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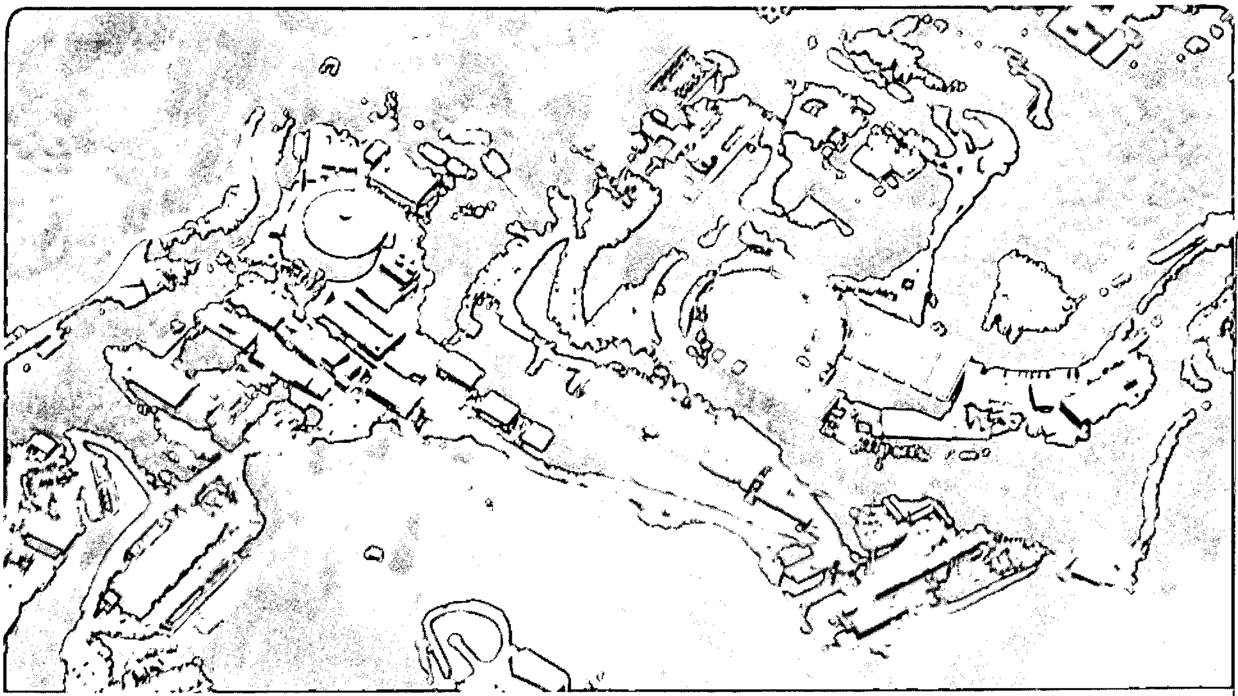
The Berkeley Programme

M. L. Stevenson

July 1993



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CONFERENCE ON THE BUBBLE CHAMBER AND ITS CONTRIBUTIONS TO PARTICLE PHYSICS
alias
"BUBBLES 40"

THE BERKELEY PROGRAMME

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14 - 18 July 1993 - CERN, Geneva Switzerland

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"Bubbles 40", The Berkeley Programme

28 September 1993

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1. Introduction

"The Berkeley Programme" of bubble chamber development was begun by the late Luis W. Alvarez after he met Don Glaser at the April 1953 Meeting of the American Physical Society in Washington DC. I will talk about the work of his group in developing liquid hydrogen chambers. Bill Fowler will speak about the work that he and Wilson Powell started at Berkeley with propane. You have already heard from Don Glaser of his work at Berkeley with the Xenon chamber that he brought with him from Michigan.

The content of my talk is contained mostly in "Alvarez, Adventures of a Physicist"[1], and "Discovering Alvarez,"[2] chapter 12 (that includes his 1968 Nobel Prize Lecture[3]). The two devices that I hold in my hands, the first glass bulb that showed tracks in liquid hydrogen, and one of the "coat hangers" from the 72"(84") chamber, span that era. Figure 1 is a visual summary of those chambers; (a photo taken Nov. '68) from left to right, 2, 4, 6, 10, 15 and 72 inch chambers. Their linear size grew at a rate that exceeded that of cyclotrons by a factor of two. Also shown are some of the people most responsible; Hernandez, Schwemin, Rinta, Watt, Alvarez and Eckman,. Nobel-Fig. 5. Not shown is the 25", the only chamber that used bellows expansion. All others used vapor expansion.

Figure 1 (XBB 680-6898)

2. The beginning

The stage was set for the "Berkeley Programme" by Alvarez and Panofsky when they began the construction of the 30 MeV proton linear accelerator. Figure 2 shows them holding an RF probe to measure the resonant cavity they had built out of war surplus radar tubes. They had earlier worked together at Los Alamos.

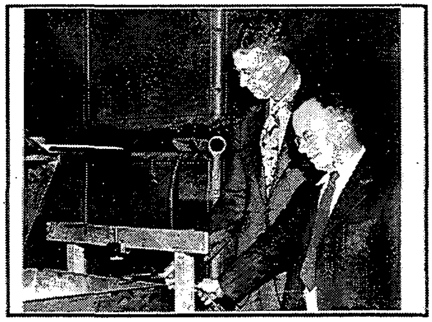
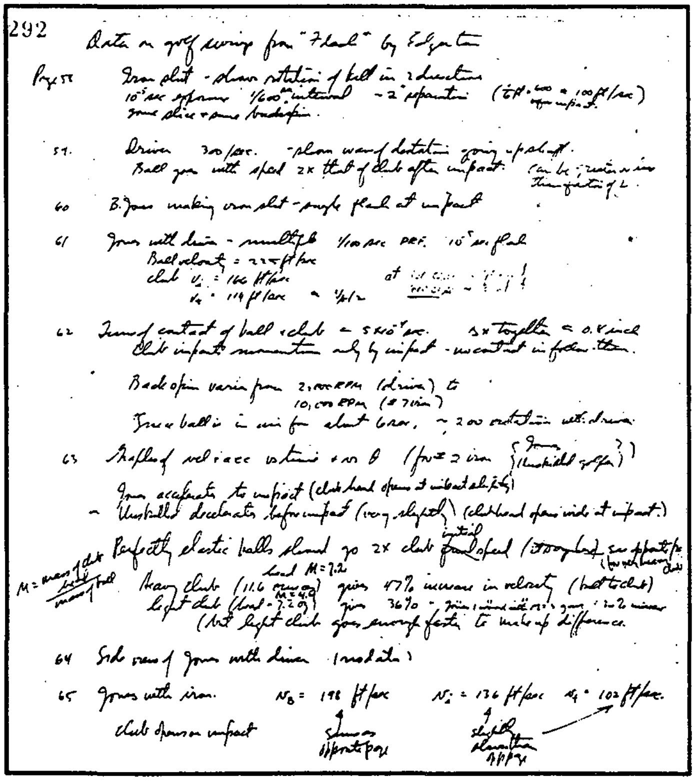


Figure 2 XBB 732-1076

After the Linac was completed and became operational, Panofsky parted from the Linac group to begin his famous pion capture experiments on proton and deuteron targets at the 184-inch synchro-cyclotron next door. Luie and his Linac group, Don Gow, Bob Watt, Jack Frank, and Hugh Bradner began working on a classified project called the Materials Test Accelerator (MTA) in a huge warehouse to the east of the Berkeley hills in the sleepy little town of Livermore. At that time the country was dependent on foreign sources of Uranium and needed a backup source of fissionable material. The purpose of MTA was to produce the fissionable isotope U^{233} from neutron bombardment of Thorium. A huge Linac was to accelerate deuterons that would, in turn, be stripped to produce the neutron beam. The MTA's vacuum tank was 60 feet in diameter! After the first section was completed (for a cost of $20 million) and under test, Uranium was discovered in the western United States. MTA became unnecessary. Luie and his Linac team were already designing and building big gadgets. Don Gow was the "Chief of Staff" of this extremely talented group. If Uranium had been discovered two years later I would not be standing before you now.

In 1952 Luis Alvarez returned from the MTA project He inherited two graduate students from Herb York (one of the discoverers of the π^0), who was to become Livermore Lab's first administrator, with Edward Teller as director. Herb, in turn, had inherited them from Panofsky, who had left Berkeley because of the "Loyalty Oath". Those two "Panofsky orphans" were Frank Crawford and myself. We had been the first to use electronic, rather than visual methods to detect pions at the 184" synchro-cyclotron, via $p + p \rightarrow \pi^+ + d$. Here, we had used liquid hydrogen targets and had developed a healthy respect for the dangers involved. Our office was room #226 at the southwest corner of the second floor of Building 50. It was later to be known as the "bullpen" as the group grew larger when his "linac group", Don Gow, Bob Watt, Hugh Bradner, and Jack Frank rejoined Luie from the classified MTA project.



292 Data on golf swings from "Flash" by Edgerton

Page 58 Iron shot - shows rotation of ball in 2 directions
10^{-5} sec exposure 1/600" interval ~2" separation ($6ft \cdot 600 = 100 ft/sec$ [illegible])
some slice + some backspin.

59. Driver 300/sec. - shows wave of distortion going up shaft.
Ball goes with speed 2x that of club after impact. (can be [illegible] in [illegible] factor of 2.

60 B. Jones making iron shot - single flash at impact

61 Jones with driver - multiple 1/100 sec PRF. 10^{-5} sec flash
Ball velocity = 225 ft/sec
club v_i = 166 ft/sec at [illegible]
v_f = 114 ft/sec [illegible]

62 Time of contact of ball + club ≈ 5×10^{-4} sec. 5× together ≈ 0.8 inch
Club imparts momentum only by impact - no contact in follow through.

Back spin varies from 2,000 RPM (driver) to
10,000 RPM (#7 iron)
Since ball is in air for about 6 sec, ~200 rotations with driver.

