

The First Muon Spin Rotation Experiment^a

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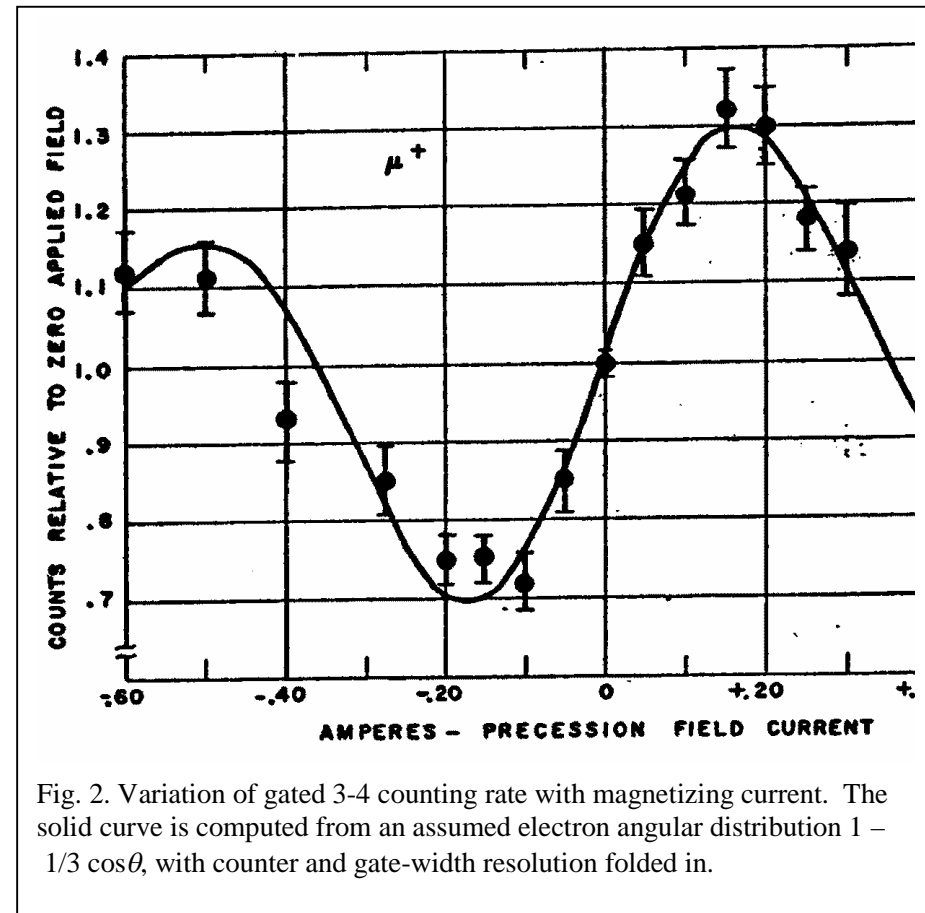
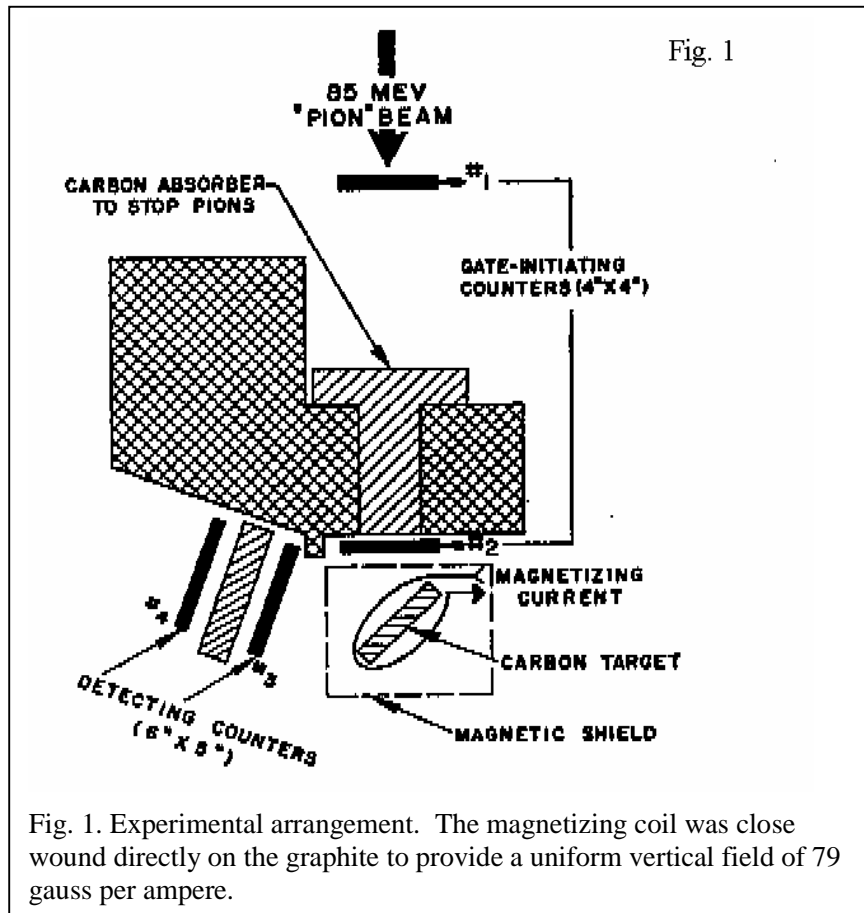
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ABSTRACT. The February 15, 1957 issue of *Physical Review Letters* shows the first muon precession curve resulting from the stopping of ‘85 MeV’ muons in graphite, and the resulting counting rate in a gate of fixed delay, duration, and orientation, as a function of an applied vertical magnetic field. The purpose of the four-day experiment was to test the conservation of parity in the weak interactions. It involved the sudden recognition that existing muon beams would be polarized if parity were not conserved, together with the appreciation that the angular distribution of decay electrons from the population of stopped muons could be observed (much more reliably and sensitively) by the variation with time or current of the detections in a fixed counter telescope than by the measurement of the decay asymmetry of nominally fixed muon spins. This retrospective paper explains the context, the state-of-the-art at the time, and what we expected as a consequence of this experiment. We went on to study more accurately the magnetic moment of the muon, its gyromagnetic ratio— g —and only to a small extent used μ SR to investigate the environment of the muon in matter. Much of the paper treats the instrumentation of the time—especially that adopted in the early 1950s. The essential tools of nanosecond-range coincidence circuits and adiabatic light pipes for scintillation counters are discussed. The paper closes with some of the later work in the field—measurements of the magnetic moment of the muon and especially the CERN measurement of muon $g-2$.

Keywords: muon spin rotation; coincidence circuits; scintillation counters; g -value; muon; $g-2$.

In our first μ SR paper [1], we reported having stopped positive muons from pion decay in flight within the Columbia University cyclotron, in the expectation that the high-energy muons from 'forward' decay of pions would as a group be polarized, if the Lee-Yang proposal were valid for parity violation. (In this web version of the paper, I have the luxury of listing my publications which would be helpful to the reader [1, 2, 3,...26].



Not all of these are specifically cited in the text.) The arrangement of Fig. 1 was designed, given our limited understanding at the time, to preserve the muon spin in the stopping process, to shield the muons from the fringe magnetic field of the cyclotron, and to allow precession of the spin of the muon population by an applied steady magnetic field.

