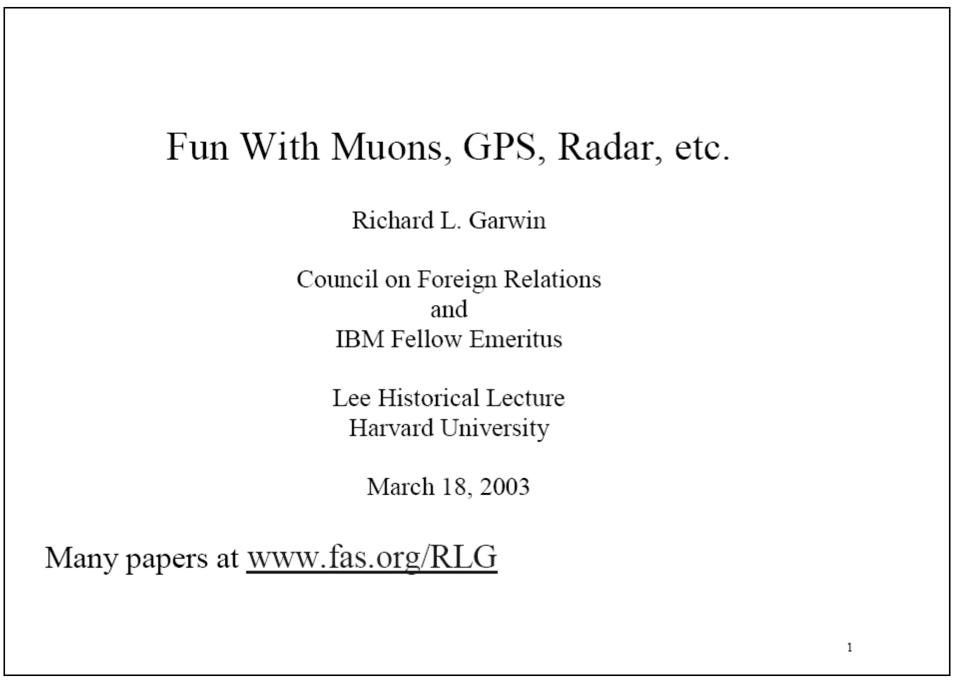
Fun With Muons, GPS, Radar, etc.

Lee Historical Lecture
Harvard University
March 18, 2003
R.L. Garwin

Thank you for the honor of presenting the Lee Historical Lecture. In thinking about this opportunity, I considered two candidates-- first the 1957 demonstration that parity is not conserved-- that a mirror universe can readily be distinguished in the case of the mu meson. The second possibility would be to present my involvement in observations from 1950 to the present regarding defense against ballistic missiles. I chose the parity experiment, but I would be glad to take questions at the end on any subject in which I have been involved.

Foil 1



I begin with a wonderful experiment I had the good fortune to conduct in January 1957 with Leon Lederman and Marcel Weinrich at the Nevis Cyclotron Laboratory of Columbia University. I want to tell briefly the story of that experiment and another which followed it, and then to describe a couple of the simple techniques involved, which I had introduced years earlier to particle physics.

Foil 2

- January 1957 "Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the FreeMuon," R.L. Garwin, L.M. Lederman, and M. Weinrich.
- Some technology of particle physics experiments from the 1950s.
- September 1961 "The Anomalous Magnetic Moment of the Muon," G. Charpak, F.J.M. Farley, R.L. Garwin, T. Muller, J. C. Sens, and A. Zichichi.
- A bit about GPS and other applied technology

2

I then leave physics for technology and policy (both in my talk and in my personal history) and touch lightly on a few topics simple in concept and incredibly complex in implementation with which I was involved.

First the muons. Muons first came on the scene (as "mesotrons" because their mass of 207 electron masses was intermediate between that of the electron and the proton) in the 1930s as the penetrating component of the cosmic radiation. Copiously produced by protons interacting with air nuclei, muons were strangely diffident about nuclear interactions themselves. Only their mass fit the requirement of the Yukawa meson.

The puzzle was solved with the discovery of the pi meson or pion, which in the fullness of time (20 nanoseconds) decays to the muon, which itself has only a weak interaction, in addition to the electromagnetic interaction of its electric charge. Cosmic ray muons contribute significantly to the radiation dose received by humans at sea level-- about twice as much annually as the dose received from the potassium-40 radioactivity built into our bodies with the stardust from which we are all made. Columbia University Physicist I.I. Rabi famously said about the muon, "Who ordered that?"

In the summer of 1956, T.D. Lee and C.N. Yang explored seriously the observable consequences of violation of the laws of conservation of parity and of charge conjugation in weak decays, as would potentially explain the ability of the theta meson to decay into two particles and the tau meson into three particles-- no problem-- unless the tau and the theta were the same, as seemed increasingly likely. To their surprise, they found that although it seemed entirely feasible to investigate parity violation in the case of ordinary radioactive materials, such experiments had not been done. In fact, I recall my mentor, Enrico Fermi, probably around 1950 telling me that he had heard from Ed Purcell of his experiment (with Norman Ramsey) to detect the electric dipole moment of the neutron, which Fermi explained would have to be zero if parity were a good quantum number.

Lee and Yang in their theoretical publication of summer 1956 showed that if a sample of polarized Co-60 nuclei emitted more electrons along the polarizing magnetic field than in the opposite direction (or vice versa), this would be proof of parity nonconservation.

They also calculated that parity violation in the decay of the positive pion would lead to a polarization of the muon along its direction of travel, and that if the muon spin direction were retained while the muon stopped in photographic emulsion about a millimeter away, the decay electrons from the muon decay (two microseconds later) might similarly be more numerous along the direction of flight than in the opposite direction, or vice versa. Prof. Chien-Shiung Wu of Columbia University in the spring of 1956 resolved to do the Co-60 experiment and that summer recruited Ernest Ambler and his colleagues at the National Bureau of Standards in Washington, DC.

Many groups began looking at pi-mu-e decay in photographic emulsion, where it was all too easy to find "effects" caused by scanning bias. To make a long story short, on Friday, January 4, 1957 when I returned from my IBM day leading a program with about 100 people to build a superconducting computer based on thin-film cryotrons, I received a call from my good friend and Columbia physics colleague, Leon Lederman, with news that Ambler, Wu, and company were obtaining positive results. Leon and I agreed to meet at the Nevis Cyclotron of Columbia University in 15 minutes, at 8 p.m.

Necessity (and even adversity) being the mother of invention, we benefited greatly from the fact that the machine shop and the stockroom were closed and that the cyclotron would begin its weekend rest Saturday morning.

Leon and his graduate student Marcel Weinrich had a setup in the external beam of the cyclotron, investigating the decay of muons, plus and minus. The muons were stopped in a block of graphite or other material to be investigated, and the electron decay product of each muon detected as a function of time after the muon(s) stopped. The muons originated from the decay in flight of pions copiously produced in an internal target in the cyclotron by a 400 MeV proton beam. The 85-MeV pions brought out from the cyclotron vacuum in a thin window have a range of about five inches of graphite. Eight inches of graphite thus blocked the pions and put the peak of the muon stopping distribution in the target under investigation. The counting rate in the electron telescope was typically 20 per minute for mu-plus and about 100 per minute for mu-minus with a background of about one count per minute.

The arrangement that Lederman and Weinrich had used is shown in the first figure. Ignore the coil!

Foil 3[1]

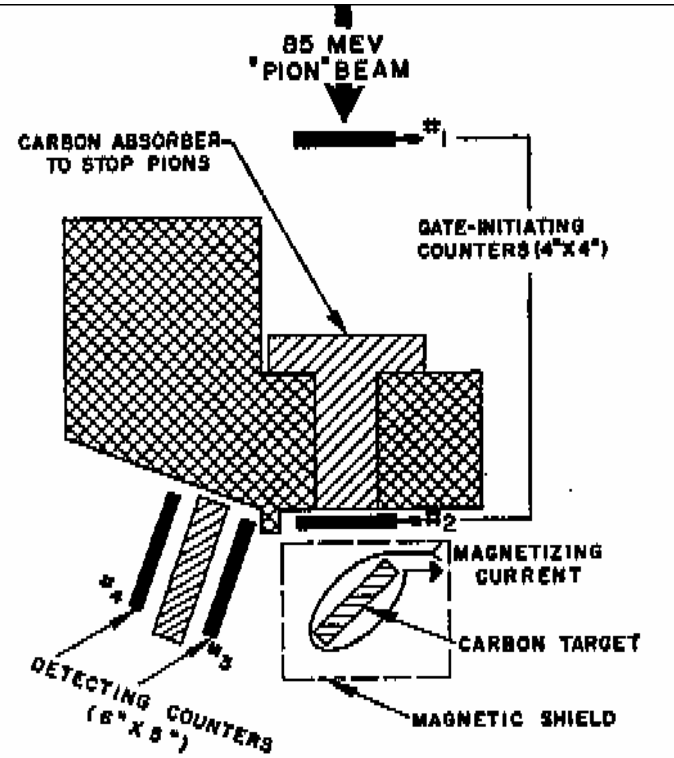


Fig. 1. Experimental arrangement. The magnetizing coil was close wound directly on the carbon to provide a uniform vertical field of 79 gauss per ampere.

Leon's brilliant idea that night was to depart from the suggestion to study muons originating from stopped pions, that would be polarized along their direction of flight. Their range was less than a millimeter, and so it was difficult to separate a population of polarized muons from the stopped pions, which had a much broader distribution in range.

