion inside an alpha-particle gave the extra boost to get above threshold).

A second approach was that of Ken Greisen at Cornell: he looked for a "knee" in the cosmic ray muon spectrum. The idea was that, for pions of energy below 117 GeV (at which energy the pion decay length is equal to the scale height (6.5 km) of the atmosphere) decay is more probable than interaction. For higher energy, the reverse is true, and the pion decay probability varies as 1/E. Hence the (negative) index of the muon energy spectrum from pion decay becomes one greater than that of the pions, so this "knee" measures the pion lifetime.

The third approach was that of Camerini and others at the Jungfraujoch. I remember Powell describing this in an evening lecture at Bristol. He pointed out that the pion lifetime was a few nanoseconds, and therefore delicate and sensitive apparatus would be necessary. So saying, he reached under the lecture bench and produced a cocoa tin! The method employed was to stick a vertical pole into the Aletsch glacier, and tie to it at different heights, a number of cocoa tins containing nuclear emulsions. Measurement of the relative numbers of upward-moving pions and muons surviving to different heights then gave a measure of the lifetime.

Finally, Martinelli and Panofsky repeated the Richardson experiment. It will be seen that all four experiments got the wrong answer, by between 5 and 9 times the stated errors; but their average is not so very far from the value accepted today!

Since those days of half a century ago, experimental particle physics has undergone profound changes. The detectors employed are incomparably more complex and sophisticated, the teams necessary to operate them run to 100s of people instead of 3 or 4, and, worst of all, a battle for funds is being continually fought with one's fellow scientists. Everyone is expected to give "value for money" in a field where the eventual values of basic research cannot possibly be predicted.

But some things have not changed at all. Fifty years ago, Cecil Powell described his feelings on finding all those wonderful new processes in nuclear emulsions. He said it was "as if, suddenly, we had broken into a walled orchard, where protected trees flourished and all kinds of exotic fruits had ripened in great profusion". Well, the walled orchards still exist today. Perhaps they are not so easy to find, but they **are** there and it is for the new generation of physicists to find them, as I am sure they will.

Table 1. Papers on Meson Physics 1946–7.

Nov. '46	Sakata Inoue	*Prog.Theor.Phys.* **1**, 143	2 meson hypothesis
Jan. '47	Perkins	*Nature* **159**, 126	First σ-star' (π^-)
Feb. '47	Conversi, Pancini Piccioni	*Phys.Rev.* **71**, 209	Negative mesotrons decay in carbon (μ^-)
Feb. '47	Occhialini Powell	*Nature* **159**, 186	6 'σ-stars'
May '47	Lattes Occhialini Muirhead Powell	*Nature* **159**, 694	2 $\pi - \mu$ decays
Sept. '47	Marshak Bethe	*Phys.Rev.* **72**, 506	2 meson hypothesis (again)
Oct. '47	Lattes, Occhialini Powell	*Nature* **160**, 453	644 mesons 105 σ-stars 11 complete $\pi - \mu$ 499 ρ-mesons ($\mu^{\pm}$)
Oct. '47	Frank	*Nature* **160**, 525	Meson-induced fusion (μ HD)
Oct. '47	Rochester	*Nature* **160**, 855	V-particles

Table 2. Early Measurements of Charged Pion Lifetime

	Method	Result (nanosecs)
Richardson (1948)	Decrease in intensity of pions spiralling in field (Berkeley SC)	8 ± 2
Greisen (1948)	Knee in cosmic ray muon spectrum	60
Camerini *et al* (1948)	Intensity of upward travelling muons above glacier	6 ± 3
Martinelli & Panofsky (1950)	Richardson method	19.7 ± 1.4
Present Value		$26.03 \pm .02$

Fig.1 Rossi-type array used by Conversi, Pancini and Piccioni, 1943–47. The two parts of the iron block are magnetized in opposite directions, focussing particles of one sign and defocussing those of the opposite sign. A meson stopping in the absorber and decaying is given by the coincidence/anticoincidence of the various trays $C_A + C_B + C_C - A + D$ (delay).