The arrival of each muon was marked by a coincidence between two scintillation counters, and counts in a decay electron telescope within a certain ‘gate’ of duration 1.25 microseconds (delayed 0.75 microseconds after the muon stopping) were recorded. The number of gated counts and the number of stopping muons were thus recorded for a typical 20-minute period, after which the magnetizing current was changed. In this way, the curve shown in Fig. 2 was created.

Here we see the recorded points of ‘counts relative to zero applied field’, and a solid curve which is a theoretical fit to a distribution of decay electrons of a form $(1 - 1/3 \cos\theta)$, where we have taken a gyromagnetic ratio of +2.00; taking into account the angular breadth of the electron telescope and the smearing due to finite duration of the gate; as well as the exponential decay rate of muons within the gate; and the small residual cyclotron stray field, which shifted the initial angle (for $H = 0$) from 100 deg to 89 deg. H is the field due to the magnetizing current.

We noted in that paper that ‘We have also detected asymmetry in negative muon decay and have verified that the moment is negative and roughly equal to that of the positive muon. The asymmetry in this case is also peaked backwards.’ We showed that the significantly weaker asymmetry for positive muons in nuclear emulsion ‘did not increase with reduced delay and gate width’ and hence must have occurred in the stopping of the muons.

And we closed with the sentence ‘It seems possible that polarized positive and negative muons will become a powerful tool for exploring magnetic fields in nuclei (even in Pb, 2% of the negative muons decay into electrons), atoms, and interatomic regions.’

So I am here to relate to you some of the circumstances of this first μ SR experiment and some of its consequences in which I was immediately involved.

As related in Ref. 25, this entire experiment was conducted-- from a glimpse of the possibility to the calculation of the curve-- from 8 p.m. Friday, January 4, 1957 to 4 p.m. Tuesday, January 8, 1957. In summer 1956, T.D. Lee and C.N. Yang published their epochal paper showing that existing experiments did not bear on the question of whether the weak interaction was invariant under charge conjugation and space reflection (parity), and suggested two experiments which might answer this question. The first was a measurement of the spatial electron decay distribution of oriented Co-60, and the second was to look at the decay of stopped pions into muons, and their subsequent decay into electrons.

The second experiment seemed tantalizingly simple, and many individuals and groups over the world began to look for a non-zero correlation between the direction of emission of the electron and that of the muon-- itself born from a pion at rest in nuclear emulsion. The idea is that parity violation would give rise to a polarization (perhaps small) of the muon along its velocity (in the pion rest frame), and the subsequent stopping of the muon and its decay could also give rise to a (possibly small) angular distribution of electrons. The problem with the experiment done in nuclear emulsion was recognized from the first as one of 'scanning bias,' since it is much easier to see the decays in which the electron is emitted 'backward' from the muon, rather than the forward decays, which might be taken simply as a scattering event rather than a muon decay.

Professor Chien-Shiung Wu of Columbia University took up the challenge of orienting radioactive Co-60 by adiabatic demagnetization cooling, using an appropriate crystal. With the group at the National Bureau of Standards, she had obtained strong indication of a 'demagnetization curve' (that is, a change in the electron distribution as the sample of oriented Co-60 warmed from its initial polarized state). At a traditional Friday Chinese lunch of the Columbia University Physics Department, she shared this news with her excited colleagues. My old friend and colleague, Leon Lederman, was at the January 4, 1957 lunch; I was not, because I was in Poughkeepsie, NY for the day, for a superconducting computer project at IBM, which I had initiated and was leading.

Returning from Poughkeepsie about 8 p.m., I was having dinner with my wife at home in Scarsdale, NY, about five miles from the Columbia University cyclotron, and returned a call from Lederman, who related the Co-60 results and observed that it had occurred to him that the muons on which he had been working for several years in the external '85-MeV pion beam' of the Nevis cyclotron were already polarized, since they were selected from the isotropic decay distribution of pions in flight as those which had been emitted 'forward' and thus had a longer range by virtue of the pion's velocity in the cyclotron.

I told Leon I would join him at the cyclotron that evening, where we met about 8:30 p.m. The setup of Fig. 1 was already operating at Nevis, as the thesis of Marcel Weinrich, with Lederman as sponsor. Missing, of course, was the magnetized target, since at that time, it had not occurred to the experimenters that the electron decay resulting from the stopped muons would be anything but isotropic. The pions of equal momentum were all stopped by eight inches of carbon since the mean range of the '85-'MeV pions about five inches of carbon.

On arriving at the cyclotron, we looked for a means for swinging a counter around the stopped muons, but found no suitable device. Furthermore, it was not permissible to stay in the counting room where the muons stopped, because of radiation levels, and so we would have had to have a remotely movable protractor. Fortunately, we didn't find such, and the machine shop was locked, so that I couldn't fashion the instrumentation on the spot.

Because of my years of experience with NMR of liquid and solid He-3, it occurred to us that we could use a fixed counter if we precessed the spin of the muon population, so as to sweep any asymmetrical decay pattern past the counters. With one lathe at our disposal, we wound a coil on a Lucite cylindrical shell, big enough to contain the graphite stopping block. And by early Saturday morning, we had substantial evidence of asymmetry. However, for the last hour or so of operation before the cyclotron shut down for its weekend maintenance, the asymmetry weakened and in fact was imperceptible over that last period.

When we approached the apparatus after the cyclotron was shut down Saturday morning, we noticed that the fine coil of insulated wire had slipped down along the coil form and was resting at the bottom, so that it provided little magnetic field at the muons. This explained the vanishing of the asymmetry. When the cyclotron resumed operation Monday evening, we were ready with a coil wound in a much more convenient if less traditional fashion-- right on the rectangular graphite block itself. Whoever said that a cylinder had to have a circular cross section? By Tuesday afternoon our experiment was complete.

Weinrich had measured the exponential decay time of positive and negative muons in various materials. Of course, in all materials the positive muons had the same 2-microsecond half-life. So he was studying negative muons stopped in various elements; the muons decayed not only by their inherent instability with the normal 2-microsecond partial decay rate, but also by interaction with the nucleons, the result being that in Pb the decay time was on the order of 40 ns.

In fact, Weinrich had been having some difficulty as his experiments became more and more precise. He was troubled by non-exponential decays, and well he might have been-- multiplying the exponential decay of the muon population around a nucleus was the unsuspected variation in electron-telescope efficiency resulting from the precession of the muon spin in the fringing field of the cyclotron at the position of the stopped muon.

