LBL-20808

Lawrence Berkeley Laboratory

UNIVERSITY OF CALIFORNIA

Physics Division

Invited talk presented at the International Symposium on Barticle Physics in the 1950's: Pions to Quarks, FNAL, Batavia, IL, May 1-4, 1985

EARLY WORK AT THE BEVATRON: A PERSONAL ACCOUNT

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December 1985

Prepared for the U.S. Department of Energy under Contract DE-AC03-76SF00098

LBL--20808

DE86 007467

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Early Work at the Bevatron: A Personal Account*

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The Bevatron started operating in early 1954 at what was then the Radiation Laboratory and is now known as the Lawrence Berkeley Laboratory.

Some personal background

Sula and I came to Berkeley from Columbia University in 1953. I to join the Physics Department and Emilio Segrè's group at the Rad Lab. She joined Walter Barkas' group and later Ed Lofgren's group. We had been working with photographic emulsions at Columbia's cyclotron located at Nevis with the help and encouragement of Gilberto Bernardini. Before then I used emulsions loaded with D_2O as a gamma-ray spectrometer for my Ph.D. thesis under Hugh Richards at the University of Wisconsin in Madison.

Setting up with photographic emulsions

While in my earlier work I had used 100 μ to 600 μ single small emulsions on glass, this was the period in which emulsion stacks started to be used in cosmic ray work at Bristol and elsewhere and the electron sensitive emulsions had recently been introduced by Kodak Ltd. of England followed by C. Waller at Ilford, in close consultation with C. F. Powell and G.P.S. Occhialini.

I thus started out at Berkeley to build up an emulsion processing plant in the Physics Department - the photographic emulsion arm of the Segrè group.This

*Work supported in part by the U.S. Department of Energy under contract DE-AC0376SF00098.

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involved new techniques for marking emulsion sheets, to allow easy track following from sheet to sheet, the modification of microscopes with special stages to hold and manipulate these large emulsion sheets after they were precision mounted on glass, and the construction of precision microscope stages for multiple scattering measurements.

I was very lucky to find that the shop foreman in the Physics Department - William Brower - loved to build precision equipment. Brower's advice and consultation were invaluable to me. In addition, Stephen Goldsack visited the Brode Fretter group for a year from England in 1954 and spent a good deal of time working with me and helped in the design of the multiple scattering equipment.

The startup of the Bevatron

From the first day - I should actually say night - the Bevatron started accelerating proton beams Sula and I were there to place emulsions into the beam. We were soon joined in these nightly vigils by Warren Chupp who was working in the Bevatron group headed by Ed Lofgren. At first we placed a few emulsions on an arm which carried the target and was introduced into the Bevatron through a Vacuum Seal. The target carried a small polyethylene "lip" - due to Ed McMillan - designed to introduce a small energy loss and scrape off a small portion of the beam*. As a result of this energy loss the proton trajectories moved to a lower radius and hit the emulsions on the next pass.

With these exposures we helped establish that one was indeed dealing with energetic protons and that one could get emulsion exposures, of sorts, inside the Bevatron vacuum tank.

The finger in the dike revisited

I remember in particular one episode when Luis Alvarez was also spending the evening at the Bevatron and observed our procedures. That night the Bevatron operator charged with pulling the target probe, with our emulsions on it, out through the vacuum lock gave a particularly vigorous pull and managed to yank the probe completely out and air started rushing into the vacuum tank. Luie, who was standing nearby, rushed over and placed the palm of his hand over the hole! This allowed the crew to close the vacuum lock without

*A similar device was introduced by R. Cool and O. Piccioni at the Cosmotron as a starting point for external beams.

the entire Bevatron coming up to air. I must admit that I would not have thought of doing this - and furthermore would probably not have done it! Luie had saved the day and the Bevatron was able to pump back down without excessive loss in time, while Luie was rubbing the sore spot on his hand.

The status of particle physics before the bevatron started

To understand where the Bevatron fitted into the physics of the day I want to review briefly where we stood in particle physics.

From the Nuclear or "Classical period" of particle physics we are well acquainted with the early discoveries of:

- The electron (J.J. Thomson 1900),
- the proton (Goldstein, Thomson, Rutherford 1886-1920's),
- the neutrino (postulated by Pauli 1930),
- and the neutron (Chadwick 1932).

The "modern period" began with particle discoveries in cosmic rays.

- the positron (Anderson 1933)
- the muon (Neddermeyer and Anderson 1936, confirmed by Street and Stevenson),
- the puzzle that despite the near coincidence in mass the muon was not Yukawa's mesotron (Conversi, Pancini and Piccioni 1946) by use of the capture rate calculations of Tomanaga and Araki,
- a hint of the K meson (Leprince-Ringuet and Lheritier 1944),
- the clear observation of strange particles (the "forked tracks" of Rochester and Butler 1947)
- the discovery of the pion(negative pion stars by D. Perkins 1947) and the π^+-μ^+ sequence (Lattes, Occhialini and Powell 1947) which resolved the puzzle as was suggested by Bethe and Marshak (1947)and independently by Sakata and Inoue and also Tanikawa.
- Then came in rapid succession the various strange particles Λ, Θ^0 (or K^0), $\tau^\pm$ (or $K_{\pi 3}$), Σ^+, hyperfragments and Ξ^-.
- Meanwhile on the theoretical side the understanding of the strange particles in terms of associated production (Pais 1952) followed by the introduction of the strangeness quantum number (Gell-Mann and independently Nishijima 1953).