63 Profile of vel + acc vs time + vs θ (for #2 iron {Jones / Unskilled golfer?})
Jones accelerates to impact (club head opens at [illegible])
- Unskilled decelerates before impact (very slightly) (clubhead opens wide at impact.)
Perfectly elastic balls should go 2x club initial speed ([illegible]) see opposite page (for infinitely heavy club)
M = mass of club head / mass of ball
head M=7.2
Heavy club (11.6 ounces, M=4.0) gives 47% increase in velocity (ball to club)
light club (head - 7.2 oz) gives 36% - [illegible]
(but light club goes enough faster to make up difference.

64 Side view of Jones with driver (no data)

65 Jones with iron. v_B = 198 ft/sec v_i = 136 ft/sec v_f = 102 ft/sec.
club opens on impact Jones opposite page slightly decelerates [illegible]

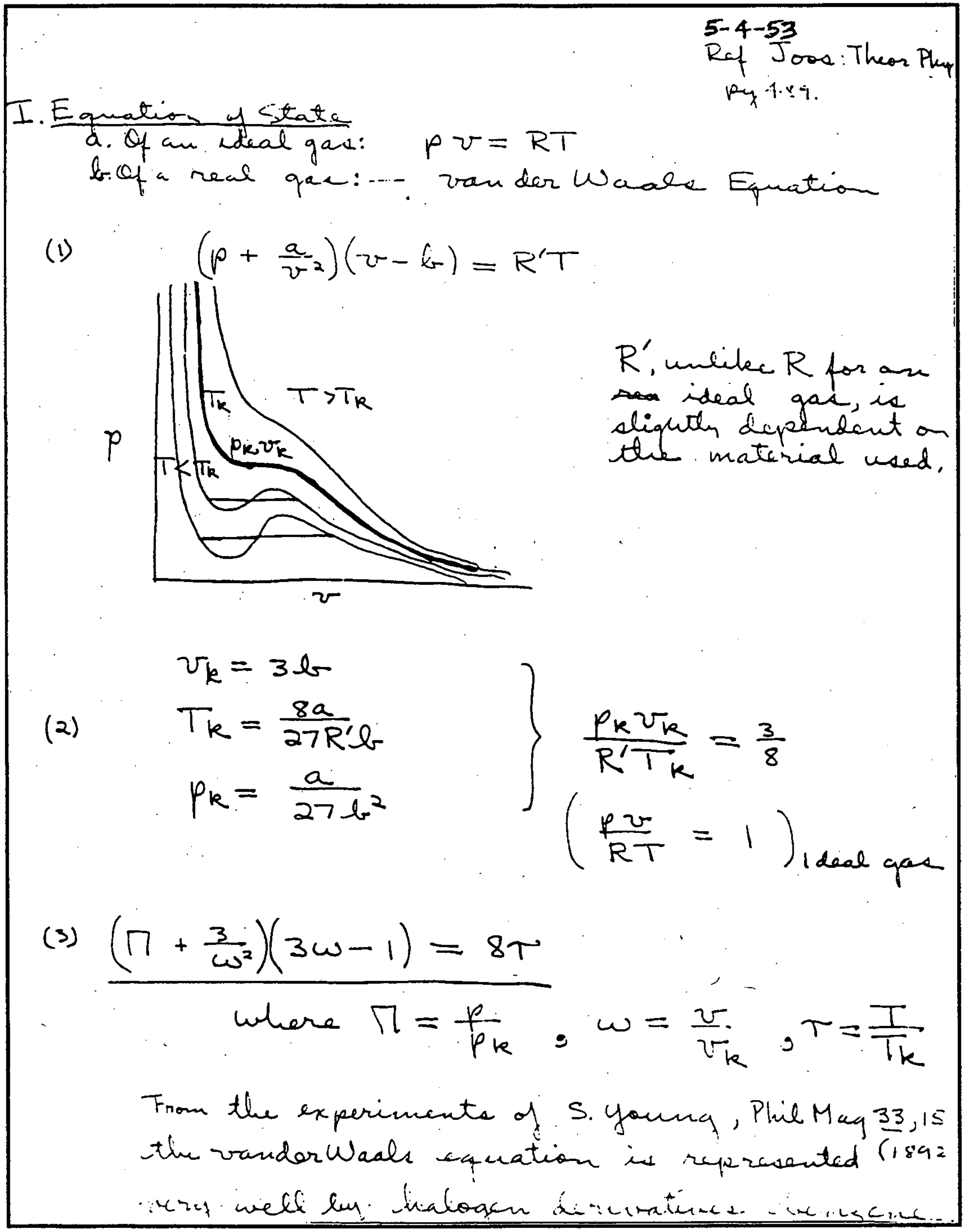
Figure 3, LWA-Notebook #16,page 292, 1952

Luie hit the ground running. His notebooks #16 (Oct. '51 to Feb. '53), #17 (Feb. '53 to .Oct. '53) record the level of his activity in transforming from classified research back to particle physics research. I have always enjoyed looking at Luie's notebooks with his confident handwriting that showed him thinking while writing. It is like studying Enrico Fermi's beautiful "Notes on Quantum Mechanics", in his own handwriting. It also displays the added dimension of Fermi thinking as he writes.

Figure 3 shows that Alvarez was interested in improving his golf game using electronics of Edgerton and his flash tubes. He delivered a golf machine that he designed and built to the new President Eisenhower while attending the April 1953 meeting of the American Physical Society in Washington DC, where he met Don Glaser.

An added bonus of Luie's contact with Edgerton was that we were able to get one of Edgerton, Germeshausen and Grier (EGG)'s fast oscilloscopes, that were generally only available for weapon work, for our counter experiments at the 184-inch cyclotron and Bevatron. It is interesting to note that EGG is one of the larger contractors at the Superconducting Super Collider (SSC).

Luie called me Sunday, May 3, 1953 after he had returned from his meeting with Don Glaser in Washington. It was obvious to all of us that we wanted to build a liquid hydrogen chamber. The next day I began a new notebook (MLS Bubble Chamber.)



5-4-53
Ref Joos: Theor Phys
pg 1.99.

I. Equation of State
a. Of an ideal gas: $pv = RT$
b. Of a real gas: --- van der Waals Equation

(1) $\left(p + \frac{a}{v^2}\right)(v - b) = R'T$

R', unlike R for an ideal gas, is slightly dependent on the material used.

(2) $v_k = 3b$
$T_K = \frac{8a}{27R'b}$
$p_K = \frac{a}{27b^2}$

$\frac{p_K v_K}{R' T_K} = \frac{3}{8}$

$\left(\frac{pv}{RT} = 1\right)_{\text{ideal gas}}$

(3) $\left(\Pi + \frac{3}{\omega^2}\right)(3\omega - 1) = 8\tau$

where $\Pi = \frac{p}{p_K}$, $\omega = \frac{v}{v_K}$, $\tau = \frac{T}{T_K}$

From the experiments of S. Young, Phil Mag 33, 15 (1892
the vanderWaals equation is represented
very well by halogen derivatives [illegible]

Figure 4

On the first page (Figure 4 above) is the Van der Waals equation of state in reduced (or universal) variables. MLS Bubble Chamber Notebook, page 1, 4 May 1953.