Our beam of cyclotron muons, however, originated from the decay in flight of pions. Muons emitted forward would have the velocity of the pion in its rest frame plus (roughly speaking, since the velocities add relativistically) the relatively small additional velocity of a muon which is born with an energy of 4 MeV in the rest frame of the pion. The forward emitted muons have greater momentum than the pions, and those emitted backward have less, so that the forward-emitted muon "contamination" of the pion beam would be strongly polarized if parity conservation were maximally violated.

So it remained only to try to measure the relative numbers of electrons in different directions from the stopped muons. Easier said than done, because we could not stay in the beam room because of radiation background, and our electronics were 150 meters away (and 30-m difference in altitude). We would need to shield the muons from the fringe field of the cyclotron so that their (unknown) magnetic moment would not lead to precession during their decay lifetime, and we would need to move a counter telescope around the muon population. Furthermore, electrons emitted in different directions would traverse different amounts of the stopping block, so that the expected small asymmetry might be masked by such dirt effects.

In my work at the IBM Laboratory at Columbia University, I had been studying liquid and solid helium 3 for several years, by the methods of spin echoes. I judged that we could make a virtue of necessity and apply a uniform magnetic field to the muon population so that any asymmetry in decay probability would precess with the muon spin, in the horizontal plane. Within a couple of hours we had fashioned from scrap a lucite cylinder surrounding the graphite stopping block, and a coil of fine enameled copper wire wound on the cylindrical lucite coil form. We started taking data with different values of applied current (hence precessing field) and found substantial effects on counting rate as a function of coil current for the telescope set at a fixed position. Unfortunately, when the cyclotron shut down for the weekend at 9 a.m. Saturday, the effect seemed to have vanished. Returning to the beam room, we found that the copper wire had overheated and expanded and was lying at the base of the coil form, no longer serving its function of precessing muon spins.

Monday was maintenance day at the cyclotron so we were runnng again only Monday night, this time with a rectangular solenoid wound directly on the graphite stopping block. I had chosen graphite for positive muons, because its low atomic number would minimize the scattering of the decay electrons, and its quasi-metallic nature would prevent the formation of muonium, which would subject the muon to the enormous magnetic field of its partner electron.

Of course, we did not know that parity was violated—that was the purpose of the experiment-- and to what extent. We did not know the spin of the muon-- 1/2 and 3/2 were candidates. And we did not know the magnetic moment or g value, which for the electron is 2.00 (almost).

We began taking data about midnight, with 20-minute counting intervals and by 6 a.m. we had the curve shown in Fig. 2. We wrote up the paper that day and were ready to publish by Tuesday afternoon January 8.

Foil 4[1]

Reprinted from THE PHYSICAL REVIEW, Vol. 105, No. 4, 1415–1417, February 15, 1957
Printed in U. S. A.

Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon*

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(Received January 15, 1957)

LEE and Yang[1–3] have proposed that the long held space-time principles of invariance under charge conjugation, time reversal, and space reflection (parity) are violated by the "weak" interactions responsible for decay of nuclei, mesons, and strange particles. Their hypothesis, born out of the $\tau-\theta$ puzzle,[4] was accompanied by the suggestion that confirmation should be sought (among other places) in the study of the successive reactions

$$\pi^+ \rightarrow \mu^+ + \nu, \qquad (1)$$

$$\mu^+ \rightarrow e^+ + 2\nu. \qquad (2)$$

They have pointed out that parity nonconservation implies a polarization of the spin of the muon emitted from stopped pions in (1) along the direction of motion and that furthermore, the angular distribution of electrons in (2) should serve as an analyzer for the muon polarization. They also point out that the longitudinal polarization of the muons offers a natural way of determining the magnetic moment.[5] Confirmation of

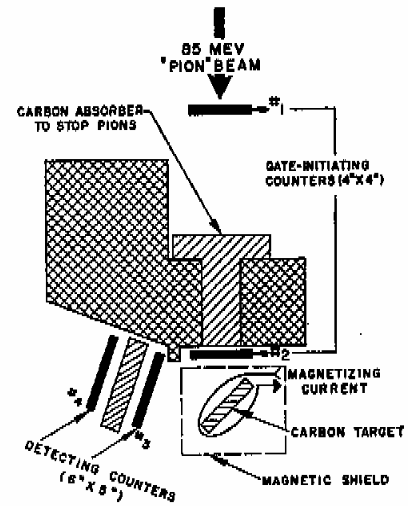


FIG. 1. Experimental arrangement. The magnetizing coil was close wound directly on the carbon to provide a uniform vertical field of 79 gauss per ampere.

Here is our paper (Fig. 3). This was the first in the new field of muon physics and opened the door to the substantial current field of muon spin rotation (or muon spin resonance)-- MuSR.

Our first experiment was not suitable for a high-precision measurement of the spin rotation rate of the muon in a large magnetic field because of the smearing on the distribution in a finite gate width. Accordingly, we used magnetic resonance intervention by flipping the muon spin, which necessitated applying an rf pulse to the sample. This meant that because of the short lifetime of the muon, the coincidence selection of the muon needed to be in the experimental room. I used a "high power" rf triode to supply a large pulse of rf to flip the presumed longitudinal muon spin from the forward to the back direction. This necessitated an insulating material for stopping the muons, and we chose a dense liquid—bromoform.

In turn, this approach could not be scaled to ever higher precision, and James Rainwater, active in physics at the cyclotron, implied that he knew a different way, not subject to that limitation. He said that he would carry out the experiment if we didn't think of the new approach, so we did, and found it good.

This did not require intervention with the muon spin, but only the use of a couple of decay telescopes with the muon stopped with spin transverse to a steady, intense magnetic field. This "stroboscopic" experiment was done in two flavors, both of which succeeded.

Foil 5[1]

$\theta = 100°$. We now apply a small vertical field in the magnetically shielded enclosure about the target, which

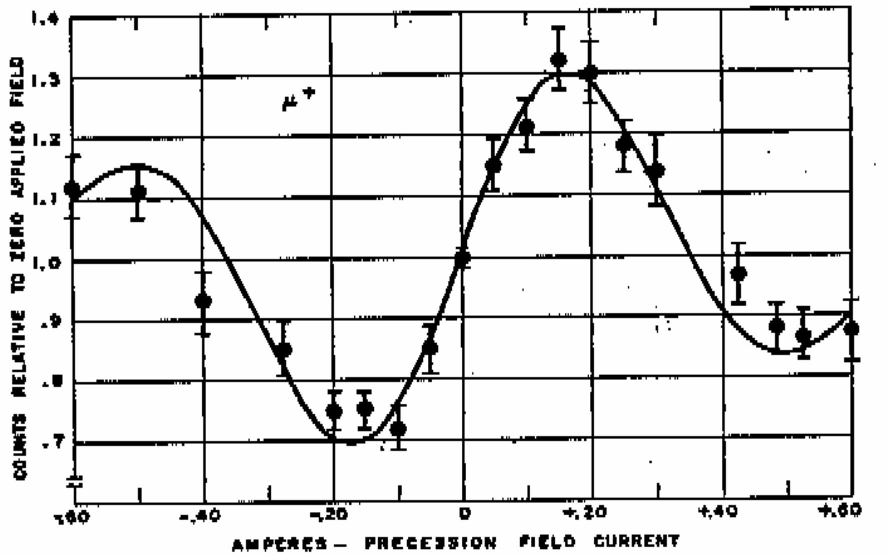


FIG. 2. Variation of gated 3–4 counting rate with magnetizing current. The solid curve is computed from an assumed electron angular distribution $1-\frac{1}{3}\cos\theta$, with counter and gate-width resolution folded in.

Foil 6[1]

nuclei (even in Pb, 2% of the μ^- decay into electrons[9]), atoms, and interatomic regions.

The authors wish to acknowledge the essential role of Professor Tsung-Dao Lee in clarifying for us the papers of Lee and Yang. We are also indebted to Professor C. S. Wu[6] for reports of her preliminary results in the Co^{60} experiment which played a crucial part in the Columbia discussions immediately preceding this experiment.

* Research supported in part by the joint program of the Office of Naval Research and the U. S. Atomic Energy Commission.

† Also at International Business Machines, Watson Scientific Laboratories, New York, New York.

1 T. D. Lee and C. N. Yang, Phys. Rev. 104, 254 (1956).

2 Lee, Oehme, and Yang, Phys. Rev. (to be published).

3 T. D. Lee and C. N. Yang, Phys. Rev. (to be published).

4 R. Dalitz, Phil. Mag. 44, 1068 (1953).

5 T. D. Lee and C. N. Yang (private communication).

6 Wu, Ambler, Hudson, Hoppes, and Hayward, Phys. Rev. 105, 1413 (1957), preceding Letter.

7 The Fierz-Pauli theory for spin $\frac{3}{2}$ particles predicts a g value of $\frac{2}{3}$. See F. J. Belinfante, Phys. Rev. 92, 997 (1953).

8 V. Fitch and J. Rainwater, Phys. Rev. 92, 789 (1953).

9 M. Weinrich and L. M. Lederman, *Proceedings of the CERN Symposium, Geneva, 1956* (European Organization of Nuclear Research, Geneva, 1956).