In this period also, accelerators began to make an impact on particle physics: from the first observation of "artificially produced" pions at the 184" cyclotron in Berkeley (Gardner and Lattes 1948) to measurements of particle properties:

- discovery of the π^0, initial indication by Bjorklund, Crandall, Moyer and York as well as in cosmic rays by Carlson, Hooper and King to the conclusive evidence by two photon coincidence measurements of Steinberger, Panofsky and Stellar using McMillan's Synchrotron.
- the Panofsky ratio (1950) and quantum numbers of the $\pi^{\pm}$.
- In 1952 Anderson, Fermi, Long and Nagle discovered evidence for the first baryon resonance at the Chicago Cyclotron the $\Delta(1238)$ which was interpreted by Keith Bruckner as an $I = 3/2$, $J = 3/2$ resonance.
- The observation of associated production of Λ and K^0 as well as the Σ^- by Fowler, Shutt, Thorndyke and Whittmore at the Cosmotron in Brookhaven.
- The observations of K^- interactions in emulsions yielding $\Sigma^{\pm}$ particles (by Hornbostel and Salant 1953) at the Cosmotron.

With all this richness there was confusion as well: there were apparently several different particles of mass $\approx$ 1000 m_e - or were they different decay modes? In particular the analysis of Dick Dalitz (and independently of Fabri) together with the meticulous collection of every single τ meson event in the world, showed clearly that the τ could not have the same spin and parity as the Θ.

Back to the Bevatron

Our first interest was to study K mesons as well as any other new particle that might show up. One of the goals was to understand the $\tau - \Theta$ puzzle. Little was known about the lifetimes of all the different charged K mesons (or were they possibly different decay modes?) and there was no reason to suppose that some of these lifetimes could not be quite short, for example as short as the K^0 or Λ lifetimes.

It was clear to me that the emulsion exposures in which the emulsions were mounted on a target holder could not be well enough controlled for accurate experiments.

The vacuum tank of the Bevatron was enormous (see Fig. 1) since the machine was designed before the invention of strong focusing (Courant, Livingston and Snyder and indepandently Nick Christofilos 1952). This meant that if emulsions were exposed in an external beam the Kaons would have to travel at least 1-2 m and hence any short-lived component would decay away.

Getting close to the target

I discussed this point with Ed Lofgren and suggested that we might introduce re-entrant wells into the vacuum tank to allow a close approach to the target from above. I also gave him a very crude sketch (for wells corresponding roughly to 45^{0}, 90^{0} and 135^{0} in the C.M. System). To my surprise and delight when I saw Ed some 10 days later he mentioned by the way that the re-entrant wells were already installed! Thus we were now able to expose emulsion stacks within a few centimeters from the target and could start looking for very short-lived particles. The first K^{+} decay event at the Bevatron was found by Don Stork in a test exposure in our re-entrant wells. See Fig. 2.

During this period also we helped in the exposure of emulsion stacks from all over the world. Frequently we also processed the stacks in Berkeley using the techniques for stack alignment we had worked out. In particular I remember an enormous stack brought over by Louis Le Prince-Ringuet from Paris, which we exposed and processed.

External beams

The next step was to expose emulsions in momentum analyzed external beams originating at an internal target. This had the advantage that the 3 types of particles π^{+}, K^{+} and p all of the same momentum had different well defined, ranges in the emulsion so that one could proceed directly to the region where the K^{+}s come to rest without scanning the entire emulsion volume.

Focused external beams

After consultation with my colleagues we decided to introduce a 90^{0} wedge magnet into the external beam to improve the intensity by focusing the beam.

While this device worked, Roy Kerth and Don Stork of the Richman group came up with a better idea at about the same time. They used a set of strong focusing quadrupoles - of the type built by Bruce Cork for focusing the proton beam at the linear accelerator - the injector to the Bevatron. See Fig. 3. With this improvement relatively clean and easily studied K^+ as well as K^- beams became available. On some of this work we shared our stacks with Aihud Pevsner and Dave Ritson et al who were both at MIT at that time.

We concentrated on interactions in flight (for cross section determinations)[1] decays in flight (for lifetime determinations)[2] decays at rest of K^+ mesons (for the study of the different particles - or decay modes)[3] and later interactions at rest of K^- mesons.[4] Birge, Haddock, Kerth, Peterson, Sandweiss, Stork and Marion Whitehead of the Richman Group concentrated on a precision range measurement of θ^+ and τ^+ mesons yielding accurate mass measurements.[5] Luis Alvarez together with Sula did the first τ^+ lifetime measurement by comparing τ production rates as observed close to the target(in the re-entrant wells) and far away (in the external beams).[6] Harry Heckman[7] in the Barkas group collected τ's for inclusion in the world-wide Dalitz plot. We found a K^+H scattering event in our emulsion stacks that allowed a precision mass measurement of a single θ^+ event.[8] All this work was reported by Don Stork at the 1955 Pisa conference. This was clearly a milestone. In less than a year the Bevatron had begun to contribute significantly to what had largely been the domain of cosmic ray physics.