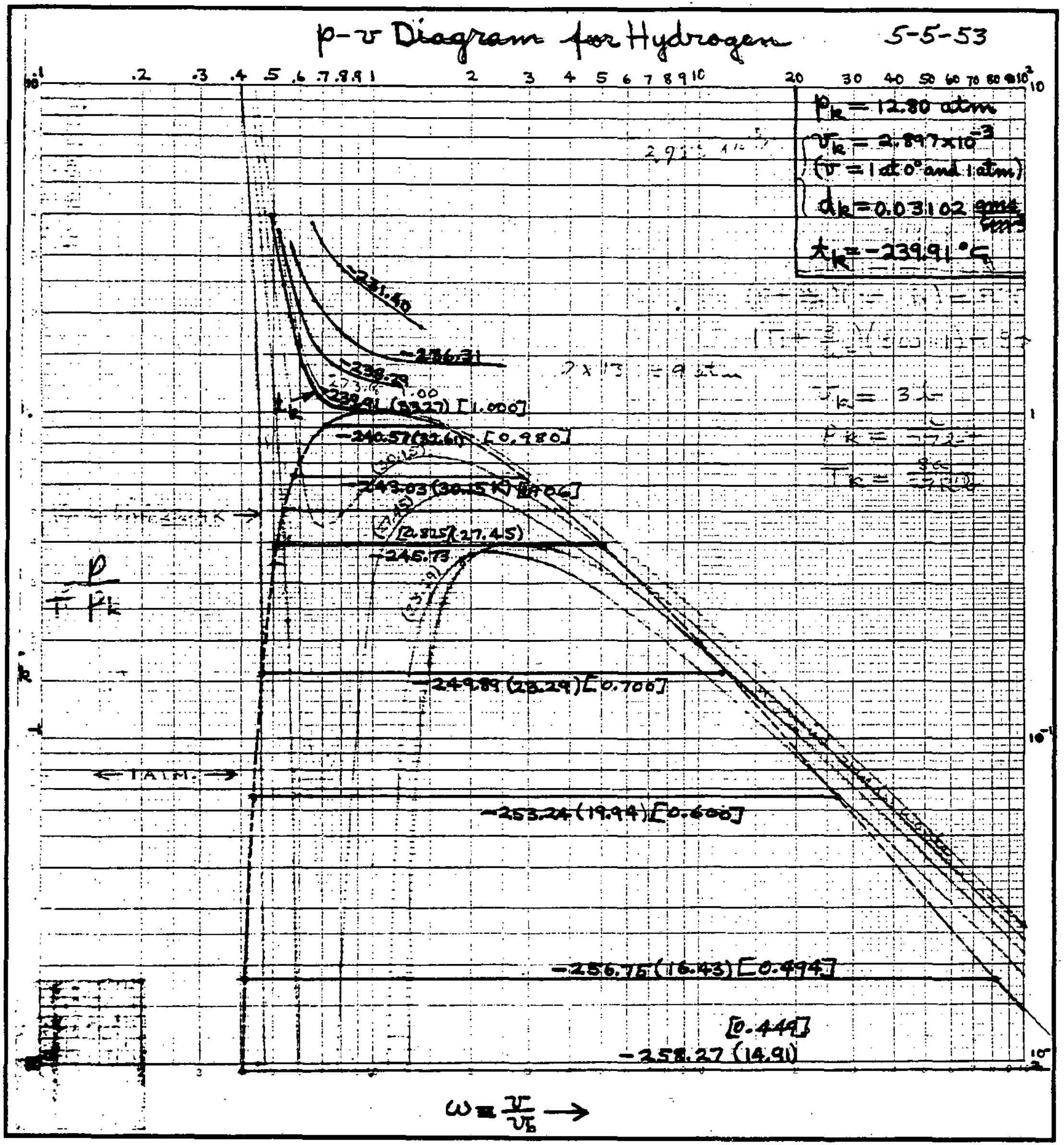
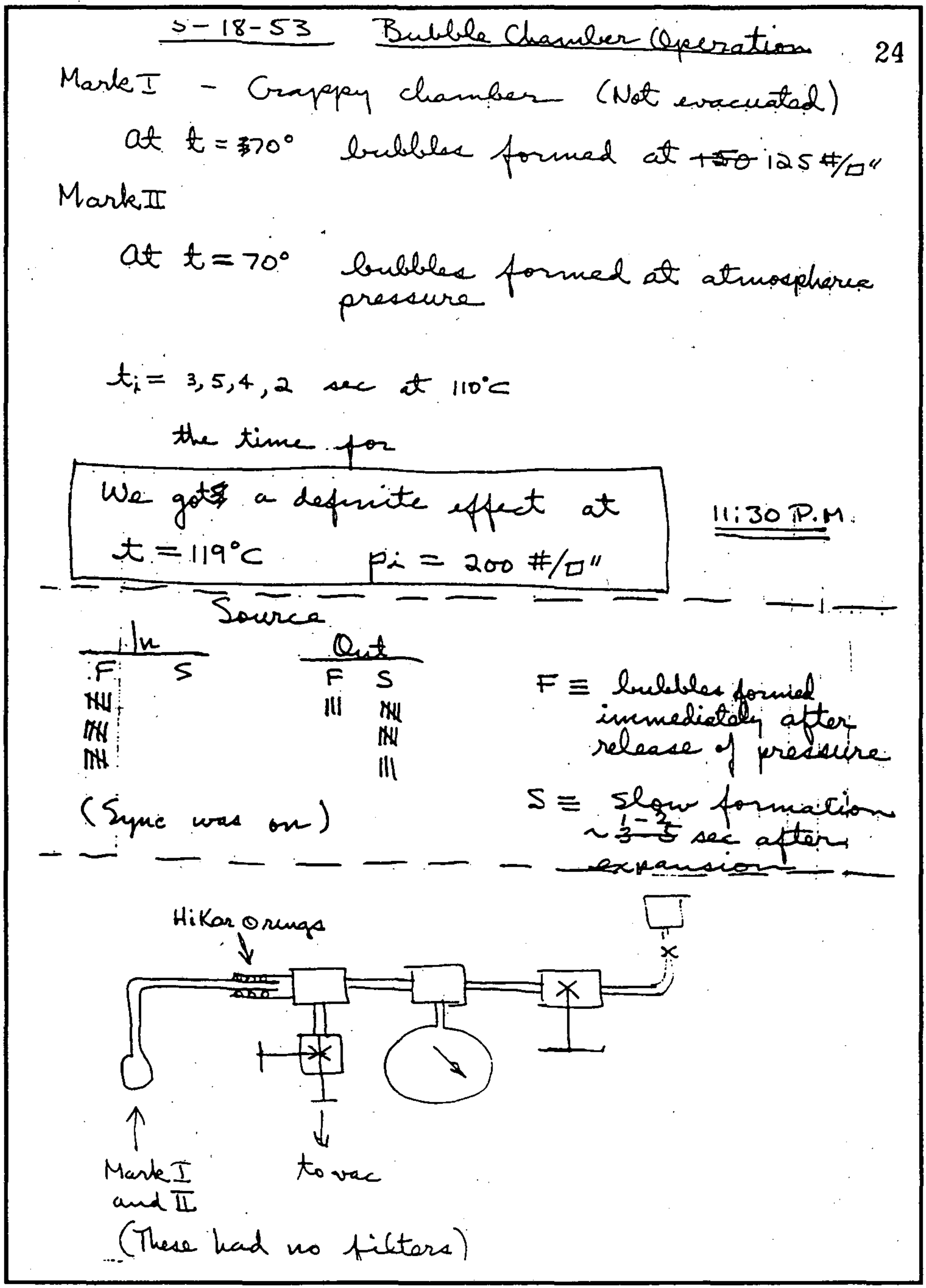


Figure 5

Appended to that page (Figure 5 above) is my "p-v Diagram for Hydrogen", dated 5-5-53.

Frank and I moved into the student shop at the electron synchrotron and enlisted the help of A. J. (Pete) Schwemin and John Wood, two of its talented technicians, to build an ethyl ether chamber in order to repeat Glaser's work.

Just two weeks after starting, we successfully tested the system outside (for safety reasons), in the rain.



5-18-53 Bubble Chamber Operation 24

Mark I - Crappy chamber (Not evacuated)

at t = 70° bubbles formed at ~~150~~ 125 #/□"

Mark II

at t = 70° bubbles formed at atmospheric pressure

t_i = 3, 5, 4, 2 sec at 110°C

the time for

We got a definite effect at
t = 119°C p_i = 200 #/□"

11:30 P.M.

Source

In F	In S	Out F	Out S
𝍸		|||	𝍸
𝍸			𝍸
𝍸			|||

(Sync was on)

F ≡ bubbles formed immediately after release of pressure

S ≡ Slow formation ~ ~~3-5~~ 1-2 sec after expansion

HiKar O rings

Mark I and II

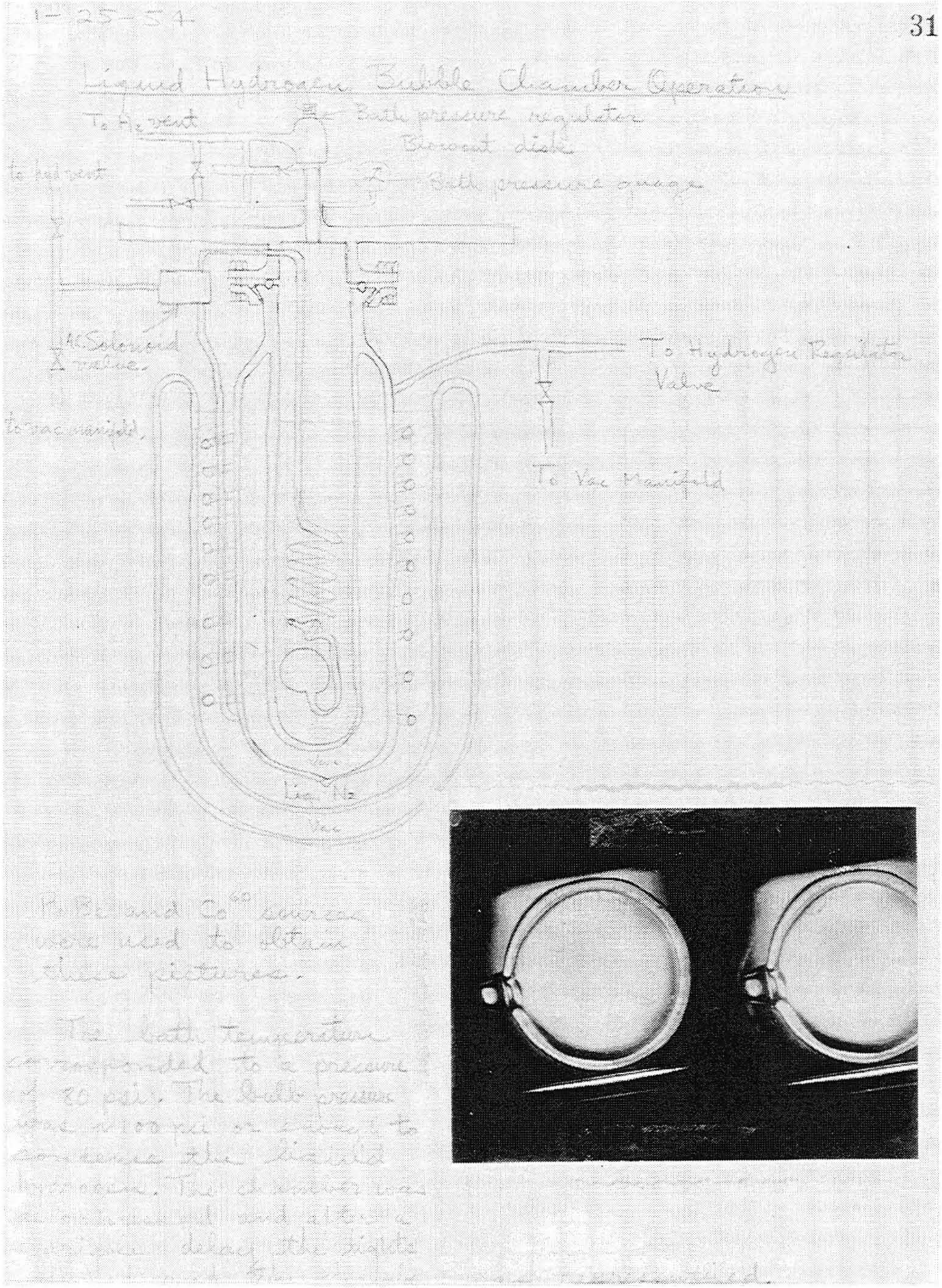
to vac

(These had no filters)

Figure 6. MLS Bubble Chamber Notebook, page 24, May 18, 1953

From the statistics of "Fast" versus "Slow" bubble formation with the radioactive source "In" versus "Out" was the closest we ever came to reproducing Glaser's work. Note that our "Slow" was defined as the time of 1 to 2 seconds that the ether remained superheated. We never reached the long times that Glaser did. We just weren't careful enough.