10 The field interval, ΔH, between peak and valley in Fig. 2 gives the magnetic moment directly by $(\mu\Delta H/s\hbar)(t_1+\frac{1}{2}T)\delta=\pi$, where $\delta=1.06$ is a first-order resolution correction which takes into account the finite gate width and muon lifetime. The 5% uncertainty comes principally from lack of knowledge of the magnetic field in carbon. Independent evidence that $g=2$ (to ~10%) comes from the coincidence of the polarization axis with the velocity vector of the stopped μ's. This implies that the spin precession frequency is identical to the μ cyclotron frequency during the 90° net magnetic deflection of the muon beam in transit from the cyclotron to the 1–2 telescope. We have designed a magnetic resonance experiment to determine the magnetic moment to ~0.03%.

11 *Note added in proof.*—We have now observed an energy dependence of a in the $1+a\cos\theta$ distribution which is somewhat less steep but in rough qualitative agreement with that predicted by the two-component neutrino theory $(\mu\rightarrow e+\nu+\bar{\nu})$ without derivative coupling. The peak-to-valley ratios for electrons traversing 9.3 g/cm², 15.6 g/cm², and 19.8 g/cm² of graphite are observed to be 1.80±0.07, 1.84±0.11, and 2.20±0.10, respectively.

Foot 7[1]

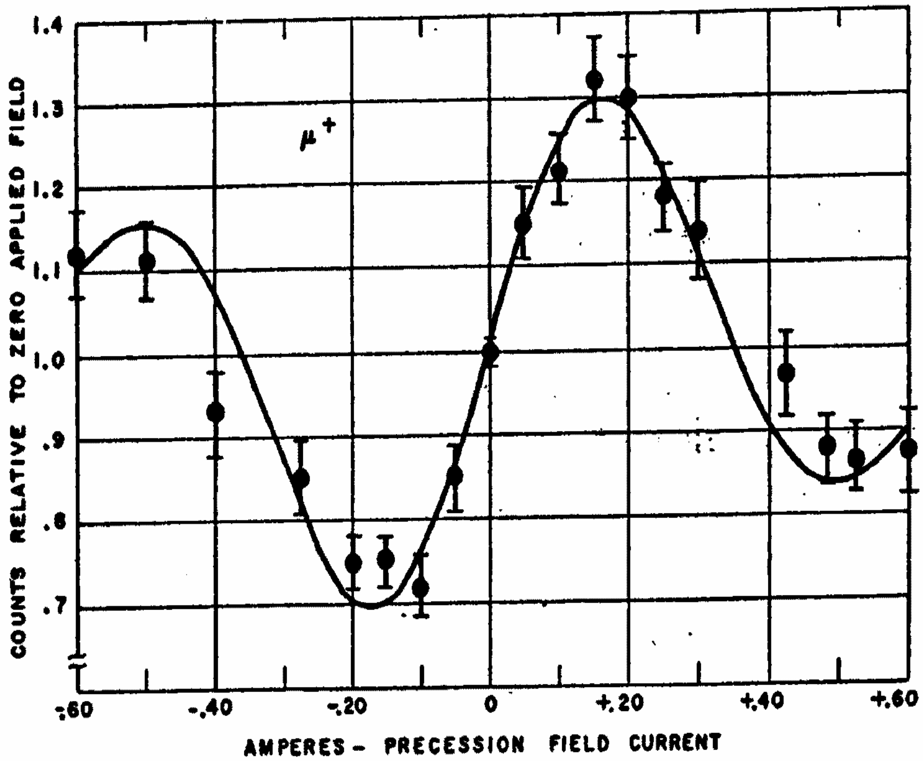
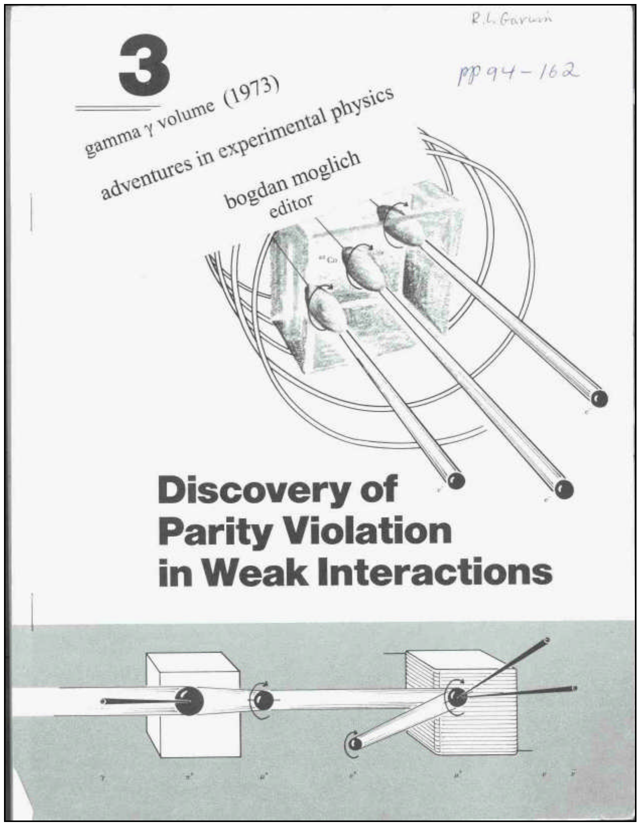


FIG. 2. Variation of gated 3–4 counting rate with magnetizing current. The solid curve is computed from an assumed electron angular distribution $1-\frac{1}{3}\cos\theta$, with counter and gate-width resolution folded in.

Foil 8[2]



Within days of our pi-mu-e discovery, I resigned my leadership of the superconducting computer program at IBM to pursue the new field opened by the discovery of parity nonconservation and the properties of muons. Here is the result of an experiment we soon did on precision measurement of the muon magnetic moment.

Foil 9[3]

The decay electrons emerging in the backward direction after the rf pulse was over were counted; the "peak" rate was obtained for no transition, a decrease in rate

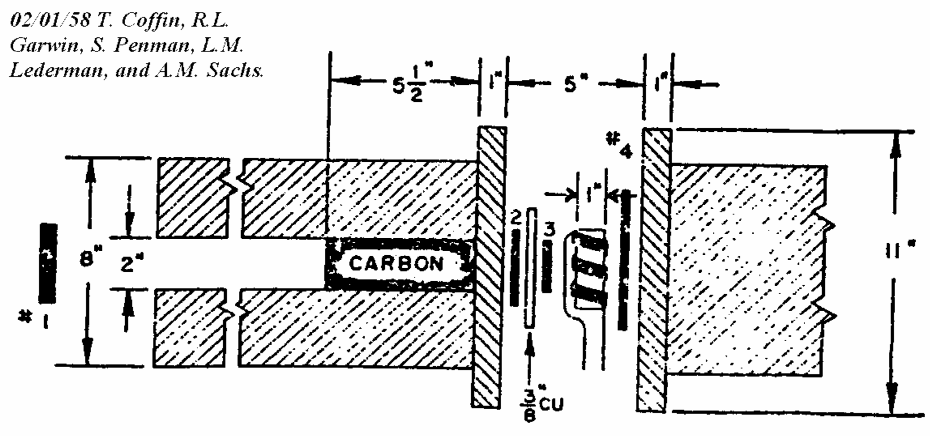


FIG. 1. Experimental arrangement.

Foil 10[3]

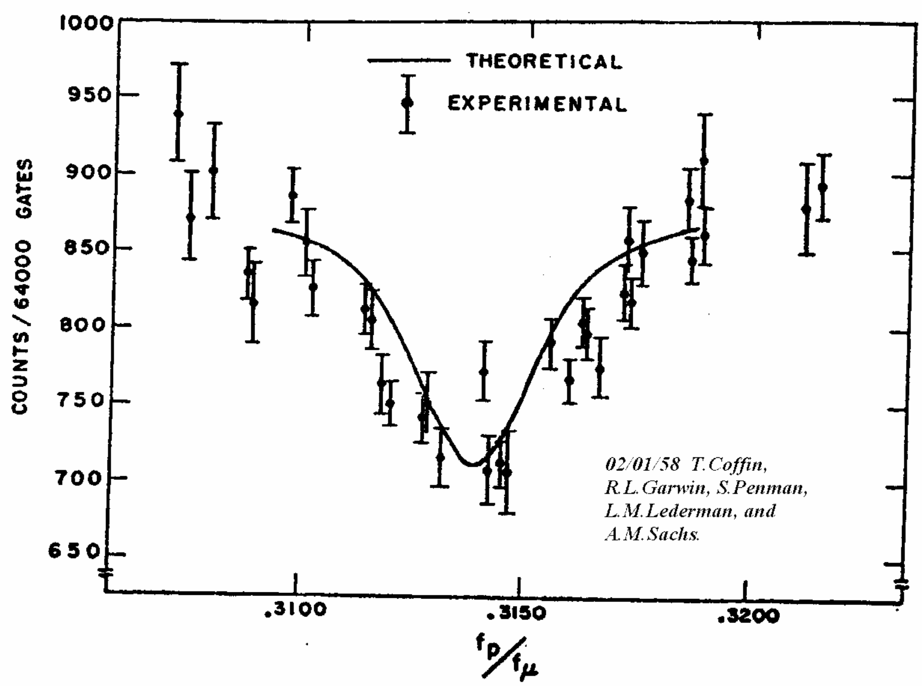


FIG. 5. Theoretical line shape and composite of the experimental points.

I was much involved at that time not only with my spin-echo research into liquid and solid helium-3 and helium-4, but on national security matters in Washington as a consultant to the President's Science Advisory Committee. When I received a Ford Foundation Fellowship to spend a year at CERN 1959-60, I wanted nothing more than to sit in the library at CERN to re-learn physics. But I was dragooned into leading a small group pursuing a suggestion Leon and I had made in our first paper-- the direct measurement of the muon g-2.