Fig.2 First negative meson capture event leading to disintegration of a light nucleus in the emulsion (B1 emulsion flown in aircraft from RAF Benson). Perkins: *Nature* **159**, 126 (1947).

Fig.3 First $\pi \rightarrow \mu$ decay observed in C2 emulsion exposed at Pic du Midi. The secondary muon does not quite come to rest before leaving the emulsion surface (Lattes, Muirhead, Occhialini and Powell: *Nature* **159**, 694 (1947)).

Fig.4 Second $\pi \rightarrow \mu$ decay observed by Bristol group. The muon comes to rest after a range of 610μm.

Fig.5 $\pi \rightarrow \mu$ decay found by author in July 1947, in B2 emulsion exposed at Vallot near Chamonix (4000m above sea level). Published only in 1948 PhD Thesis by the author.

Fig.6 Histogram of ranges of muons in 11 complete $\pi \rightarrow \mu$ decays, proving that the decay is a two-body process (Bristol events of October 1947).

Fig.7 Four complete $\pi \rightarrow \mu \rightarrow e$ decays in G5 emulsion, showing the constancy of the muon range.

Fig.8 Hydrogen bubble chamber picture of HD $\rightarrow^3$He reaction catalyzed by negative muon capture into μHD molecule. The incident muon comes to rest, drifts as a neutral mesic atom, and is ejected with 5.4 MeV energy in the exoergic fusion reaction.

Fig.9 The year 1947 was rounded off by the publication of a neutral and a charged 'V-event' in a cloud chamber at Manchester. The upper neutral V on the right photo probably corresponds to $K_s^0 \rightarrow \pi^+\pi^-$, and the left one charged V, to $K^+ \rightarrow \mu + \nu$.

