How the first neutral-current experiments ended

Peter Galison

Lyman Laboratory of Physics, Harvard University, Cambridge, Massachusetts 02138

At the beginning of the 1970s there seemed little reason to believe that strangeness-conserving neutral currents existed: theoreticians had no pressing need for them and several experiments suggested that they were suppressed if they were present at all. Indeed the two remarkable neutrino experiments that eventually led to their discovery were designed and built for very different purposes, including the search for the vector boson and the investigation of the parton model. In retrospect we know that certain gauge theories (notably the Weinberg-Salam model) predicted that neutral currents exist. But until 't Hooft and Veltman proved that such theories were renormalizable, little effort was made to test the new theories. After the proof the two experimental groups began to reorient their goals to settle an increasingly central issue of physics. Do neutral currents exist? We ask here: What kind of evidence and arguments persuaded the participants that they had before them a real effect and not an artifact of the apparatus? What eventually convinced them that their experiment was over? An answer to these questions requires an examination of the organization of the experiments, the nature of the apparatus, and the previous work of the experimentalists. Finally, some general observations are made about the recent evolution of experimental physics.

CONTENTS

Over the course of a year and a half—from the fall of 1972 to the spring of 1974—photographs such as Fig. 1 and Fig. 2 that at first appeared to be mere curiosities came to be seen as powerful evidence for the existence of weak neutral currents. Slowly, the experimentalists embedded these photographs in a persuasive demonstration based on a variety of technical, theoretical, and experimental advances. In so doing they presented the physics community with one of the most significant discoveries of recent physics. The subsequent developments in gauge theories and tests of the standard model are well known. But how did the experimentalists themselves come to believe in this result? What persuaded them that they were looking at a real effect and not at an artifact of the machine or the environment?

To understand how the evidence became convincing to the experimentalists we shall need to situate the experiment in the context in which it was planned and built. We need to know something of the experimental and theoretical assumptions held by the physicists involved. Finally, we must trace not only the positive results obtained, but also the myriad of false leads and technical difficulties that arose in the course of the work. In this sense the study will be historical, unlike the excellent and comprehensive review articles that have appeared such as Baltay (1979), Cline and Fry (1977), Cundy (1974), Faissner (1979), Kim *et al.* (1981), Mann (1977), Myatt (1974), and Rousset (1974).

I. INTRODUCTION

The blessing and curse of Fermi's (1934) theory of beta decay was that it skirted the fundamental dynamics of the weak interaction. Certainly the provisional advance afforded by such a move had many historical precedents. A hundred years earlier Ampère unravelled many of the laws of electrodynamics by studying the interactions of electrical currents. Even in the absence of Maxwellian theory much could be learned. Facing the largely unexplored weak interaction, Fermi drew explicitly on the ideas of quantum-electrodynamic currents for his theory. Just as an electron can produce a photon, Fermi reasoned, so could a nucleon emit the light electron and neutrino. The salient difference between electrodynamic and weak currents was this: While an electron retained its charge during the emissive process the nucleon did not—it changed from a neutron to a proton. [See Figs. 3(a) and 3(b).]

Subsequently currents without a change of heavy particle charge were dubbed "neutral" and those with such a change, "charged." For over 30 years after Fermi's paper
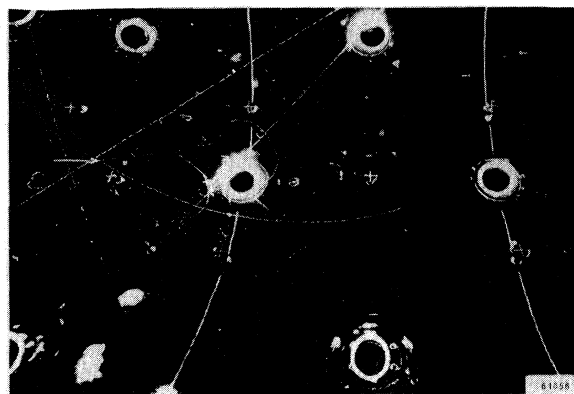

FIG. 1. Neutral-current event. Bubble-chamber photographs from Gargamelle resembling and including this one were at first mistakenly classified as neutron stars. (These are events in which a neutron—putatively at the arrow's end—collided with a nucleus to create a right-moving shower of particles.) Later many of these events were understood to be neutral-current events in which an unseen right-moving neutrino scattered elastically from a quark, creating a right-moving hadronic shower.
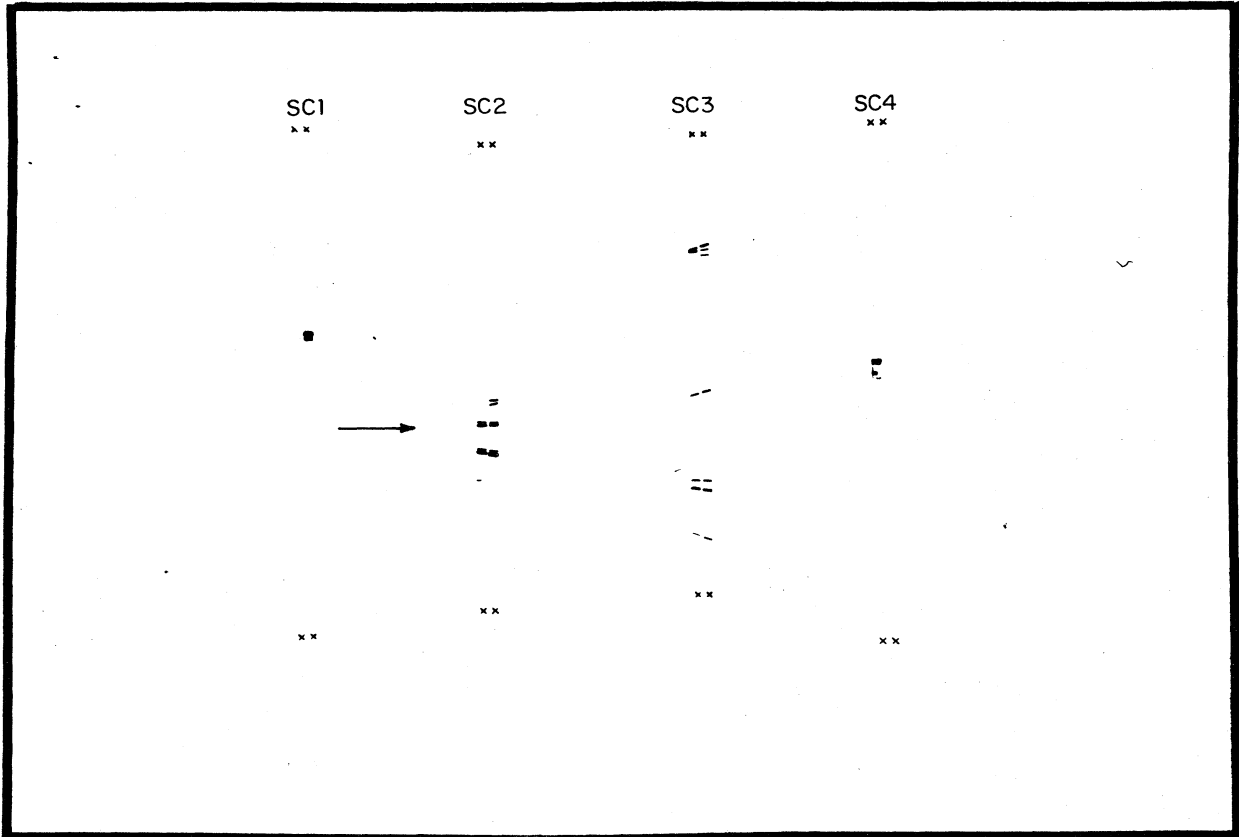
477

FIG. 2. Neutral-current event. Spark-chamber photographs from E1A like this one depicted a right-moving neutrino that collided at the arrow's end with a hadron. At first it was suspected that the neutrino changed charge to become a muon that escaped at a large angle. The event therefore would *appear* to have produced only hadrons. Later many events like this one were understood to be neutral-current events in which the neutrino scattered elastically from a hadron creating a right-moving hadronic shower.

was published, it was almost axiomatic to assume that weak currents were charged. Virtually every text on weak interactions would begin with this assumption and a discussion of the hypothesis that all weak interactions could be described as a product of two currents, $J^{\mu a}J^b_\mu$ in which the charge changed in both of them.

"It is a remarkable fact," one author wrote in 1964, "without known exception that the two leptons in a weak current always consist of a charged and a neutral particle...this could only imply that neutral (weak) currents must be absent" (Feinberg, 1964, p. 282). During the years between 1932 and 1964 a wealth of experimental

data indicated that beta decay was just one of many processes that could be explained using a modified version of Fermi's theory. Just as Fermi had counseled, physicists modified his proposed coupling, eventually arriving at an adequate phenomenological theory. Especially striking were the extremely low experimental limits on neutral-current decay processes such as the one diagrammed in Fig. 4. The message seemed clear: no neutral currents. Further evidence came from experiments that appeared to show that the rate of neutral-current processes, like that shown in Fig. 5(a), was but a small fraction of the rate of related charged-current processes like that shown in Fig.
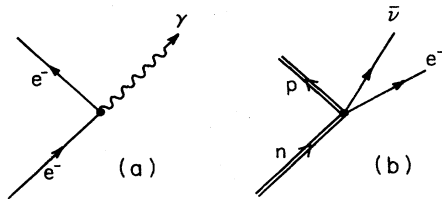

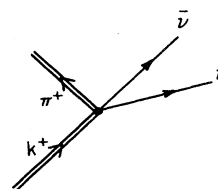FIG. 3. Electrodynamic current (neutral). Weak current (charged).

FIG. 4. $k^+ \rightarrow \pi^+ \nu\bar{\nu}$. Low experimental limits had been placed on neutral-current decays of this type.
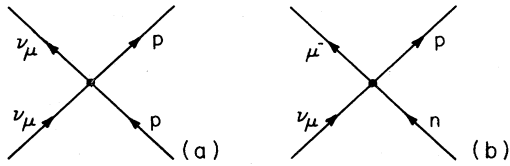
FIG. 5. (a) Neutral-current neutrino scattering. (b) Charged-current neutrino scattering.

FIG. 6. Analogy of intermediate vector boson with intermediate photon.

5(b).

G. Bernardini (1966) mentioned similar results in his introductory speech to the 1964 Enrico Fermi Summer School, arguing that, "neutral leptonic currents if they do exist are coupled with hadronic currents more weakly by several orders of magnitude than the charged ones." In a widely used textbook, R. E. Marshak, Riazuddin, and C. P. Ryan (1969, p. 319) included a section entitled, "Absence of Neutral Lepton Currents," in which they concluded that results similar to those just mentioned "support the absence of neutral lepton (or at least neutrino) current(s)...." As late as 1973, E. Commins contended, "purely leptonic weak interactions are forbidden by the selection rule *no neutral currents*" (Commins, 1973, p. 235).

So things stood at the end of the 1960s. Occasionally a new lower limit on a neutral-current decay process would be published, pounding one more nail into the neutral current's coffin. From time to time an experimental proposal would be made to search for neutral currents in scattering processes, with the goal of testing higher-order corrections to the current-current theory, but neutral currents appeared to be ruled out in first order experimentally, and the theorists had no pressing need for them.

By the early 1970s however, the virtue of Fermi's "point interaction" came to look increasingly like a cardinal sin. Though it allowed the construction of an enormously successful heuristic theory, it had several major failings. Again the electrodynamic analogy was invoked. Above all, the Fermi theory, unlike electrodynamics, was manifestly not renormalizable. Thus, unlike electrodynamics, in order to calculate higher and higher-order corrections to scattering processes one is forced to introduce ever more parameters. In this sense the theory was not predictive. [The authors of at least one textbook went so far as to label their final section on weak interactions, "There is No Theory of Weak Interactions" (Frauenfelder and Henley, 1974, p. 313).]