What did we learn from all this work at the Bevatron?

- We established that K^+ cross sections were significantly lower than K^- cross sections. That low energy K^+ interactions did not produce pions but only underwent either scattering or charge exchange. A clear confirmation of the Gell-Mann Nishijima strangeness scheme. Furthermore we confirmed the observations at the Cosmotron that K^- interactions produce Σ^+ and Σ^- hyperons and noted in particular from a few capture events on hydrogen in the emulsions that $M(\Sigma^-)$ was 14 m_e larger than $M(\Sigma^+)$, a surprising result at first.[4]
- The θ^+ and τ^+ mass measurements[5,8] coupled with lifetime measurements[2,6] and particularly later lifetime measurement with counters (Alvarez,Crawford, Good and Stevenson[9] as well as those of Fitch and Motley[10] at the Cosmotron, see discussion by Val Fitch at this conference) pointed clearly to the puzzle that the Θ and τ had nearly indistinguishable masses and lifetimes!

The stage was set, the culmination of the cosmic ray, Cosmotron and Bevatron work coupled with Dalitz's analysis led Lee and Yang to postulate two alternate possibilities:

a) either there is a parity doubling of particles or

b) parity is violated in weak decays and furthermore they suggested how this could be tested.

As is well known the experiments of Wu and Ambler et al., Garwin, Lederman and Weinrich, and Friedman and Telegdi gave a resounding confirmation to hypothesis b!

<u>The hunt for the antiproton</u>

The Bevatron was designed to have enough energy for antiproton production in a $\bar{p}p$ collision. To search for the $\bar{p}$ was thus clearly on many people's minds.

In the Segrè group we decided to attempt a double barrel attack on the antiproton. On the one hand Owen Chamberlain, Clyde Wiegand and Tom Ypsilantis went ahead with the preparation of a beam for a counter experiment (the details are given in Owen's talk at this conference), on the other hand Emilio Segrè and I went ahead to plan for an emulsion experiment in collaboration with Eduardo Amaldi and his group in Rome. When the $\bar{p}$ beam under construction by Chamberlain et al.[11] reached the first focus (i.e., about half done), we exposed our emulsion stack (see Fig. 4), processed it in Berkeley, divided it in two parts, and started scanning it both at Berkeley and in [illegible]e.

As it turned out in this emulsion experiment we outsmarted ourselves. We calculated the effect of the Fermi motion and concluded that in order to get a reasonable $\bar{p}$ flux we had to run at a momentum of 1090 MeV/c rather than 700 MeV/c. At the latter momentum the $\bar{p}$'s could reach the end of their range in the stack. This meant that in order to stop $\bar{p}$'s in our emulsion stack we had to place a sizable Cu absorber (132 gm/cm^2) ahead of our emulsion stack. This had two deliterious effects. First of all interactions of the beam particles in the absorber gave rise to a large number of protons which managed to enter our stack together with the negative particles. This made track following of about 1.5 x minimum ionizing tracks very difficult, and meant that we had to rely in part on the very slow and laborious method of area scanning. Secondly $\bar{p}$'s have a cross section which is considerably larger than the proton cross section. This fact, which we did not anticipate,* reduced our $\bar{p}$ flux by more than a factor of 2 from what we expected.

By October 1955 the counter experiment had clearly demonstrated:

1. There were negative particles of protonic mass within an accuracy of ±5%
2. There was a threshold for the production of these particles at about 4 GeV incident proton beam kinetic energy.

Clearly necessary conditions for the identification of $\bar{p}$'s.

Then in November 1955 our efforts in the emulsion experiment, despite the handicaps mentioned above, yielded 1 event, found in Rome, which came to rest and produced a star with a visible energy release of about 826 MeV.[12] See Fig. 5. Again a necessary condition for $\bar{p}$'s.

About that same time Brabant, Cork, Horowitz, Moyer, Murray, Wallace and Wenzel[13] of the Lofgren and Moyer groups placed their special lead glass Čerenkov counter behind the Chamberlain et al beam and observed "large pulses" consistent with the properties expected for $\bar{p}$'s.

In December 1955 we decided to try another emulsion exposure - this time at 700 MeV/c so that the $\bar{p}$'s could enter the emulsion stack and come to rest in it. I furthermore introduced a special sweeping magnet this time to guard against stray protons entering our stack. On this occasion all emulsion groups at the Laboratory participated in the exposure : Birge et al of the Richman group, and Barkas et al who supplied their own emulsion stacks, as well as Amaldi's group in Rome who shared our stack together with Sula Goldhaber and Warren Chupp of the Lofgren group. Also, in September 1955 Gösta Ekspong came to visit from Sweden and joined me in my efforts to find more $\bar{p}$'s in emulsions.

Just before we started the exposure we went through the usual period of doubt - had all the magnets been connected up correctly? As a last check we brought out a battery and connected a piece of thin wire and checked the

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*Gösta Ekspong tells me that one day Edward Teller came rushing into my lab looking for me. Edward was all excited - he had the explanation why we were not seeing any events in our emulsions - the large cross section was the cause! H.P. Duerr and E. Teller, Phys. Rev. 101, 494(1956).

direction of the forces on it in the various magnets. This was followed by all present holding up either their right hand or their left hand with three fingers held perpendicular to each other to ascertain that negative particles would be bent correctly by the magnets.