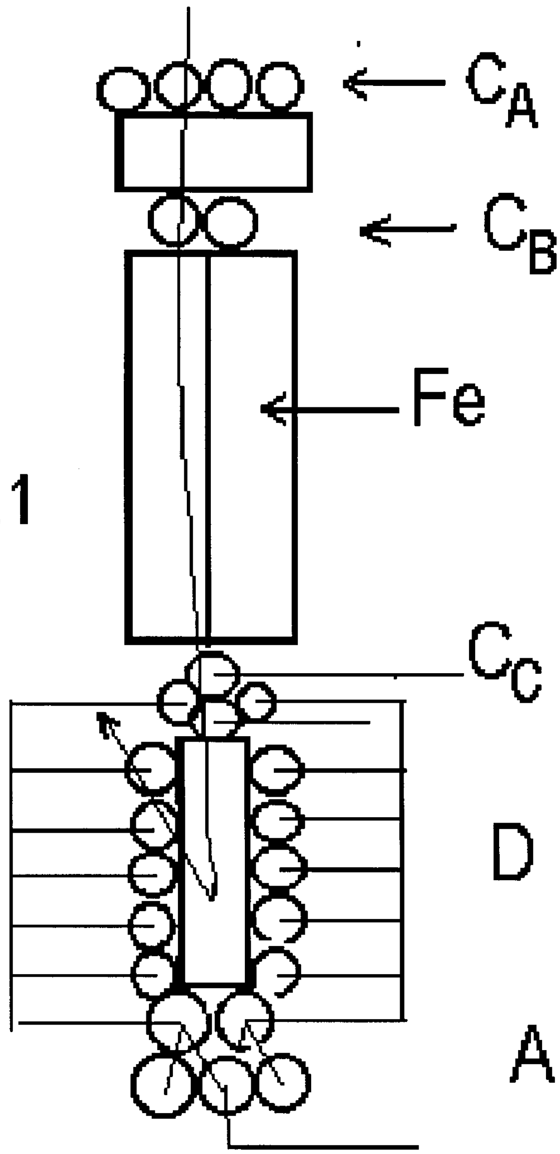

Fig.1

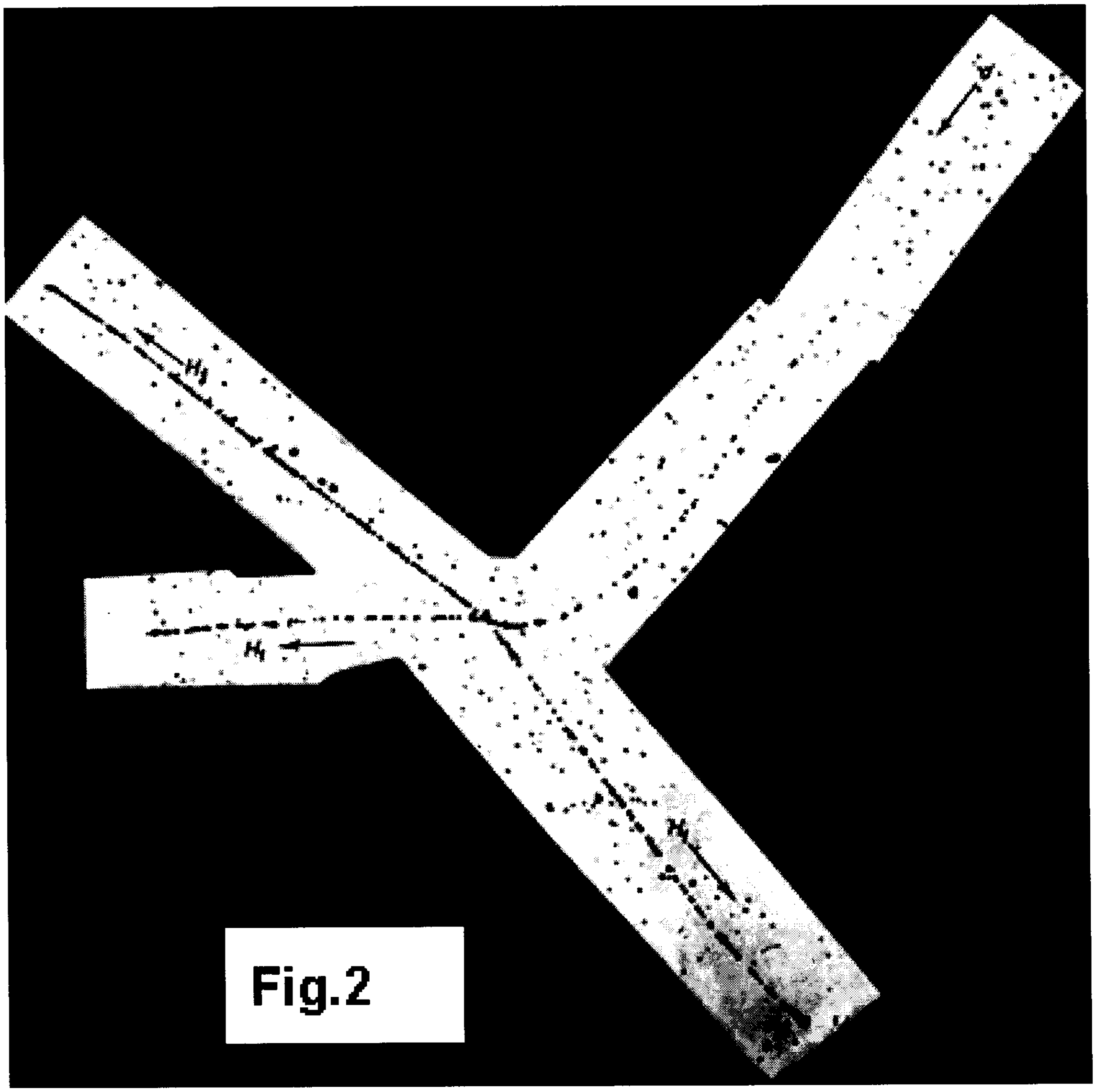

Fig.2

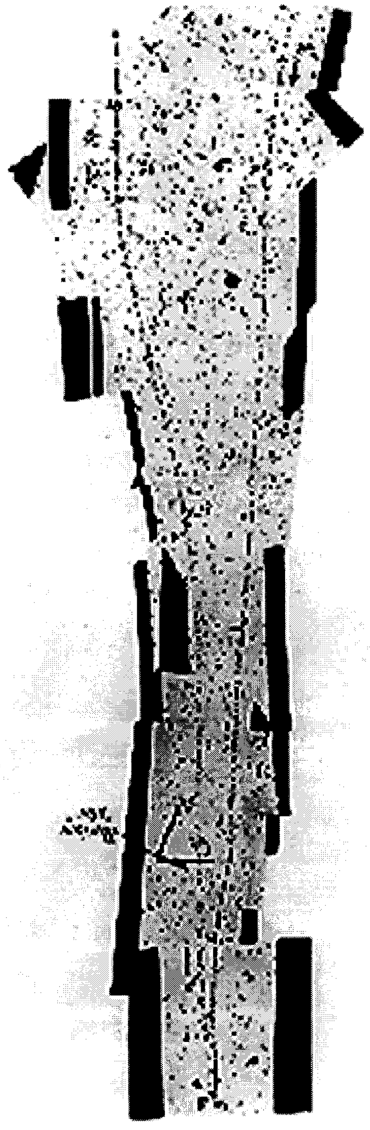