John Wood began building a chamber, following the design of Hildebrand and Nagle[4], that would work first on Nitrogen and then on Hydrogen. Five months later we had failed to prove that the chamber worked on Nitrogen. Finally, John Wood, on January 25, 1954, successfully photographed tracks in Hydrogen[5].



1-25-54

31

Liquid Hydrogen Bubble Chamber Operation

Po-Be and Co^{60} sources were used to obtain these pictures.

The bath temperature corresponded to a pressure of 80 psi. The bulb pressure was ~100 psi or enough to condense the liquid hydrogen. The chamber was [illegible] expanded and after a [illegible] delay the lights [illegible]

[illegible] reconnected.

Figure 7 MLS Bubble Chamber Notebook, page 31, January 25, 1954 shows on this date a sketch of this chamber, along with two Polaroid pictures of tracks.

We noticed an unusual phenomenon in some of the pictures. There were tracks in the liquid of a partially filled chamber! It implied that there could be boiling on the walls, or even a vapor phase, and the main volume of liquid would still be superheated and capable of producing tracks.

Schwemin, together with Doug Parmentier, then launched a vigorous program of building chambers with metal bodies and glass windows that culminated 10 months later with a 4-inch chamber being placed in a 10 MeV π^+ beam at the 184" cyclotron.

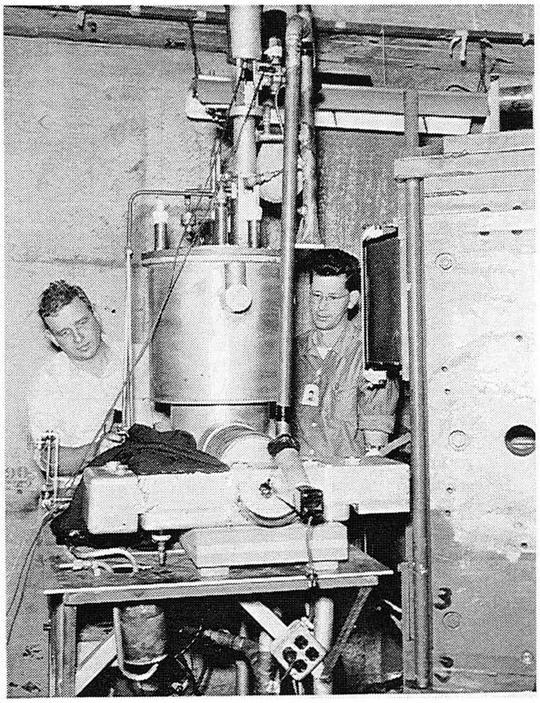


Figure 8 GPR-1348

Safety was constantly on our minds. Our checklist for the 4-inch chamber had 43 items that had to be completed before liquid hydrogen could even be brought into the building, and 10 more before the chamber was allowed to operate.

Figure 8 shows the 4-inch chamber system, with Doug Parmentier (left), and Pete Schwemin (right). in the "Cave" of the 184' cyclotron. Figure 9 shows Schwemin holding the 4-inch and Watt pointing.

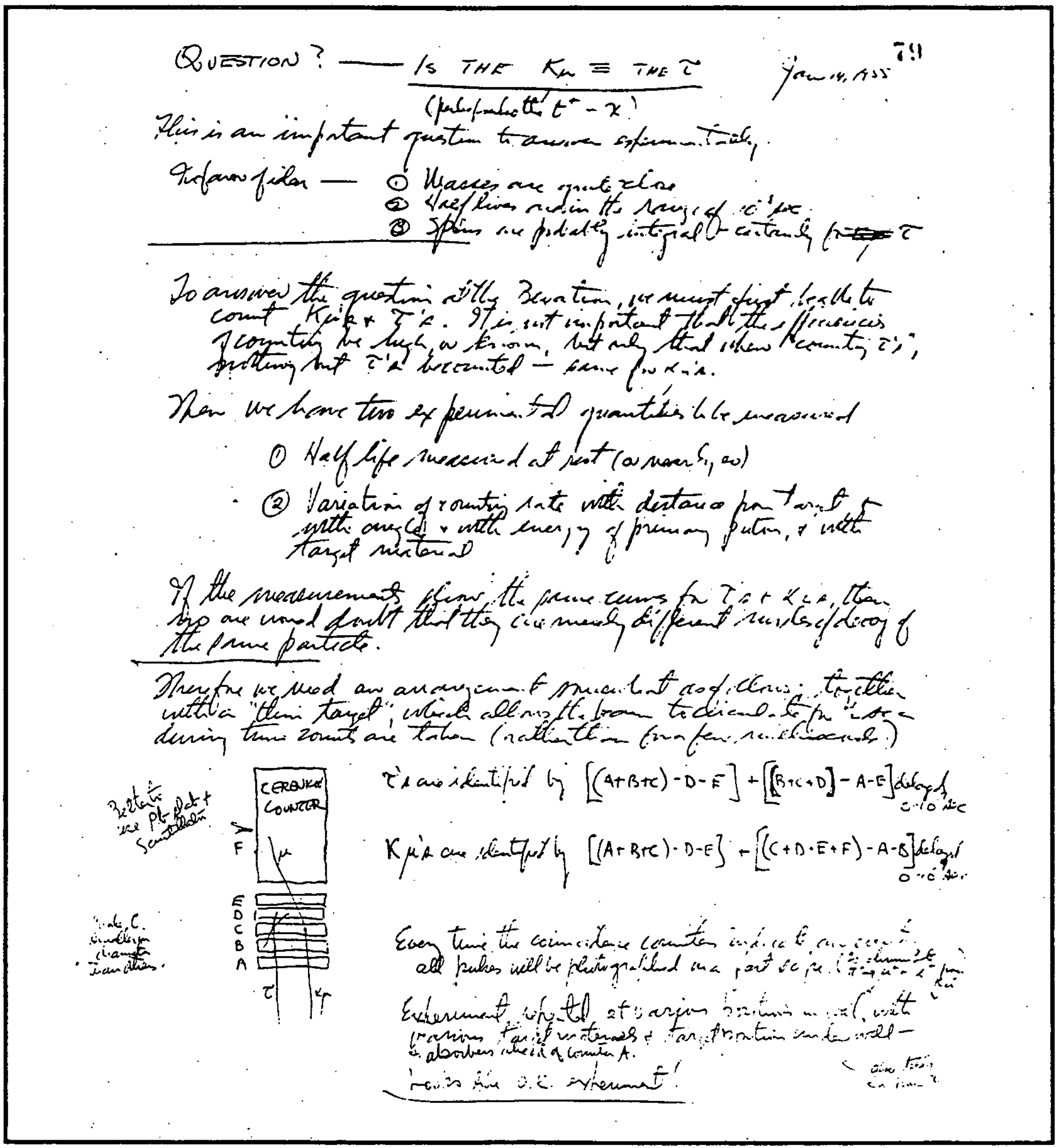
Figure 9 XBB 680-6895

I would define this time, November of 1954, as the end of the Beginning. Up to this time, Luie, Frank and I were also fully involved in doing counter experiments.

3. Our Counter Experiment Program

On May 18, 1953, three weeks after his historic meeting with Don Glaser, we heard an interesting talk by a theorist at Berkeley asserting that the pion cloud surrounding the proton would enhance the gamma ray-proton cross section by a factor of ten. We launched a vigorous program that ultimately proved the theorist wrong. We had added an engineer, Dick Blumberg to the "bullpen" to help with that project and to start designing for the 10-inch chamber that would be used at the Bevatron. Bud Good had joined us as Frank and I repeated the measurement of $p + p \rightarrow \pi^+ + d$, this time with a highly polarized proton beam.

I recall vividly the day Luie's distinguished old friend, Le Prince Ringuet, visited the "bullpen. He summarized for us the status of the strange particle data from both emulsions and cloud chambers. Some of the particles had masses within about 10 Mev of each other. Two of them would become known as the tau and theta mesons. Luie's comment, after the summary, was, "They must all be the same particle!" Thus began our measurement of the tau and theta meson lifetimes on January 14, 1955, a month after starting the design of the 10-inch chamber.. See Figure 10



QUESTION? —— IS THE $K_\mu \equiv$ THE τ Jan 14, 1955   79

(preferably the $\tau^+ - \chi$)

This is an important question to answer experimentally.

In favor of idea — ① Masses are quite close
② Half lives are in the range of 10^{-8} sec.
③ Spins are probably integral — certainly for τ

To answer the question with the Bevatron, we must first be able to count K_μ's + τ's. It is not important that the efficiencies of counting be high or known, but only that when counting τ's, nothing but τ's be counted — same for K_μ's.

Then we have two experimental quantities to be measured

① Half life measured at rest (or nearly so)

② Variation of counting rate with distance from target + with angle + with energy of primary proton, + with target material

If the measurements show the same curves for τ's + K_μ's, then [illegible] one would doubt that they are merely different modes of decay of the same particle.

Therefore we need an arrangement such that as follows: together with a "thin target", which allows the beam to circulate for [illegible] during time counts are taken (rather than for a few microseconds.)

Better use Pb plate + scintillator

CERENKOV COUNTER

F

μ

E
D
C
B
A

τ

K_μ

[illegible] scintillation [illegible]

τ's are identified by $[(A+B+C)-D-E] + [[B+C+D]-A-E]_{\text{delayed}}$ 0.10 μsec

K_μ's are identified by $[(A+B+C)-D-E] + [(C+D+E+F)-A-B]_{\text{delayed}}$ 0.10 μsec

Every time the coincidence counters indicate an event, all pulses will be photographed on a fast scope. ([illegible])

Experiment repeated at various positions [illegible], with various target materials + target positions in the well — + absorbers ahead of counter A.