This exposure was extremely successful. As soon as the emulsions were developed we could see $\bar{p}$ candidates entering the emulsion stack. Our procedure was to scan along the upstream edge of each emulsion and look for about twice minimum ionization tracks - which were easily distinguishable from the large background of 700 MeV/c pions which were at minimum ionization.

The emulsion processing was started over New Year's and early in January 1956 as soon as the emulsions were dry from the developing, fixing and washing cycle, Gösta would scan the leading edge and look for $\bar{p}$ candidates. We found a few twice minimum tracks and Gösta started to follow along the track through a series of plates and in the morning of January 11, 1956, he followed a track to the end of its range where it came to rest and formed a large star! Thus within about 3 weeks from the exposure we found our first star! That same afternoon a scanner working with Sula found another star! The first star occurred at the interface between 2 emulsion sheets with half the tracks going upwards, the other half downwards. We had to wait another week or so, until the rest of the stack was developed, before we could follow all the tracks from this star. After Gösta and I developed a new method for the multiple scattering measurements of steep tracks - and here our precision placement of the emulsion sheets was of crucial importance - we evaluated the total visible energy. This event turned out to be particularly important because it gave the conclusive proof ("sufficient condition" for those who were still in doubt) of the annihilation process. The visible energy released in this star was 1300 ± 50 MeV.[14] Clearly greater than the mass of the incident negative particle! See Fig. 6.

I remember two amusing consequences of our discovery.

- The day after the annihilation event was found, Segrè saw to it that a phone was installed in my Lab in LeConte Hall.
- Chamberlain gave an invited talk at the 1956 New York meeting of the APS. There he reported on both the counter experiment and on our annihilation event. He told me afterwards that the proof supplied by the annihilation event was an important ingredient in the minds of the audience. In fact,

in a subsequent interview with the press, my hand drawing of the first annihilation event was reproduced in "Time" magazine.[15]

Subsequently all the groups participating in this exposure found $\bar{p}$ events in their emulsion searches. We pooled our data and published our results as the "Antiproton Collaboration Experiment" 35 events and 18 authors![16] Fig. 7 shows the visible energy distribution for these 35 events in units of 2 M_p. About 2/3 of the events showed a visible energy release above 0.5 i.e., above M_p. Aside from proving that $\bar{p}$ annihilation occurs, we found many interesting properties of the annihilation process. When we were first looking for $\bar{p}$ events in emulsion some expectations were that we would see $\bar{p}p \rightarrow e^+e^-$ or $\bar{p}p \rightarrow \pi^+\pi^-$, so called "T events". This was certainly not the case. We found a surprisingly large pion multiplicity $\bar{N} = 5.3 \pm 0.4$ which, if one took Fermi's statistical model seriously, implied a rather large interaction volume of radius over 2 times the expected radius $(\hbar/m_\pi c) \approx$ 1 fermi.

Actually the high multiplicity is probably the result of the fact that meson resonances rather than individual particles are produced in the $\bar{p}$ annihilation process. But the discovery of meson resonances at the Bevatron came nearly 5 years later and are discussed in Luis Alvarez's talk at this conference.

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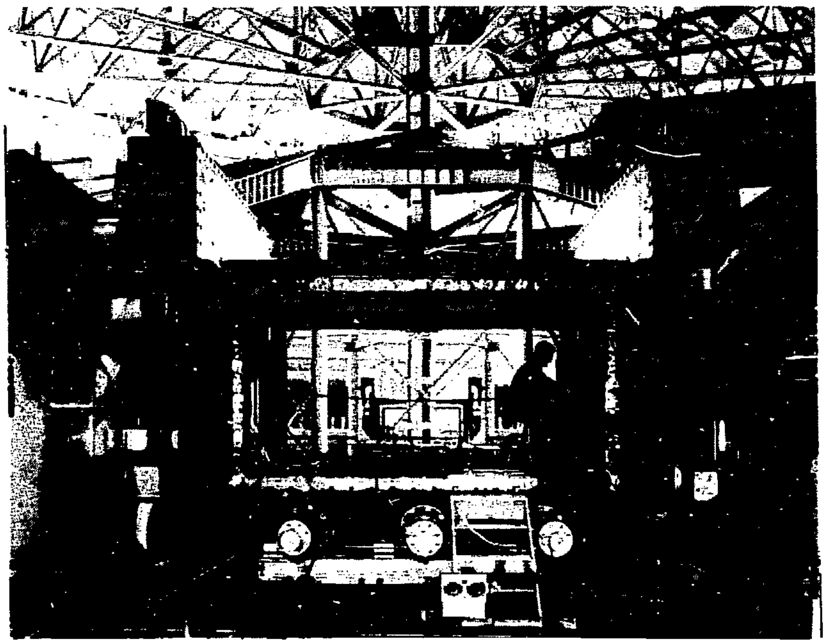
a)



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b)

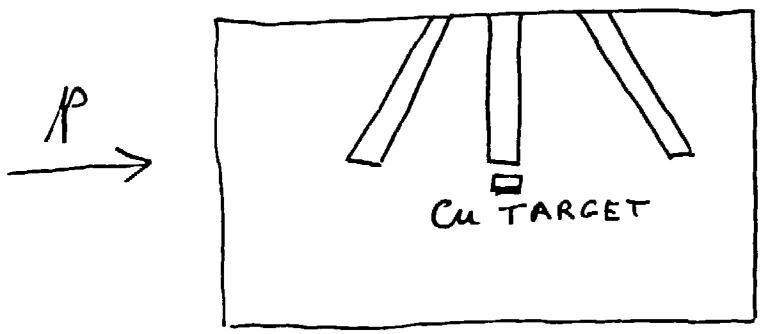

Fig. 1 a) The Bevatron vacuum tank. b) Sketch of the re-entrant wells.