Fig.3

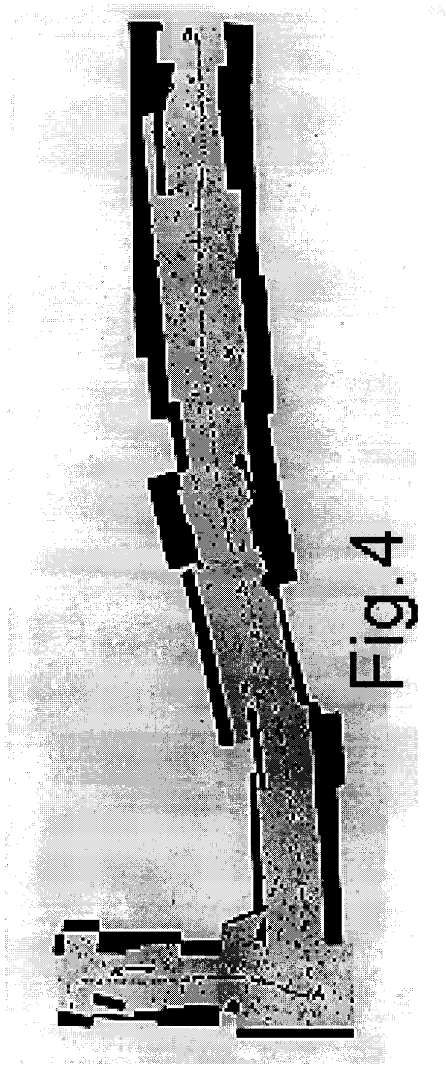

Fig.4

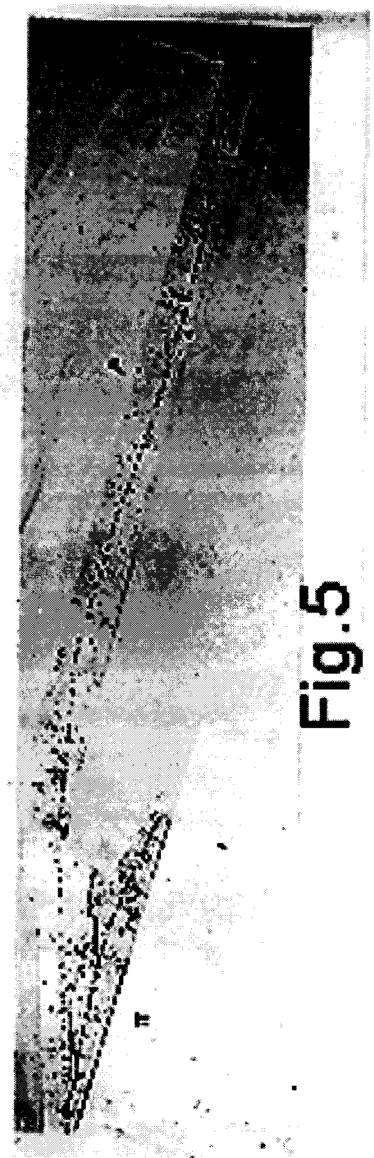

Fig.5