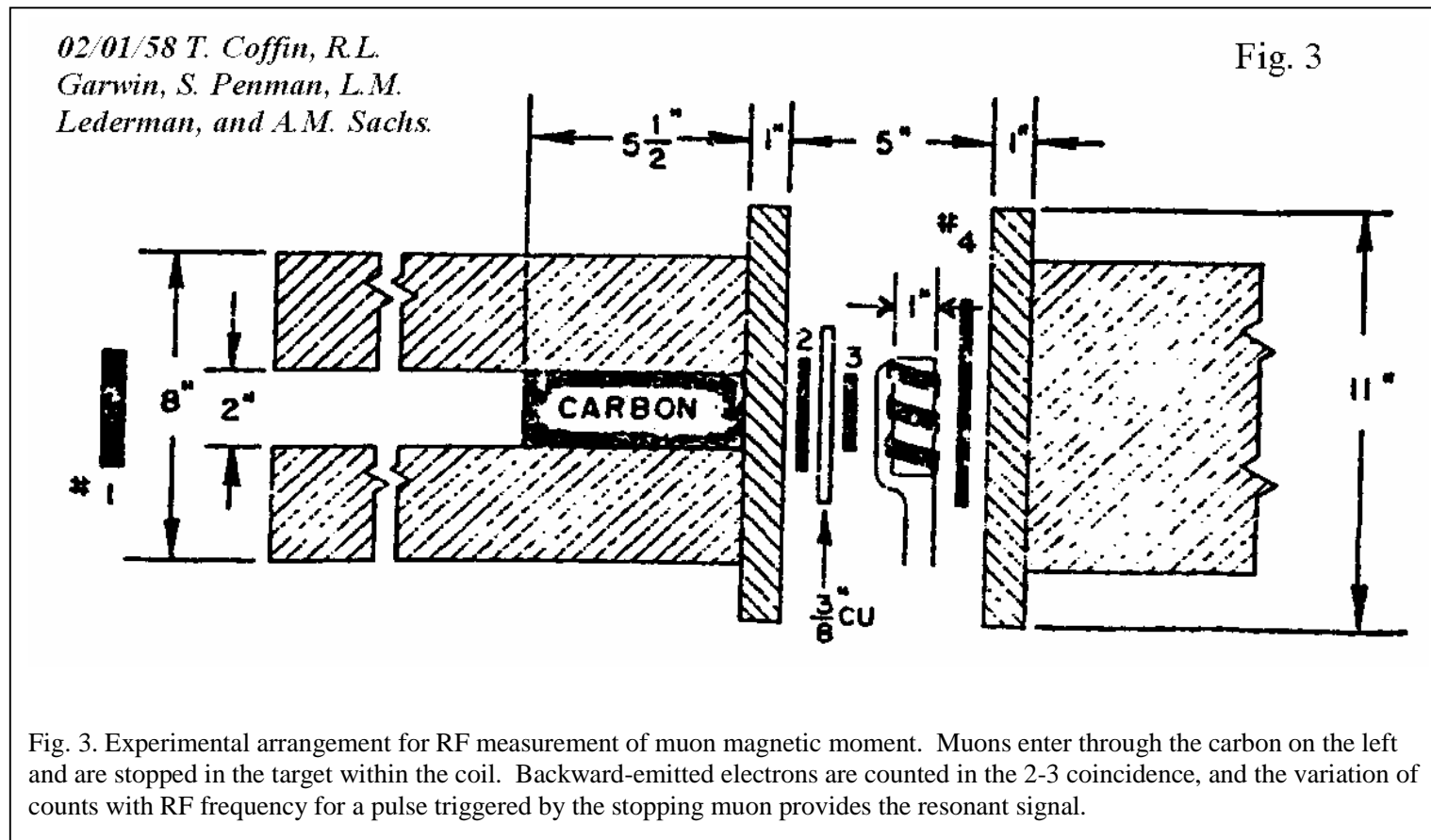
Useful for future work by ourselves and others were our findings of a large asymmetry, establishing that the muon beam is strongly polarized; the $(1 - 1/3 \cos\theta)$ angular distribution; the establishment of nonconservation of parity and violation of charge conjugation; the g value of the free muon as $+2.00 \pm 0.10$; the likelihood that the muon spin was $1/2$; the similarly backward-peaked asymmetry for negative muons stopped in carbon with about 15% as large an asymmetry; and a similar but negative magnetic moment for the negative muon.

We found large asymmetries for the positive muon stopped in polyethylene and calcium, but only about half as large when we used nuclear emulsion as a target.

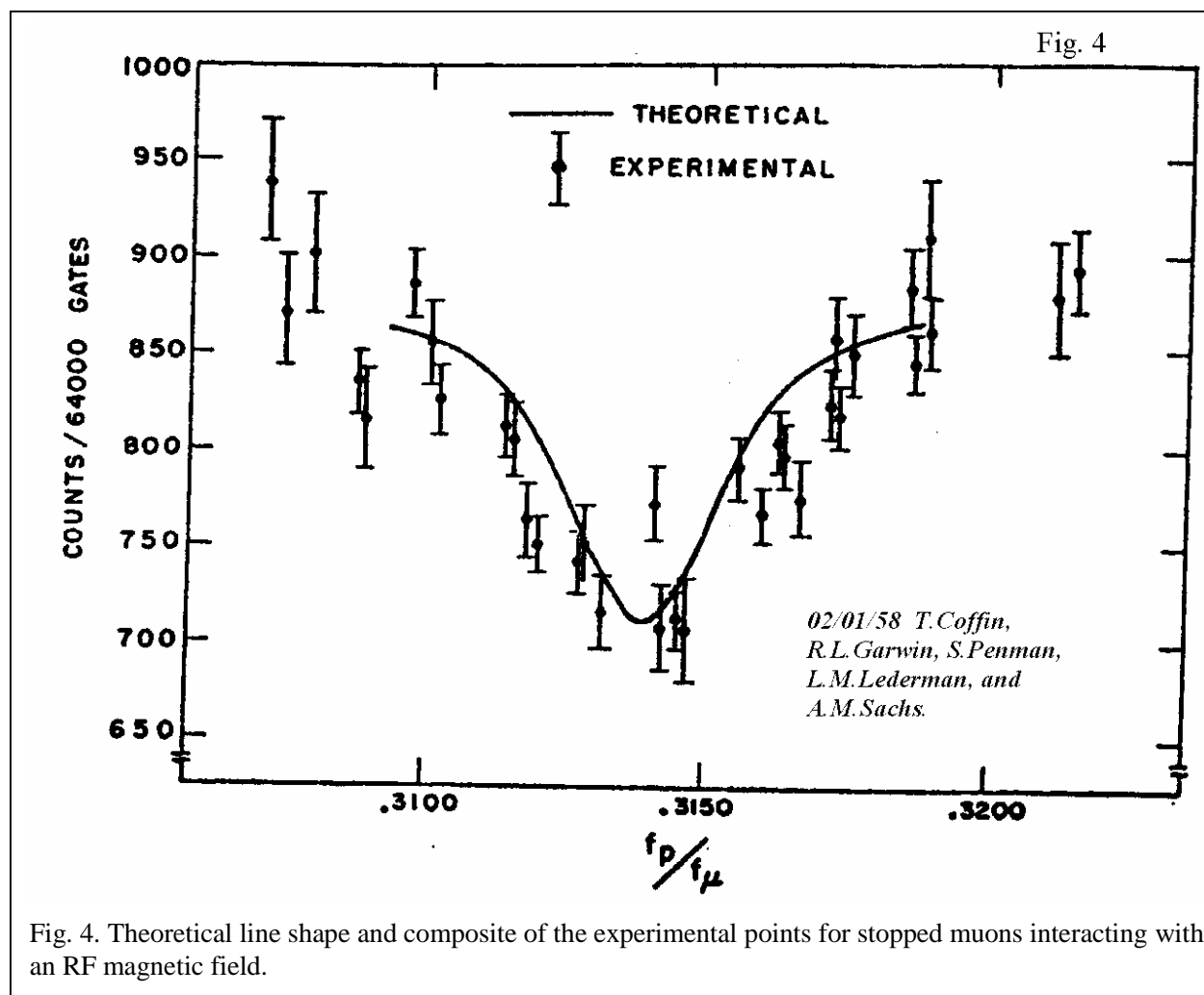
FURTHER EXPERIMENTS.

