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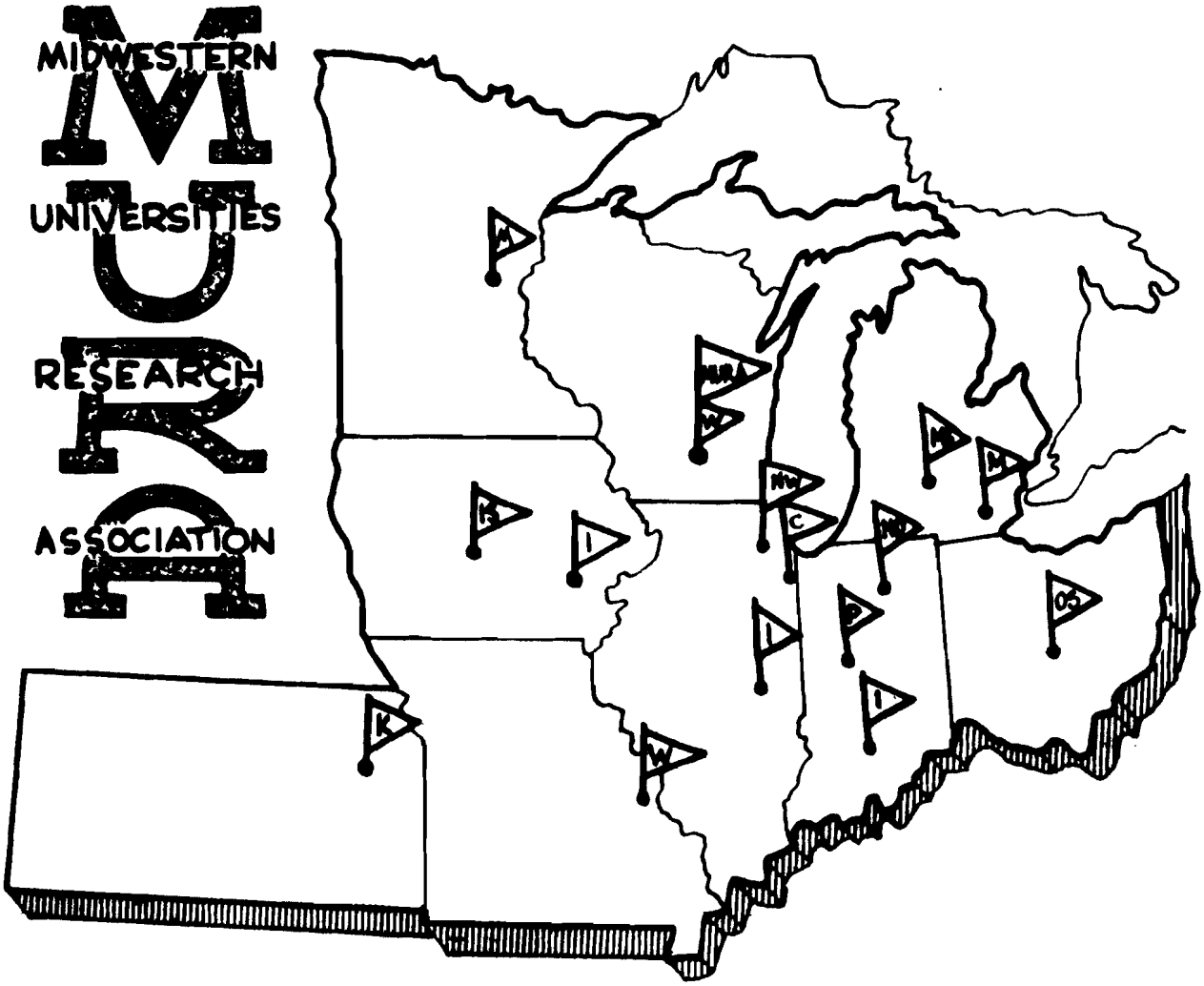

REPORT ON THE HIGH ENERGY PHYSICS CONFERENCE IN GENEVA AND ON A VISIT TO THE CERN LABORATORY

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REPORT ON THE HIGH ENERGY PHYSICS CONFERENCE IN GENEVA AND ON A VISIT TO THE CERN LABORATORY

Keith R. Symon

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ABSTRACT

This brief report on the 1958 Conference on High Energy Physics is devoted primarily to those aspects of the conference of particular interest to MURA. It includes a summary of discussions on colliding electron beams, very high energy cosmic ray events, and a report on the accelerator study group at CERN. Some information about Russian accelerator developments is also included.