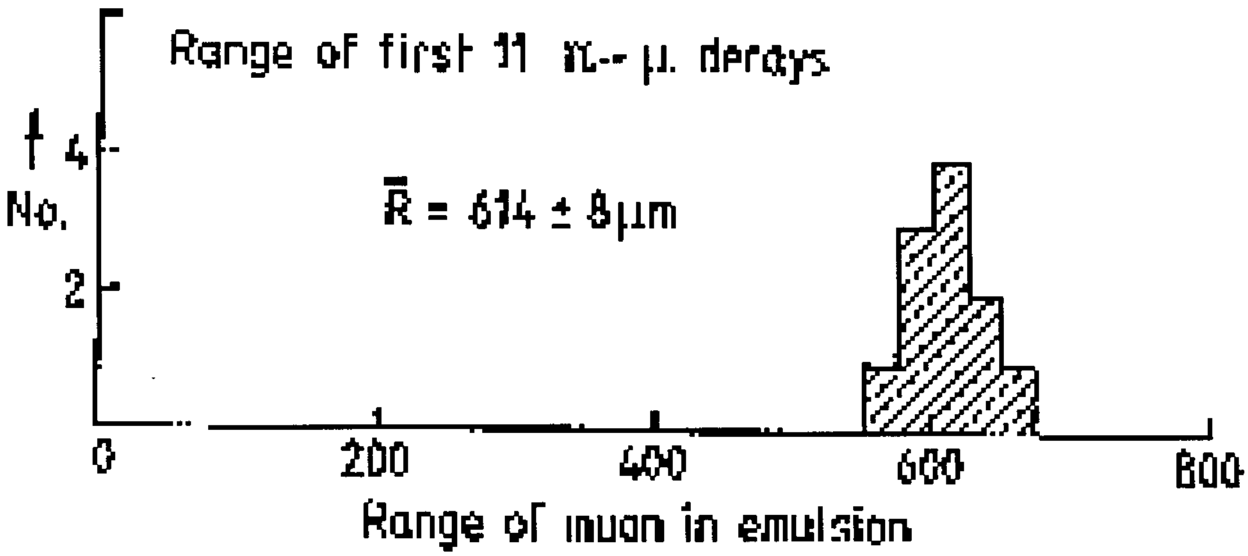
Range of first 11 π–μ decays
$\bar{R}$ = 614 ± 8μm
No.
4
2
0
200
400
600
800
Range of muon in emulsion