On the μ SR side, our next significant experiment was a magnetic resonance measurement of the muon magnetic moment-- hence the precession frequency in a known magnetic field. For this, we needed to have a substantial field, so we used iron-core magnets, properly shimmed and measured. How to invoke and measure the precession?

So our first resonance experiment was a tour de force-- maybe even a tour de brute force.



We brought the muons in, allowed them to precess, and after a certain time, flipped the spin by 90 degrees with the application of a 0.5 microsecond damped sinusoid of RF at the assumed resonant frequency. Varying the magnetic field would allow us to trace out the width of the resonance line, with precision limited by the finite time available for measurement because of the two microsecond muon lifetime. The results are shown in Fig. 4.



This experiment was conducted with RF triodes usually used in radio transmitters, to which we applied a 10 kV pulse. The triode was sent into oscillation by a large pulse from a secondary-emission pentode-- of about 600 V. To extend this experiment to improve the measurement of the magnetic moment by a factor ten seemed possible, but difficult.

Our colleague, Prof. Leo James (Jim) Rainwater, suggested to me that there was another way, but he was not about to tell me. If I didn't think of it myself, he proposed to do that experiment.

Well, given this incentive, I did think about it, and decided that Rainwater might have in mind a synchronously gated detector: The entry of a particular muon into the intense, uniform, and static magnetic field would give a signal. This signal could trigger a reference sine wave, and a counter (or several counters) could be detected in coincidence with the half cycles of the reference wave. Depending on the orientation of the counter and the deviation of the muon spin precession frequency from the reference frequency, there would be a *slow* change with time of the counting rate in a particular counter. So this change could be measured just as we had measured the variation of counting rate with time in our first experiment, and this would serve as a *vernier* measurement of the offset of the precession frequency from the reference frequency. Of course, we would need to know the magnetic field, and that we measured by conventional proton spin resonance probe.

Naturally, there was all sorts of fun involved in this experiment, and in fact I devised two approaches, both of which were implemented with the help of my colleagues, Alvin Patlach at IBM, and Sheldon Penman at Columbia University.

One of these approaches is as indicated; the problem is starting a sine wave of constant frequency accurately with a fixed phase. The other was a pulsed measurement of the actual time to detection of the electron in a counter in a particular direction, as well as timing the incident muon-- all with a continuously operating crystal-controlled oscillator.

Finally, when I went to CERN in the Fall of 1959 for a year of reading in the library to recharge my knowledge and energy, I was pressed into leadership of a group which had been considering with Leon Lederman measurement of the $g-2$ of the muon. Note that our first measurements of January 1957 included a statement on $g-2$:

Independent evidence that $g = 2$ (to $\sim 10\%$) comes from the coincidence of the polarization axis with the velocity vector of the stopped muons. This implies that the spin precession frequency is identical to the muon cyclotron frequency during the 90 deg net magnetic deflection of the muon beam in transit from the cyclotron to the 1-2 telescope.'

But now we were going to do it right, and again there were two possibilities. One of these was the favorite of V.L. Telegdi. It would confine the muons in a 'screw' for a fixed number of turns determined by the geometry of the screw. The deviation of the orientation of the mean spin from that of the mean momentum of the muon population would then constitute a direct measurement of $g-2$.

Because of the 'Thomas factor' first explored by L.H. Thomas, the magnetic moment associated with an electron charge in an orbit with angular momentum $\hbar/2\pi$ is also associated with the spinning electron itself-- with half a unit of angular momentum.

If g were precisely 2 (that is, if there were no influence of virtual photons and pairs), the orientation of the spin would remain precisely aligned with that of the muon momentum, as the muon traversed an arbitrary static magnetic field. But the 1947 measurements and theory for the electron showed that one could expect for a pure Dirac particle $g = 2(1 + \alpha/2\pi)$. It was this second term (about 0.1%) which was the object of our measurement-- in order to detect new particles or some non-QED interactions of the muons.

The approach actually adopted was to measure the relative precession of muon spin with respect to momentum as a function of time, for a muon orbiting in a magnetic field of known intensity (Fig. 5). Our experiment was ultimately published in *Nuovo Cimento*, because the *Physical Review* would not accept a 133-page paper, and I so loved the experimental details that I hated to cut any of them.

G. CHARPAK, *et al.*
 16 Giugno 1965
 Il Nuovo Cimento
 Serie X, Vol. 37, pag. 1241-1363

Fig 5

The Anomalous Magnetic Moment of the Muon.