[illegible] the O.C. experiment!

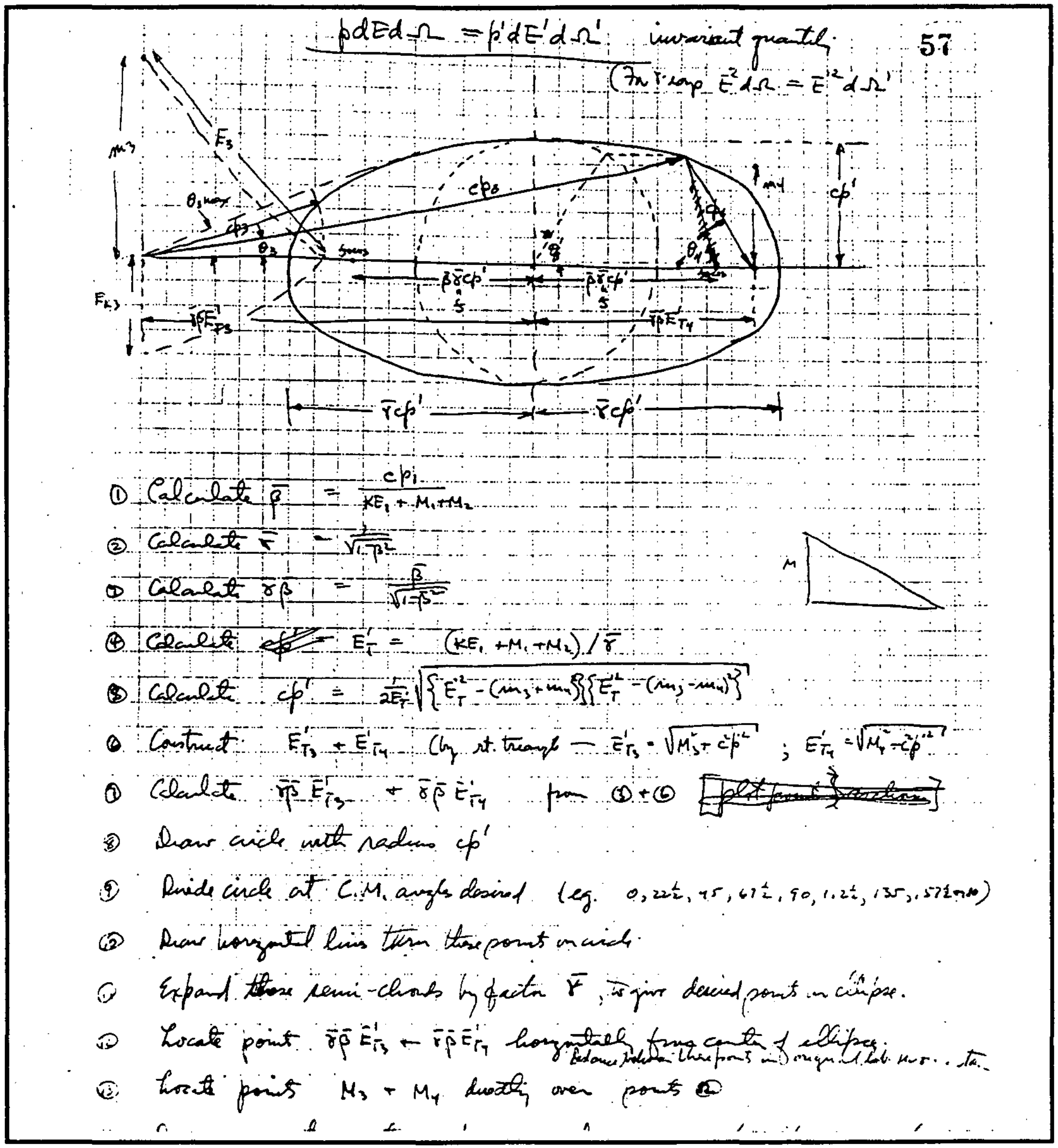
< also [illegible]

Figure 10 shows Luis Alvarez's design of the tau-theta meson lifetime experiment. (LWA-Notebook #18 , page 79, January 14, 1955)

Bud, Frank and I were exhausting ourselves trying to keep up with Luie's ideas, both with counters and bubble chambers. We needed help!

Since Enrico Fermi had taught an undergraduate quantum mechanics course at Berkeley one summer, we had idolized him. We were also very impressed with the quality of his graduate students. We convinced Luie to offer Fermi's last student, Art Rosenfeld, a position at Berkeley. Not only did Art accept, but he brought his good friend and fellow student, Frank Solmitz with him. Solmitz was a theoretical physics student of Gregor Wentzel. His part of the story, data processing, will be told by Rudi Bock.

We borrowed one of Wilson Powell's cloud chamber magnets for the 10-inch chamber. It had the disadvantage of restricting the stereo angle and compromising the accuracy of depth measurements. But with it came the advantage that the human eyes could view two stereo pictures, placed side-by-side, and see a three dimensional image. Luie had the ability to scan the pictures side-by-side and view them as three dimensional images. Most of us could not do this. We needed special glasses to do what Luie could do unaided. I have brought with me a set of those special glasses and some of the stereo pairs from that chamber.



$p\,dE\,d\Omega = p'\,dE'\,d\Omega'$ invariant quantity 57

(For 1-comp $E^2\,d\Omega = E'^2\,d\Omega'$)

① Calculate $\bar{\beta} = \frac{cp_1}{KE_1 + M_1 + M_2}$

② Calculate $\bar{\gamma} = \frac{1}{\sqrt{1-\bar{\beta}^2}}$

③ Calculate $\bar{\gamma}\bar{\beta} = \frac{\bar{\beta}}{\sqrt{1-\bar{\beta}^2}}$

④ Calculate ~~cp'~~ $E_T' = (KE_1 + M_1 + M_2)/\bar{\gamma}$

⑤ Calculate $cp' = \frac{1}{2E_T'}\sqrt{\{E_T'^2 - (m_3+m_4)^2\}\{E_T'^2 - (m_3-m_4)^2\}}$

⑥ Construct $E_{T_3}' + E_{T_4}'$ (by rt. triangle — $E_{T_3}' = \sqrt{M_3^2 + c^2p'^2}$; $E_{T_4}' = \sqrt{M_4^2 + c^2p'^2}$

⑦ Calculate $\bar{\gamma}\bar{\beta}E_{T_3}' + \bar{\gamma}\bar{\beta}E_{T_4}'$ from ③ + ⑥ ~~[plot point [illegible]]~~

⑧ Draw circle with radius cp'

⑨ Divide circle at C.M. angles desired (e.g. 0, 22½, 45, 67½, 90, 112½, 135, 157½ & 180)

⑩ Draw horizontal lines thru those points on circle

⑪ Expand these semi-chords by factor $\bar{\gamma}$, to give desired points on ellipse.

⑫ Locate point $\bar{\gamma}\bar{\beta}E_{T_3}' + \bar{\gamma}\bar{\beta}E_{T_4}'$ horizontally from center of ellipse. Between these points [illegible] original lab. [illegible]

⑬ Locate points $M_3 + M_4$ directly over point ⑫

Figure 11
Luis Alvarez learned about the Blaton ellipse method. (It may be the only thing that Frank and I taught him) (LWA-Notebook #18, page 57, November 1953)

3. Alvarez's proton linac group (Don Gow, Bob Watt, Hugh Bradner, Jack Frank) join the effort ("they come in out of the cold" of doing classified work on MTA).

The next part of the story should be told by Bob Watt, who knows more important facts than anyone about the development of the 10-inch, 15-inch, and 72-inch chambers.(The 72-inch later became the 84-inch at SLAC). Unfortunately, ill health prevents his attending this conference. Paul Hernandez, the chief engineer, who could also speak about this period, is also unable to attend.

Hardly had the design of the 10-inch been completed, than we were using the Blaton ellipse method of representing the relativistic momenta of the final state Vees of associated production[6],

$\pi^- + p \to \Lambda + K^0$.

This method allowed us to plot the mean decay positions of the Λ and K^0 's and helped us to determine the optimum chamber size.

I will let Luie's Nobel lecture (in italics) tell of the development of the 72-inch. My interjections will be enclosed in boxed paragraphs.