Fig.6

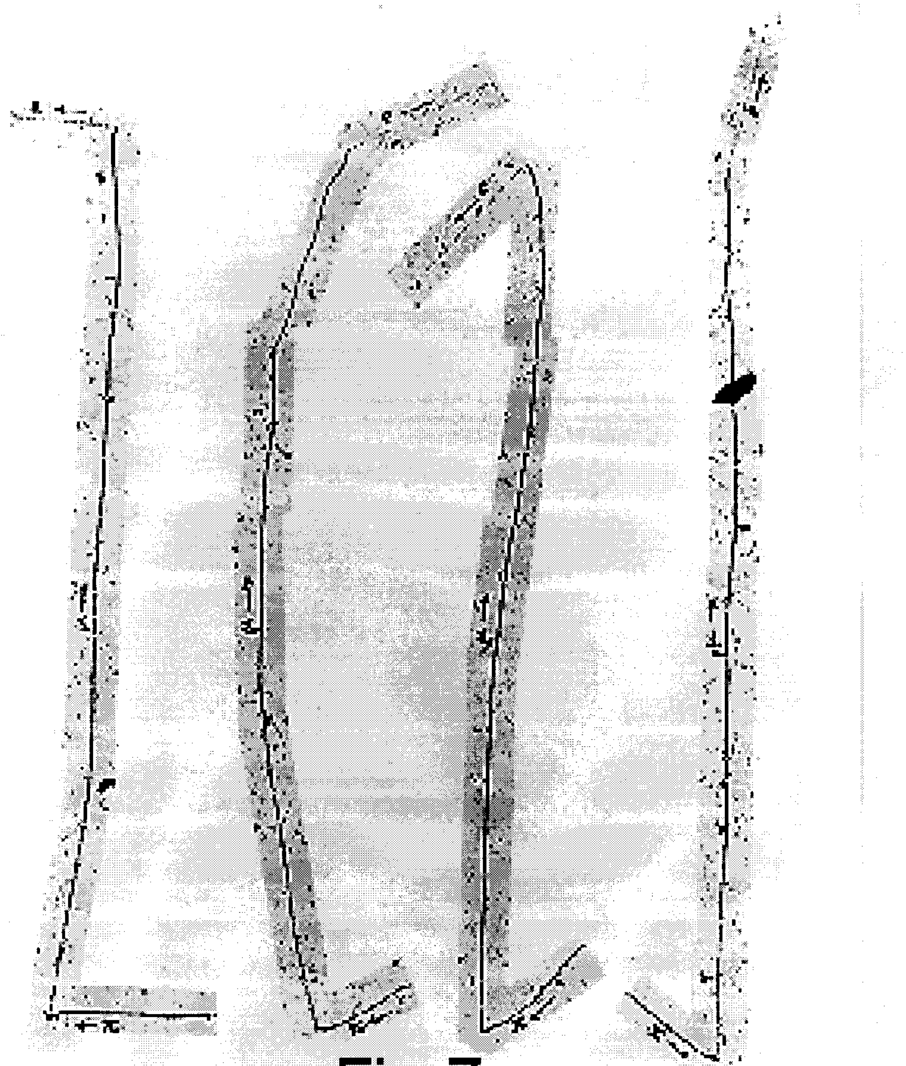

Fig.7