G. CHARPAK (*), F. J. M. FARLEY, R. L. GARWIN (**), T. MULLER (***)
 J. C. SENS and A. ZICHICHI

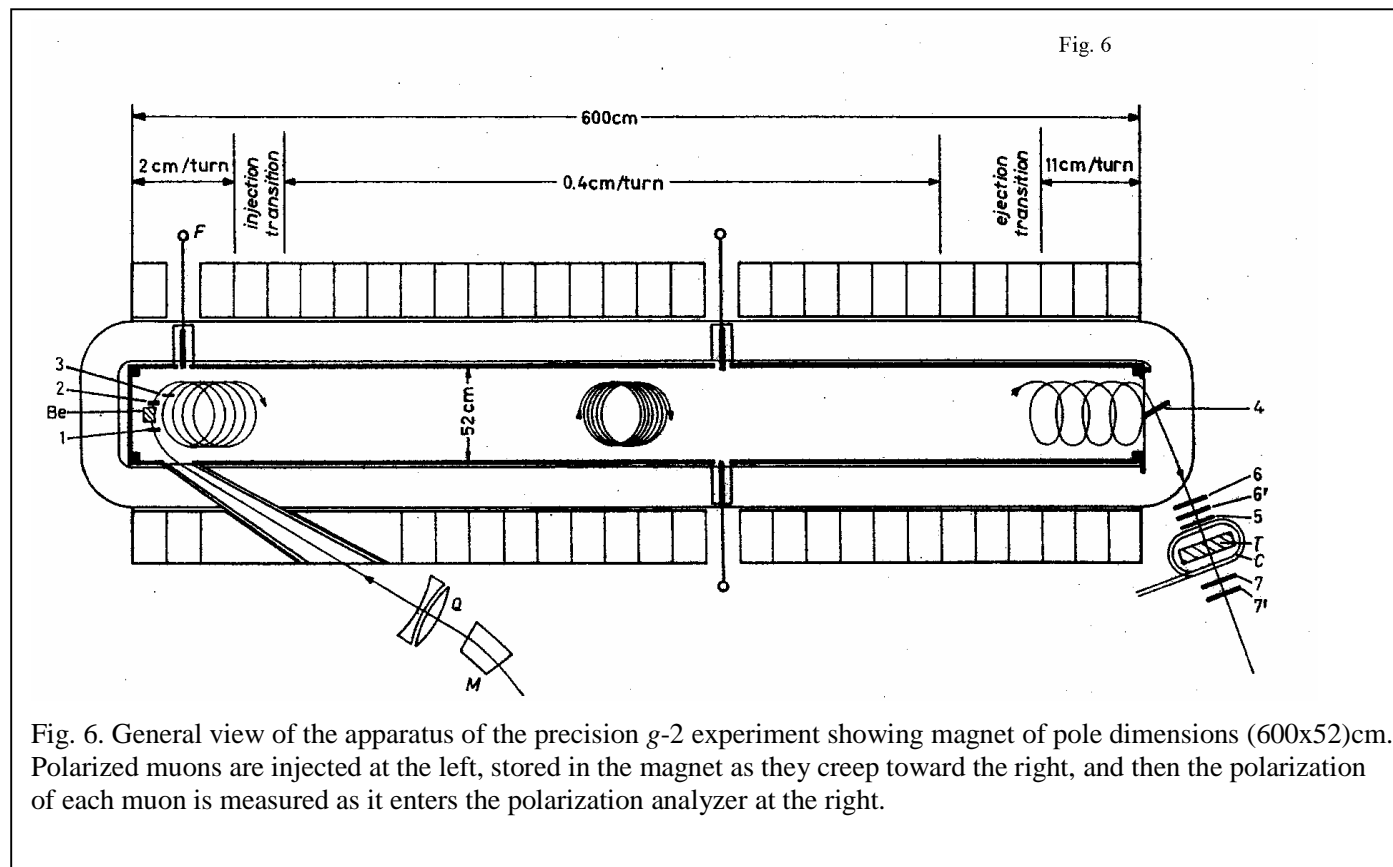
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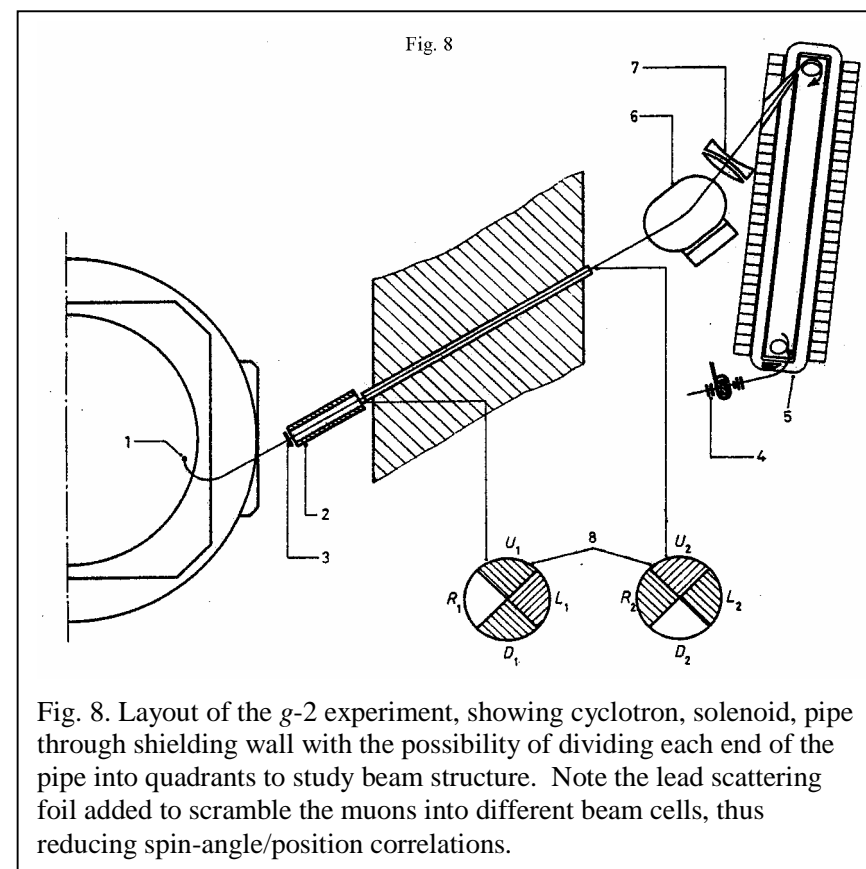
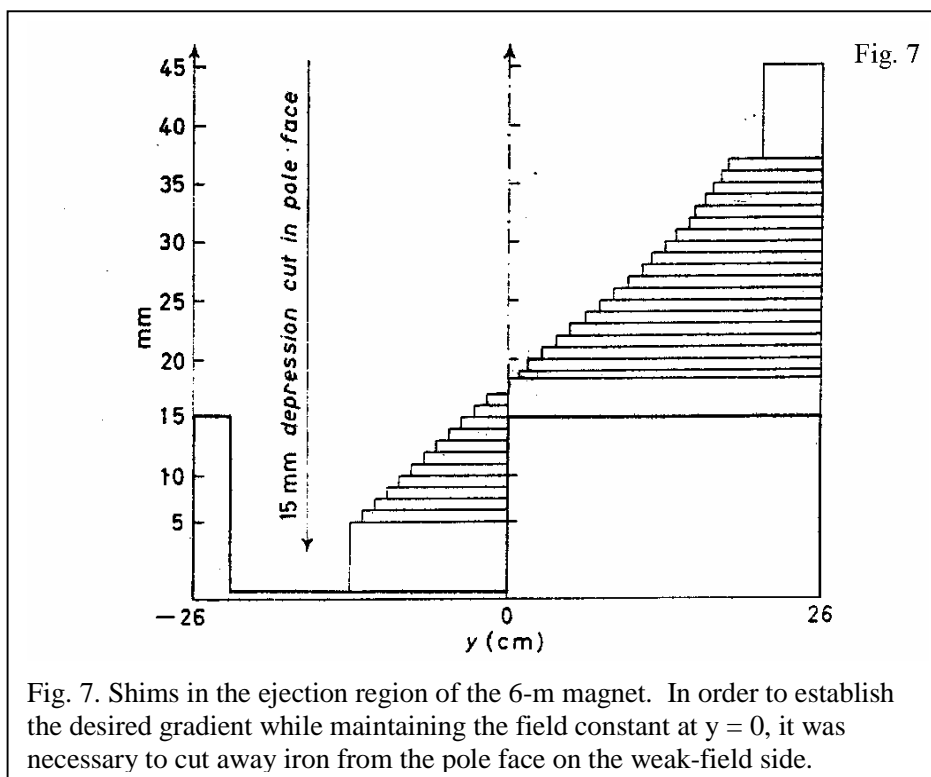
(ricevuto il 18 Settembre 1964)

Summary. — The anomalous part of the gyromagnetic ratio, $a \equiv \frac{1}{2}(g - 2)$ of the muon has been measured by determining the precession $\theta = a\omega_0 \bar{B}t$ for 100 MeV/c muons as a function of storage time t in a known static magnetic field of the form $B = B_0(1 + ay + by^2 + cy^3 + dy^4)$. The result is $a_{\text{exp}} = (1162 \pm 5) \cdot 10^{-6}$ compared with the theoretical value $a_{\text{th}} = \alpha/2\pi + 0.76\alpha^2/\pi^2 = 1165 \cdot 10^{-6}$. This agreement shows that the muon obeys standard quantum electrodynamics down to distances ~ 0.1 fermi. Details are given of the methods used to store muons for $\sim 10^8$ turns in the field, and of measuring techniques and precautions necessary to achieve the final accuracy. Some of the methods of orbit analysis, magnet construction shimming and measurement, polarization analysis, and digital timing electronics may be of more general interest.