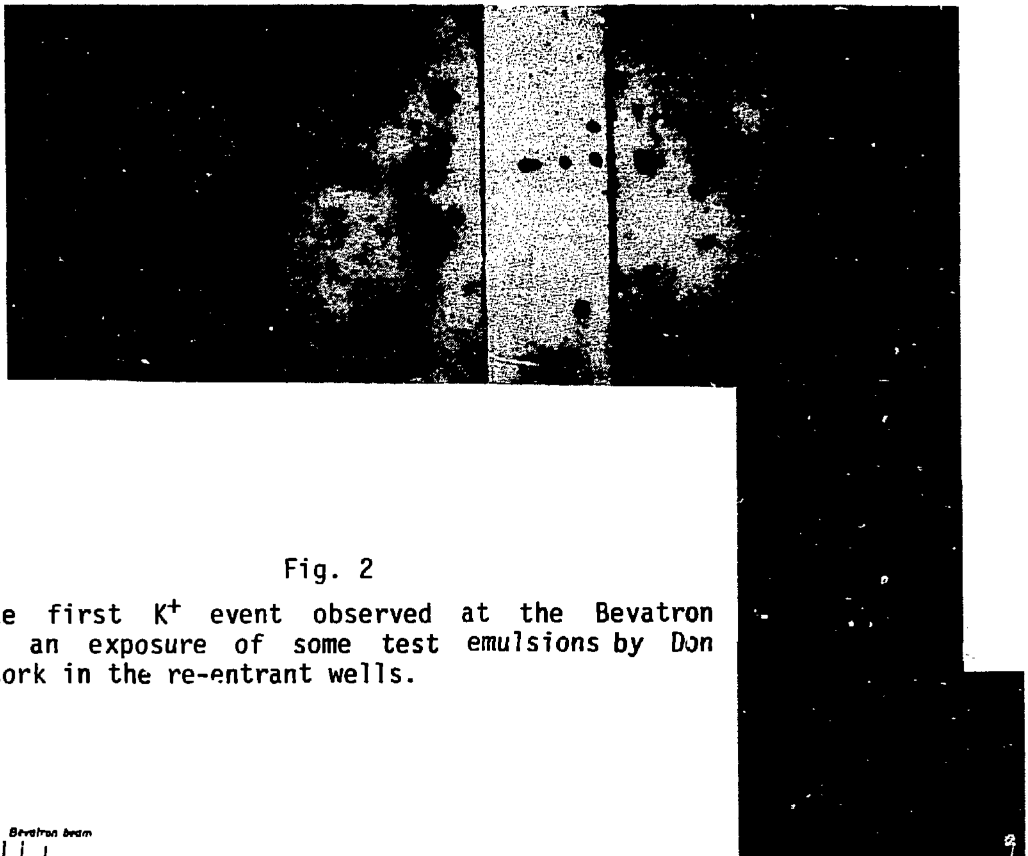

Fig. 2

The first K^+ event observed at the Bevatron in an exposure of some test emulsions by Don Stork in the re-entrant wells.

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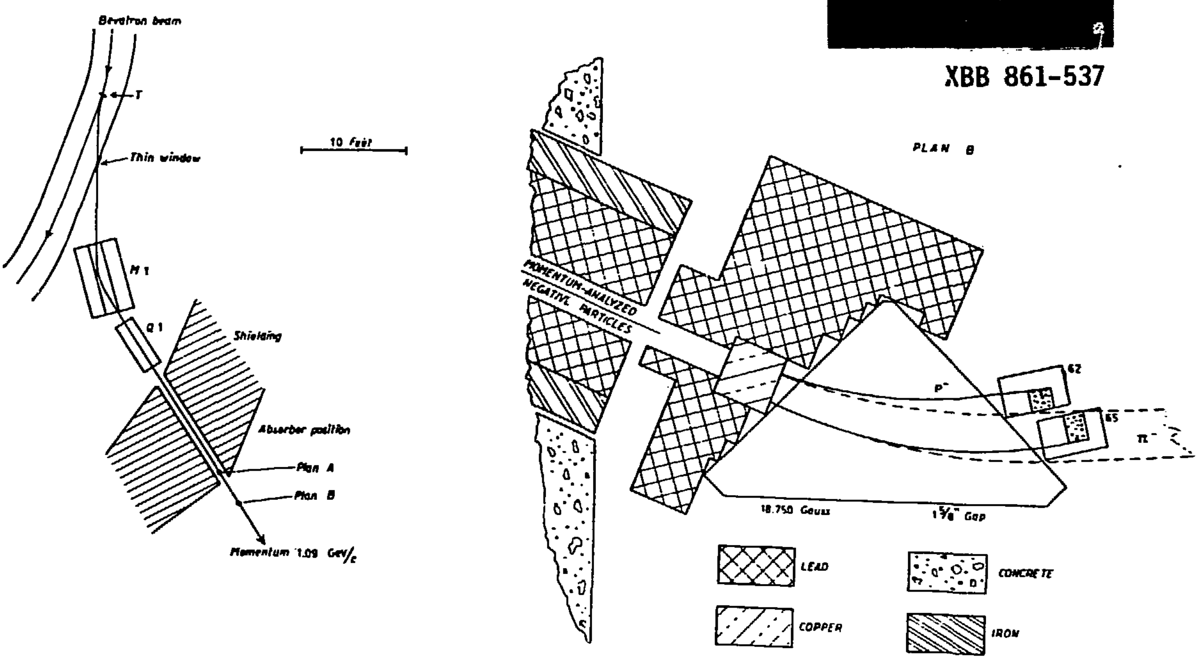