The natural solution to the problem was to invoke an intermediate vector boson by analogy to electrodynamics. (See Fig. 6.) While the Fermi-theory cross section σ grew like G^2mE with energy (disaster at high energy), the introduction of an intermediate massive particle spread out the interaction, allowing $\sigma \rightarrow G^2M_W^2$. Since this quantity was finite, the theory at least had a chance at being renormalizable. Ideas about massive particles mediating the nuclear forces had been bandied about the physics community since the early work of Yukawa and Fermi. Since virtually everyone worked under the assumption that
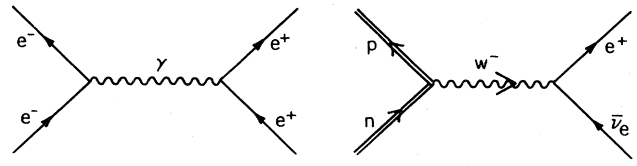
weak currents were charged, it was usually tacitly assumed that the W's were charged as well. Unsuccessful experimental searches for the W continued throughout the 1960s; with each successive effort the lower limit on its mass was raised.

Among the planned searches for the W were the two high-energy neutrino experiments that eventually led to the discovery of neutral currents. This is not to imply that neutral currents were an important original motivation for the experiment—they were not. Of course with hindsight the now "standard" spontaneously broken gauge theory of S. Weinberg (1967) and A. Salam (1968) could have provided the original motivation for the neutrino experiments; in fact their influence was exerted only several years later.

Gauge theories are based on this idea: One starts with a simple Lagrangian of matter but demands that the complete Lagrangian be invariant under some continuous symmetry transformation. To enforce this demand, extra fields ("gauge fields") need to be added. These gauge fields are interpreted as the fields of the intermediate force-carrying particles. For example, in quantum electrodynamics, the matter field could be the electron, while the symmetry demanded is that the complete Lagrangian be invariant under a local charge of phase: $\psi \rightarrow e^{i\theta(x)}\psi$. Wonderfully, the gauge field that when added makes the Lagrangian invariant is just that of the photon.

In the theory of Weinberg and Salam, the more general symmetry of $SU(2)_L \otimes U(1)$ is postulated, and the gauge fields required are a singlet B and triplet W^1, W^2, W^3. Unfortunately, the gauge symmetry forbids giving an explicit mass to these W particles, and the main point of introducing them was to make them massive. To circumvent the difficulty, a scalar potential is added to the Lagrangian. The potential is chosen to have a form that, while originally symmetric, spontaneously falls into its lowest energy field configuration, thus breaking the gauge symmetry. In doing so it gives rise to mass terms for three of the gauge fields. These now massive fields are called W^+, W^-, and Z^0; the fourth particle, the photon, remains massless (Abers and Lee, 1973).

The Z^0 is neutral. It follows immediately from the Weinberg-Salam theory that previously ignored or forbidden processes, such as the one shown in Fig. 7, should occur at a rate commensurate with charged-current interactions. Until 't Hooft (1971a) proved the theory renormalizable, it languished among many competing models. Once 't Hooft's proof appeared, some theorists began to take the model seriously.
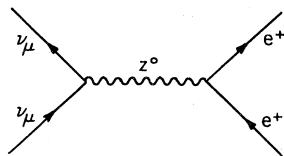
FIG. 7. Neutral-current event as Z^0 exchange between neutrino and electron.

By the time the experiments to be discussed here were completed, they had helped bring the Weinberg-Salam theory to the center of the physics community's attention. More generally, the experimental results precipitated a shift of particle physics away from an assemblage of heuristic and phenomenological techniques to a field-theoretical description not only of the electroweak interaction but of hadrons as well. As one recent reviewer of the subject commented, we now have "a real theory of weak interactions, approaching Maxwell's theory of electromagnetism" (Taylor, 1976). Building on this success, theorists constructed a gauge theory of the strong interactions, setting the whole in the still untested framework of grand unified theories. The discovery of neutral currents played a crucial role in catalyzing the gauge program both in theoretical and experimental quarters.

In this historical study of the discovery of neutral currents I shall focus on two sets of experiments, one on each side of the Atlantic. Though these particular experiments left many issues about the properties of neutral currents to be resolved by later, more precise experiments (e.g., space-time and isotopic spin of neutral currents), they were the first to persuade many physicists, both theorists and experimentalists, that neutral currents existed at a high level. The two experiments were Experiment 1A (E1A) conducted at the Fermi National Accelerator Laboratory (Fermilab or FNAL) in Batavia, Illinois, and the "Gargamelle" collaborative study conducted at CERN, the European Organization for Nuclear Research. At Fermilab the collaboration that undertook E1A was composed of groups from Harvard University, The University of Wisconsin at Madison, the University of Pennsylvania at Philadelphia, and Fermilab, often abbreviated as the HWPF collaboration.

In Europe the Gargamelle team was composed of groups from the University of Oxford, the Laboratoire de l'Accélérateur Linéaire at Orsay, the Physikalisches Institut der Technischen Hochschule, Aachen, the Università degla studi di Milano, the University of London, the École Polytechnique in Paris, the Interuniversity Institute for High Energies, U.I.B., V.U.B. Brussels, and CERN. Over seventy physicists eventually signed the various early reports on neutral currents.[1] Many other administrators, experimentalists, theorists, students, and technicians participated in innumerable stages of planning and analysis.

---

[1]Below are listed the authors of the first papers from the Gargamelle and HWPF groups that were directly concerned with neutral currents. The authors are indicated with their institutional affiliations during the experiment.

F. J. Hasert, H. Faissner, W. Krenz, J. Von Krogh, D. Lanske, J. Morfin, K. Schultze, and H. Weertz—III Physikalisches Institut der technischen Hochschule, Aachen, Germany.

G. H. Bertrand-Coremans, J. Lemonne, J. Sacton, W. Van Doninck, and P. Vilain—Interuniversity Institute for High Energies, U.I.B., V.U.B., Brussels, Belgium.

C. Baltay, D. C. Cundy, D. Haidt, M. Jaffre, P. Musset, A. Pullia, S. Natali, J. B. M. Pattison, D. H. Perkins, A. Rousset, W. Venus, and H. W. Wachsmuth, CERN, Geneva, Switzerland.

V. Brisson, B. Degrange, M. Haguenauer, L. Kluberg, U. Nguyen-Khac, and P. Petiau—Laboratoire de Physique des Hautes Energies, École Polytechnique, Paris, France.

E. Bellotti, S. Bonetti, D. Cavalli, C. Conta, E. Fiorini, and M. Rollier—Instituto di Fisica dell'Università, Milano, and I.N.F.N., Milano, Italy.

B. Aubert, L. M. Chounet, P. Heusse, A. Lagarrigue, A. M. Lutz, and J. P. Vialle—Laboratoire de l'Accélérateur Linéaire, Orsay, France.

F. W. Bullock, M. J. Esten, T. Jones, J. McKenzie, A. G. Michette, G. Myatt, J. Pinfold, and W. G. Scott—University College, University of London, London, England.

"Search for Elastic Muon Neutrino-Electron Scattering," Physics Letters **46**, 121–124. Received 2 July 1973, appeared 3 September 1973. Hereinafter Hasert, 1973a.

F. J. Hasert, S. Kabe, W. Krenz, J. Von Krogh, D. Lanske, J. Morfin, K. Schultze, and H. Weertz—Physikalisches Institut der Technische Hochschule, Aachen, Germany.

G. H. Bertrand-Coremans, J. Sacton, W. Van Doninck, and P. Vilain—U.L.B., V.U.B., Brussels, Belgium.

U. Camerini, D. C. Cundy, R. Baldi, I. Danilchenko, W. F. Fry, D. Haidt, S. Natali, P. Musset, B. Osculati, R. Palmer, J. B. M. Pattison, D. H. Perkins, A. Pullia, A. Rousset, W. Venus, and H. Wachsmuth—CERN, Geneva, Switzerland.

V. Brisson, B. Degrange, M. Haguenauer, L. Kluberg, U. Nguyen-Khac, and P. Petiau— Laboratoire de Physique des Hautes Energies, Ecole Polytechnique, Paris, France.

The experimental and theoretical consequences of these neutral-current experiments provide one rationale for examining them historically; but beyond the framework of the history of gauge theories, the events surrounding the discovery of neutral currents present a particularly interesting opportunity to study the relation of experiment to theory in elementary particle physics. In addition, the history of neutral currents raises a number of methodological issues for the historian: Can the tools of history of physics, as they have been developed for the study of earlier periods, such as the scientific revolution, statistical mechanics, relativity, and quantum mechanics, be applied to events which are or are almost contemporary with us? Must events be fifty or sixty years distant before a historical perspective is possible? Finally, are the tools developed for the study of the history of quantum mechanics (for example) appropriate to a history of events half a century more recent?

This analysis presupposes that a contemporary history of physics is possible, but based on an archival record with a different character from that of even thirty years ago. No one writes the kind of scientific correspondence that formed the backbone of the classic works of the history of physics from Descartes to Einstein and Bohr. Some of the older types of sources remain, such as draft manuscripts, transcripts of conference meetings, and occasionally notebooks. In addition new source material can be exploited: preprints, computer simulations, computer calculations, internal memoranda, minutes of collaboration meetings, log books, experimental proposals, and grant applications. I have used such sources in addition to the more usual ones, along with the interviews conducted in the United States, France, England, and Switzerland.

With these tools, I hope to offer an account of how two high-energy physics experiments—admittedly exceptional ones—took place. In some respects, as we shall see, these experiments structurally resemble much older experiments. In other respects, amid the new technology, organization, and subject matter of high-energy physics, the nature of experimental demonstration has been changed.

II. THE EXPERIMENT "GARGAMELLE": FROM W SEARCH TO NEUTRAL-CURRENT TEST

It is beyond the scope of this study to review the development of weak-interaction theory up to the early 1960s. However, from the experimentalists' point of view, the theoretical interests were woven together concisely into a broad experimental program outlined by M. Schwartz (1960) and by T. D. Lee and C. N. Yang (1960a) in two consecutive (and related) *Physical Review Letters*. A number of their suggestions became the guiding principles for experiments that were conducted over the next decade, including the two-neutrino experiments, tests of

E. Belotti, S. Bonetti, D. Cavalli, C. Conta, E. Fiorini, and M. Rollier—Instituto di Fisica dell'Università, Milano and I.N.F.N., Milano, Italy.

B. Aubert, D. Blum, L. M. Chounet, P. Heusse, A. Lagarrigue, A. M. Lutz, A. Orkin-Lecourtois, and J. P. Vialle—Laboratoire de l'Accélérateur Linéaire, Orsay, France.

F. W. Bullock, M. J. Esten, T. W. Jones, J. McKenzie, A. G. Michette, G. Myatt, and W. G. Scott—University College, London, England.

"Observation of Neutrino-Like Interactions without Muon or Electron in the Gargamelle Neutrino Experiment," *Physics Letters B* **46**, 138—140. Received 25 July 1973, appeared 3 September 1973. Hereinafter Hasert et al., 1973b.

F. J. Hasert and same authors as Hasert et al., 1973b. "Observation of Neutrino-like Interactions without Muon or Electron in the Gargamelle Neutrino Experiment, *Nuclear Physics B* **73**, 1. Received 10 January 1974. Hereinafter Hasert et al., 1974.