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Electron-Nucleon and Electron-Electron Scattering

Of direct interest to MURA was a discussion by W. K. H. Panofsky of the Stanford electron-nucleon scattering experiments in which he referred in particular to the possibility of an experimental test of the breakdown of quantum electrodynamics. If one assumes the validity of quantum electrodynamics for describing electrons and photons, then there are certain difficulties in the interpretation of the electron-nucleon scattering experiments. For a discussion of this point see Reference 1. A possible way of interpreting the experiments is to assume that they represent not the structure of the nucleons but rather a breakdown of quantum electrodynamics. Panofsky pointed out that none of the experiments hitherto performed exclude the possibility of a breakdown of quantum electrodynamics at moderately high momentum transfers. One direct test of such a breakdown would be electron-electron scattering experiments but these would require either a multi-Bev electron beam or a colliding electron beam experiment. Panofsky remarked that MURA was building a two-way electron machine at 50 Mev (sic) in which it would be possible to do colliding electron beam experiments at about 100 Mev in the center of mass. Although no one expects quantum electrodynamics to break down at this energy, the possibility is not excluded by any experiments yet performed and if one blames on the electron all of the anomalous structure found in the Stanford experiments, one might just expect to be able to see something in a one per cent experiment at this energy. O'Neill and Panofsky are proposing to do an electron colliding beam experiment using storage rings with electron energies up to 500 Mev.

Very High Energy Events

Also of direct interest to MURA was an informal evening session concerned with high energy cosmic ray jets. Cocconi began with the discussion of data which he had collected on 50 high energy stars of energies greater than 10^{12} electron volts. These are from high altitude exposures of emulsion stacks by various groups. Since he wants to study a simple p-p or p-n event, he requires that the primary particle be a nucleon, i.e., be only singly ionized, and he eliminates those stars showing heavy prongs. The events which remain presumably then correspond to a primary proton and involve only one target nucleon. Unfortunately only eight of the 50 stars are included if we adopt these criteria. In order to get better (!) statistics, Cocconi also includes stars having one heavy prong. He then has 16 events. For each jet he makes a plot of $F/(1-F)$ vs. $\tan\theta$ on a log-log scale. F is the fraction of particles emitted at angles greater than θ, (in center-of-mass system). For isotropic emission of particles from a common center, one can show that the graph should be a straight line with a slope 2 as shown in Fig. 1. A number of theories, as for example that of Heisenberg, predict a graph like that in Fig. 2, where the curve has a

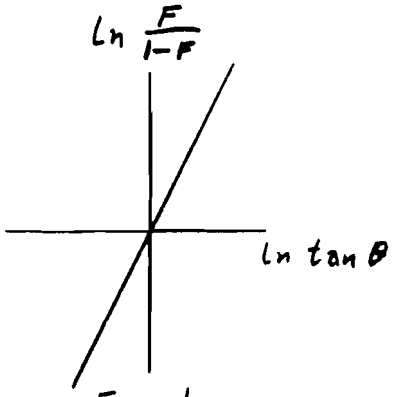

Fig. 1

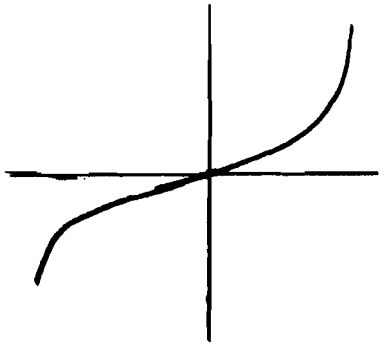

Fig. 2

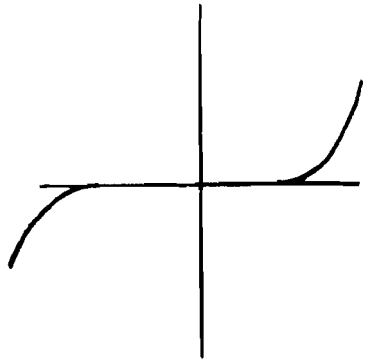

Fig. 3

slope of approximately 1. For two jets coming from two excited centers traveling at different values of γ, we can predict a curve like that shown in Fig. 3. 12 of Cocconi's 16 jets give plots like that in Fig. 3. An interesting feature of all the jets observed and which is also true for lower energy showers is that the transverse momentum of secondary particles is always of the order of magnitude 0.5 - 1.0 Gev/c. This is also true for nucleon-nuclei collisions. A number of hypotheses were discussed to try and account for this fact. If one can assume some kind of mechanism which will result in a common temperature for all of the excited centers which radiate the secondary particles, then this would account for the observed distribution of transverse momenta. It was also pointed out that this value of transverse momenta is just what one gets from the uncertainty relations for particles coming from a region of the order of the nucleon size.