We had established that the muon had spin 1/2 and g very nearly 2. Because the muon is 207 times as heavy as the electron, the contributions to its anomalous magnetic moment probe energies that much higher than are involved in the electron anomaly. So this would be a significant experiment, despite the fact that it would yield only a single number. After my initial reluctance, I threw myself into this experiment, where we all had a lot of fun. Here is a sketch of the experiment and just a mention of the wonderful techniques and some of those who had primary responsibility for different aspects of the work. H.R. Crane at the University of Michigan had performed high-precision measurements of the electron g-2, taking advantage of the fact that for g=2 the longitudinal polarization of the electron spin does not change with time in an arbitrary static magnetic field. But the anomalous magnetic moment (i.e., the deviation from g=2) can result in a secular precession of the spin relative to the momentum.

Foil 11[4]

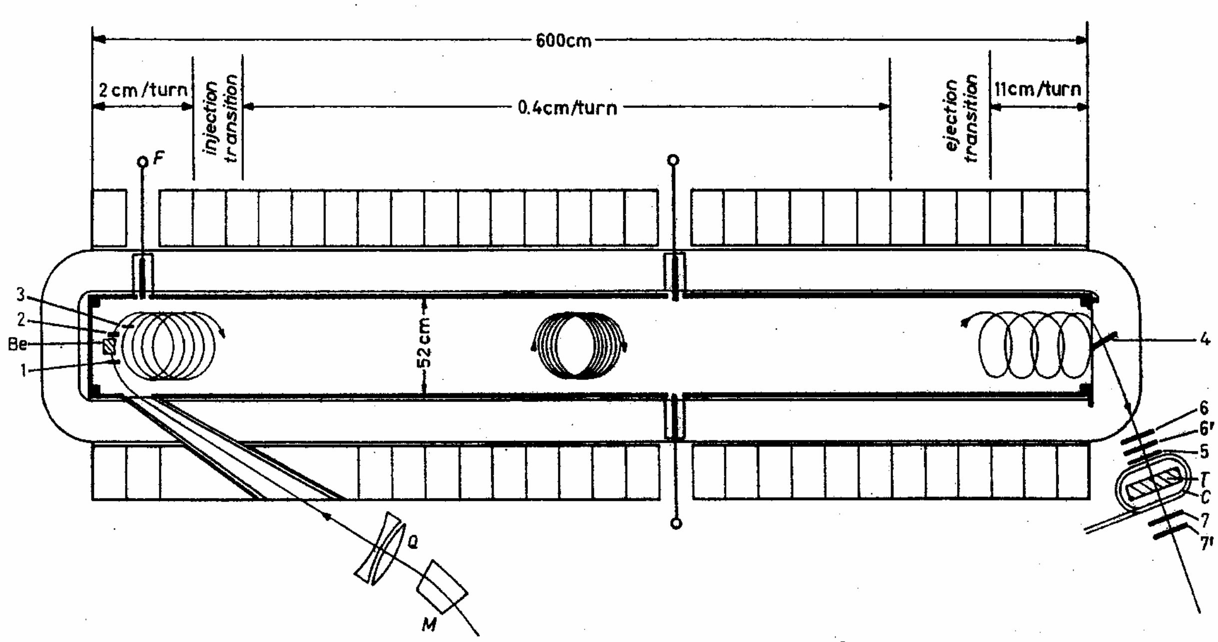


Fig. 1. – General view of the apparatus, showing magnet of pole surface (600 × 52) cm². Muons, deflected by the bending magnet *M* and focused by the quadrupole pair *Q* enter the magnet via a shielded channel. After slowing down in the beryllium moderator Be they describe many turns in the field. The quasi-circular orbit is slowly displaced by the field gradient (2 cm/turn in the *injection* region, 0.4 cm/turn in the *storage* region, and 11 cm/turn in the final *ejection* region). Muons ejected from the magnet are stopped in target *T* of the polarization analyser where the spin direction is determined by recording the decay electrons. Injected muons are indicated by the counter signature 123. Ejected muons by the signature $466'\,5\bar{7}$. Decay electrons by $66'\,4(\overline{77'})$ and $77'\,\bar{4}(\overline{66'})$. The time of flight of muons between counters 2 and 4 is recorded.

Preliminary explorations of this muon g-2 experiment had been carried out under the leadership of Leon Lederman the previous year at CERN. The paths of muons in complicated magnetic fields could be mimicked by alpha-particle trajectories from natural sources, and we did put alpha particles in our 6-m magnet. Ironically, a far better vacuum is required for alpha particles than for muons, because alphas are lost if they snatch an electron from any residual gas-- a process with an atomic cross-section rather than a much smaller nuclear cross-section. So our vacuum system for the 6-m magnet was designed for this simulation rather than for the much easier conduct of the actual experiment.

Foil 12[4]

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

123pps.

Foil 13[4]

The Anomalous Magnetic Moment of the Muon.

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

(ricevuto il 18 Settembre 1964)

Summary. — The anomalous part of the gyromagnetic ratio, $\mathbf{a} = \frac{1}{2}(g-2)$ of the muon has been measured by determining the precession $\theta = \mathbf{a}\omega_0 \bar{B} t$ 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 $\mathbf{a}_{exp} = (1162 \pm 5)\cdot 10^{-6}$ compared with the theoretical value $\mathbf{a}_{th} = \alpha/2\pi + 0.76\,\alpha^2/\pi^2 = 1165\cdot 10^{-6}$. 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 $\sim 10^3$ 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.

- Antonino Zichichi accepted the responsibility of shimming the 6-m magnet in order to obtain the trajectories as shown in Foil 11. This was preceded by work by Panofsky on the orbit stability. The magnetic field perpendicular to the plane of the paper, Z, in Foil 11 needed to have a gradient in the transverse Y direction, to provide the orbit drift along X (left to right in the figure, from the injection end of the magnet to the extraction end). The drift per orbit needed to be substantial--2 cm/turn in the injection region-- in order that the second orbit should miss the Be block in which the orbital radius was reduced to remain within the magnet. But in order to use the space efficiently and to obtain thousands of turns in the magnetic field, the gradient was reduced by a factor five to provide orbit drift of 0.4 cm/turn in the storage region. The orbits would simply have drifted to the right end but would not have emerged had the field gradient not been increased to the point where the muon orbit encountered the rapid falloff in magnetic field in a non-adiabatic fashion. We designed the magnetic field, but it was Zichichi's job to produce it in reality by a succession of measurements throughout the magnet and by tailoring the field primarily with magnetic shims, except in the extraction region, where the large field gradient required milling 15 mm into the removable pole face.

Foil 14[4]

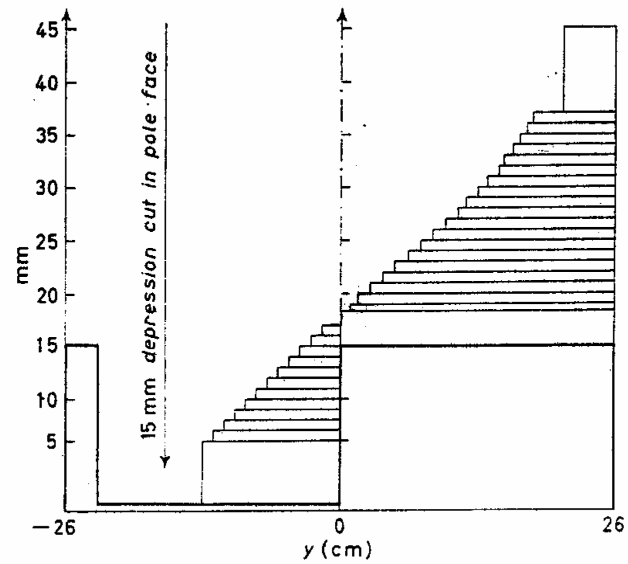


Fig. 19. – Shims for ejection field. In order to establish the desired gradient while maintaining the field constant at $y=0$, it was necessary to cut away iron from the pole face on the weak-field side.

Foil 15[4]

42 G. CHARPAK, F. J. M. FARLEY, R. L. GARWIN, ETC. [1282]

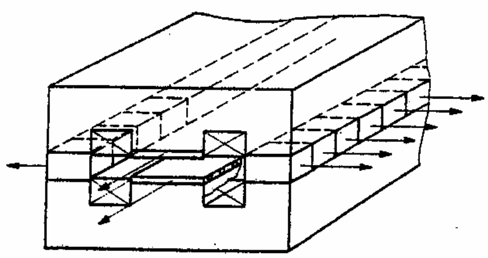


Fig. 23. – Overall view of the magnet showing the removable iron blocks ($20\times20\times\times31$ cm^3) in the yokes, and the removable top and bottom pole pieces (5 cm thick). To remove the poles the lower pole was first raised by hydraulic jacks via vertical push rods passing through the lower half of the magnet. An assembly of rollers was then introduced below this pole, and finally the upper and lower poles were rolled out of the magnet together onto a special table also equipped with rollers. Arrows indicate those parts of the magnet which can be taken out.

- Managing injection, transition, storage, and ejection—Farley

The required field was calculated by Francis Farley, who went on to make further exquisitely sensitive measurements of the muon gyromagnetic ratio.

- Theo Muller and Hans (J.C.) Sens accepted responsibility for the electronics.

All this is described in great detail in our 123-page paper[4].