A. Benvenuti, D. Cline, R. Imlay, and D. D. Reeder—University of Wisconsin, Madison, Wisconsin.

D. C. Cheng, R. L. Piccioni, J. Pilcher, C. Rubbia, and L. Sulak—Harvard University, Cambridge, Massachusetts.

W. T. Ford, T. Y. Ling, A. K. Mann, and F. Messing—University of Pennsylvania, Philadelphia, Pennsylvania. R. Stefanski—National Accelerator Laboratory, Batavia, Illinois.

"Observation of Muonless Neutrino-Induced Inelastic Interactions," *Physical Review Letters* **32**, 800—803. First version received 3 August 1973. Hereinafter Benvenuti et al., 1974a. Slightly revised version received 14 September 1973, appeared 8 April 1974. Hereinafter Benvenuti et al., 1974b. Details in footnote 6.

B. Aubert—Laboratoire de l'Accélérateur Linéaire, Orsay, France.

A. Benvenuti, D. Cline, R. Imlay, and D. D. Reeder—University of Wisconsin, Madison, Wisconsin.

W. T. Ford, T. Y. Ling, A. K. Mann, and F. Messing—University of Pennsylvania, Philadelphia, Pennsylvania.

R. L. Piccioni, J. Pilcher, C. Rubbia, and L. Sulak—Harvard University, Cambridge, Massachusetts.

R. Stefanski—National Accelerator Laboratory, Batavia, Illinois. "Further Observations of Muonless Neutrino-Induced Inelastic Interactions," *Physical Review Letters* **32**, 1454—1457. Received 19 March 1974, appeared 24 June 1974. Hereinafter Aubert et al., 1974.

the conserved vector current hypothesis, lepton conservation, and electron-muon universality. Most important for our interest in neutral currents was their suggestion that neutrino interactions could be used in the search for the intermediate vector boson, W. At the time there were two reasons to believe the W might exist: First, it would base weak interactions on an exchange force similar to the successful quantum electrodynamics, and second, it offered at least a hope that the theory could then be renormalized (Kabir, 1963).

In their own work on the W published that same year, Lee and Yang (1960b) concluded that the search for charged W's could be made by studying the neutrino reaction

$$\nu + Z \rightarrow W^+ + l^- + Z \;,$$

where Z is a nucleus of charge Z and l^- a negatively charged lepton. Furthermore, since kaons did not undergo a fast decay to $W + \gamma$, Lee and Yang concluded that the mass of the W must be greater than that of the kaon; from the form of the V-A interaction they could say that $W^\pm$ must be spin 1. Beyond that little was known. The search suggested by Yang and Lee assumed that the W has approximately the mass of the heaviest known particles, nucleons. With this assumption, they could make a rough calculation of the cross section of reaction (1), and the rate of production of the W decay products, $\mu + \nu_\mu$. "If experimentally no $W^\pm$ is found," they wrote, "it would be possible to set a lower limit on the values of m_W" (1960a, p. 310).

The search of $W^\pm$ formed one of the main motivations for the construction of Experiment 1A at the National Accelerator Laboratory and of Gargamelle at CERN. By contrast, the search for neutral currents had low priority, since there seemed to be no pressing reason for neutral currents to exist in the phenomenological theory. Thus when in February of 1964 A. Lagarrigue, A. Rousset, and P. Musset (1964) assembled a preliminary project proposal for a new bubble chamber, their interest centered on the search for charged intermediate vector bosons, even though the phenomenological theory put no upper limit on the mass of the W. Consequently, there was no assurance that the neutrino energies in the new proposal would be sufficient to produce the W. Still, as the Sienna conference of 1963 approached, it was hoped that the particle was somewhere in the range of a few GeV, i.e., within the grasp of the next generation of experiments.[2]

Other projects were mentioned by Musset and Rousset, but even as the draft went through several revisions, neutral currents were not mentioned. Some six years later, with Gargamelle nearing completion, D. H. Perkins (1970) rewrote the proposed physics program for Gargamelle. At the top of the list of physics projects remained the search for the intermediate vector boson. Then came the study of various processes predicted from

the older theory of weak interactions which did not use W's. Still, not everything was the same. Since the original project proposals had been submitted, a group at SLAC had conducted an experiment in which they inelastically scattered electrons off nuclei with a large transfer of momentum (Briedenbach et al., 1969). The astonishing result of the SLAC work was that the double differential cross section $(d^2\sigma/dE'd\Omega)$, when divided by the simple point Mott scattering, was independent of momentum transfer and center-of-mass hadron energy values up to 5 GeV.

The constant value of the ratio $(d^2\sigma/dE\,d\Omega)/(d\sigma/d\Omega)_{\text{Mott}}$ suggested to Feynman and Bjorken that the electrons were scattering from point scatterers that, in a not yet clear sense, were included in the protons and neutrons. Feynman (1969) christened these point scatterers *partons*. [Partons were first identified with quarks by Bjorken and Paschos (1969).] Tests of other consequences of the parton model suddenly became the order of the day.

As a result of this new interest, there was an added incentive to study the behavior of the neutrino interactions at high energies. If the SLAC results held good at even higher energies, the neutrino-parton cross section was expected to rise linearly with energy. While the search for the W was now accompanied by excitement over the parton model, neutral currents remained relegated to a secondary place. "In addition," Perkins (1970, p. 1) appended to his list of proposed experiments, "there are, of course, many other topics of interest, for example, neutral currents, ... However, these problems can also be investigated with [other] chambers. On the other hand Gargamelle is, we claim, a unique instrument for investigating problems [like the W and the parton hypothesis]." When the French group responded with a new draft of the proposal, they advocated increased attention to tests of the parton model. Neutral currents did not even merit a separate section. They concluded: "We don't mention several outstanding problems where the existing limitation come[s] not only [from] the statistics, but mainly [from the] background: elastic process, lepton conservation, neutral currents" (Aubert et al., 1970).

In addition to setting experimental priorities there were many technical problems associated with starting a new and complicated detector. During 1971, a great number of machine tests had to be completed in Gargamelle, and the physics program began to take shape. In the course of this work, the analysis of preliminary bubble-chamber photographs revealed some unusual events. As J. P. Vialle (1980) later recollected:

> One thing we saw right away on the photographs was that there were very energetic events in the bubble chamber with no muons. But obviously, we couldn't have said these are neutral currents, no, it wasn't like that. I think our first thought was that it was curious we were observing stars that could come from neutrons but were much too energetic, and that we would have to look into that.

The experimental program continued as planned, with the

---

[2]The Weinberg-Salam theory puts the mass of the W at about 80 GeV, though as of May 1982 it still had not been found.

study of neutral currents of secondary importance (Musset, 1982).

The poor showing of neutral currents in the list of priorities had three causes. First, as Perkins pointed out, there were many other subjects of more immediate concern: searches for the W, heavy leptons, and scaling violations. Second, bubble-chamber evidence seemed to indicate unambiguously that neutral currents either did not exist or were astonishingly well suppressed. Roughly speaking the argument ran as follows: neutral currents either conserved or changed strangeness. Unfortunately, strangeness-conserving processes involving charged leptons were overwhelmed by competing electromagnetic decays. But electromagnetism conserves strangeness, so processes such as $K^+ \to \pi^+ \nu \bar{\nu}$ that changed strangeness could not occur electromagnetically. By 1969 Camerini et al. had shown such processes were well suppressed:

$$\frac{\Gamma(K^+ \to \pi^+ \nu \bar{\nu})}{\Gamma(K^+ \to \text{anything})} \leq 5 \times 10^{-5} .$$

Many other similarly low limits had been placed on strangeness-changing neutral currents. Since there was no reason at the time to believe there was any relevant distinction to be made between $\Delta S = 0$ and $\Delta S \neq 0$ neutral currents, it was widely held that neutral currents simply did not exist.

I must qualify "no reason" with a parenthetical aside. In (1970) S. L. Glashow, J. Iliopoulos, and L. Maiani (GIM) postulated a mechanism invoking a fourth quark. They hoped to explain, for example, the small $K_1 K_2$ mass difference by suppressing strangeness-changing neutral currents. Only several years later was the GIM mechanism seen to fit naturally into the Weinberg-Salam model, allowing $\Delta S = 0$ neutral currents while suppressing those with $\Delta S \neq 0$. But in 1970–71 this was either not known or not accepted in the experimental community. Considering that even Glashow (1980) did not connect the mechanisms with gauge theories until 1972, this is not very surprising.

Along with these two arguments against according neutral-current searches a high priority was a third. There were two very low limits on strangeness-conserving neutral currents that had been imposed by earlier experiments. One relevant result came from W. Lee's (1972) study of the exclusive (i.e., final states specified) $\nu_\mu + p \to \nu_\mu + p + \pi^0$ channel and its interpretation by B. W. Lee (1972a). The latter argued that the new experimental result effectively "rules out the existence of the neutral current predicted by Weinberg's model of weak interactions." By the time Musset was studying these papers, however, Lee and Lee's conclusions had already been cast into doubt on the grounds that they had not adequately treated charge exchange processes. Perkins (1972a), for example, presented criticism along this line a few months later. [B. W. Lee (1972b) soon admitted that strangeness-conserving neutral currents should not be categorically ruled out.]

Another upper limit had been given by the 1963 CERN

bubble-chamber work in which Cundy and Perkins had participated. This group had widely presented a limit on an exclusive hadronic channel that was far below the Weinberg level, indeed below 3%.[3] In a more recent publication (Cundy et al., 1970), many of the same authors withdrew the original result, offering in its place the less stringent bound on the exclusive channel

$$\frac{\Gamma(\nu_\mu + p \to \nu_\mu + \pi^+ + n)}{\Gamma(\nu_\mu + p \to \mu^- + \pi^+ + p)} \leq 0.08 \pm 0.04 .$$

(Although this was not discussed at the time, such a bound does not contradict the Weinberg-Salam theory.) The received view that neutral currents did not exist seems to have contributed to the incorrect 1964 results. "That we did not believe what we saw," Helmut Faissner (1979) has written, "was an unfortunate conspiration of mental blocking, by theoretical prejudice, and experimental mischief." Whatever the cause, at the time the added weight of the 1964 results seemed to rule out conclusively all neutral currents.

Suddenly, in the spring of 1971, theoreticians took a new interest in neutral currents. Ever since 1967, when it was first put forward, the Weinberg-Salam theory had played absolutely no role in the experimentalists' planning. One reason for this was that 't Hooft's proof that the theory was renormalizable came almost four years after Weinberg's original paper. In the absence of such a proof Weinberg's model seemed to be but one among many theories vying for attention. Once the proof was made known and accepted, theorists such as E. A. Paschos and L. Wolfenstein (1973), A. Pais and S. B. Treiman (1972), S. Weinberg (1972), and G. 't Hooft (1971b) began to calculate some of the experimental consequences of the theory. A combination of the renewed theoretical interest and the availability of cross sections they could test awakened the interest of the experimental community (Sullivan et al., 1980).

Not long after the appearance of 't Hooft's paper in November of 1971, B. Zumino, J. Prentki, and M. K. Gaillard spoke to a group of experimentalists and theorists in the small library room in the building that housed Gargamelle at CERN. Zumino explained to them the sudden theoretical interest in the now renormalizable Weinberg-Salam theory. Musset recalls being a little discouraged at the test the theorists were most in favor of: scattering a muon neutrino off an electron. Though extremely "clean" of background effects because no strong interactions were involved, the cross section (or likelihood) of such an event was extremely small. By contrast,

---

[3]D. H. Perkins presenting work of H. H. Bingham et al. (1963). In an appended conclusion, J. S. Bell, J. Løvseth, and M. Veltman (1963) wrote, "Thus the ratio of neutral-current elastic events is less than about 3%. Clearly neutral lepton currents cannot be admitted on a symmetrical basis with the charged." See also M. M. Block et al. (1964) for the same upper bound on neutral currents by these authors. Their result was also presented in Dubna by D. C. Cundy (1964).

the hadronic weak—neutral-current reaction seemed to have a chance of having a much larger cross section by analogy with the charged-current cross section. Unfortunately, the theorists felt any calculation involving hadrons and the strong interaction would become much too complicated.