Miesowicz of the Polish Academy discussed their work on high energy cosmic ray events. He writes the equation

$$1.5\, n_s\, E\, \overline{\gamma} = 2\, K\, \gamma_c\, M c^2,$$

where n_s is the number of charged secondary particles, $\overline{\gamma}$ is the γ of each of the two excited centers (assumed equal in mass) in the center-of-mass coordinate system, K is the inelasticity coefficient, γ_c is the γ of the primary particles in the center-of-mass system, and E is the mean energy of the secondaries in the coordinate systems of the excited centers. It is of interest to try and determine the inelasticity coefficient K. Miesowicz makes a plot of the quantity $n_s \overline{\gamma} / \gamma_c$ vs. γ. If E is constant, this quantity should be proportional to K. The experimental data are roughly as shown in Fig. 4.

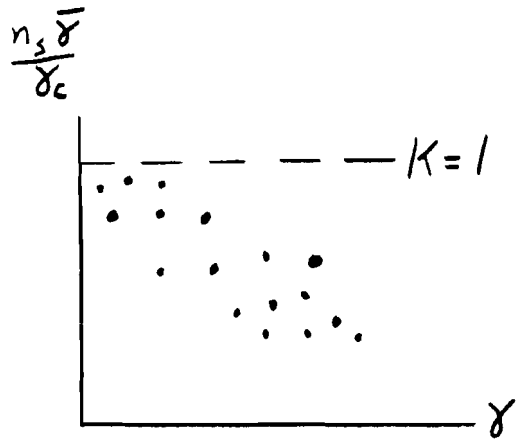

Fig. 4

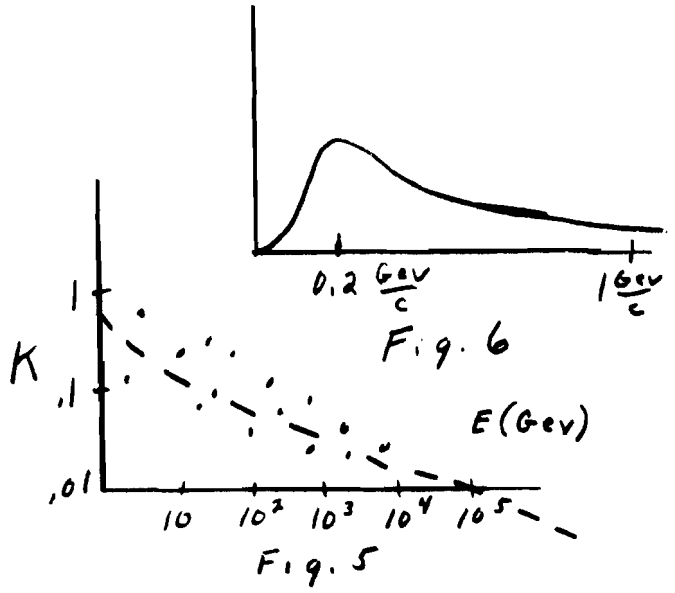

Fig. 6

Fig. 5

Perkins of Bristol made a similar plot of K versus primary energy as shown in Fig. 5. It is clear that K tends to decrease with increasing primary energy. Perkins also presented some plots of the distribution of transverse momenta of which a typical one is given in Fig. 6, which shows the distribution of 43 secondary particles emitted at wide angles which could be easily measured. The mean transverse momentum was 0.4 Gev/c. Less accurate measurements of a 40 small angle secondaries give a similar distribution with a mean transverse momentum of about 0.8 Gev/c. Measurements of 70 electron pairs gave the same general result. Farley of CERN presented a phenomenological calculation based on the assumption that the two excited centers have the same energy and that the secondary particles are all emitted with the same energy. This theory predicts that the elasticity coefficient K is a definite function of the number n_s of secondary particles. The agreement with the experimental data is rather poor. Farley has also made a calculation by statistical methods of the evaporation of the excited centers and finds that with from 1 to 10 Gev excitation energy, the mean energy of the emitted secondaries is 0.55 Gev. In this

calculation he assumes only that the excited center has a size of the order of 1.4×10^{-13} cm. He also claims that the predicted angular distribution fit the data.