Foil 16[4]

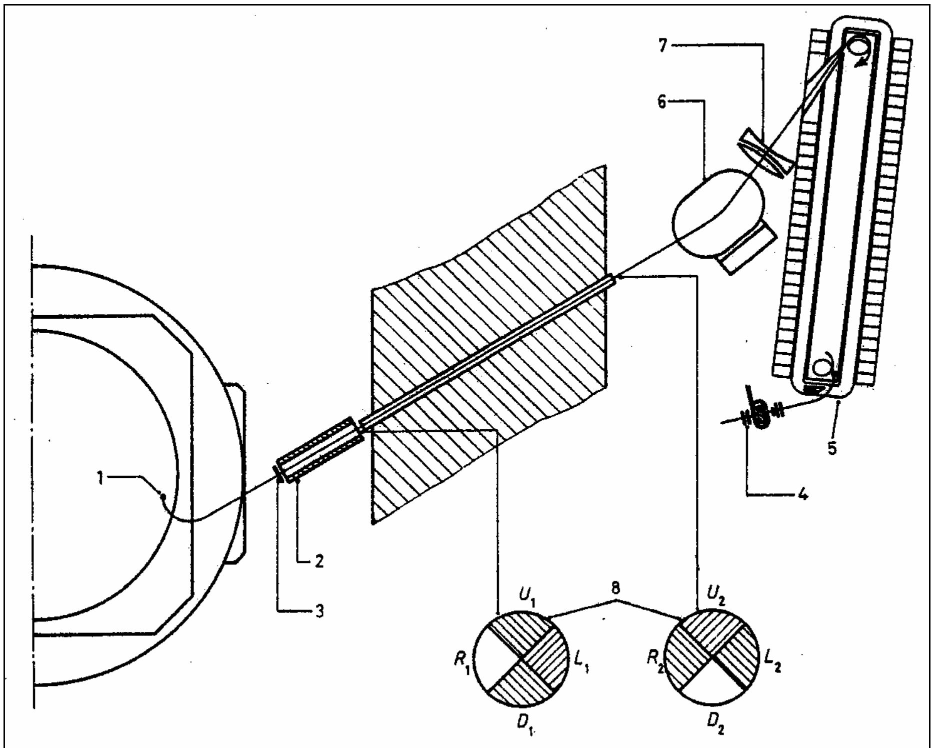


Fig. 48. – Final layout of experiment, showing cyclotron, solenoid, pipe through shielding wall with the possibility of dividing each end of the pipe into quadrants to study beam structure. Note the lead scattering foil added to scramble the muons into different beam cells, thus reducing spin-angle/position correlations. 1) target; 2) solenoid; 3) lead foil; 4) polarization analyser; 5) storage magnet; 6) bending magnet; 7) quadrupole lenses.

- Polarization detection telescope—Charpak

The spin polarization was determined for each muon by an analysis telescope for which Georges Charpak was responsible. It measured the number of electrons emitted forward and backward with respect to the trajectory of the muon entering the counters shown as "4" in Foil 16. Many measurements needed to be made in order to investigate and compensate small effects due to non-uniformity of muon beam, etc.

Charpak was so enthusiastic about the results of the experiment and the quality of collaboration that he concentrated his career on inventing and improving particle detectors for physics, biology, and medicine, which won him the Nobel Prize in 1992.

- As for myself, I provided an end-to-end simulation of the experiment so that I could put in an assumed anomaly, a, such that $g = 2(1+a)$ so that $a = (g-2)/2$ and a simple computer program that would produce mock results such like those shown in Foil 18. These mock results were supplied to Francis Farley just as if actual muons had been detected by Charpak's polarization telescope and their counts stored in the electronics of Muller and Sens.

Farley would give me the resulting (g-2), and after a few tries, we got it right—in that the analytic result from the simulated counts reproduced the input value of "a" that I had provided.

We published in *Nuovo Cimento* because the *Physical Review* declined to allow us enough pages to present what I strongly believed were important experimental details of measurement of magnetic field, trimming of the magnet, analysis of uncertainties, and the like.

By the time data were being obtained from this experiment, I had returned to my normal work at the IBM Laboratory at Columbia University. In those pre-email days, the data were provided by telegraph. I fit them by least squares and also by a maximum likelihood method to the curve of expected form, as shown in Foil 18.

Foil 17[4]

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noid of length 200 cm (see Fig. 48). The field and length of the solenoid are chosen to rotate the transverse component of the muon spin by 90°, whereas the component along the axis of the solenoid is not affected. For our 150 MeV/c muons and 200 cm available length, a magnetic field $B = 3\,950$ G is required to obtain 90° rotation.

The polarization was re-measured with the *solenoid on* as a function of range with the results of Table VI, which are plotted in Fig. 49. The results of a least-squares fit to this data are

(86) θ_{beam} (at range $X = 10$ cm carbon) $= -(\ 0.5 \pm 12)$ mrad ,

(87) and slope $(d\theta/dR)_{beam}$ $= -(19 \pm 12)$ mrad/cm .

Foil 18[4]

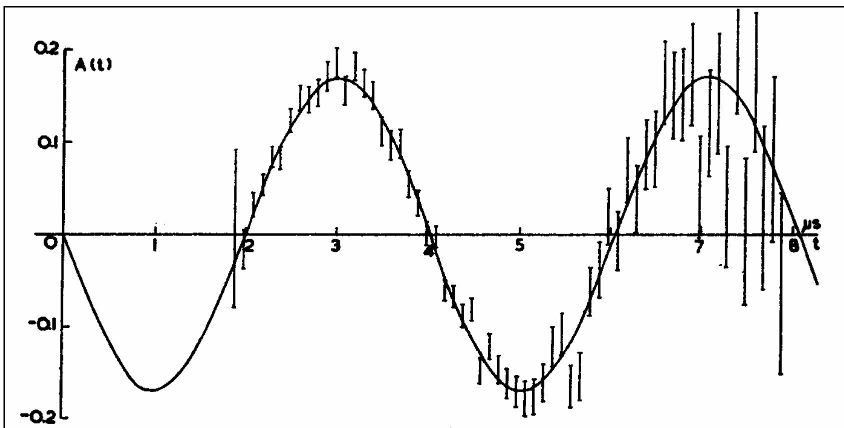

Fig. 4. Electron asymmetry $A(t)$ for $\pm$ 90° flipping (combined data from forward and backward telescopes) as a function of storage time t with curve showing the best fit obtained by varying A_0 and a in eq. (8).

Foil 19[4]

Table 3

Anomalous moment evaluation for separate runs of $A(t)$ vs. t. The distribution of the individual results about the combined value 1162 is statistical, $\chi^2 = 20.3$, expected value 19.3.

Run	$a_{exp} \times 10^6$	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

→ 1162 ± 5

Needless to say, we were all pleased with this result, to be compared with the quantum electrodynamics prediction of 1165, as shown in Foil 12.

Which reminds me why our first hydrogen bomb "experiment" November 1, 1952 was the test of a full-scale explosion—it was just easier than to do than a convincing experiment at a smaller scale. But that is another story.

I had left particle physics when I joined IBM in December 1952, but that had been my field at the University of Chicago since 1949. And the experiments at Columbia and throughout the physics community were done with technology I had helped pioneer at Chicago. I will mention two aspects here-- fast, flexible coincidence circuits and "adiabatic light pipes" for efficiently gathering light from scintillation counters and transferring it to the cathode of a photomultiplier tube.

For my Ph.D. thesis in 1949-- the first study of beta-gamma angular correlation in the decay of radioactive materials-- I wanted to take advantage of the new technique of end-window photomultiplier tubes and fast organic scintillators. But my experiments would have gone slowly had I been limited to the existing Rossi-type coincidence circuits with coincidence windows of about a microsecond. I needed something better, and so did the entire nuclear and high-energy physics field. So I began first with a convenient laboratory pulse generator capable of sub-nanosecond pulses. As shown in Foil 20, I could in this way conveniently produce square pulses of duration a fractional nanosecond up to a good fraction of a microsecond, and split the pulse into three channels, each of which could have independent cable delays.

Foil 20[5]

If now the copper shield diameter is so chosen as to make an approximate impedance match with some

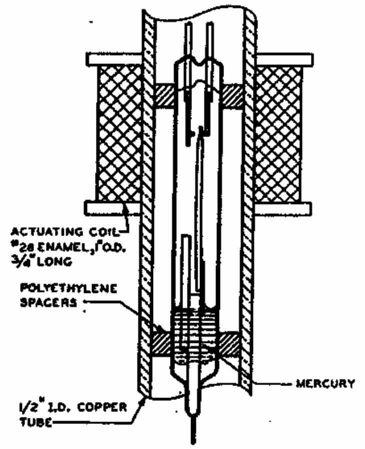

Fig. 1. Rapid coaxial switch.

[1] The glass-enclosed switch is taken from a Western Electric D-168479 synchronous relay.

The indicated pulse heights hold, of course, only if the system is matched in impedance at each point. Thus, if an output is not being used, it must be terminated in its characteristic impedance to avoid reflections. The system impedance used in the pulser here constructed is 75 ohms, for which no constant-impedance connectors are available. Accordingly, the AN-83 series connectors

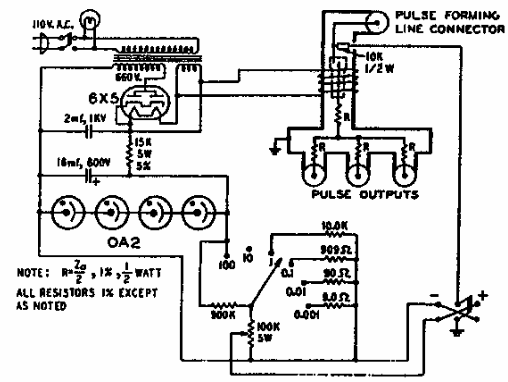

Fig. 2. Pulser schematic.