Musset (1980) recalled later that his suggestion that the group study the hadronic neutral currents was met with no great enthusiasm by some other members of the collaboration. Their lack of enthusiasm was certainly not because of any lack of interest in the question of neutral currents. Cundy, Perkins, H. Wachsmuth, H. Faissner, and G. Myatt, for instance, had vast experience in hadronic neutral-current searches conducted over almost ten years. Indeed, it was precisely by their earlier experience that many of the Gargamelle collaborators knew at first hand the extreme difficulty of extracting any information on neutral currents from the background. Consequently some felt it would be easier and more reliable to include a search for the rare neutrino-electron events in the routine scanning procedure. The problem with the hadronic processes was that the neutrinos from the beam inevitably caused a large but unknown number of neutrons to enter the chamber from the surrounding magnets, floor, and structure. If one of these secondary neutrons then hit a neutron or proton in the bubble chamber, the resulting shower of hadrons could look like a genuine neutrino neutral-current event. In both cases no muon would emerge.

Gargamelle was much bigger than any previous bubble chamber; for this reason, it alone provided the opportunity to determine the rate of neutron background. For it was known at the time that neutrons had an interaction length in the bubble-chamber liquid longer than the dimensions of the older bubble chambers. This meant that there was no way in the old experiments to see the exponential decrease of neutron-induced events as one looked further from the walls; hence it was impossible to figure out precisely how problematic neutron events would be. In Gargamelle, by contrast, not only could one see the exponential decay, one could do better: it was possible to study the entire career of a neutron in the liquid by examining the so-called "associated events." (See Fig. 8.) Upstream they have a normal charge-current event from which a neutron is emitted, creating within the visible volume of the bubble chamber a "fake" neutral-current event. By studying the length and angle of the neutron's path, one could then program the computer simulation to describe such events even where one did not
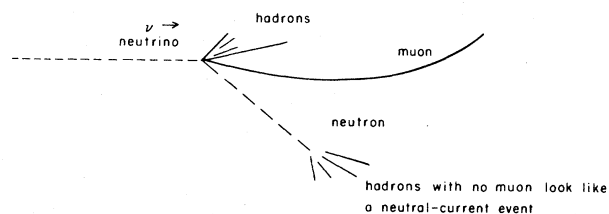

FIG. 8. Schematic diagram of associated event.

see the neutron's beginnings. Unfortunately, especially at the beginning of the experiment, the associated events were quite rare.

In addition to the study of associated events, Lagarrigue, Musset, and Pullia, insofar as possible, attempted to treat the charged and "neutral" events on equal footing. That is, criteria for selection of the hadronic part of the neutron interactions (location, energy, etc.) were chosen to be precisely the same for charged and muonless events. Furthermore, since the primary question at the time was whether neutral currents existed (not yet in what proportion), only completely unambiguous events were used so as not to confuse charged with neutral currents. Finally, to reduce the effects of any remaining biases, and to make the measurement less sensitive to flux calculations, the group chose to focus attention on the ratio of neutral to charged currents. Since both charged- and neutral-current interactions increased linearly with neutrino energy, the ratio was independent of energy.

These innovations proved crucial in demonstrating the existence of neutral-current events, because in muonless events a large fraction of energy is carried off by the (unseen) neutrino. (About the same fraction is carried off in charged-current events by the muon.) Consequently, if one naively compared the total visible energy deposited for both charged and muonless events, one would then be measuring charged-current events' energy by hadron energy plus muon energy, and neutral events by hadron energy alone. Therefore, since the number of events of both types falls off very quickly with energy, at a given total visible energy one would find an extremely and artifically low ratio of neutral to charged events. The failure to treat both kinds of events by hadron energy alone may well have been partially responsible for some earlier experiments' mistakenly low upper bound on the rate of neutral-current events.

III. BACKGROUND AND SIGNAL

By April 1972, Lagarrigue (1972) considered the search for neutral currents to be one of the three primary goals of the neutrino program for, as he wrote to Jentschke, then Director General of CERN, "following Weinberg's theoretical publication, everyone is anxious to discover whether neutral currents really exist." The search for neutral currents, which had begun in January 1972 at Gargamelle, had become by late spring of 1972 one of the important areas of investigation for a number of physicists in the CERN group. Individuals disagreed, however, as to whether they thought the experiment would confirm or refute Weinberg's theory, and as to whether they thought the leptonic or hadronic channels should be pursued preferentially. By the spring, for example, Cundy and Baltay (1972) concentrated almost exclusive on the single-electron search.

During this time Perkins was in Oxford, where he composed a technical memorandum that was sent to the Gargamelle collaboration. Its object was, as Perkins (1972b) stated,

to encourage people in the collaboration to study careful-ly the question of neutral hadronic cur-rents, ...because (i) there probably is an appreciable ef-fect, (ii) conditions for proving the existence of neutral currents are much more favorable [than in the old bubble chambers at CERN].... This is a big effect, large enough that a detailed and systematic analysis in Gar-gamelle, using the position of interactions in the chamber as well as the much better statistics of events, should be able to demonstrate, for the first time, the existence of neutral currents.

Charles Baltay wrote back criticizing the memoran-dum, arguing that excess neutral-current events could be accounted for by low-energy muons alone, a position Per-kins (1972c) took issue with. Responding to Baltay's cri-ticism, Perkins argued that the data for the old bubble-chamber experiment showed very few low-energy muon events. "To summarize," Perkins wrote, "I don't think your explanation works, and I still cannot account for the excess neutral events, although I am certainly not going to claim that they prove the existence of neutral currents.... In providing the final solution (if there is one), one cer-tainly needs to find a satisfactory explanation of the old data." As did several other members of the collaboration, Baltay remained an enthusiast of the leptonic search, which was cleaner. He continued this work later at the 15-foot FNAL bubble chamber.

Musset, Pullia, and others at Milan continued work on the hadronic channel, and in June, Pullia (1972) presented a progress report on the hadronic neutral currents, sug-gesting that the neutron problem could be solved but offering no definite opinion on whether a significant level of neutral currents would remain when the background was removed.

Sufficient interest in the neutral-current question had developed by this time for the group to meet by itself in Paris, apart from the rest of the neutrino collaboration, in order to prepare a report for a conference at Batavia in September (Baltay et al., 1972a). Before the meeting in Paris, the organizers requested that photographs of all neutral-current "candidates" be sent ahead to CERN. By doing so, the authors of the memorandum hoped to stand-ardize the criteria used to separate charged from neutral events. The data cards would be processed in order to plot energy of the events against the total longitudinal and transverse momenta, as well as against position. From these data the group hoped to get a first glimpse of the background problem with some statistical signifi-cance. Changing the strategy employed in Pullia's report, the hadron group now dropped the search for the relative-ly rare pion-producing events. Instead they now chose to determine the much larger total (inclusive) cross section for $v_\mu + N \rightarrow v_\mu +$ (hadrons).

Along with the proposal for a meeting, the first memorandum dealing purely with the subject of neutral current was issued (Baltay et al., 1972b). At the very be-ginning of this report, the group noted that their best chance of isolating the neutrino from neutron interactions would be at high energies (the neutron spectrum was peaked below that of the neutrinos). By this time, then, the hadronic group's effort was entirely concentrated on the background problem, which they described as five-fold:

(1) particles entering the chamber with the beam and which interact in the chamber;
(2) neutrons or kaons coming from outside the chamber generated by neutrinos;
(3) cosmic rays;
(4) μ^-'s sufficiently slow to stop in the liquid;
(5) K_2^0's whose interaction length might be greatly ex-tended by regeneration effects.

Not everyone in the group was equally worried about all of these problems. For instance, Fry was especially con-cerned about the possibility of K_2^0 regeneration, no one was especially worried about cosmic rays, and everyone was interested in the slow muon and neutron problem.

The authors argued that the problem of stopping muons (which, because they stop in the bubble chamber, look like hadrons) could be attacked in several ways. Their number could be estimated from the scaling hy-pothesis, but this was considered a bit tenuous since the center-of-mass hadron energies were much higher than those studied at SLAC. All short unidentifiable tracks could be discarded, or, finally, an upper limit to the num-ber of "hidden μ contamination" could be set as follows. The muon spectrum had been measured in the liquid as had been the decay rate. From these facts, the number of muons below a certain energy in Gargamelle could be cal-culated. Then from the theoretical ratio (of muon capture to muon decays) the number of muons captured below a certain energy could be found.

Lastly, the group set up a standardized system for recording neutral-current candidates. At least one physi-cist would review each event, and bit by bit the data would be assembled in preparation for Batavia. At Bata-via, Perkins (1972a) summarized the group's work from his perspective. It is worth quoting from his assessment of the future prospects of the hadronic and electron neu-trino experiments:

As far as the Weinberg theory is concerned, the most definitive and unambiguous evidence, for or against, must come from the purely leptonic reactions . . . since the hadronic processes involve details of strong interac-tions which might contain unknown suppression effects .... As I have tried to indicate, the reactor ex-periment $[v_e + e \rightarrow v_e + e]$ is beset with severe back-ground problems. Even if in future improved experi-ments, a clean signal is detected, it is necessary, in order to finally demolish the Weinberg theory, to prove that the observed signal rate is consistent with the V-A pre-dictions within close limits. It is difficult to believe that this could be achieved to a precision of better than 20%.[4]

---

[4]The reactor experiment referred to is the one reported on, for example, in H. S. Gurr, F. Reines, and H. W. Sobel, 1972, *Phys-ical Review Letters* 28, 1406. Antineutrinos interacted in a tar-get or plastic scintillator, and recoil electrons were detected. Severe background was encountered due to inverse beta decay.

By contrast, Perkins pointed out, certain purely leptonic interactions could occur only in a theory other than the old phenomenological V-A theory. This was a process Perkins felt the Gargamelle group could profitably investigate; he continued, "In the CERN Gargamelle experiment to date, the expected number of events was between 1 and 9, and none was observed . . . . If none were observed (in the remainder of the experiment), this would be fairly conclusive evidence against the Weinberg theory." It would seem that the division in the Gargamelle collaboration, which many of the members recalled very vividly (Musset, 1980; Vialle, 1980; Cundy, 1980), was based not on whether neutral currents should be searched for, but in which process.

During the fall of 1972 each of the subgroups conducted their respective data analyses, a long, often frustrating task in which hundreds of events had to be compared, definitions modified, and criteria adjusted. By January 1973, Musset and the others had gathered sufficient data to present their findings to the Americal Physical Society meeting in New York.

The emphasis of Musset's talk was almost entirely on the neutron background problem, and the data presented was in the form of number of events (charged, neutral, associated) plotted against longitudinal and radial position in the bubble chamber. His goal was to demonstrate that the events occurred relatively evenly throughout the volume of the machine, as would real neutrino events, as opposed to neutron-induced events. This was not the most reliable check, but there were at the time very few associated events to study. This was because, as the group had pushed up the minimum required energy of the hadrons (in order to exclude neutron-induced events which tended to have lower energy), they eliminated the vast majority of their data. The cutoff especially reduced the number of associated events. They therefore had to rely entirely on the spatial distribution of events. (See Fig. 9.)