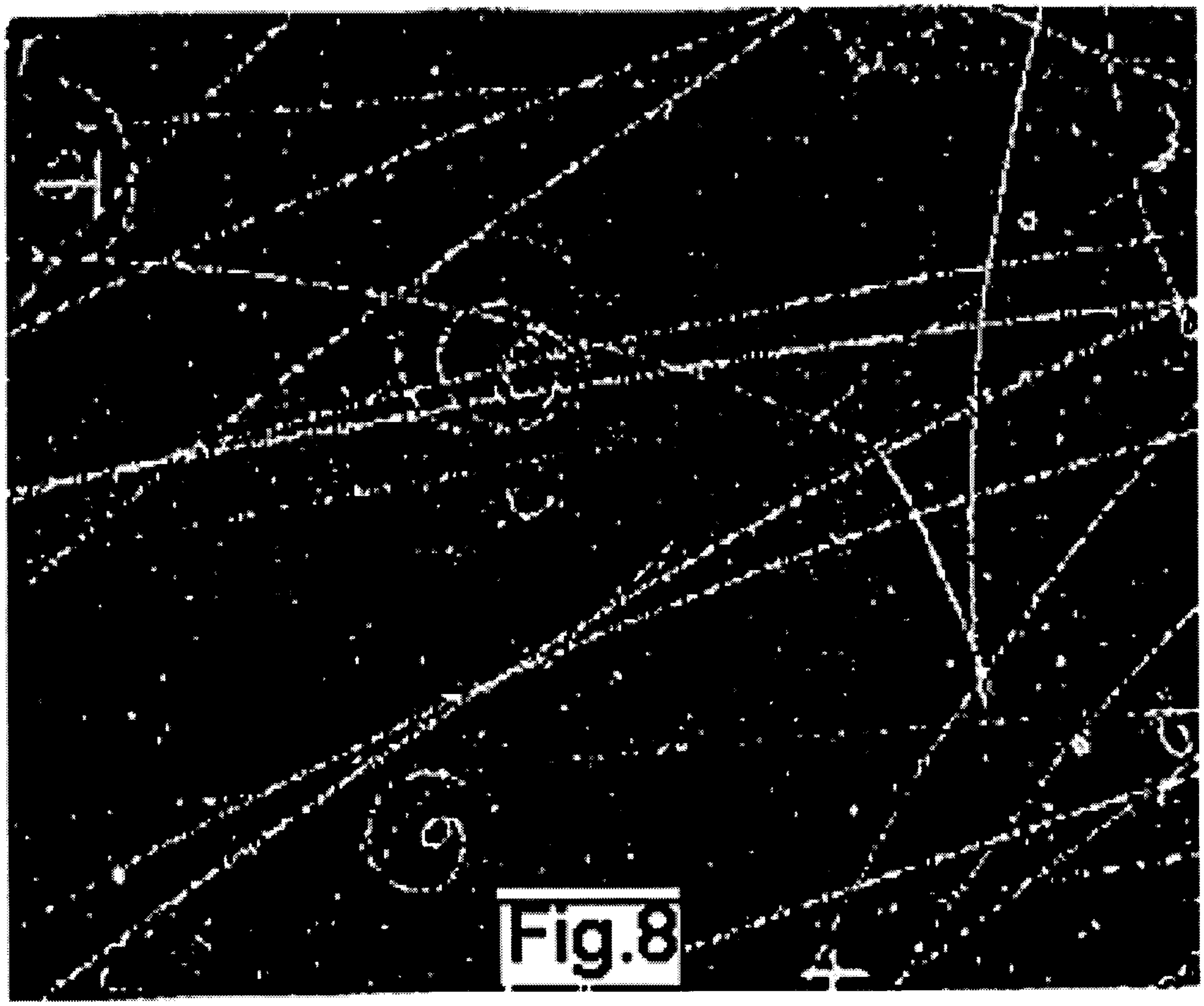

Fig.8

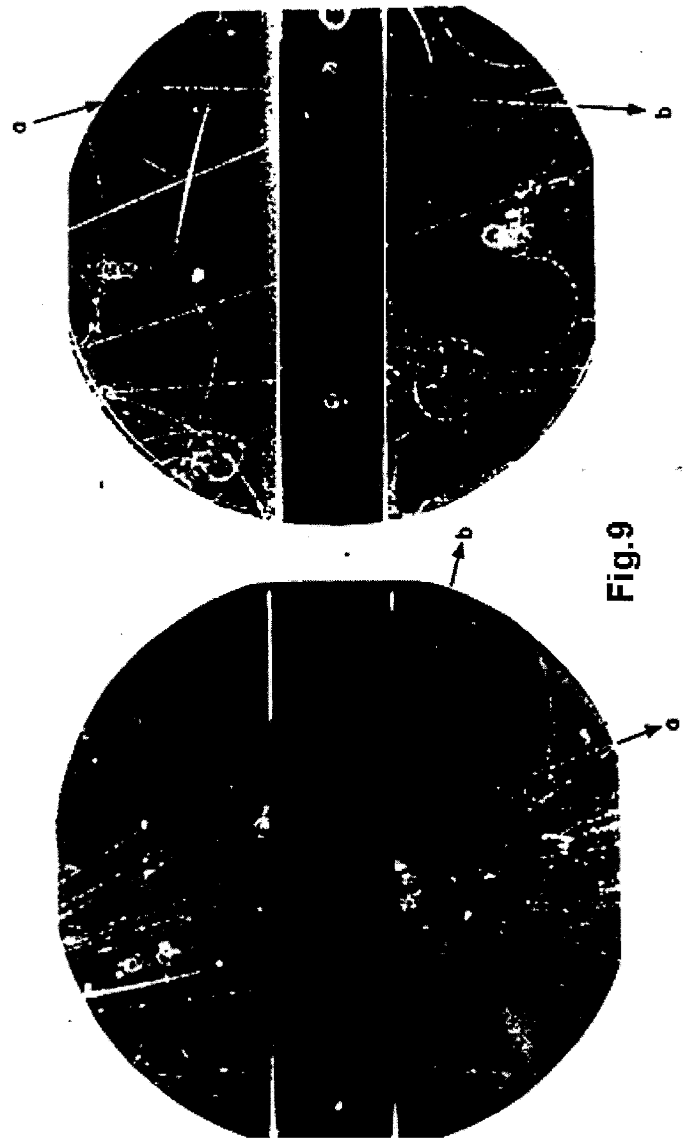

Fig.9