Fig. 4 The first emulsion exposure to $\bar{p}$'s at 1090 MeV/c.

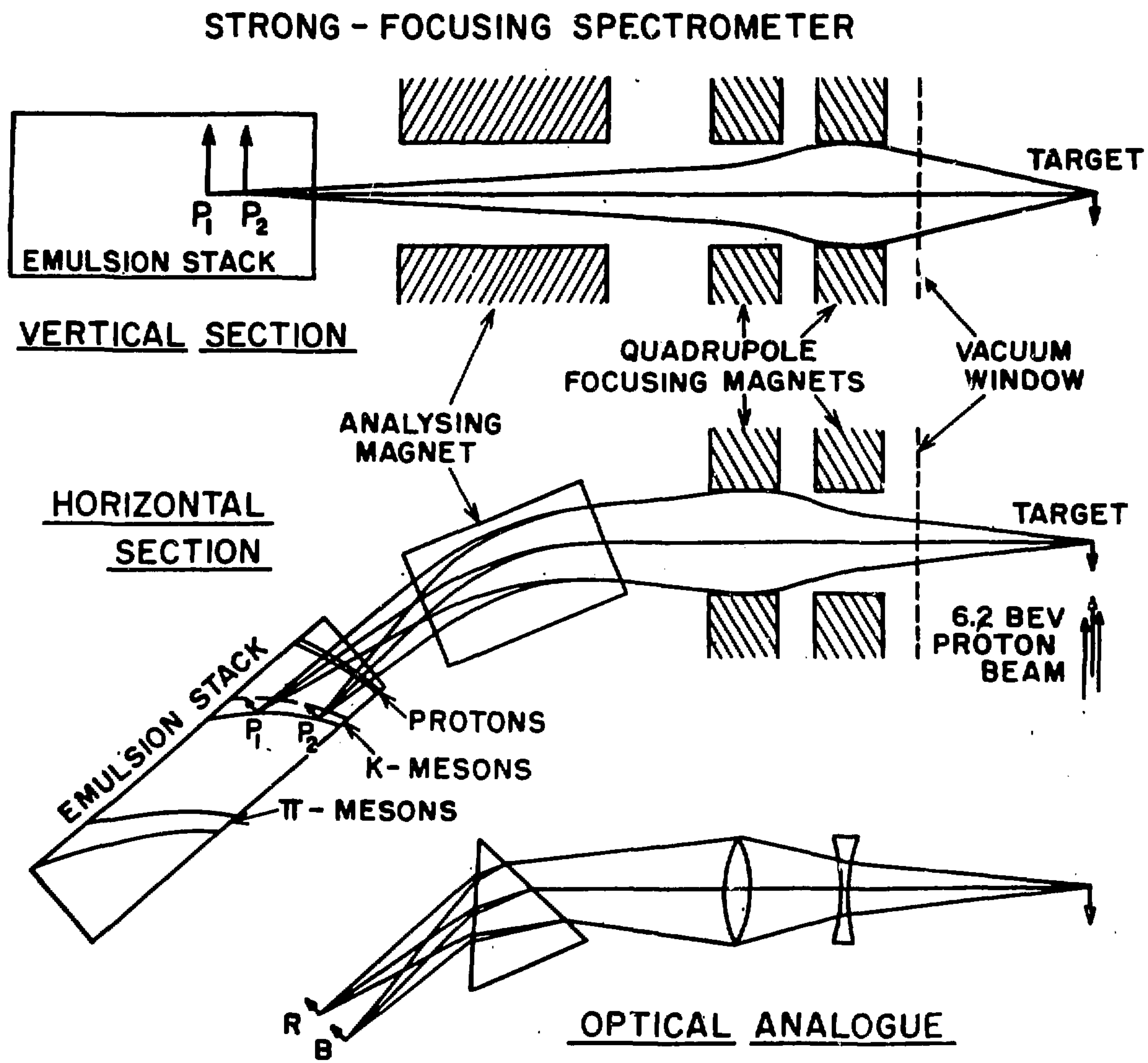

Fig. 3 The quadrupole lenses used in the focused K^+ beam.