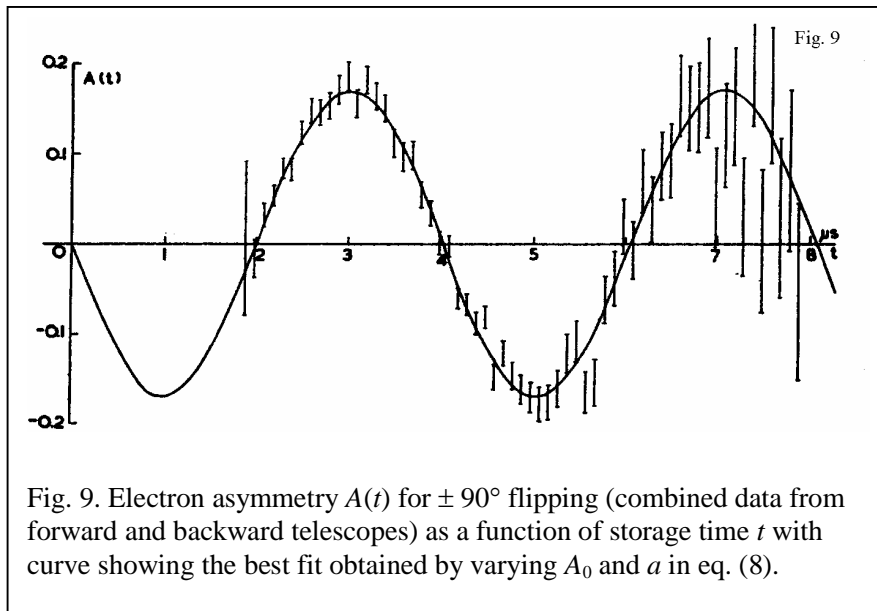
Fig. 5. Title, authors, and abstract of the first precision muon $g-2$ experiment.

Our confinement magnet weighed 80 tons and held the muons for about six microseconds-- a long time for a non-relativistic muon (Figs. 6,7).





The magnet was shimmed in a wondrous fashion-- responsibility of Nino Zichichi-- and the polarization measured as the muons emerged from the static magnetic field by a system perfected by Georges Charpak (Figs. 8,9,10).



Run	$a_{\text{exp}} \times 10^6$	χ^2 for this fit (expectation value = 58)	Difference from final value of a_{exp}	Statistical error $\times 10^6$
1	1169	65	+ 7	13
2	1155	70	- 7	14
3	1135	58	-27	15
4	1165	49	+ 3	16
5	1149	76	-13	19
6	1183	50	+21	14
7	1162	53	0	13
8	1154	61	- 8	15
9	1197	68	+35	16
10	1132	72	-30	14
11	1162	86	0	19
12	1178	50	+16	16
13	1133	62	-29	23
14	1160	63	- 2	13
15	1154	50	- 8	36
16	1174	71	+12	16
17	1150	57	-12	26
18	1145	51	-17	17
19	1146	48	-16	23
20	1181	70	+19	19
21	1173	66	+11	27

Fig. 10. Results for individual runs of the $g-2$ experiment.

Of course, the measurements of $g-2$ for the muon have progressed far beyond our early efforts, with Frances Farley remaining a key participant. Many clever techniques have been used, and the result was most recently in the news as (temporarily) disagreeing with theory. It turns out, as I understand, that the theory was wrong-- not in any important sense of the experiment having indicated new forces or new particles, but simply because the sign of one of the complicated terms in the series for $g-2$ had been reversed.

To return to the 'four-day experiment', I want to close by indicating some of the experimental infrastructure which was used. I do this from a personal point of view.

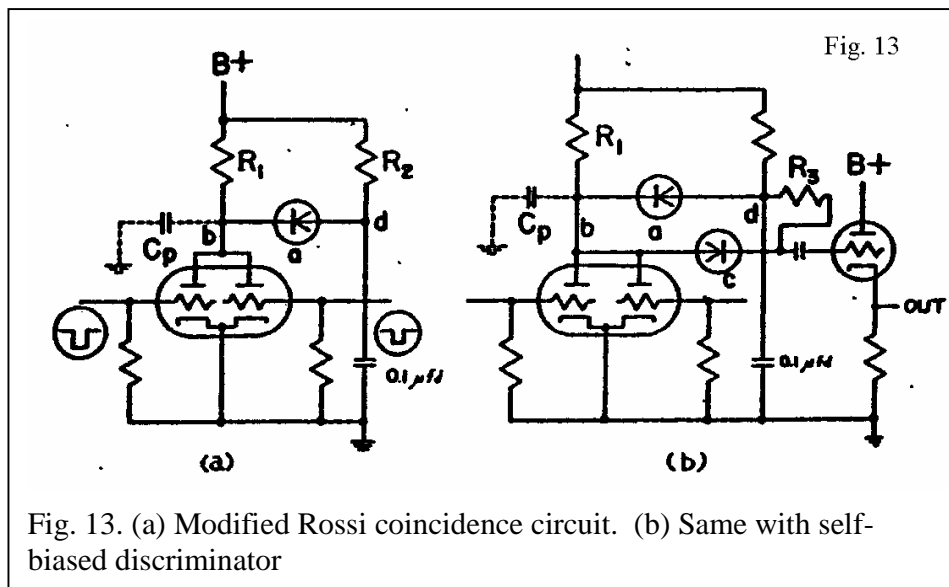
Enrico Fermi was my graduate supervisor at the University of Chicago. I entered Chicago in 1947 to obtain my Master's degree and obtain my Ph.D. in Physics there in December 1949. After some months of academic work, I became restless to do some work in the laboratory and volunteered to Fermi to serve as some kind of research assistant. He took me up on this, for which I was grateful. Fermi and Leona Woods Marshall were working on some experiments on positronium, and I noticed they were using Rossi coincidence circuits of resolving time perhaps one microsecond. Since the 'accidental coincidence' rate is proportional to the square of the count rate and the resolving time (for two pulses in coincidence or for one pulse beginning a gate and the other pulse falling within it) a higher source strength could be used if the coincidence rate could be reduced. Furthermore, various short-time phenomena could be better studied with a shorter coincidence resolving time.

Since this was a field of nuclear physics in which I might work, I made some effort to contribute to the instrumentation, first building pulse generators that would work in the 'milli-microsecond range' (now called the 'nanosecond range') (Fig. 11).

The Rossi circuit of the 1930s was the workhorse of the nuclear physics community. Its principle was simple: two (or more) vacuum tubes operated in parallel from the same anode load. When one cut off the current in one of the tubes with a negative pulse to its grid (supplied by a Geiger counter or other detector), the anode voltage rose until the other tubes shared the current loss. Typically, triodes were used for this purpose. When the grids of ALL tubes were similarly turned off, the common anode voltage rose to the power supply potential of, say, 200 volts. So one was discriminating signals of typically 50 volts or so from a signal of 200 volts, and this could be done very reliably by the ‘discriminator’ of the next stage in the counting train. However, a typical tube current of 10 mA with the shared capacitance of the tubes and wiring, say 40 picofarads, rose only slowly (250 V/nS), so that the anodes required almost a microsecond to reach the discriminator threshold.