Foil 21[6]

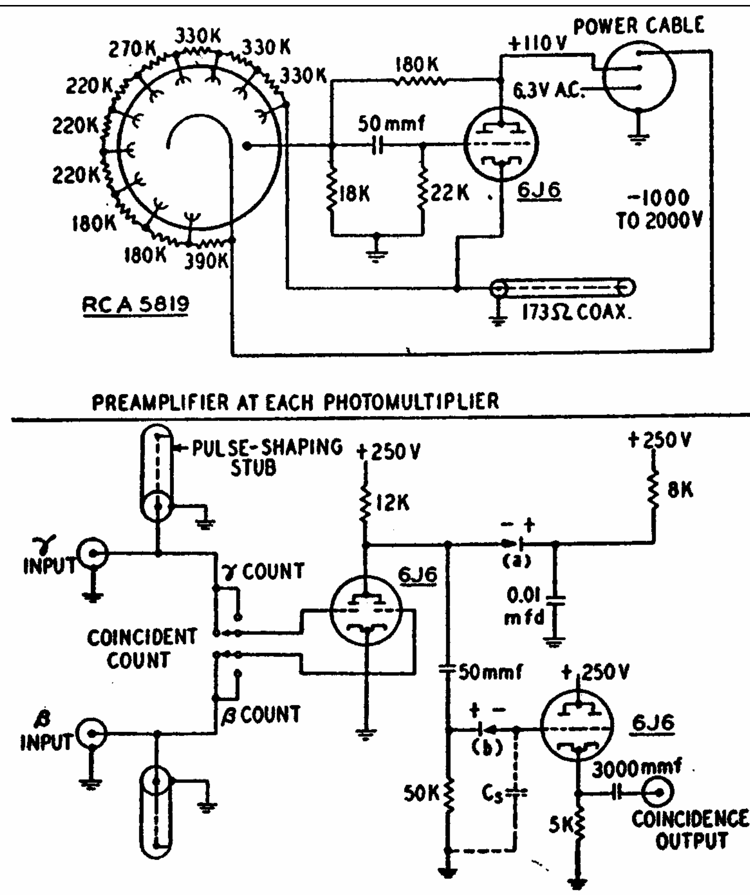

Fig. 1.

Foil 22[6]

Reprinted from THE REVIEW OF SCIENTIFIC INSTRUMENTS, Vol. 21, No. 6, 569, June, 1950
Printed in U. S. A.

A Useful Fast Coincidence Circuit

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Institute for Nuclear Studies and Department of Physics, University of Chicago, Chicago, Illinois

February 17, 1950

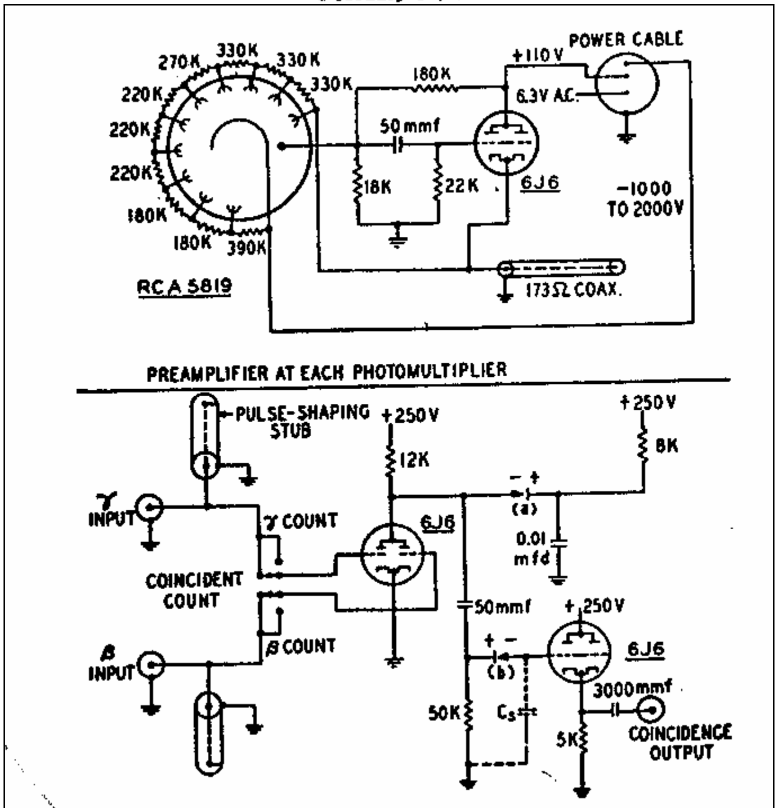

FIG. 1.

Foil 23[7]

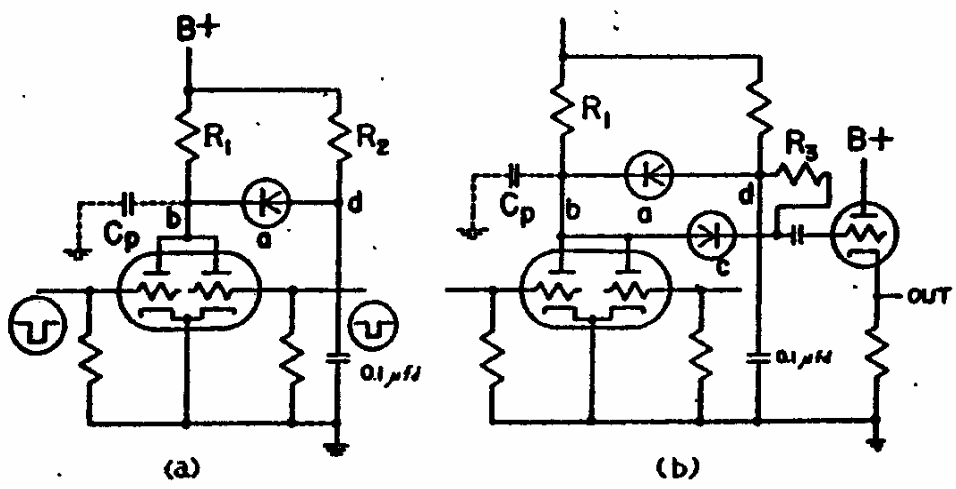

FIG. 1. (a) Modified Rossi coincidence circuit. (b) Same with self-biased discriminator.

Foil 24[7]

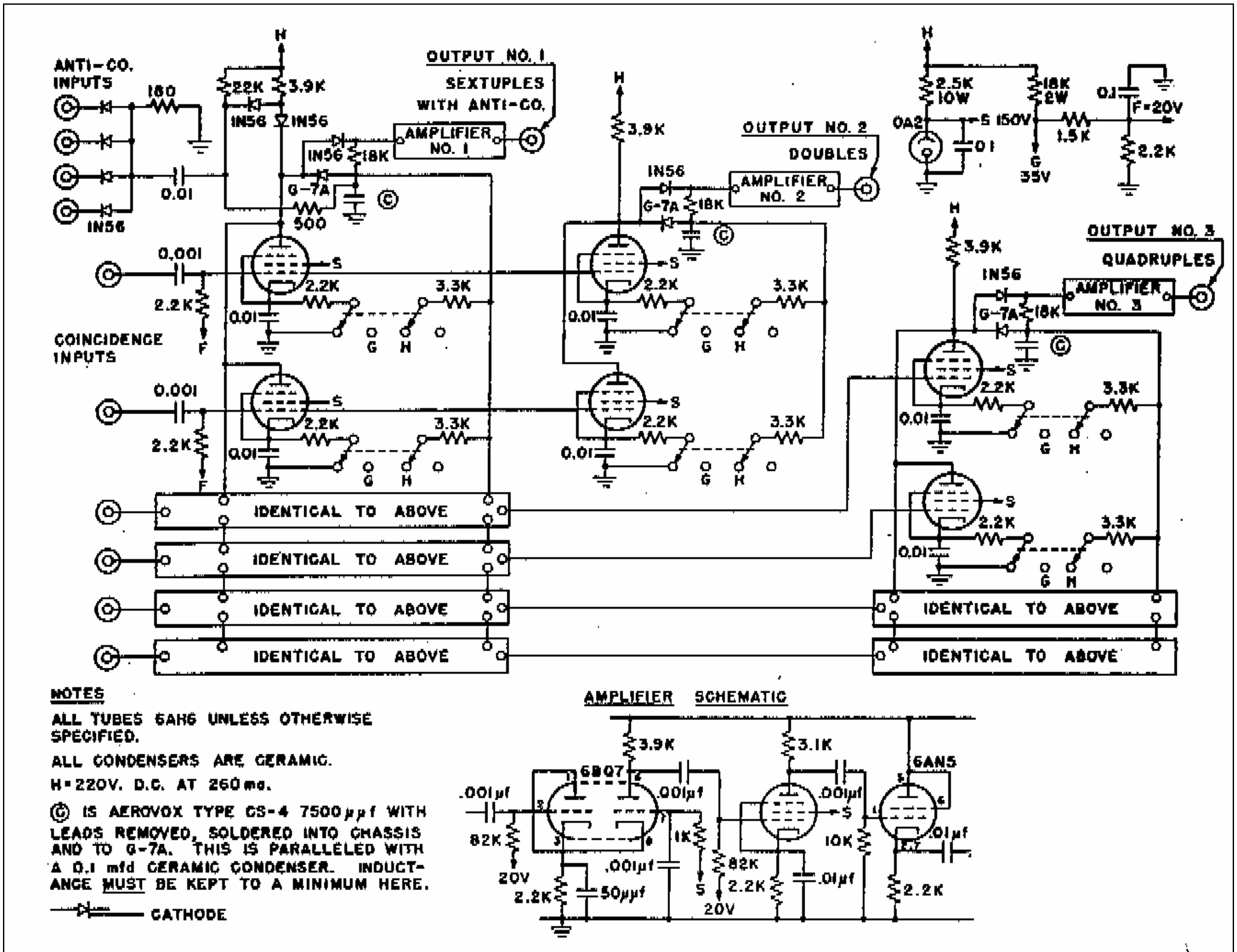

FIG. 4. Fast coincidence-anticoincidence analyzer schematic.