Vernov of Moscow described data on showers which they had observed at energies of the order of 10^{15} ev. They found K less than 0.5 and a cross section greater than half the geometrical cross section.

General Impression of the High Energy Conference

My general impression of the results reported at the conference was that although there has been a good deal of experimental and theoretical work during the past year on the outstanding problems in particle physics, there were no outstanding new results to be reported.

There are a number of plausible general conjectures which have been made with regard to possible symmetry laws in particle physics. Examples are the hypothesis of the conservation of leptons, the hypothesis of a universal Fermi interaction according to which all weak interactions would have the same universal form, and the hypothesis of global symmetry according to which all strong interactions would have the same general form. A good deal of additional data has been collected during the past year bearing on each of these conjectures and most of it is consistent with them. While none of these can yet be regarded as well established, they may furnish convenient working hypotheses which are at least not disproved as yet by the experimental data.

A great deal of experimental and theoretical work has been concerned with the dispersion relations. In spite of much theoretical effort the dispersion

relations have been rigorously proved only for a few cases. Most of the experimental data is consistent with the dispersion relations and there is as yet no clear experimental contradiction of these relations.

No new particles appear to have been discovered during the past year. A number of groups have attempted to find the mass 500 particle reported by Alikhanian. None of the other groups succeeded in finding the particle and it appears that if it exists, it is present in numbers definitely less than about one per one thousand μ mesons in cosmic rays. Alikhanian claimed to have found about one mass 500 particle for every 200 μ mesons.

Visit to CERN

The laboratory and experimental buildings, foundation and tunnel for the CERN AGS are all complete. The CERN AGS staff now occupy the laboratory building, which unfortunately appears already to be too small. The magnets have been delivered and the coils are being assembled on the magnets and magnetic measurements are being made. It is hoped that the machine will be in operation in 1961. Some of the AGS group feel that it will be necessary to provide a one-mile straight away for anti-proton beams in order to allow time of flight separation of π mesons and anti-protons.

The pendulum analogue constructed by Barbier for the study of non-linear AG betatron oscillations and reported at the 1956 CERN conference is still in operation. One of the problems being studied is the possibility of using measured data on beam position to calculate magnet misalignments. They will have data on horizontal and vertical beam position at some 30 to 40 points around the accelerator.

Unfortunately they are somewhat discouraged about the possibility of obtaining from this data sufficiently accurate information to determine magnet misalignments. Another problem which they have found it possible to simulate on the pendulum analogue is the solution of the phase oscillation equations. In particular they are studying the problem of beam stacking in the 600 Mev synchro-cyclotron. A few experiments in beam stacking on the synchro-cyclotron were carried out one Saturday during the CERN conference. Keller told me that he believed they had succeeded in stacking three or four pulses and carrying half of these out to the target on the last rf sweep. Since they simply sweep the rf through the stacked beam on the last sweep, it is not at all clear to me how they manage to carry the stacked beam out to the target.

Barbier has under construction an electron analogue for studying betatron oscillations. This consists essentially in a glass ball about two inches in diameter into which a group of essentially stationary electrons is injected. Electrodes are painted on the outside of the glass ball and by applying suitable alternating voltages to these electrodes, alternating focusing forces can be applied to the electrons inside. The oscillations of electrons about the center of the ball can then be made to simulate the betatron oscillations in an accelerator. The purpose is to be able to study many oscillating trajectories at once and thus obtain the statistical properties of the orbit more quickly than would be possible with the pendulum analogue which allows the study of only one orbit at a time. W. Paul of Bonn has constructed an rf mass spectrograph which makes use of the same principle of subjecting a group of ions to alternating focusing forces from an rf electric field.

The CERN study group on new accelerator ideas is under the direction of A. Schoch. It consists of about a dozen people including Linhart, Schneider, and Schoch from the regular CERN staff, and a number of visitors from other places including Emigh from Los Alamos, Reynolds from Harwell, Pentz from England, Vogt-Nilsen from Norway, Nielsen from MURA, Schrader from Holland, and Maisonnier from Saclay. A group including Pentz, Vogt-Nilsen and Nielsen are studying the possibility of a two-way electron synchroclash around 100 Mev very similar to the MURA two-way model except at twice the energy. Another group have been constructing a plasma betatron. This is an air-core betatron with a maximum energy of 2 Mev, a rise time of 7 microseconds developing a betatron voltage of 20 volts/cm. There is in addition a toroidal field of the order of 2000 gauss. A sketch of the cross section of the machine is shown in Fig. 7.