(L. W. Alvarez, Les Prix Nobel en 1968, pg. 14-17)

> *"The size of the "big chamber" was set by several different criteria, and fortunately all of them could be satisfied by one design. By studying the relativistic kinematics of strange particles produced by Bevatron beams, and more particularly by studying the decay of these particles, I convinced myself that the big chamber should be rectangular, with a length of at least 30 inches. This length was next increased to 50 inches in order that there would be adequate amounts of hydrogen upstream from the required decay region, in which production reactions could take place. Later the length was changed to 72 inches, when it was realized that the depth of the chamber could properly be less than its width and that the change could be made without altering the volume. The production region corresponded to about 10 % of a typical pion-proton mean free path, and the size of the decay region was set by the relativistic time-dilated decay lengths of the strange particles, plus the requirement that there be a sufficient track length available in which to measure magnetic curvature in a "practical magnetic field" of 15,000 gauss.*
>
> *In summary, then, the width and depth of the chamber came rather simply from an examination of the shape of the ellipses that characterize relativistic transformations at Bevatron energies, plus the fact that the magnetic field spreads the particles across the width but not along the depth of the chamber.*
>
> *The result of this straightforward analysis was a rather frightening set of numbers: The chamber length was 72 inches; its width was 20 inches, and its depth was 15 inches. It had to be pervaded by a magnetic field of 15,000 gauss, so its magnet would weigh at least 100 tons and would require 2 or 3 megawatts to energize it.*

Physics Motivation for "72-inch" chamber (Blaton ellipses for, $\pi^- + p \to \Lambda + K^0$)
Luis Alvarez, "The Bubble Chamber Program at UCRL" (18 April 1955), Fig. 5, page 339, Peter Galison[7]

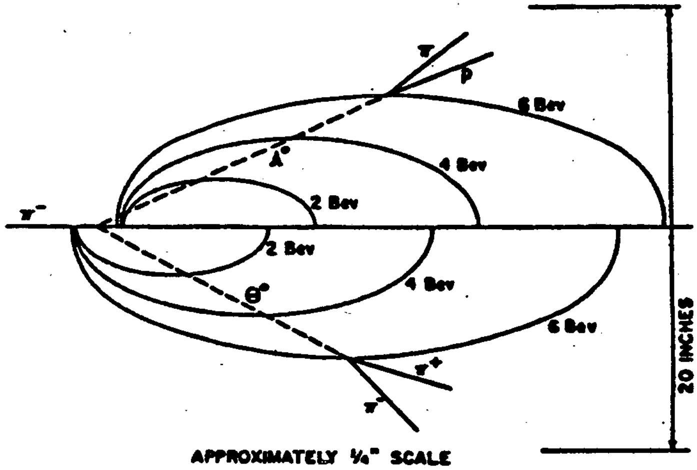


Figure 12

It would require a window 75 inches long by 23 inches wide and 5 inches thick to withstand the (deuterium) operating pressure of 8 atmospheres, exerting force of 100 tons on the glass.

Figure 13 shows the longitudinal cross section of the 72-inch [Bradner[8],pg. 30, UCRL-9199 (1960)]

No one had any experience with such large volumes of liquid hydrogen; the hydrogen-oxygen rocket engines that now power the upper stages of the Saturn boosters were still gleams in the eyes of their designers; these were pre-Sputnik days. The safety aspects of the big chamber were particularly worrisome. Low temperature laboratories had a reputation for being dangerous places in which to work, and they didn't deal with such large quantities of liquid hydrogen, and what supplies they did use were kept at atmospheric pressure.

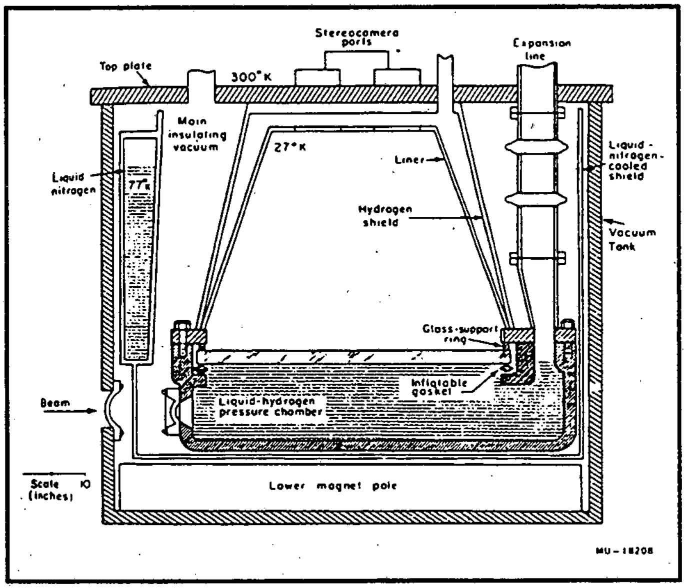


Figure 13

For some time, the glass window problem seemed insurmountable; no one had ever cast and polished such a large piece of optical glass. Fortunately for the eventual success of the project, I was able to persuade myself that the chamber body could be constructed of a transparent plastic cylinder with metallic end plates. This notion was later demolished by my engineering colleagues, but it played an important role in keeping the project alive in my own mind until I was convinced that the glass window could be built. As an indication of the cryogenic "state of the art" at the time we worried about the big window, I can recall the following anecdote. One day, while looking through a list of titles of talks at a recent cryogenic conference, I spotted one that read, "Large glass window for viewing liquid hydrogen." Eagerly I turned to the paper, but it described a metallic Dewar vessel equipped with a glass window 1 inch in diameter!

Don Gow was now devoting all his time to hydrogen bubble chambers, and in January of 1955 we interested Paul Hernandez in taking a good hard engineering look at the problems involved in building and housing the 72-inch bubble chamber. We were also extremely fortunate in being able to interest the cryogenic engineers at the Boulder, Colorado, branch of the National Bureau of Standards in the project. Dudley Chelton, Bascomb Birmingham and Doug Mann spent a great deal of time with us, first educating us in large-scale liquid hydrogen techniques, and later cooperating with us in the design and initial operation of the big chamber.

Bob Watt and Paul Hernandez collaborate with the National Bureau of standards. While testing the consequence of a sudden escape of liquid hydrogen from the chamber into its surrounding vacuum tank they accidental blow down a wall of the NBS lab. It was a sobering experience. They imposed extreme safety measures that included special static-free phones and metallic leg straps to prevent static charge buildup. Nylon shirts were not allowed. All these measures paid off when we had an enormous leak of liquid hydrogen directly into the building containing the 72-inch chamber.

In April of 1955, after several months of discussion of the large chamber, I wrote a document entitled "The Bubble Chamber Program at UCRL." This paper showed in some detail why it was important to build *the large chamber, and outlined a whole new way of doing high energy physics with such a device.*

It stressed the need for semiautomatic measuring devices (which had not previously been proposed), and described how electronic computers would reconstruct tracks in space, compute momenta, and solve problems in relativistic mechanics. All these techniques are now part of the "standard bubble chamber method", but in April of 1955 no one had yet applied them. Of all the papers I have written in my life, none gives me so much satisfaction on rereading as does this unpublished prospectus.

In this important paper, Luie stressed the importance of hydrogen. He took verbal potshots at emulsions and bubble chambers containing hydrocarbons, referring to them as like strawberry jam One day a jar of strawberry jam with an Ilford Emulsion label on it showed up on his desk(Roy Kerth, and Bob Birge the culprits). He treasured that gift. and kept it on his desk. It is in the archives, and I would have brought it with me if I had thought I could get it across your borders. It shows its age.

After Paul Hernandez and Don Gow had estimated that the big chamber, including its building and power supplies, would cost about 2.5 million dollars, it was clear that a special AEC appropriation was required; we could no longer build our chambers out of ordinary laboratory operating money. In fact, the document I've just described was written as a sort of proposal to the AEC for financial support; but without mentioning money! I asked Ernest Lawrence if he would help me in requesting extra funds from the AEC. He read the document, and agreed with the points I had made. He then asked me to remind him of the size of the world's largest hydrogen chamber. When I replied that it was 4 inches in diameter, he said he thought I was making too large an extrapolation in one step, to 72 inches. I told him that the 10-inch chamber was on the drawing board, and if we could make it work, the operation of the 72-inch chamber was assured. (And if we couldn't make it work, we could refund most of the 2.5 million.) This wasn't obvious until I explained the hydraulic aspects of the expansion system of the 72-inch chamber; it was arranged so that the 20-inch wide, 72-inch long chamber could be considered to be a large collection of essentially independently expanded 10-inch square chambers. He wasn't convinced of the wisdom of the program, but in a characteristic gesture, he said, "I don't believe in your big chamber, but I do believe in you, and I'll help you to obtain the money." I therefore accompanied him on his next trip to Washington, and we talked in one day to three of the five Commissioners: Lewis Strauss, Willard Libby (who later spoke from this podium), and the late John Von Neumann, the greatest mathematical physicist then living. That evening, at a cocktail party at Johnny Von Neumann's home, I was told that the Commission had voted that afternoon to give the laboratory the 2.5 million dollars we had requested. All we had to do now was build the thing and make it work!

Design work had of course been under way for some time, but it was now rapidly accelerated. Don Gow assumed a new role that is not common in physics laboratories, but is well known in military organizations; he became my "chief of staff'. In this position, he coordinated the efforts of the physicists and engineers; he had full responsibility for the careful spending of our precious 2.5 million dollars, and he undertook to become an expert second to none in all the technical phases of the operation, from low temperature thermodynamics to safety engineering. His success in this difficult task can be recognized most easily in the success of the whole program, culminating in the fact that I am speaking here this afternoon. I am sorry that Don Gow can't be here today; he died several years ago, but I am reminded of him every day; my three-year-old son is named Donald in his memory.

The engineering team under Paul Hernandez's direction proceeded rapidly with the design, and in the process solved a number of difficult problems in ways that have become standard "in the industry". A typical problem involved the very considerable differential expansion between the stainless steel chamber and the glass window. This could be lived with in the 10-inch chamber, but not in the 72-inch. Jack Franck's "inflatable gasket" allowed the glass to be seated against the chamber body only after both had been cooled to liquid hydrogen temperature."