By this time the data were beginning to indicate that neutrons were not sufficient to account for all the neutral-current candidates, but the group was not confident enough to phrase their results in any terms but an upper bound on the ratio of neutral to charged events (Musset, 1980). After the talk Paschos called Musset to discuss the new results with him. Only a few weeks earlier, Paschos and Wolfenstein (1973) had published theoretical limits from the Weinberg model for the inclusive channel $v+N \rightarrow v+X$ that Pullia, Musset, Lagarrigue, and the hadronic group were studying. The two theorists now possessed the result Musset had so wanted at that first meeting with Zumino in November 1971: a prediction that the NC/CC ratio would be above eighteen percent. Such a fraction was just on the limit of earlier bounds, and so was potentially compatible with what was known, yet it was sufficiently large to be easily detectable.

Musset's excitement over the new theoretical results was augmented by another development he had heard about just a few days before leaving for the United States: in early January 1973, a single electron had been found during a routine re-scan of some photographs at Aachen. The Aachen electron satisfied all the criteria the electron
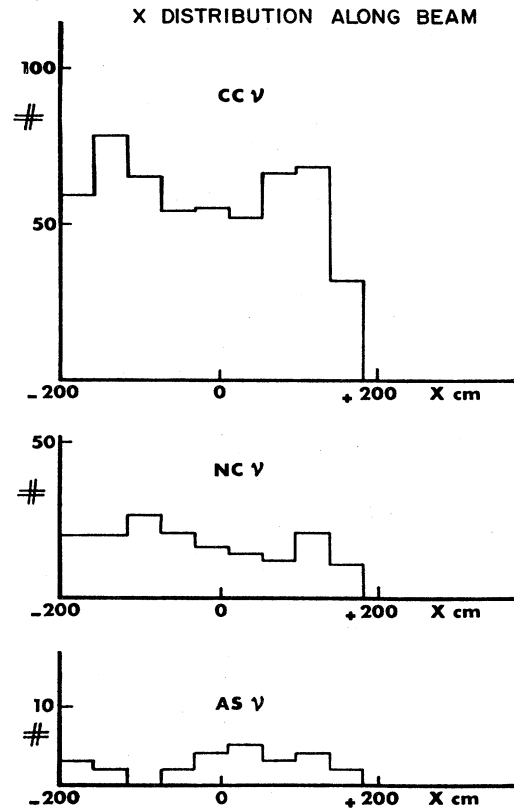

FIG. 9. Preliminary Gargamelle data on spatial distribution of neutrino events as presented on a transparency by Musset to the American Physical Society meeting in January 1973. From Musset, 1972. Object is to show that the events are relatively constant in distribution, as they would be in true neutrino interactions (but as they would not be in the background neutron-induced events). #CC is the number of charged-current events (those with a muon); #NC is number of neutral-current events (those without a muon); and #AS is the number of associated events as defined in the text.

group had imposed on it—it was well within the fiducial volume, making it almost certainly not due to a photon, it was of very high energy, there were no nearby events, and it was oriented in the direction of the neutrino beam. In Aachen the discovery of the electron event had occurred in four stages, as the crucial photograph rose through the hierarchy. At the first level one of the women scanning the bubble-chamber negatives (H. von Hoengen) noticed an unusual event (see Fig. 10). She (mistakenly) classified it as $\mu^- + \gamma$. While checking the scanners' work, one of the research students, Franz Hasert, grew curious about this unusual signature. He went back to the film and recognized the spiralling particles in Fig. 10 as electrons. The next day Hasert brought the picture another step up the ladder to Deputy Group Leader Jürgen von Krogh. Krogh agreed that the event was of considerable interest and brought it to Helmut Faissner, who later wrote (1981),
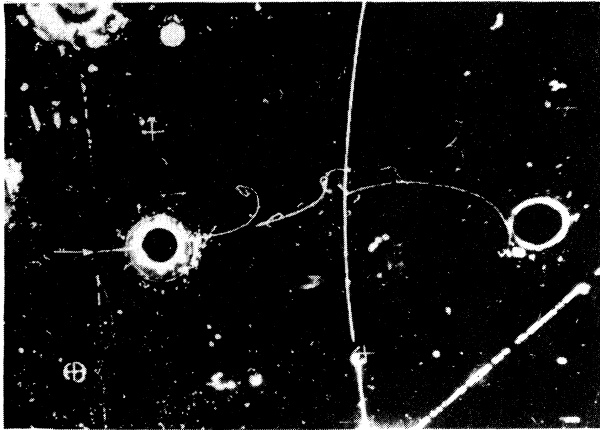
FIG. 10. First single-electron event from Gargamelle. Found at Aachen early January 1973. The electron's trajectory goes from left to right, beginning at the arrow's end, where it ostensibly was hit by a right-moving neutrino. The haloed black circles are lights to illuminate the bubble-chamber liquid.

I got to realize that this event was a "Bilderbuch-example" of what we had been expecting ... (for) months to show up: a candidate for neutrino electron scattering. But the crucial point to assess was (the) background."

The dominant background to $\overset{(-)}{\nu}_\mu e^-$ scattering is inverse β decay:

$$\overset{(-)}{\nu}_e + \begin{Bmatrix} n \\ (p) \end{Bmatrix} \to e^{(\mp)} + \begin{Bmatrix} p \\ (n) \end{Bmatrix}.$$

Since the photograph definitely involved an e^-, the only background was the (small) contribution $\nu_e + n \to e^- + p$, whence Faissner's excitement.

Faissner flew to England with the prints to show them to Perkins, who had conducted several studies of the background to $\nu_\mu e$ scattering. When Perkins had convinced himself that the event looked reasonable, Faissner (1973a) wrote Lagarrigue on 11 January: "The event has excited us a great deal; it is in effect a lovely candidate for an example of the neutral current."

Lagarrigue (1973a) replied shortly afterwards, counseling the Aachen group to continue background studies on the inverse β decay. Faissner (1973b) in turn enthusiastically wrote to W. Jentschke, declaring the discovery "would be a great one not just for Aachen."

An event like the Aachen electron was very striking, above all to the experienced bubble-chamber experimentalists who were used to working with a small number of well-defined events. Many of the bubble chamber's great successes had come with single events such as the famous Ω^-. In a letter Faissner (1973a) reminded Lagarrigue of such a case: "I still vividly remember your declaration of twelve years ago, that a single distinct electron would be sufficient to demonstrate the identity of the muon-neutrino and the electron-neutrino." Nonetheless, the group realized that too much was at stake to publish be-

fore the background was clearly understood. This task took almost seven months.

However, the effect on the collaboration's priorities was immediate. Until January neutral currents had been the primary occupation of only a few of the Gargamelle team, primarily the members of the hadron group. When Musset returned from the United States, this had begun to change, partly as a result of the increasing numbers of hadronic muonless events, but more directly as a consequence of the Aachen electron. With mounting evidence now in hand from both the hadronic and leptonic groups, A. Rousset had the ammunition to request authorization from CERN for two more experiments, each with a million pictures. The tone of his request, dated 19 February 1973 (Rousset, 1973a), reflected the now more confident attitude of the neutrino group.

The search for hadronic neutral currents in Gargamelle shows an appreciable amount of possible hadronic events (i.e., without charged lepton). These events have to be distinguished from neutron background. Severe cuts in the fiducial volume, in energy, in angle are needed and the resulting statistics are then very small. A quick increase of statistics by a factor 2 to 3, possible with 2 to 3 weeks of running time , ... would increase the significance of the results. In addition, one candidate of leptonic neutral current $(\nu_\mu + e \to \nu_\mu + e)$ has been found in the present film and therefore we can hope to detect other events in the new film.

With Lagarrigue and Rousset now both strong advocates of the neutral-current search, this part of the program began to dominate all other neutrino work. By mid-March the CERN and Orsay groups (Musset, 1973a) had finished a new study of the old data tapes that had been prepared in December for the American Physical Society meeting. This time they put an even higher energy cut on the events in order to be even more sure the events were unaffected by the neutron background. An independent analysis of the same data was made by the Orsay group to check the results. After the cut, they found that for neutrinos the ratio of neutral-current (NC) to charged-current (CC) candidates was 130 NC/551 CC $(=0.24)$, and for antineutrinos 83 NC/191 CC $(=0.43)$. (See Fig. 11.) Immediately after presenting the numbers in the memorandum, the authors added the results of the Weinberg model. It is clear that the data were prefectly compatible with the theory, but the crucial question remained: How many additional events needed to be subtracted because of background effects? Not surprisingly, the CERN and Orsay groups ended with a plea to "put priority on this study" of associated events which would help determine the neutron background.

The help Musset needed was with the extraordinary amount of work required to study each neutral-current candidate in detail. Huge enlargements of the appropriate photographs were made so that the group as a whole could judge their validity. The records from these meetings (see Fig. 12) contain long lists of such judgments: "*OUT* possible μ," "*OK* one track badly measurable," "*OUT* cosmic [ray]," "*OUT* entering track," "*OUT* out-

V Distributions spatiales $E_{vis}^{had} \geqslant 1 GeV$

Ratios $\frac{NC}{CC}$

13. R^2

$\bar{V}$ Distributions spatiales $E_{vis}^{had} \geq 1 GeV$
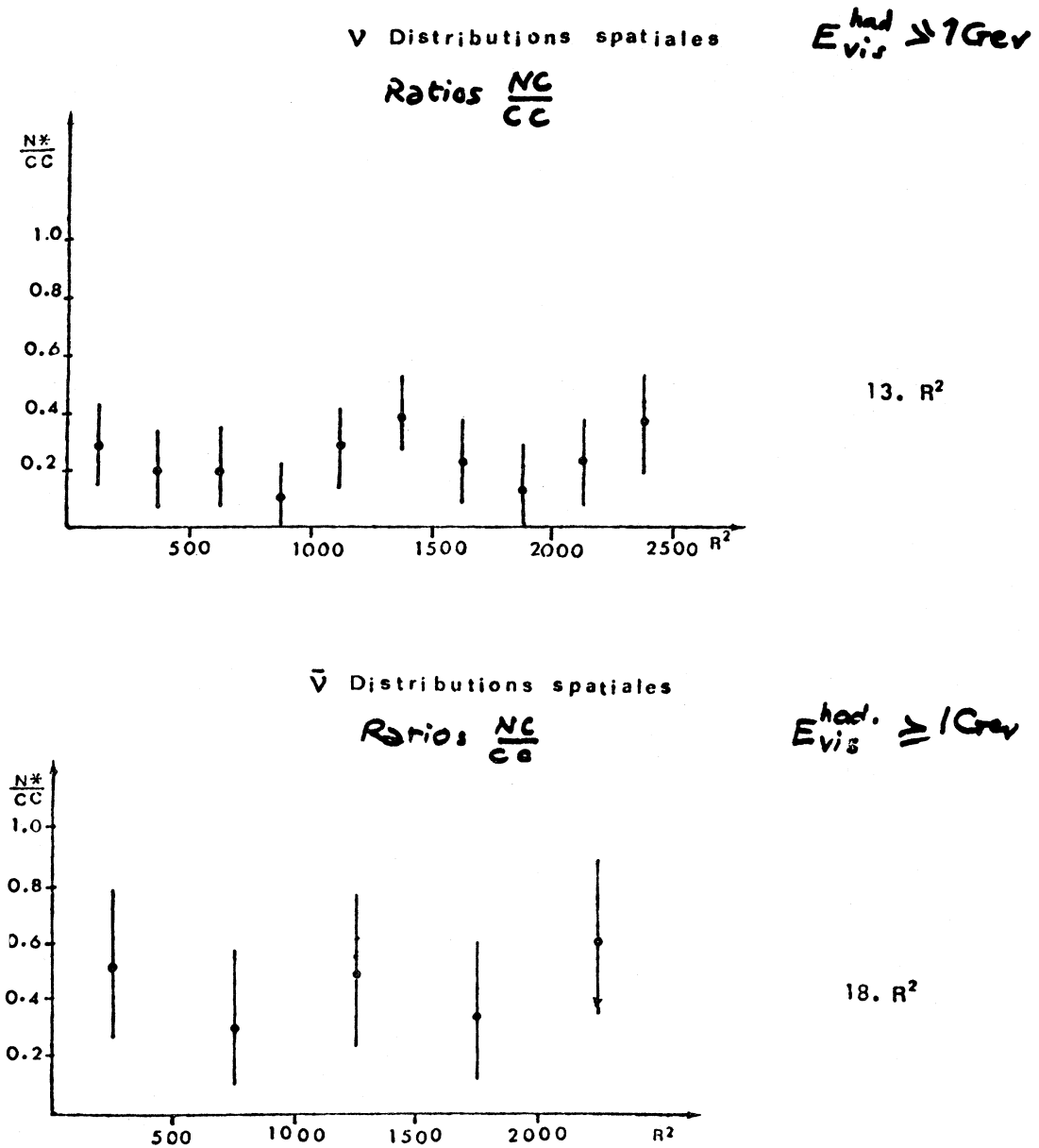
Ratios $\frac{NC}{CC}$