Still using vacuum tubes of that era, I resolved to separate the function of the amplifier tubes with their common anodes from the function of providing a small voltage rise when one tube was cut off and a large voltage rise when all tubes were cut off.

Evidently, what was needed, was a very low impedance for flow for all but, say, 9 mA in this case, and a high impedance for the last current. Such could be achieved if there were a diode which carried the current for $N - 1$ tubes, and a little bit of the current of the N 'th tube. Ref. [14].**JAH: Please check that Ref. 14 belongs here. It is also mentioned on p.15.** I found a semiconductor diode of that era with a drop of 0.5 V, and arranged in principle N resistors from the power supply through that diode to the common ‘plates’ (anodes) of the vacuum tubes. So a little bit more than $N - 1$ times a single tube current was used in this way. The other tube's current went directly to the common anodes. In order that the system be self adjusting, rather than to provide the current from a fixed voltage, I used a special fast capacitor-- that is, one with low inductance which would not provide a voltage kick when the current was changed-- rather than a fixed supply to feed the diode. And in order to obtain equal current in all the tubes, each had a cathode resistor from a substantial negative voltage, so that no matching of tubes would be required. Published first as a two-input coincidence circuit, I soon expanded this to a multiple-output coincidence-anticoincidence analyzer with six coincidence inputs and four anticoincidence inputs—and three logically independent outputs, as shown in Figs. 13,14 from Ref. 16.



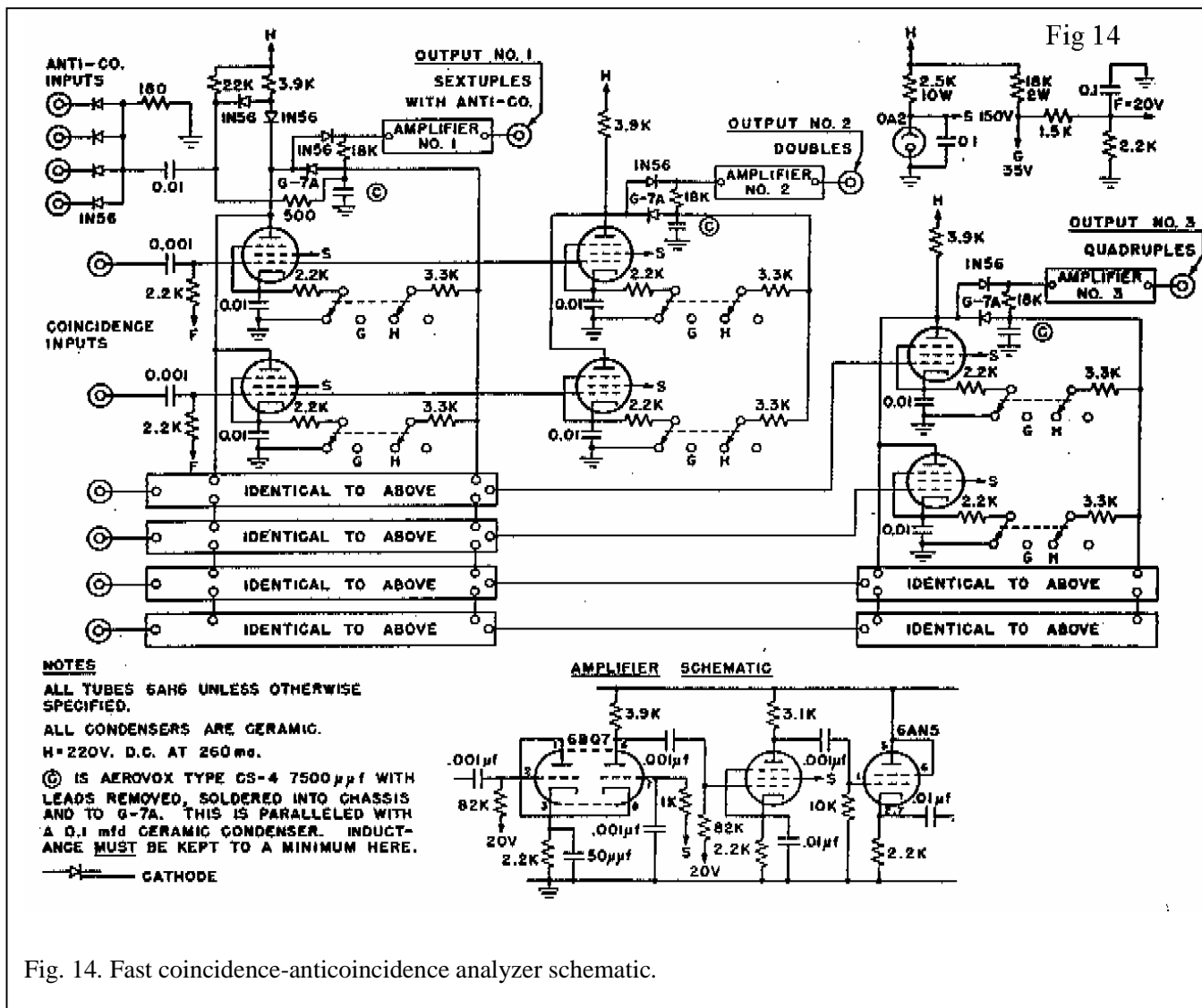


Fig. 14. Fast coincidence-anticoincidence analyzer schematic.

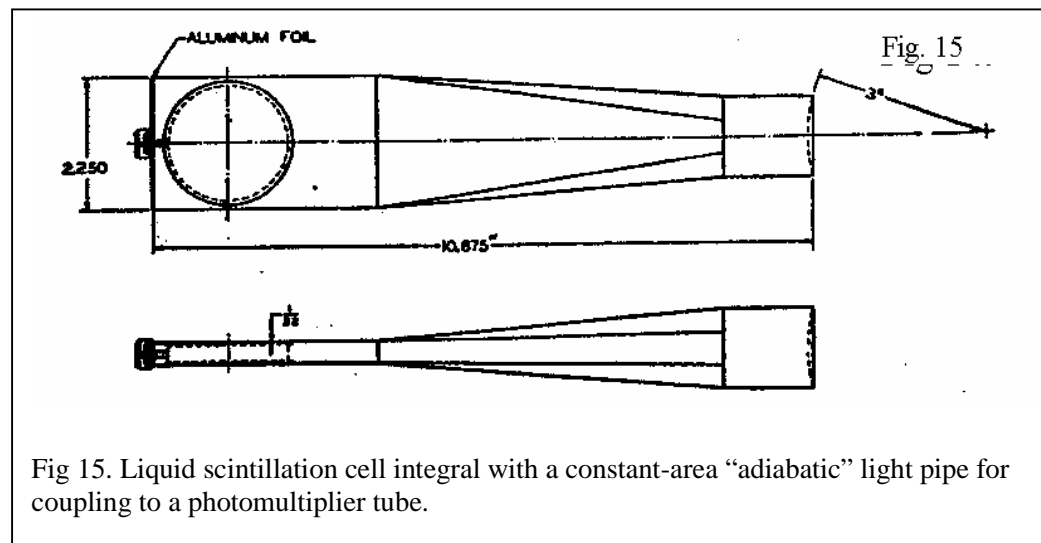
My own Ph.D. thesis topic was a search for angular correlation between the emission of a nuclear beta ray and the ensuing gamma ray decay. For this experimental work, I used the fast coincidence circuit I have just described, but also employed the new photomultiplier tubes and scintillation counters which were

just coming into use. Fermi and Marshall had been using Geiger tubes for their measurements on positronium decay. Their competitor at MIT, Martin Deutch, showed Fermi his own experimental setup, which used the new-found photomultiplier tubes, but not only the side-window type 931 tubes, but also the end-window tubes which Deutch had obtained as developmental models from RCA.