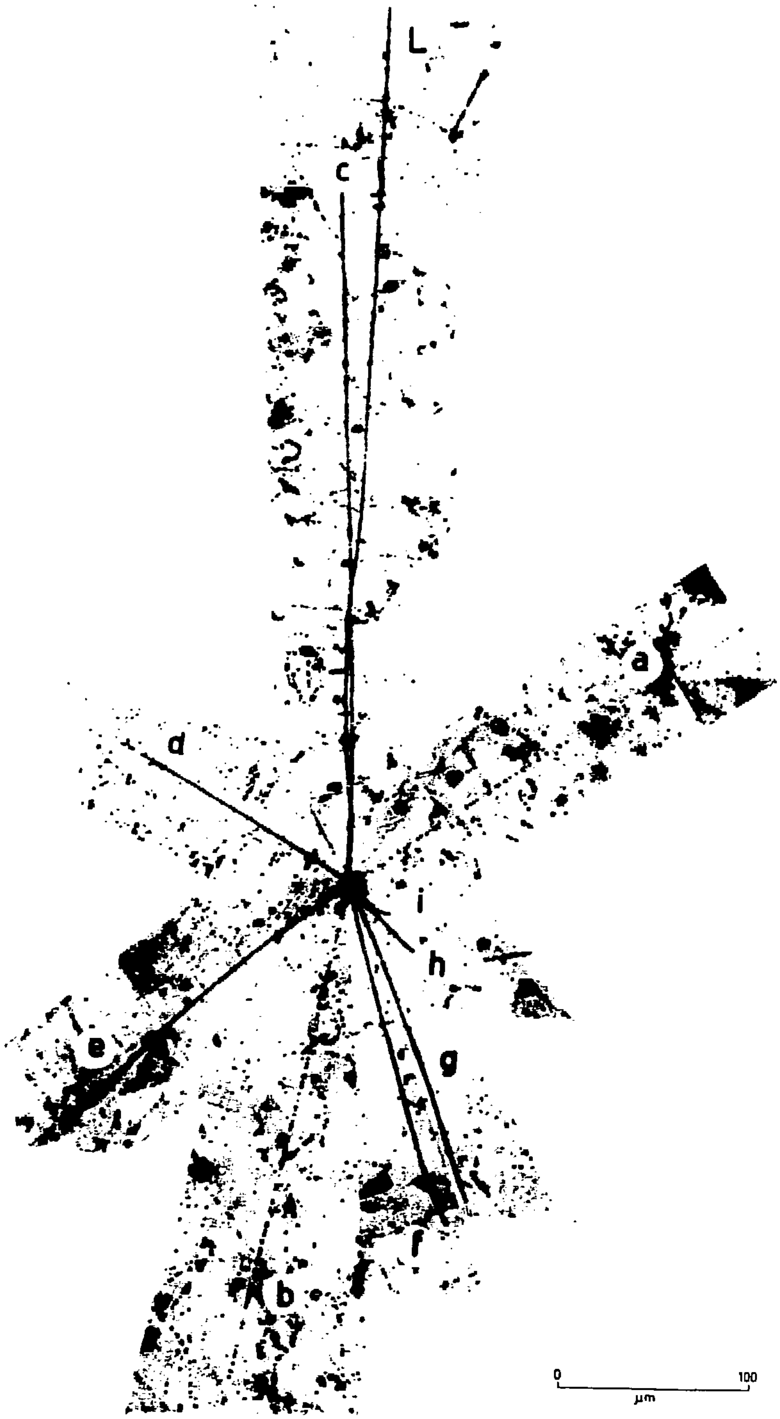

Fig. 5 Photo micrograph of first event in our emulsion exposure, found in Rome. The star. **L** indicates the incoming antiproton track. Tracks **a** and **b** are pions, and **c** is a proton. The remaining tracks could be protons or α-particles. (XBB 861-318)

Example of an Antiproton-Nucleon Annihilation

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(Received March 8, 1956)