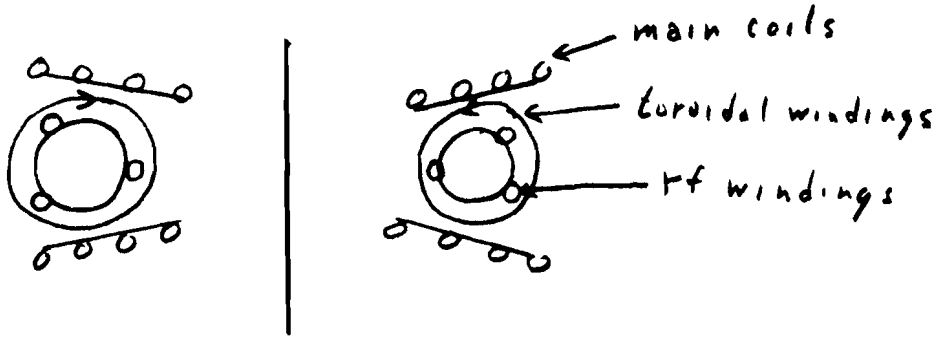

Fig. 7

Three azimuthal rf windings are provided which develop an rf ionizing betatron voltage and also provide additional focusing forces. The idea is to

develop a plasma in the donut and then to accelerate electrons out of this plasma from rest to 2 Mev by means of the betatron voltage. So far currents of electrons up to 0.1 amperes have been accelerated to 2 Mev. This is very much less than the expected currents which would be developed if all of the plasma electrons were accelerated. Among other plasma experiments which they are working on is one being built up by Schrader based on an idea of Linhart. A longitudinal magnetic field is developed along a vacuum tube about a meter long. A plasma column is injected at one end and a uniform column of plasma along the tube is produced. Into this column they hope to be able to inject electromagnetic waves using the plasma as a wave guide. By increasing the longitudinal magnetic field, it will then be possible to pinch down the plasma and thus squeeze the trapped electromagnetic field with the hope of developing high energy densities and intense electromagnetic fields.

Accelerator Work in Russia

The 10 Gev proton-synchrotron at Dubna is still not operating satisfactorily. According to the Russians I talked to at the conference (Dzhelepov, Nikitin and Blokhintsev), the Dubna synchrotron is now producing from 10^7 to 10^9 protons per pulse. A number of different explanations for the poor performance were circulated. The one thing that the Russians all seem to agree on was that the difficulties with the injector have now been corrected. One story which I received thirdhand was that the difficulty is due to the fact that the large amount of iron required had to be supplied from a number of different suppliers and that consequently there was considerable variation between magnets. This story however was denied

secondhand by Nikitin who is reported to have said that the iron all came from one supplier. Dzhelepov said that there seemed to be some source of noise or other perturbation which resulted in losing a large fraction of the injected beam after the first few turns. Nikitin told me that the difficulty was entirely due to a rather poor vacuum and that leaks in the vacuum system were gradually being found and closed up.

I tried hard to find out something about the electron colliding beam experiments of which we have heard rumors and seen news items. None of the Russians at the conference seemed to know very much about this and in fact they all gave me the impression that I knew more about it than they did. Blokhintsev told me he knew of no such work going on. He is the director of the Joint Institute for Nuclear Research. Nikitin and Dzhelepov said that they believed that there had been one or two persons thinking about the possibility but so far as they knew, no project had been started.

Dzhelepov gave a talk at CERN, translated by Nikitin, on the work at the 680 Mev synchro-cyclotron. He stated that the machine now operates 140 hours per week. At 15 meters from the extractor, they have a beam of about 2×10^9 protons per square centimeter per second with a cross section of 4 to 5 cm^2. Most of the work is p-p and p-d scattering including scattering of polarized protons. They also do some radio-chemistry. They expect to modify the synchro-cyclotron to accelerate deuterons to 420 Mev, and perhaps some other particles. With this modification they will be able to do deuteron-deuteron scattering and to look for the hypothetical π^0_0 particle if it exists. They now extract about one seventh of the internal protons.