Fermi soon obtained some of these, which were evidently critical if one were to be competitive in nuclear or particle physics. Fermi wisely abandoned the work on positronium, since Deutch was so far ahead, and I was able to use the end-window tubes for my beta-gamma work. They became standard for the particle physics of the era.

The 100-MeV betatron was about to begin operation at the University of Chicago, largely as a kind of training machine for the 450-MeV synchrocyclotron under construction there. We needed to prepare for experiments using the external beams of particles from the cyclotron, and much work went on adapting scintillation crystals to photomultipliers, providing magnetic shields against the fringing field of the cyclotron, and the like.

Ever reluctant to spend money, I decided that rather than grow crystals (although I had already grown naphthalene and anthracene crystals for my Ph.D. thesis (with stilbene doping), I decided that it would be far more economical to use liquid scintillators. So I helped pioneer convenient construction of liquid scintillators and provided some which were integral with a light pipe (Fig. 15) Ref. [15].



Some of my colleagues in the field of particle physics ('high-energy physics', as we called it then), were attempting to collect efficiently the light from large scintillators, to be viewed by one of the expensive end-window phototubes. This could be done with multiple reflections by surrounding the scintillator with a good reflector such as shiny aluminum foil, but if the signal were to be carried a considerable distance, the total internal reflection light pipe would have limitations not previously quantified. In brief, the phase space of the light could not be reduced, and so light could not be squeezed into a light pipe of smaller cross-section ultimately than the entry of the pipe. I published a paper evaluating this limitation, drawing the conclusion that light pipes of constant area could with sufficient ingenuity, be used to carry light long distances without loss despite changes in aspect ratio from a narrow rectangle to a round cylinder the same diameter as the photomultiplier tube. I had some such adiabatic light pipes constructed for my own work.

Following this 1952 paper, I left the University of Chicago for the IBM Laboratory in New York, primarily because I did not want to be limited by describing my own work six weeks in advance to a scheduling committee and having to be part of a team of perhaps six people. Over the years, these numbers have grown to six years and 600 people, so I am glad I made the change.

One further footnote to my activities in this field-- although not particularly relevant to μ SR. In 1960, I published another paper on the collection of light from scintillation counters, pointing at that my adiabatic limit applied only to the original photons. Ref. [19]. If one were willing to take the loss involved in a fluorescent converter to change the frequency of the light, new vistas opened. Fig. 16 makes the point.

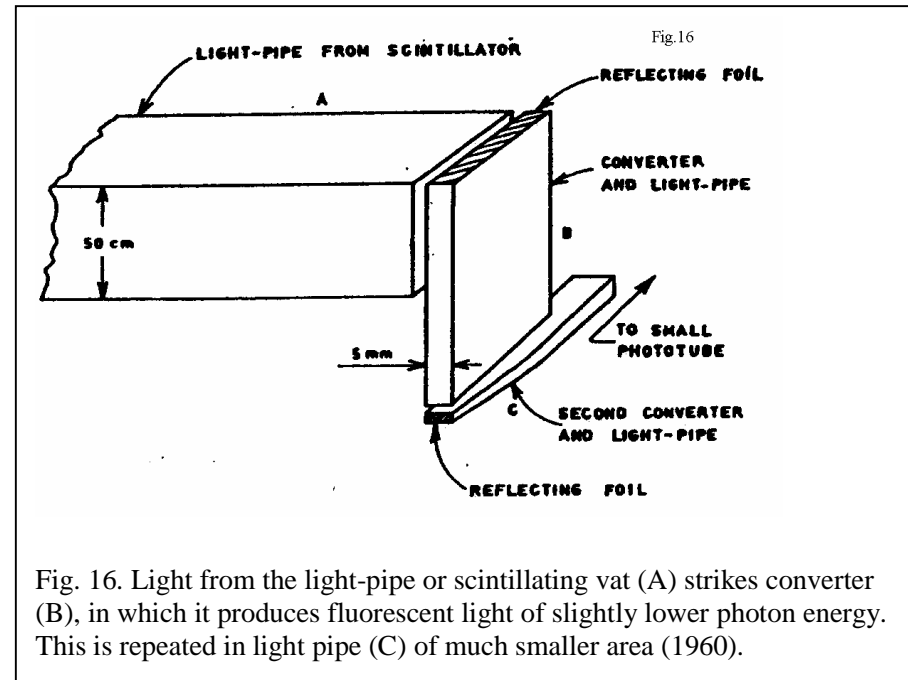


Fig. 16. Light from the light-pipe or scintillating vat (A) strikes converter (B), in which it produces fluorescent light of slightly lower photon energy. This is repeated in light pipe (C) of much smaller area (1960).

Here light is traveling through a light pipe shown conveniently of rectangular cross-section. Across the gap at the end, is another light pipe of the same width, locally doped with a fluorescent converter which can be excited by the original photons from the scintillator carried by the first light pipe. Although the width of the second light pipe is the same as that of the first, it can be very thin, so that there can be a substantial area reduction. And instead of 100 phototubes, a single phototube might do to view a considerable fraction of the light (limited not only by a fluorescent converter efficiency which is almost 100%, but by the fraction of the isotropic fluorescence which can be captured within the acceptance solid angle of the light pipe).

If one is a little bit more greedy, one can have a second converter and a light pipe which is now smaller in both dimensions of cross-section than the original light pipe. So one could use a single phototube instead of 10,000—still seeing perhaps 10% of the initial optical energy.

In particle physics experiments these days, many of the enormous scintillation counter tanks are viewed through a thin 'cable' or fiber optic 'tail' going to phototubes at a considerable distance.

My own work as I have indicated went in December 1952 from particle physics to condensed matter physics, only to resume with parity violation and muons in 1957 through about 1962. After that, I returned entirely to condensed matter physics, to an attempt to detect gravitational radiation, following the erroneous 'discovery' by Joe Weber, and continued work on many applied programs for IBM and the U.S. government.

But that is a long story of little relevance to the wonderful work now being done in muon spin rotation, for which you, the current practitioners, have my thanks and admiration.

With Leon Lederman:

- [1] R.L. Garwin, L.M. Lederman and M. Weinrich 'Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon,' *Physical Review* 105, No. 4, pp. 1415-1417, February 15, 1957.
- [2] Columbia University Physics Department announcement of parity experiments by C.S. Wu, E. Ambler, R.L. Garwin, L.M. Lederman, et al., January 15, 1957.
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