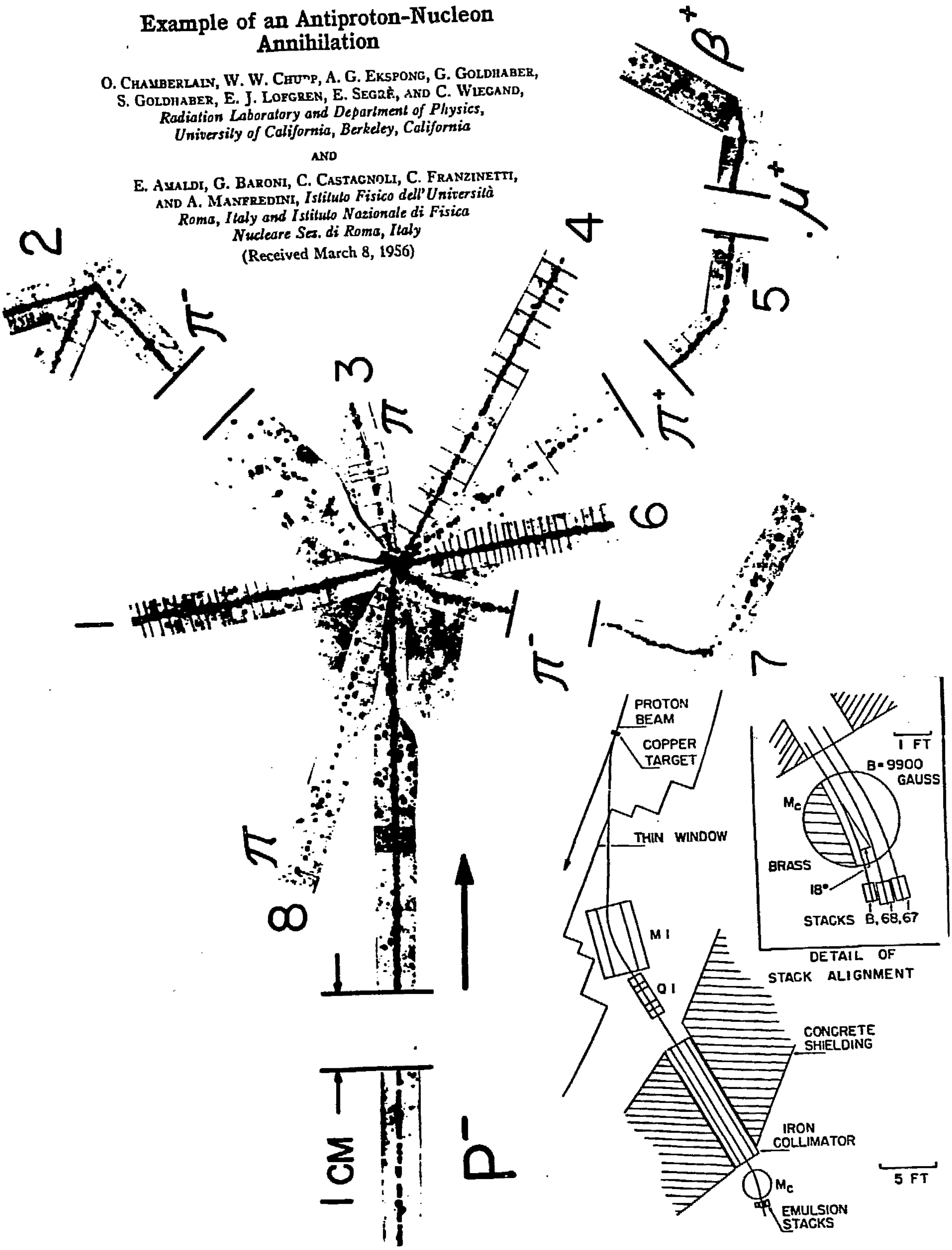

Fig. 6 Photo micrograph of first event found by "along the track" scanning in the second exposure. This event, which released 1300 ± 50 MeV of visible energy gave the conclusive proof for the annihilation process.

Antiproton-Nucleon Annihilation Process* (Antiproton Collaboration Experiment)

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(Received October 26, 1956)

Thirty-five antiproton stars have been found in an emulsion stack exposed to a 700-Mev/c negative particle beam. Of these antiprotons, 21 annihilate in flight and three give large-angle scatters ($\theta > 15°$, $T_{\bar{p}} > 50$ Mev), while 14 annihilate at rest. From the interactions in flight we obtain the total cross section for antiproton interaction: $\sigma_{\bar{p}}/\sigma_0 = 2.9 \pm 0.7$, where $\sigma_0 = \pi R_0^2$ and $R_0 = 1.2 \times 10^{-13} A^{\frac{1}{3}}$ cm. This cross section was measured at an average antiproton energy of $\bar{T}_{\bar{p}} = 140$ Mev.

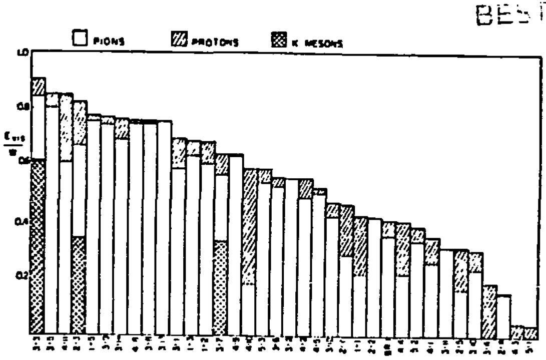
We also find that the antiproton-nucleon annihilation proceeds primarily through pion production with occasional emission of K particles. On the average 5.3 ± 0.4 pions are produced in the primary process; of these, 1 pion is absorbed and 0.3 inelastically scattered. From the small fraction of pions absorbed, we conclude that the annihilation occurs mainly at the surface of the nucleus at a distance larger than the conventional radius.

A total energy balance of particles emitted in the annihilation gives a ratio of charged to neutral pions consistent with charge independence. Conversely, assuming charge independence, we conclude that the energy going into electromagnetic radiation or neutrinos is small.

Comparisons with the Fermi statistical model and the Lepore-Neuman statistical model have been made. Good agreement with the experimental results on the annihilation process can be obtained through appropriate choice of the interaction volume parameters.

Several different estimates of the antiproton mass are in good agreement and suggest strongly that the antiproton mass is the same as the proton mass within an accuracy of 2½%.

A study of the elastic scattering of the antiprotons down to angles of 2° suggests a possible destructive interference between nuclear and Coulomb scattering.



Visible energy release in antiproton annihilation stars, expressed as a fraction of the available energy. The star reference number is given for each entry.

Fig. 7 Energy release for 35 $\bar{p}$ events observed in the "Antiproton Collaboration Experiment". Energy is given in units of the total available energy: 2 M_p.