Following Dzhelepov's talk, Nikitin described an experiment which he has suggested to measure the factor g - 2 for the μ meson. A sketch of the apparatus is shown in Fig. 8. The overall diameter is 9 cm. The magnetic guide field consists of an outer region having n = 0.25 to provide focusing for the meson orbit and an inner region of uniform magnetic field (3000 gauss). Longitudinally polarized μ mesons from the synchro-cyclotron at 90 Mev and with an angular divergence of $\pm$ 3.5 degrees enter the device and pass through an absorber where they lose about 5 to 6 Mev and become trapped in an orbit around the outside. The absorber has a radial extent of 1 cm and a vertical extent of $\pm$ 3 cm. A series of absorbers at the position of the first orbit reduced the energy further so that on the first turn the orbit spirals in to miss the large capturing absorber. Two scintillation counters are placed along one diameter as shown. The orbit spirals in at first rapidly until it leaves the system of absorbers whereupon it spirals more slowly losing only 0.15 Mev per turn in the scintillation counters. After 100 to 200 turns the orbit spirals into a final absorber which causes it to make one last half turn and enter a detector at the center of the device in the uniform 3000 gauss field. At this point the polarization of the μ meson is measured "by the method

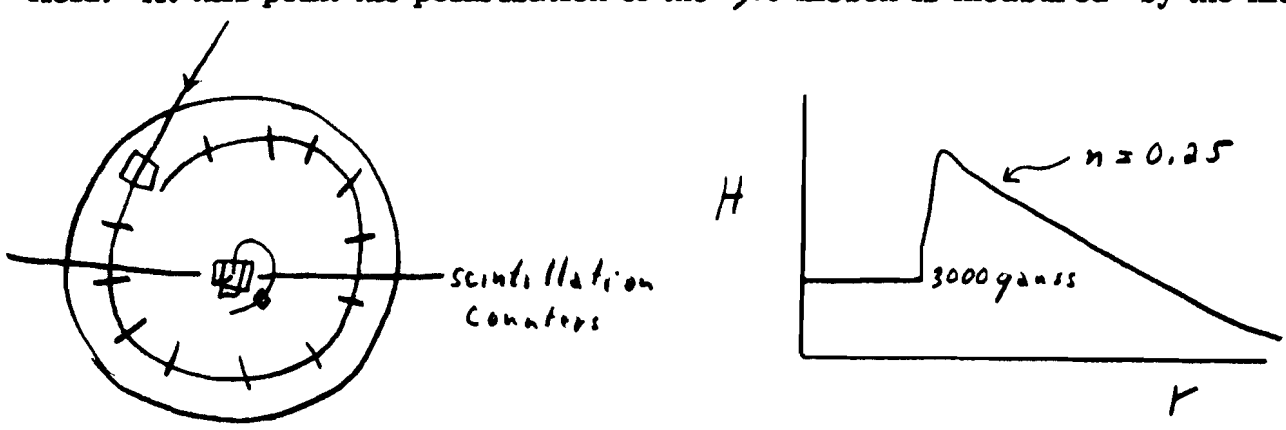

Fig. 8

of Lederman, Garwin and Telegdi." The idea of the measurement is that if $g = 2$ for the μ meson, then the spin precession frequency is exactly equal to the orbital frequency of revolution in a magnetic field and mesons initially polarized longitudinally will continue to be polarized longitudinally as they circulate in the magnetic field. If the final polarization of the μ meson can be measured, its deviation from the longitudinal direction is a measure of the quantity $g - 2$. By means of the scintillation counter it will be possible to count the number of revolutions which the meson makes before being finally absorbed. Only one meson at a time will be present in the apparatus. With the intensities available from the synchro-cyclotron, they will get about 1000 μ mesons per minute into the initial 6 cm^2 absorber. As each μ meson finally decays at the center of the apparatus, the angle of emission of its electron is measured and from this the polarization can be determined. Depolarization due to scattering in the absorbers is apparently not a serious factor. One difficulty about the experiment which I was unable to resolve is how it is possible to correct for the precession which takes place while the meson is almost at rest at the center of the apparatus before it decays. It would seem from the figures given that the meson would precess an average of perhaps a thousand turns while at the center of the apparatus so that one might expect to lose nearly all of the precision gained by counting orbital revolutions during the first few hundred turns.

Reference 1. D. R. Yennie, M. M. Levy, D. G. Ravenhall, Rev. Modern Physics 28, 144 (1957).