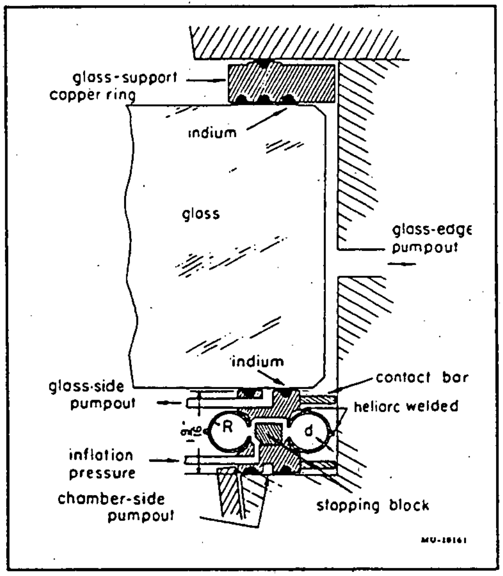


Figure 14, The inflatable gasket (UCRL-9199 pg. 33 1960)

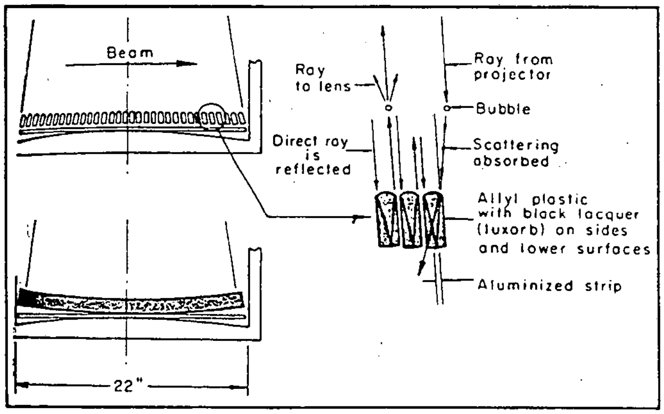


Figure 15, The "coathanger" retrodirective illumination

Just before leaving for Stockholm, I attended a ceremony at which Paul Hernandez was presented with a trophy honoring him as a "Master Designer" for his achievements in the engineering of the 72-inch chamber. I had the pleasure of telling in more detail than I can today of his many contributions to the success of our program. One of his associates recalled a special service that he rendered not only to our group but to all those who followed us in building liquid hydrogen bubble chambers. Hernandez and his associates wrote a series of "Engineering Notes", on matters of interest to designers of hydrogen bubble chambers, that soon filled a series of notebooks that spanned 3 feet of shelf space. Copies of these were sent to all interested parties on both sides of the Atlantic, and I am sure that they resulted in a cumulative savings to all bubble chamber builders of several million dollars; had not all this information been readily available, the test programs and calculations of our engineering group would have required duplication at many laboratories, at a large expense of money and time. Our program moved so rapidly that there was never time to put the Engineering Notes into finished form for publication in the regular literature. For this reason, one can now read review articles on bubble chamber technology, and be quite unaware of the part that our Laboratory played in its development. There are no references to papers by members of our group, since those papers were never written; the data that would have been in them had been made available to everyone who needed them at a much earlier date.

And just to show that I was also deeply involved in the chamber design, I might recount how I purposely "designed myself into a corner" because I thought the results *were important, and I thought I could invent a way out of a severe difficulty, if given the time. All previous chambers had two windows, with "straight through" illumination. Such a configuration reduces the attainable magnetic field, because the existence of a rear pole piece would interfere with the light-projection system. I made the decision that the 72-inch chamber would have only a top window, thereby permitting the magnetic field to be increased by a lower pole piece and at the same time saving the cost of the extra glass window, and also providing added safety by eliminating the possibility that liquid hydrogen could spill through a broken lower window. The only difficulty was that for more than a year, as the design was firmed up and the parts were fabricated, none of us could invent a way both to illuminate and to photograph the bubbles through the same window. Duane Norgren, who has been responsible for the design of all our bubble chamber cameras, discussed the matter with me at least once a week in that critical year, and we tried dozens of schemes that didn't quite do the job. But as a result of our many failures, we finally came to understand all the problems, and we eventually hit on the retrodirecting system known as coat hangers. This solution came none too soon, if it had been delayed by a month or more, the initial operation of the 72-inch chamber would have been correspondingly delayed.*

Figure 15 is a schematic view of "coat hangers" for retrodirective illumination
[Fig. 20, page 48, UCRL-9199 (23 May 1960)]

(end, Alvarez Les Prix Nobel en 1968, pgs. 14-17)

5. The 72-inch, as several 10-inch chambers working simultaneously.

Here, I include Bob Watt's published account of this period of the "The Berkeley Programme."

"Life with Luie"[9] by Robert D. Watt

My association with Luis W. Alvarez began in 1949 and lasted until I took his 82-inch bubble chamber to the Stanford Linear Accelerator Center, seventeen exciting years later. During this time I came to understand that, although Luie might have 100 ideas each day, 50 were probably useless, another 25 too difficult to do, and among the remaining 25 one or two would be worth a Nobel Prize. It was then left to us to drag our feet on all but the best of these ideas. Luie's really good suggestions are well known, while the others are too numerous to mention.

When I first joined Luie's group, it was to supervise the operation of the 40 foot Linear Accelerator. This machine had been designed and constructed by a group of young physicists among whom were doctors Hugh Bradner and W. K. H. Panofsky. The group soon became bored with the routine care and maintenance of the accelerator and decided it was time to turn this task over to professional operators. Ollie Olson, Wilfred Kimlinger, Jack Franck, and a number of us slowly improved the reliability of the equipment by redesigning and replacing the weak components.

Our greatest problem was the reliability of the 202 megacycle power sources used to excite the cavity. For the first few years, we had one man, full time, rebuilding oscillators and replacing their small radar power tubes. On a normal day of operation, the loud call, "New head!" could be heard every twenty minutes. Although the practice made the crew expert at replacing these power sources, 25% of the machine time was lost to this problem. The oscillator problem was solved by Jack Franck, who designed and built a half-dozen large oscillator amplifiers to feed the power transmission lines into the cavity. After this, radiofrequency problems were very rare indeed.

Another recurrent problem was breakage of the Textolite supports that held the Van de Graaff high-voltage head in position. Luie suggested that I visit Van de Graaff installations around the country to find out how they solved the problem. It quickly became apparent that most places *had no problem; because their Van de Graaffs were vertical machines. Ours was horizontal, with the high-voltage sections cantilevered out some 15 feet. We solved our Textolite problem with diagonal supports in tension made of glass fibers embedded in a matrix of epoxy. The Textolite supports did not break after that.*

Conversation with Luie in those days was always a pleasure, because I never needed to say more than the first three words of a sentence to convey an idea to him. Within the space of these few words, he always understood the total content of my sentence, and any further utterance on my part would have been redundant. What Luie never understood, however, was that although his answer to my words was always correct, what I would have said, had I continued, usually was not. In this way, I was credited with much more knowledge than I truly had.

The birth and training of the 72 inch bubble chamber was one of our most trying periods. It was horrible to start, make sensitive, and operate. Once, when I was very discouraged, Luie backed me into a corner in the control room and said, "Bob, you're doing this all wrong. Don't think of it as a large 72 inch chamber. Treat it as a group of small 10 inch chambers in series and in parallel." And as soon as I changed my thinking to that mode, the bubble chamber, obligingly, began operating better. Unfortunately, it was a few months before all these " 10 inch" chambers realized that they should be sensitive simultaneously and have the same bubble density.

I observed during my years with Luie that he always knew the capability of each of us and expected us to perform at that level. He was quick to congratulate a good performance and rarely failed to notice a bad one.

The most striking example of his ability to share credit with others happened several years after I moved to Stanford University. At 4:00 in the morning, I was awakened by the telephone and greeted with this message: "Hi Bob, this is Luie. We just won the Nobel Prize!" He didn't say "I"; he said "we," and this is typical of his assumption that a team of people who work together deserve credit together. His Nobel lecture is more of a tribute to the accomplishments of his associates than it is a description of his contributions. This trait of Luie's has made it very satisfying for those of us who have worked with him.

(end "Life with Luie" by Robert D. Watt)

Bob Watt's shout "SIX FOOT LONG TRACKS!" echoes through the Bevatron bldg.

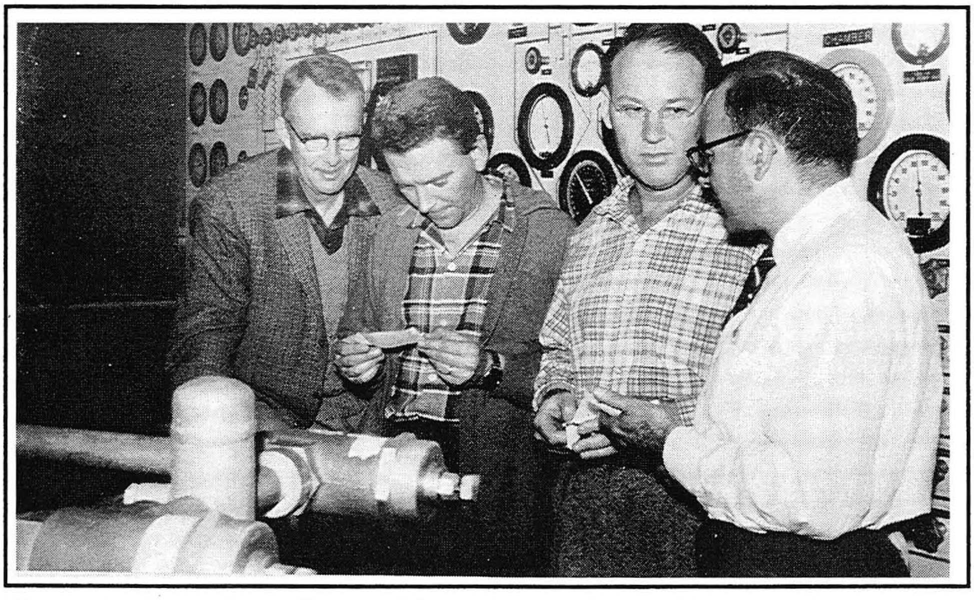


Figure 16, Alvarez, Gow, Watt, and Hernandez at 72" control deck after first tracks

My responsibility was to deliver a beam of antiprotons to the 72-inch for the primary purpose of studying anti-hyperon production. Figure 17 shows the first anti-lambda event with the scanner who found it , Bonnie Thompson. She became the first Mrs. Don Glaser. Those of us who attended the "Perkinsfest" at Oxford last week learned from Peter Fowler that he married the scanner who found the first example of the pion → muon → elctron decay sequence in nuclear emulsions.

Figure 17 XBB 936-3689

This event, shown in more detail in Figure 18, had such beautiful symmetry that it was displayed for years in the New York Meuseum of Modern Art.

6. The 25-inch chamber (with omega bellows), completed in 1962,was the only chamber that did not use vapor expansion. It was designed to triple pulse on the Bevatron flat-top. It was very turbulent, Don Miller called it the "chamber pot".