18. R^2

FIG. 11. $R \equiv NC/CC$ plotted as a function of position in chamber after a cut was made eliminating any event with an energy of less than 1 GeV. From Musset, 1973a.

side FV [fiducial volume] when best vertex," "K^0?," "*OUT* possible μ-kink," "*OUT E* > 1 GeV." These comments reflect the various tests to which events were subject. The π's and p's could look like muons—this meant that, to be conservative, possible charged events had to be discarded, as did cosmic-ray events. Similarly, if there was evidence that a particle had entered with the beam in line with a vertex, the photograph was discarded as an "entering track." A "μ-kink" meant that the track suddenly bent—a possible sign that the "muon" was really a pion or proton interacting with a nucleus. Other criteria such as the hadron energy cutoff at 1 GeV and the re-

striction of vertices to a fiducial volume helped statistically to ensure that neutron events to a fiducial volume helped statistically to ensure that neutron events would not be counted as neutrino events. All of these individual analyses, in addition, further guaranteed that the same criteria were applied to neutral, charged, and associated events.

These small-scale debates occurred hundreds of times across Europe both at individual laboratories and at the larger neutrino collaboration meetings. Each decision added to what the participants hoped was a reasonably conservative estimate of the ratio, NC/CC. By April, the

<u>List of the NC events > 1 GeV</u>
<u>controlled at CERN on the 12 & 13 April 73</u>

Total ν : 96
Total ν̄ : 61

<u>Event number</u>	<u>Classif.</u>	<u>Comments</u>
<u>AACHEN</u>		ν <u>7 OK</u>
399/276	<u>OK</u>	2 possible vertices
441/642	OUT	ν associated
556/077	OUT	only electrons possible ν_e
556/307	OUT	ν associated
556/543	(OUT)	probably < 1 GeV. Short track
570/097	<u>OK</u>	check if no possible μ. π^+ downstream ?
570/174	OUT	ν associated
570/253	OUT	< 1 GeV
570/399	<u>OK</u>	E badly known
707/244	OUT	ν_e associated
707/208	OUT	ν_e
714/252	<u>OK</u>	
742/203	OUT	entering track or ν event
749/449	OUT	E < 1 GeV possible μ
756/726	<u>OK</u>	
756/682	OUT	possible μ
756/376	<u>OK</u>	(> 1 GeV if π hypothesis)
763/150	OUT	possible μ
813/392	<u>OK</u>	

FIG. 12. Typical summary sheet of scanning meeting in which individual neutral-current candidates were evaluated and event categories were standardized. From Musset, 1973c.

question of finding an upper limit for the ratio had been abandoned. The goal thereafter was to justify a number in accord with the Weinberg-Salam theory.

In the single-electron search, confidence was also building up: Cundy's minutes from the meeting of 21 March 1973 began with the remark that "There was general agreement that a paper should be published as soon as possible concerning the electron search and the one event found" (Cundy, 1973). Even though all preliminary estimates indicated a very small background, some of the details needed to be cleaned up, such as scanning efficiency. Also, beta decay could yield a free electron which might be mistaken for a neutral-current event if the proton had sufficiently low energy to remain undetected.

Simultaneously, several different groups sought to determine the neutron background with more precision. Vialle and Blum at Orsay set up one type of Monte Carlo simulation, the CERN group wrote another (Vialle, and Blum 1973). W. Fry and D. Haidt (1973) were especially concerned with the possibility that neutrons would induce a shower of other neutrons, thereby dangerously extending their effective range; H. Wachsmuth (1973) exploited an existing CERN program to address the same problem. In Milan, Fiorini (1973) tackled the problem of neutral kaons, while Pullia focused his attention on the attenuation of neutrons in the liquid. Lagarrigue came to believe

that the signal was significant on the basis of his own rough calculation of the background. Finally, Rousset arrived at a thermodynamic analysis of the neutron background that treated the neutrons as in equilibrium with the neutrino beam (Rousset, 1973b). Some years before a graduate student, E. Young (1967), had developed a similar strategy for analyzing neutron background in the smaller bubble chamber.

Each of the background studies helped persuade those members of the collaboration who were not already convinced that neutral currents existed. Of these various approaches, many of the experimentalists found the Young-Rousset thermodynamic analysis of early spring 1973 to be particularly compelling (Cundy, 1980). It was simple and easily generalized to more realistic models. As in other studies, the key quantity to estimate was the ratio of B (the unknown number of neutron-induced "fake" NC events) to AS (the rate of associated events). The analysis was based on three simple equations:

(i) $N = B + \mathrm{AS}$ (N=total rate of neutron interactions that look like NC events),

(ii) $N = \alpha N_\nu$ (N_ν is the rate of neutrino events producing neutrons, and α is a proportion of neutrons that create events satisfying NC criteria, assuming an infinite length of liquid in which the neutron can interact),

(iii) $AS = \alpha N_v \langle p \rangle$ ($\langle p \rangle$ = probability of detecting a neutron interaction if the neutron's origin is inside the fiducial volume).

Therefore,

$$B/AS = (1/\langle p \rangle) - 1 .$$

Suppose a neutron is created in the fiducial volume by a neutrino at a distance L from the downstream end of the fiducial volume. p, the probability that this neutron will engender a NC event inside the volume, will then have the form

$$p = 1 - \exp(-L/\lambda) ,$$

where λ is the measured characteristic neutron interaction length in bubble-chamber liquid. Thus

$$\langle p \rangle = \langle 1 - \exp(-L/\lambda) \rangle$$

and

$$\frac{B}{AS} = \frac{1}{\langle 1 - \exp(-L/\lambda) \rangle} - 1 .$$

From this formula and the measured number of associated events (AS) the background N is immediately given. When other factors such as the radial distribution of neutrino flux, the density of matter surrounding the bubble chamber, the characteristics of neutron cascades, and the energy spectrum of neutrons are added, this result varied. But even when computer simulations were undertaken, the neutron background could account for no more than 20% of the excess of NC events (Rousset, 1973, 1974).

During these weeks, argument within the collaboration went back and forth as various members of the group suggested possible new sources of background, and others sought to demonstrate they could not be large enough to account for the excess of neutral-current candidates. Vialle (1980), for example, remembers Lagarrigue coming into his office practically every day with a new source of possible background. Only days before Musset's seminar announcing the discovery of neutral currents, Fiorini (1973) became very concerned about kaon regeneration, only to write to Musset shortly afterwards that he had convinced himself it would not be a problem. Thus, using a variety of approaches, techniques, and approximations, the members of the collaboration persuaded themselves they were looking at a real effect.

However, the final argument not to delay publication any longer had little to do with the physics at Gargamelle. In early July, Carlo Rubbia, who also held a position at CERN, let it be known that the FNAL group was close on Gargamelle's heels. According to many of the participants, this tipped the already tilting balance, and the decision to publish was made. Not everyone was entirely happy with the arguments presented in the final draft,[5]

---

[5]Cundy, for instance, did not feel the paper was sufficiently convincing at that time. (Cundy interview, 1980; Musset interview, 1980; Vialle interview, 1980.)

but they believed they had the background under control. On 19 July 1973, Musset gave a seminar at CERN announcing the discovery; four days later, on 23 July, the paper was sent to *Physics Letters* (Hasert *et al.*, 1973b). The single-electron paper was received on 2 July 1973 (Hasert *et al.*, 1973a).

In the *Physics Letters* article, the authors relied almost entirely on two arguments: (i) that various criteria such as spatial distribution and energy distribution were the same for CC and NC events and (ii) that the Monte Carlo model predicted a neutron background significantly below the level of NC events found. Rousset's equilibrium argument and the associated studies of cascades, etc. were reduced to a single sentence. Their evidence was summarized in the following figures. (See Fig. 13.) The group put their results in a conservative form, allowing that their data "could be attributed to neutral-current-induced reactions, other penetrating particles than v_μ and v_e, heavy leptons decaying mainly into hadrons, or by penetrating particles produced by neutrinos and in equilibrium with the v beam." Nonetheless, the final sentence returned to the Weinberg model, concluding that their results would imply a Weinberg parameter $\sin^2\theta_W$ between 0.3 and 0.4.

By January of 1974, a more comprehensive summary of the work was prepared for *Nuclear Physics B* (Hasert *et al.*, 1974) that included some of the values of B/AS generated by the Monte Carlo program for a variety of values of neutron energy and angular distribution. Even under the worst case, the excess of muonless events to charged events was too large to be accounted for by the neutron background. In sum, they wrote, "The events behave as expected if they arise from neutral-current processes induced by neutrinos and antineutrinos."

IV. THE FIRST HWPF EXPERIMENT 1A

Neutrino physics was one of the major justifications for building the Fermi National Accelerator Laboratory. The predecessor to experiment 1A (experiment 1), however, had its beginnings somewhat later, in the Summer Study Program held by Fermilab in Aspen, Colorado. In 1969, while the accelerator itself was still under construction, many proposals were put forward for possible search programs for the W. One of the participants, A. K. Mann, presented a report at the school (Mann, 1969) on the possibility of producing the W by means of a high-energy neutrino source incident on a high-Z material, then detecting the particle's decay products using spark chambers between segments of earth. From his preliminary calculations, Mann argued, such a search could be effected up to a W mass of about 5 GeV. As it had been in the Schwartz and Yang and Lee program and the proposals drafted at CERN, the W search was given the highest priority by many American experimentalists at Aspen.