Reprinted from THE REVIEW OF SCIENTIFIC INSTRUMENTS, Vol. 23, No. 12, 755–757, December, 1952
Printed in U. S. A.

The Design of Liquid Scintillation Cells*

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(Received April 18, 1952)

NEW types of liquid scintillator cells have been developed here, of such excellence that they have entirely supplanted ast crystal scintillators. The new cells are made possible by the liscoveries that the scintillation fluid (3 g terphenyl and 10 mg

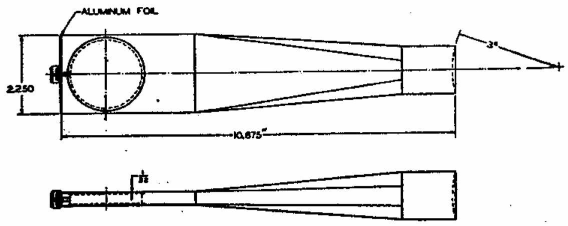

FIG. 1.

liphenylhexatriene[1,2] per liter of phenylcyclohexane) does not lissolve appreciable amounts of Lucite (first observed here by G. Yodh) and that small amount that does dissolve has no effect on the scintillations. The diphenylhexatriene increases the photomultiplier response by a factor two, by changing the emitted spectrum from the invisible ultraviolet to the blue, thus reducing self-absorption in the liquid and in the Lucite light pipes and matching the 5819 photosensitivity more accurately. Clearly, any phenylcyclohexane sample that looks water-white in the depth to be used does not absorb appreciably the spectrum emitted from a scintillation.

Figure 1 shows a 2-in. diameter by 1-cm thick liquid cell with $\frac{1}{32}$-in. Lucite windows. The windows are attached with glacial acetic acid, which makes an optically clear joint unaffected by this scintillation fluid. The cell shown is polished on all surfaces and acts as a light pipe to conduct the light from the scintillation to the 5819 photomultiplier which is attached by means of a film of grease to the spherical depression at the left. It is easy to show that one can conduct essentially all the light traveling by total internal reflection along a light pipe of any cross-sectional shape into another light pipe of any other cross-sectional form, so long as the cross-sectional *areas* of the two light pipes are equal.[3] The only requirement on the transition section is that it be adiabatic, i.e., have small angles of taper and maintain constant area. The construction of Fig. 1 is an approximation to the adiabatic taper between the rectangular cross section and the circular. It is estimated that 25 percent of the light emitted in a scintillation arrives at the photocathode[4] (with aluminum foil attached by a film of grease to the transverse surface at the right end). Figure 2 shows the success of this type of construction. We plot the relative dc current observed at the anode of the photomultiplier as a function of position of a beam of gamma-rays perpendicular to the cell. The gamma-rays are from 150 mc of Co^{60} collimated by 4 in. of lead to a $\frac{3}{16}$-in. diameter beam.

It is evident that the optical efficiency is uniform to 1 percent over the volume of the cell. The fall-off at the edge is the result of the finite size of the gamma-ray beam and the finite range of the Compton recoils. The pulse-height spread from minimum ionization particles passing through the cell is about ±5 percent, allowing one to discriminate among particles by their velocity, i.e., specific ionization. This pulse-height spread is entirely a result of the

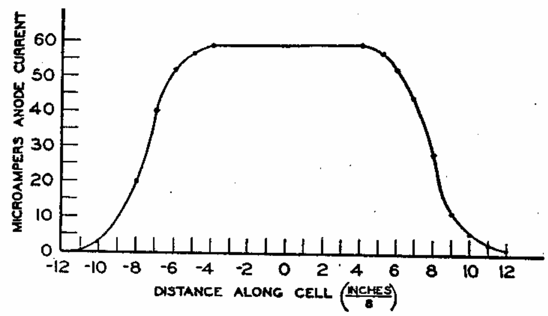

FIG. 2.

statistical fluctuations in the finite number of photoelectrons from the multiplier cathode.

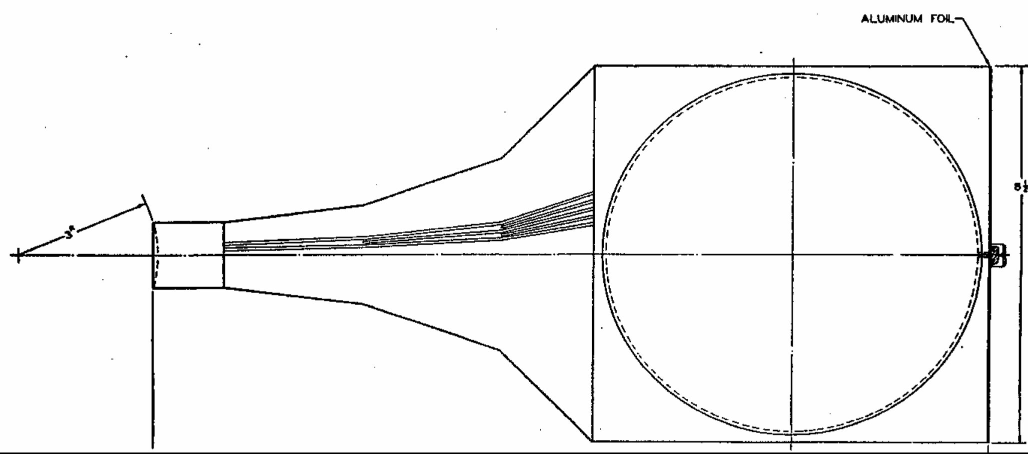

FIG. 3.

Foil 26[9]

Reprinted from THE REVIEW OF SCIENTIFIC INSTRUMENTS, Vol. 31, No. 9, 1010–1011, September, 1960
Printed in U. S. A.

The Collection of Light from Scintillation Counters

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(Received June 14, 1960)

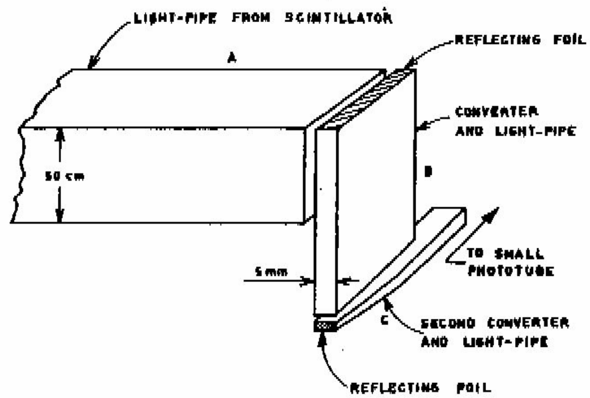


FIG. 1. Light from the light-pipe or scintillating vat (A) strikes the converter (B), in which it produces fluorescent light of slightly lower photon energy. Some of this light, being isotropic, can now be piped along the converter without further loss; and, in principle, can be used a second time to produce fluorescence in the second converter (C) to concentrate further the light onto a small phototube.

Foil 21 shows a typical circuit I used with the photomultiplier tubes, and my contribution to modifying the Rossi circuit. In Foil 21 and more clearly in Foil 23, the Rossi approach would be to cut off with one pulse or two the current in each of the two triodes. However, as explained in the paper (Foil 22), a long single pulse might result in the rise of the common anode ("plate") by 30 volts, while a short coincidence might result in a rise of 20 volts. The diode shown on the left side of Foil 23 from an essentially fixed voltage at (d) will not allow (b) to drop more than about one volt below (d). Nor will (b) rise more than one volt when the current to one plate is cut off. But as soon as current to both plates is cut off, (b) begins a rise toward the power supply voltage, B+.

In a later innovation published in 1952 the diode (c) was added (Foil 23). No pulse is communicated to the output tube until (b) has risen above (d), thus improving discrimination against single inputs.

The six-fold three-channel coincidence-anticoincidence analyzer shown in Foil 24 was that which I used in my experiments at Chicago and which I thought was so meritorious that not only did I publish it, but I had IBM pay the University of Chicago $350 (as I recall) to make another copy which I then brought to IBM in December 1952, even though I was leaving particle physics. I contributed that circuit to the group at Nevis, and it was that unit on which we later did our pi-mu-e parity experiment in 1957.