7. I get a phone call from Luie at 6 AM October 30, 1968 telling me that "we" had won the Nobel Prize in Physics. Remember that Bob Watt got his call at 4 AM! That's called calibration!.

8 The Berkeley Programme beyond the Alvarez era.

In the mid 60's I dreamed about a 100 m^3 hydrogen chamber to do neutrino physics at the 200 BeV accelerator.

When reality set in and the accelerator was built at Fermilab, we developed the External Muon Identifier (EMI), the Internal Picket Fence (IPF), and the Quantameter (never built) for the Fermilab 15-foot chamber.[10], in collaboration with the University of Hawaii. Figure 19 is a schematic 3-D sketch of these devices and a typical di-muon event (Oxford 1978 Conference Proceedings.Fig. 10.4, pg. 374, RL-78-081). Vince Peterson (Hawaii) will give more details of this collaboration in his talk tomorrow, "The EMI for the 15 ft. chamber".

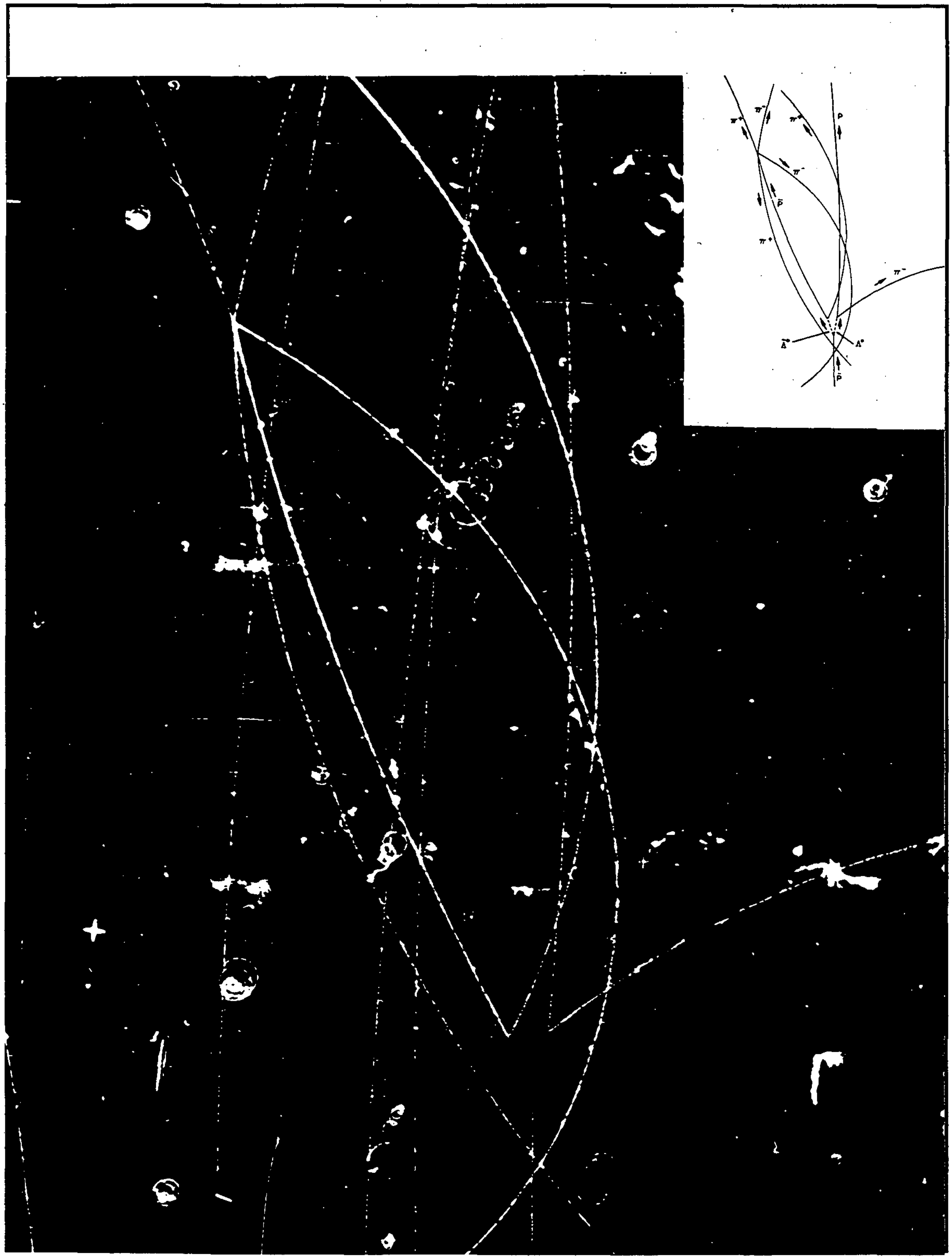


Figure 18 $\overline{p}+p\rightarrow\overline{\Lambda}+\Lambda$ (BC 772A)

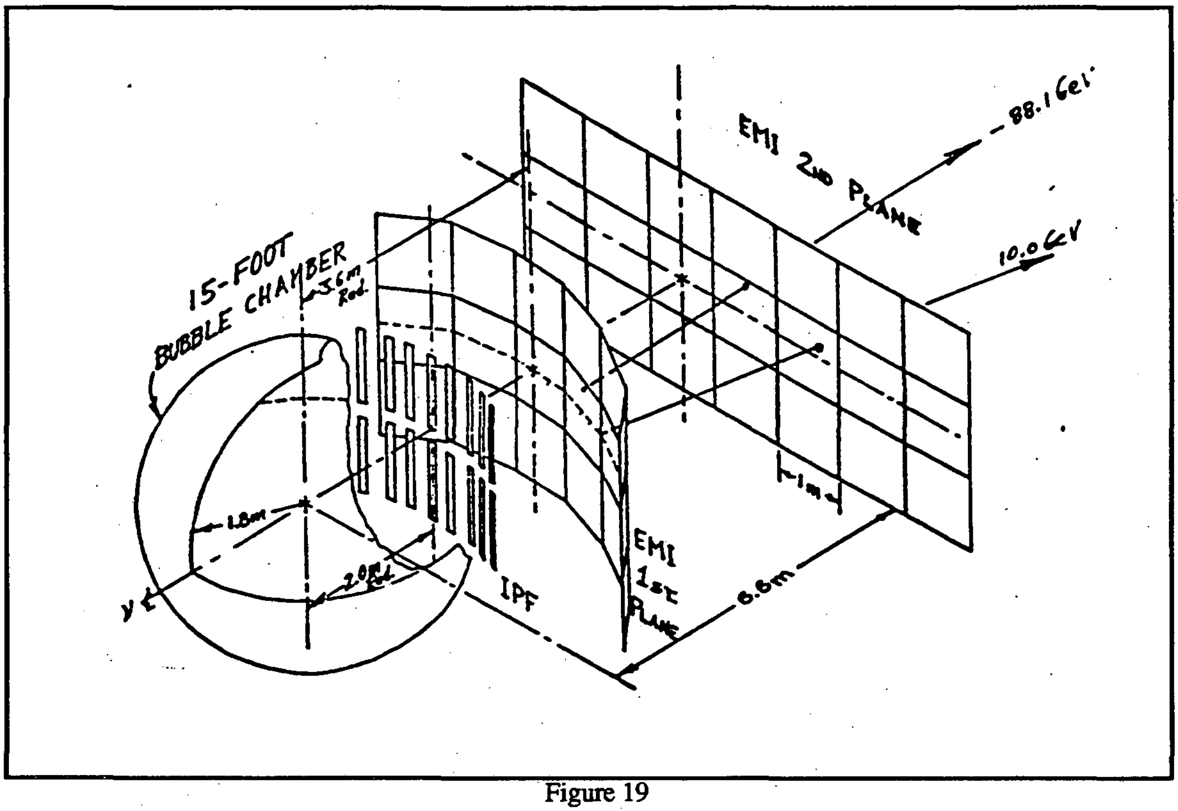


Figure 19

[1] "Alvarez, Adventures of a Physicist," Luis W. Alvarez, Basic Books, Inc. (1987), and the unedited original manuscript.

[2] "Discovering Alvarez," Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues, Ch. 12 "The Development of the Hydrogen Bubble Chamber", M. Lynn Stevenson; Edited by W. Peter Trower, University of Chicago Press (1987).

[3] Les Prix Nobel en 1968, "Recent Developments in Particle Physics" Nobel Lecture by Luis W. Alvarez, The Nobel Foundation 1969.

[4] R. H. Hildebrand and D. E. Nagle, Phys. Rev. 92, 517 (1953)

[5] J. G. Wood, Phys. Rev. 94, 731 (1954)

[6] W. B. Fowler, R. P. Shutt, A.M. Thorndike, and W. L. Whitmore, Phys. Rev. 91,1287 (1953); 93, 861 (1954); 98, 121 (1955)

[7] Luis Alvarez, "The Bubble Chamber Program at UCRL." (18 April 1955); from Peter Galison, Chapter 10 "Bubble Chambers and the Experimental Workplace, pg. 339; in Observation, Experiment and Hypothesis in Modern Physical Science, 1985, edited by Peter Achinstein and Owen Hannaway, The MIT Press

[8] Hugh Bradner, "Bubble Chambers", UCRL-9199 (23 May 1960)

[9] Discovering Alvarez," Selected Works of Luis W. Alvarez with Commentary by His Students and Colleagues, Ch. 12 "Life with Luie", Robert D. Watt; Edited by W. Peter Trower, University of Chicago Press.

[10] M. Lynn Stevenson, "Performance Characteristics of the Fermilab 15-Foot Bubble Chamber with a 1/3-Scale Internal Picket Fence (IPF) and a Two-Plane External Muon Identifier (EMI),Proceedings; Topical Conference on Neutrino Physics at Accelerators, Oxford, July 4th-7th 1978, Rutherford Laboratory Publication RL-78-081.