Partly as a result of his own work prepared for the conference and partly as a consequence of the other studies presented there, Mann (1980) advocated running a
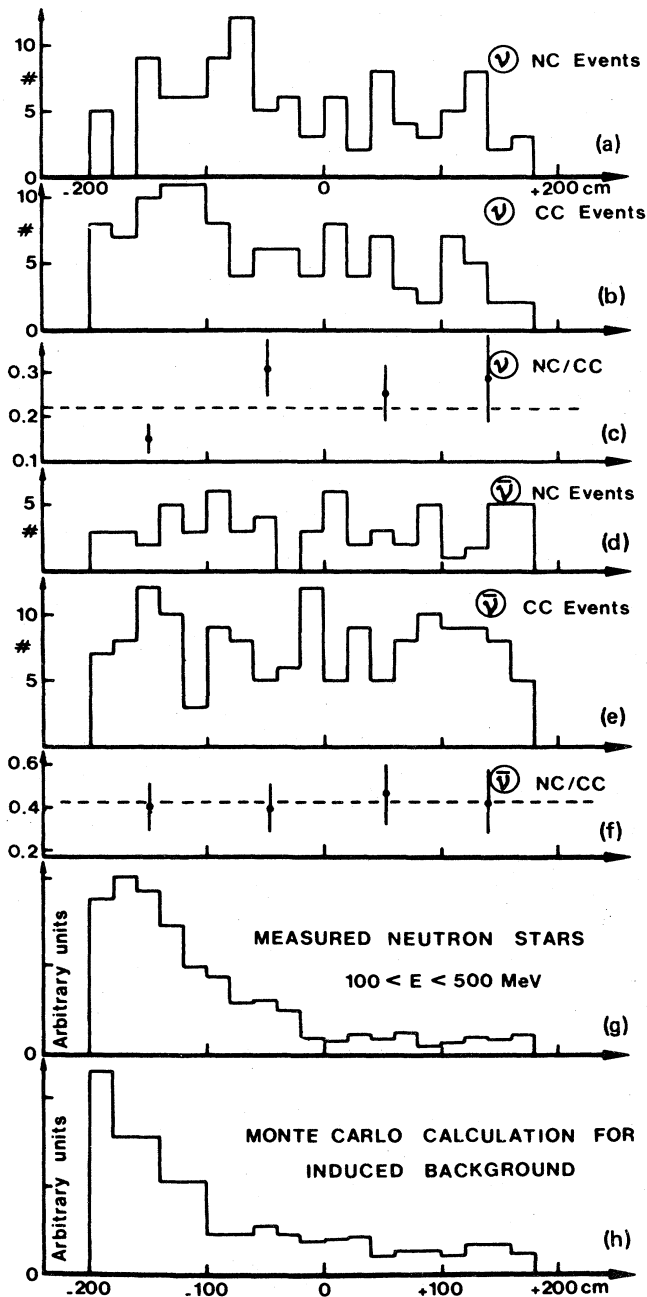
10
#
5
0
-200   0   +200 cm

(v) NC Events

(a)

10
#
5
0

(v) CC Events

(b)

0.3
0.2
0.1

(v) NC/CC

(c)

5
#

(v̄) NC Events

(d)

10
#
5

(v̄) CC Events

(e)

0.6
0.4
0.2

(v̄) NC/CC

(f)

Arbitrary units

MEASURED NEUTRON STARS

100 < E < 500 MeV

(g)

0

Arbitrary units

MONTE CARLO CALCULATION FOR

INDUCED BACKGROUND

(h)

0
-200   -100   0   +100   +200 cm

FIG. 13. Figures from the first published paper on hadronic neutral currents by the CERN-Gargamelle collaboration, Hasert *et al.*, 1973b, p. 139. Distributions along the ν-beam axis. (a) NC events with ν. (b) CC events with ν (distribution based on a reference sample of $\sim \frac{1}{4}$ of the total ν film. (c) Normalized NC/CC ratio with ν. (d) NC with $\bar{\nu}$. (e) CC events with $\bar{\nu}$. (f) Ratio NC/CC with $\bar{\nu}$. (g) Measured neutron stars with $100 < E < 500$ MeV having protons only. (h) Computed distribution of the background events from the Monte Carlo.

high-energy neutrino experiment at FNAL. But Mann was not the only physicist with his eye on the first neutrino experiment at FNAL; it was clear from the start that whoever ran that first neutrino experiment would be in an excellent position to explore a region of energy high above

that of all previous work. Thus, as Mann began to draft the proposal, it seemed to him likely that if the collaboration of Schwartz, Steinberger, and Lederman also submitted an application for the first neutrino experiment, they would present very stiff competition. Not only had these three experimentalists worked together before on the two-neutrino experiments, but the apparatus they had been using was quite similar to the spark-chamber detector Mann hoped to build. In addition, J. Walker presented a proposal that remained in direct competition with experiment 1 until the final decision by the planning committee. To add more weight to his proposal, Mann turned to a younger physicist with whom he had beem impressed, David Cline.

Cline, like Mann, brought with him much experience in experimental weak-interaction physics. Even since his Ph.D. he had been interested in the problem of neutral weak currents in kaon decay. As was mentioned earlier, such strangeness-changing neutral currents had quite severe upper limits placed on them by a variety of experiments. Almost all his career in physics Cline had been involved in these determinations using bubble chambers. For example, in 1964 with Camerini, Fry, and Powell he had shown that the branching ratio of $K^+ \rightarrow \pi^+ e^+ e^-$ was less than 10^{-6} of all K^+ decays (Camerini *et al.*, 1964).

Much of Cline's careful work on the strangeness-changing neutral currents involved the identification of characteristic "signatures" of the various rare processes he was studying. The problem was to identify carefully as many unambiguous events as he could, thereby setting limits on the process. In the Camerini, Cline, Fry, and Powell paper, the authors had found three candidates for the $K^+ \rightarrow \pi^+ e^+ e^-$ decay in the bubble-chamber pictures and then had devoted a considerable part of the letter to an argument that two of the three must be due to a background process "faking" the neutral-current events. Even the third, they concluded, was not "an unambiguous event and [we] shall consider it as an upper limit." It was not, to use one of Cline's favorite phrases, the "gold-plated event" they were searching for. Still, it was good enough not to be discarded.

A few years later, after Cline and many others had conducted quite a variety of other experiments on strangeness-changing decays in a variety of channels, Cline reviewed the subject at the École Internationale de la Physique des Particules Élémentaires (Cline, 1967). The tenor of the article was that the limits on neutral currents seemed to indicate such currents did not exist. Indeed, Cline summarized his review by declaring:

...the crucial tests of such models [of weak interactions by Salam and Ward, Good, Michel, de Rafael, d'Espagnat, and Bludman] will probably come from experimental studies of lepton-lepton scattering which presently seem virtually impossible. Nevertheless, the successful explanation of the absence of neutral lepton couplings (and possibly of primative neutral hadron couplings) will undoubtedly be a very significant factor in the ultimate theory of the weak interactions.

Cline was an important addition to the neutrino project, and, in December 1969, Cline and Mann (1969) drafted a more complete proposal for the experiment by elaborating Mann's earlier Summer School report. Their goals were stated as threefold. First, they wanted to measure the double differential cross section $d^2\sigma/d(q^2)d(E_\nu -E_h)$ and the corresponding total cross section for $\nu_\mu +p\to\mu^- +$ anything. (E_h is the energy of hadrons.)

The second goal of the experiment would then be the W search. Assuming the mass of the W to be less than about 8 GeV/c^2, they would look for the particles through the reaction

$$\nu_\mu +Z\to\mu^- +W^+ +Z ,$$

which corresponds to the Feynman diagram in Fig. 14.

If, on the other hand, the W was significantly more massive than 8 GeV/c^2, the authors hoped to look at the "point" interaction in which the decay of the W^+ would be seen. Nominally, the decay products of the W^+ would be $\mu^+ +\nu_\mu$, i.e.,

$$\nu_\mu +Z\to Z +\mu^+ +\mu^- +\nu_\mu ,$$

which corresponds to the Feynman diagram in Fig. 15.

The proposed physics goals required a more sophisticated apparatus than a simple spark chamber, so Mann and Cline modified Mann's original detector in several ways. First, they proposed using liquid scintillator containers alternately placed between iron blocks to form a sampling ionization calorimeter. In this device, when hadrons hit the iron they caused further showers of charged particles. Cascading charged particles caused light to be emitted as they collided with atoms in the scintillator. The light could then be collected and measured by phototubes. Second, 25 m downstream from the calorimeter, they used blocks of iron alternately placed between spark chambers to determine the range (and therefore the remaining energy) of the muons. To determine the sign of the muons, the first section of the muon detector was to have been magnetized. By measuring the hadron and muon energies, the experiment could be used to determine the energy of the original neutrino, making possible the various cross-section measurements proposed by the authors.

Mann nonetheless felt the apparatus was not yet well enough formulated to sway the planning committee at FNAL, so he and Cline turned to Carlo Rubbia (then at Harvard), whom Mann knew from a leave of absence Mann had taken at CERN (Mann, 1980). Above all,
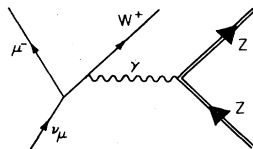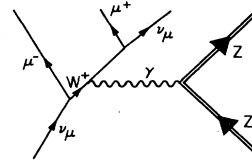

FIG. 15. W decay products.

Rubbia brought with him his experience designing and building large and sophisticated detectors.

During the previous several years, Rubbia had used large detectors to study the interference of K_S and K_L. This line of investigation, which had begun as a test of CP conservation, had yielded a wealth of new discoveries, including Fitch and Cronin's discovery of CP violation (Christenson et al., 1964; Fitch, 1981; Cronin, 1981). One goal of the various collaborations in which Rubbia took part was to confirm the earlier results. Other objectives were to determine more precisely the $K_L -K_S$ mass difference and to obtain a better understanding of the empirical aspects of regeneration phenomena. All three of the principal collaborators of experiment 1A thus came to the experiment with a strong background in weak-interaction physics, though the kinds of experiments in which they had participated were quite diverse.

The three principals—Mann, Cline, and Rubbia—planned a meeting in the lobby of the JFK airport in late 1969. Before they parted ways they had agreed to proceed with a joint proposal for the neutrino experiment. When the proposal was finished, it had become the "Harvard-Pennsylvania-Wisconsin collaboration," and the four goals they set down were the same as the four main objectives of the CERN project being formulated on the other side of the Atlantic:

(1) The W search (which they claimed could be undertaken up to 10 GeV);

(2) The point interaction $\nu_\mu +Z\to\nu_\mu +\mu^+ +\mu^- +Z$; and

(3) The double differential and total cross sections for $\nu_\mu +p\to\mu^- +$ anything.

In addition the group now identified the probing of hadronic structure in the deep-inelastic region (large $E_\nu -E_h$) as one of their primary interests. For only since the SLAC deep-inelastic results and the corresponding speculation on their theoretical origin, had hadronic structure become a major concern. As it had at CERN, investigation of the parton model took its place beside the W search as a major goal of the experiment.

To accomplish these goals, Rubbia and his collaborators redesigned the earlier two-stage detector in several important ways (see Fig. 16). The calorimeter was rebuilt to be totally active, that is, all the energy deposited in the mineral-oil-based liquid scintillator would be collected by phototubes. Between the scintillator containers were placed spark chambers to record both hadron and muon tracks. In addition, counters A, B, C, and D could

FIG. 14. Neutrino production of W. An early goal of high-energy neutrino experiments was to produce W's at several GeV.
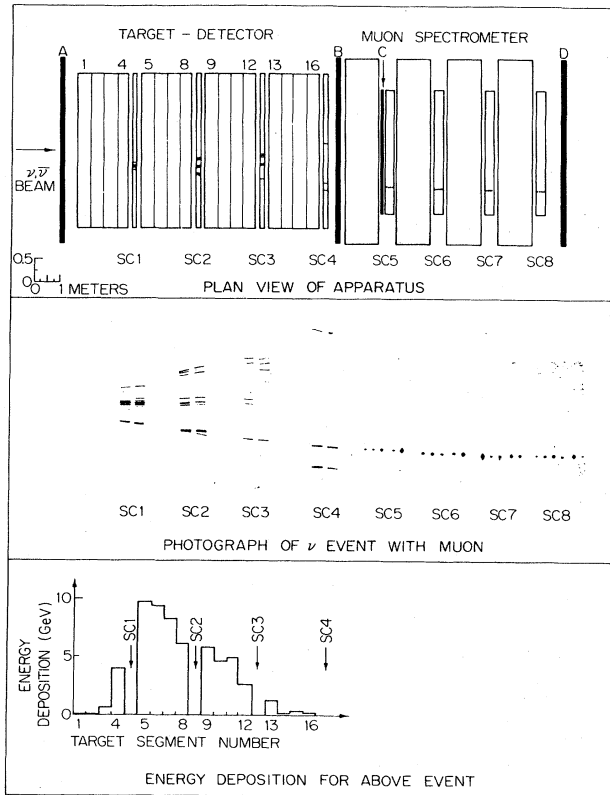
FIG. 16. Experimental 1A. Top is a schematic depiction of the apparatus. The first stage consists of units of four liquid scintillator segments (labeled 1—12) alternately placed between double-gap spark chambers that are labeled SC1—SC4. The light produced when charged particles pass through the scintillator is collected by phototubes. When added together, the phototube's output is proportional to the energy deposited. Simultaneously the spark chambers produce tracks from the event which can be used to analyze events later or to trigger on-line electronics. In addition, the scintillators and spark chambers serve as a neutrino target. The second stage consists of iron slabs alternately placed between spark chambers. By placing the whole second stage in a magnetic field, it serves as a muon detector (muons and not hadrons easily penetrate such a barrier). Middle shows a typical spark-chamber display for a charged-current event. The single track through SC5—SC8 is a muon. All the other tracks end, indicating that they are probably hadrons. Bottom is the calorimeter display recording the energy deposition of the event in middle by scintillation segment. Figure after Benvenuti et al., 1974, p. 801.

be used to trigger the recording devices selectively. For instance, they would be triggered only when no charged particles entered the device through A in time with a hadron shower in the calorimeter. The second stage of the detector was improved as well. Instead of determining the energy of the muons by their range through blocks of iron, the group installed large magnetized iron blocks, which served to measure the muon's momentum by its deflection. From the total energy deposited in the calorimeter and the muon energy as determined in the spectrome-

ter, the neutrino energy could be calculated (Beier et al., 1970).