Foil 25 shows the “adiabatic” lightpipe I introduced in 1952. In “Fig. 1.”, we see on the left a thin cylindrical scintillator through which charged "beam" particles pass and give rise to light. The light is conducted down the lucite lightpipe toward the right, where optical contact is made via a grease film with the glass outside the photocathode of an end-window photomultiplier tube (PMT). The PMT needs to be shielded from the fringe field of the accelerator, and that is one of the reasons for length of the lightpipe so that the PMT can be immersed in a cylindrical magnetic shield. The contribution of this paper was an analysis that showed that efforts to reduce the cross-sectional area of a lightpipe in order to view larger scintillators or use smaller photocathodes were doomed to failure (because of phase-space considerations or (equivalently) conservation of brightness). More positively, an "adiabatic" transformation of the shape of the lightpipe could allow essentially all of the light channeled from the edge of a thin scintillating disk (as in Fig. 1) to be conducted to an equal-area photocathode. In Fig. 1 we see a two-inch diameter scintillator, 1-cm thick. The performance is shown in “Fig. 2.”

Fig. 3 shows a proposed 8-inch diam scintillation cell 0.5 cm thick, in which the "gradual transformation of the shape of the lightpipe is proposed to be done essentially by a large number of small lightpipes in parallel. This is often realized in the modern era by having individual strips of plastic lightpipe, about 2-cm wide by 0.5 cm thick, and twisting and bending them so that they approximate the cylinder at the left.

Having demonstrated in 1952 that one cannot concentrate scintillation light (and hence increase its brightness) it proved to be awkward to have to obey this limit, so in 1960 I showed how the limit could be avoided by changing some of the assumptions. In Foil 26 one sees a massive lightpipe or scintillator tank on the left 50-cm high by 70-cm wide coupled via an air gap to a lightpipe 70-cm wide by 0.5 cm thick. But this is not just a lightpipe; it is an additional scintillator with a converter which absorbs the scintillation light for Vat A and down-shifts it a bit toward the red. Thirty percent of that light can be trapped and conducted indefinitely downward in lightpipe B, and the same trick can be performed again with a different scintillation material in lightpipe C.

In the modern era, one often sees large-scale scintillation detectors in particle physics experiments which are "read out" by what seems to be a thin cable, which is actually the doubly wavelength-shifted light concentrated in an optical pigtail on the way to a photomultiplier tube.

I promised in the title that I would talk about GPS and radar, and I will do so but very briefly.

I spent the first of many summers at Los Alamos in 1950 working on the development and testing of nuclear weapons, and especially the first hydrogen bomb. When I joined IBM it was only a very short time before I was asked to work half-time with the Harvard-MIT crowd in your fair city, Cambridge, on extending the air defense of the United States to the sea lines of approach for Soviet bombers-- the Lamp Light study led by Jerome Wiesner and Jerrold Zacharias of MIT. There I contributed to various broader elements of national security, including the invention (with Wiesner and Dave Sunstein) of a multiple-access single-frequency radio communication system in which each second of speech would be compressed by a factor hundred (and its bandwidth broadened correspondingly), so that 50 users could share the same frequency.

I became familiar at this time with the magnificent 28-volume MIT Radiation Laboratory Series, edited by Louis N. Ridenour-- a record of the radar science and technology developed during World War II. So I had a great appreciation for the individuals involved, and the effectiveness of the organization of scientists and engineers. My

1953-1954 involvement with the Cambridge community led to my becoming a consultant with the President's Science Advisory Committee under President Eisenhower, initially chaired by Jim Killian, my membership on that Committee 1962-65 and 1969-73, and my chairing a good number of national security-oriented panels such as the Military Aircraft Panel, the Naval Warfare Panel, and the like. In 1958-59 I was again with Jerry Wiesner in Geneva for the 10-nation "Conference for the Prevention of Surprise Attack," and there I was promoting GPS-like navigation and monitoring systems. In July 1958 I had proposed a system using time-difference-of-arrival of radio pulses from aircraft, relayed by multiple satellites to a ground station. This ”inverse GPS” system would allow ground-based computers to determine the position of 30,000 aircraft (and 500,000 ships) to a fraction of 1 km or so, and also to relay these positions to the individual vehicles.

By December 1960, I was proposing to E.R. Piore, then IBM Director of Research, that IBM deploy a system for civil aircraft navigation "using difference in arrival times" and, with typical caution, I wrote

> "Should one doubt the feasibility of relay satellites (which would be a great mistake) one could still have a system which would protect and land airplanes automatically by siting about a dozen relay receivers within about 30 miles radius of the major airport ..."

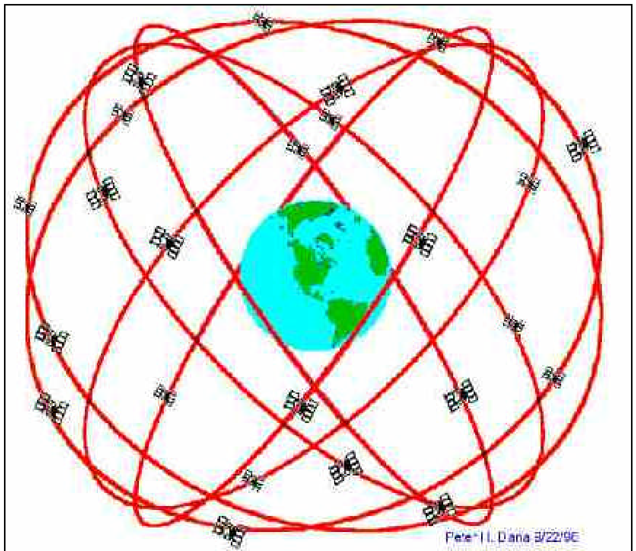
Naturally, when I headed the PSAC aircraft panel and then an Air Traffic Control Panel, it was no accident that our 1971 report on air traffic control came down firmly on a proposal to deploy an all-satellite system for aircraft navigation, communication, and surveillance. This panel included recognized experts, including Charles Zraket, later to head the MITRE Corporation.

An engineering study in support of our activity showed that one could build the airborne tranceiver for $900 to obtain navigation accuracy on the order of 100 ft. This would provide computation on the ground, with relaying of the position to the light aircraft. Airliners would carry a box capable of doing the computation, which would cost about $2100.

Rather than criticize our design, the government chartered a study from the Institute for Defense Analyses which looked not at how we proposed to do the job but at the results-- about 0.1 seconds to obtain position information, and an accuracy of 100 ft. Looking at all <u>other</u> approaches to accuracy and "time to obtain a navigation fix," the IDA study estimated $200,000 per unit user equipment. This is not a prescription for progress!

Of course, the real work on GPS was done by many others, one of whom-- Colonel and now Professor Brad Parkinson—last month received the Charles Stark Draper Award from the National Academy of Engineering. In 1980, I had pressed the head of the uniformed Air Force, General Lew Allen to move ahead with GPS; instead, he reprogrammed the entire $2 M GPS allocation to another purpose. But in 1996 he characterized these and other interventions as "... given with vigor and a unique style which is impossible to ignore ...".

Foil 27



GPS Nominal Constellation
24 Satellites in 6 Orbital Planes
4 Satellites in each Plane
20,200 km Altitudes, 55 Degree Inclination

Now you can buy fingers-size GPS systems which not only give your coordinates to an accuracy of a few meters, but also place your location on a digital map, which is usually what you want to know.

In 1999 the U.S. first employed the Joint Direct Attack Munition--JDAM--a bomb which uses GPS and an auxiliary inertial navigation system to guide it to the specified coordinates of a target. This finally realizes the promise highlighted by the PSAC Military Aircraft Panel of the 1960s to strike targets on the battlefield by their coordinates on a navigation grid. Instead of millions of tons of bombs, 1% as many will do a better job, with far less wanton destruction.

And it shows the degree to which once difficult problems of guiding long-range missiles to their targets are now trivially solved. Our own cities may be those targets, as may US forces abroad.

I hope that I have conveyed not only the joy of experimental physics, but also some enthusiasm for practical applications of technology in science, commerce, and national security.

Foil 28[1]

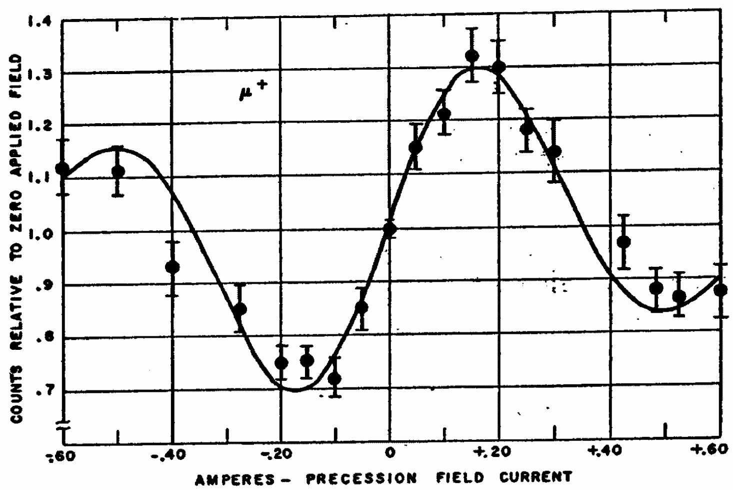


FIG. 2. Variation of gated 3–4 counting rate with magnetizing current. The solid curve is computed from an assumed electron angular distribution $1-\frac{1}{3}\cos\theta$, with counter and gate-width resolution folded in.

Foil 29

Many papers at
www.fas.org/RLG

It is difficult to provide in cold print the flavor of a talk, but I have attempted to do so here by the use of the actual foils presented in the Lee Lecture. Most of the text is that of the lecture, but since there was no recording, some of it has been reconstructed.

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