Thus, if the physics goals resembled those of CERN, the experiment itself certainly did not. The idea behind the design of the apparatus remained as a two-stage analysis of the neutrino interactions: calorimeter and muon spectrometer. By combining these two detectors, the E1A group would record more information than one would in a simple spark-chamber experiment, and therefore the group would be able to compete favorably with the bubble-chamber neutrino groups. In addition, the spark-chamber calorimeter had two other important advantages. First, spark chambers could be built much larger than bubble chambers, giving a ratio of 10 (100 tons versus 10 tons) in the target mass. Further E1A would operate at 10 times the energy of CERN-Gargamelle (20 GeV versus 2 GeV), providing yet another factor of 10 in the expected rate of neutrino interaction. Thus on the order of 100 times the Gargamelle rate per day could be expected at FNAL. A second advantage of the spark chamber was that by being active, it could exclude events in which an unseen neutron interacted with nuclei creating a shower of charged particles.

The competition between the two types of detectors thus reflected a deep experimental conundrum: Bubble chambers provide great detail on particle momenta and identification, but they are passive devices requiring vast amounts of film and running time to locate rare events. Spark chambers, by contrast, normally offer less detail in the event analysis, but are active, recording event information only when specific logic circuits are fired, and providing a much higher rate of interactions. E1A used a detector which was designed to try to bridge this gap, if only partially. As we shall see, this dilemma, pitting particle identification against high statistics, played a crucial role in the subsequent neutral-current search.

Neutral currents, it should be added, figured but little in the Harvard-Wisconsin-Pennsylvania proposal. They are not mentioned in the primary physics objectives. But more important, the design of the apparatus was such that, even in principle (in its original form), the experiment was not capable of a neutral-current search. The reason neutral currents could not be found is that the logic circuits would have an event recorded only if a muon penetrated into the muon spectrometer; unfortunately, neutral-current events were characterized precisely by having no muon. This feature of the trigger had been borrowed (along with much else) from an earlier experiment where such a trigger was crucial to eliminate extraneous events where no muon was produced. Finally, as in the CERN proposals, even where neutral currents were mentioned (in the context of dimuon production), there was no mention of Weinberg-Salam theory at all, and no quantitative prediction of the order of magnitude effect to be expected.

During the winter and spring of 1970, plans for the experiment advanced, and in the summer of 1970 Cline, Mann, and Rubbia (1970) published an article describing another channel through which they could use their ap-

paratus to detect the decaying W's. This article, "Detection of the Weak Intermediate Boson Through Its Hadronic Decay Modes," again focused on the search for intermediate bosons in the energy range of 5–10 GeV/c^2. In print, the HWPF collaboration did not, however, discuss neutral currents (or the weak interaction in general) in relation to the Weinberg-Salam model before 1972.

In January, February, and March of 1972, E1A and E21 (Barish *et al.*) began skirmishing over who would actually be the first to run a neutrino experiment on the new beam. After several exchanges of letters and meetings with the Director, Wilson let E1A proceed as the first experiment.

Meanwhile, 't Hooft's renormalization proof of Weinberg and Salam's theory reopened interest in the gauge-theoretical unification ideas. Once again, the HWPF and Gargamelle collaborations continued to move in parallel. Whereas in Switzerland, Zumino had come to speak to the experimentalists about the consequences of the gauge theories, in America, after the renormalization proof, Weinberg began calculating some experimental consequences of his theory—calculations which until then had not seemed worth undertaking. Some nine years later, Weinberg (1980) recalled,

> Now we had a comprehensive quantum field theory of the weak and electromagnetic interactions that was physically and mathematically satisfactory in the same sense as quantum electrodynamics—a theory that treated photons and intermediate vector bosons on the same footing, that was based on an exact symmetry principle, and that allowed one to carry calculations to any desired degree of accuracy. To test this theory, it had now become urgent to settle the question of the existence of the neutral currents.

Weinberg published calculations of the cross sections to be expected for neutral-current production. In addition, from M.I.T., he called Rubbia at Harvard to tell him how important it was for the FNAL group to search for the expected muonless events. Rubbia (1980) recalled that,

> Steven Weinberg was the one who, with rare insistence . . . was chasing me and many other people [to do the neutral-current search]. I learned all these things [about gauge theories and neutral currents] from him directly. I remember I was down in the old cyclotron at 44 Oxford Street. He called me up—in the beginning I thought, my God, what [is] he asking me to think? [Then] I realized how beautiful things were.

Soon, the E1A collaboration decided to do the search; it fit in with some of their earlier interests and seemed possible without extensive modification of the apparatus. It also added yet another reason for the steering committee at FNAL to choose E1A, as they were quick to point out (Benvenuti *et al.*, 1972) to the Director of the laboratory, Robert Wilson:

> There has recently been increasing awareness of the need of more sensitive searches for neutral weak currents and neutral weak intermediate bosons. The existence of a neutral weak current or a neutral-weak propagator would cast additional light on the connection between weak and electromagnetic interactions. As the center-

of-mass energy, $S^{1/2}$, available to experiments increases, and GS moves closer in magnitude to α, the possibility of finding such a connection becomes more realistic. We might now stand in a position analogous to that of Oersted, Ampère, and Faraday 150 years ago as they attempted to elicit the connection between electricity and magnetism.

> We have observed, along with others, that a sensitive test of a recent, possibly renormalizable, theory of weak interactions may be made through comparison of the observed rates for the processes

> $$\nu_\mu(\bar{\nu}_\mu) + N \rightarrow \nu_\mu(\bar{\nu}_\mu) + \text{anything}$$

> and

> $$\nu_\mu(\bar{\nu}_\mu) + N \rightarrow \mu^{\mp} + \text{anything} ,$$

> where N is a nucleon. Different models allow for some leeway in the expected value of the ratio $\sigma(\nu_\mu)/\sigma(\mu)$, but a value $\lesssim 0.01$ would be quite difficult to accommodate in that theory.

The immediate experimental necessity was to install a trigger on the calorimeter that would fire if *either* the hadron energy was above a certain minimum in the calorimeter *or* a muon passed into the muon spectrometer. Rubbia (1980) later commented that he had been in favor of putting the trigger into the experiment, "not because I had decided it [beforehand], but because Steve Weinberg gave me a good reason for it." The actual construction of the trigger was the first independent task that Larry Sulak, then a young assistant professor at Harvard, undertook on the project. Aside from the immediate problem of putting together the electronics, it engaged Sulak full time in the problem of the neutral-current search.

Data from the experiment came in painfully slowly. The beam was on for a few days near Thanksgiving 1972, then again for a short time near Christmas. Between the two runs, the energy trigger yielded some 150 events to be examined; these were first assessed by the Wisconsin group with Sulak flying out to help. Soon, however, the data were brought to Harvard, which became, for the first part of the experiment (up to August 1973), the focal point for the neutral-current search.

Almost as soon as the energy trigger was installed, pictures began to show up without muons (pictures like the one reproduced in Fig. 2, at the beginning of this essay). Much later these were taken to be photographs of a process including weak neutral currents. But at the time at least some members of the group saw them quite differently. Mann (1980) later commented in an interview:

> You can say, well, we came to the conclusion immediately that we had seen weak neutral currents. But you'd be suprised, that was the last conclusion we came to. Our first conclusion was that we were making some mistake and that these muons were somehow escaping the apparatus or being missed by us in some way and that no effect of that magnitude could exist.

It must be reemphasized that Cline and Mann, independently (Beier *et al.*, 1972), had conducted precise measurements to show that neutral currents in kaon decay did not exist in some channels above one part in a million. As

was mentioned above, it was only later accepted that charm suppressed neutral-current decays if they were strangeness-changing, but did not affect the strangeness-conserving processes considered by E1A and Gargamelle. It bears repeating that, at the time, neither the E1A group nor practically anyone else sought to draw a radical division between strangeness-changing and strangeness-conserving decays. It was therefore natural when dealing with a new machine for the experimentalists to suspect that some error was producing the ratio of over 30% muonless events to events with muons.

Consequently, during the spring of 1973, Mann and Cline were concerned principally with understanding the physics of charged-current events and various other projects originally set out as goals for experiment E1A. Their reasoning was that the charged-current events would yield information about the properties of the detector as well as about the charge-current events themselves. With so many aspects of the beam and detector still untested, this seemed a necessary prerequisite for the study of any new physics, including neutral currents, heavy leptons, violations of scaling, and other topics. Culminating these first efforts was a paper submitted by the group to *Physical Review Letters* entitled, "Early Observation of Neutrino and Antineutrino events at High Energies" (Benvenuti et al., 1973).

Meanwhile, during the spring of 1973, Sulak began the analysis of the films brought back from the experiment. Several undergraduates assisted him, and the small group remained in contact with the larger group and with Rubbia, who was traveling back and forth between CERN and Harvard. From the computer tape, Sulak determined the frame numbers on the film of events where more than a minimum cutoff amount of energy was deposited in the calorimeter. Then, frame by frame, in a fourth-floor room in Lyman Laboratory at Harvard, he and the undergraduates studied the photographs in a high-accuracy film projector, sorting muonless from charged-current events, and measuring the properties of both.

The problem of escaping muons was their overriding concern. (See Fig. 17.) Since any individual "muonless" event might have a muon escaping detection by exiting from the calorimeter at a large angle, it was necessary to work out a computer-simulated model for wide-angle muons. By comparing the number of muons expected not to reach the muon spectrometer with the number of measured muonless events, they could determine if there was a statistically significant excess of neutral candidates. Two Monte Carlo programs, one at Wisconsin and one at Harvard, simulated the distribution of muons, using an angular distribution given by the parton model (Rubbia and Sulak, 1973).

When the Monte Carlo results were ready and compared with the first batch of photographs, it became clear that there *was* an excess of muonless events. After correction, the ratio, R was found (Benvenuti, 1974a) to be[6]

$$R = NC/CC = 0.42 \pm 0.08 .$$

During June and July, Sulak and the undergraduates prepared an article for *Physical Review Letters*. Meanwhile, at FNAL, Bill Ford, an assistant professor at Pennsylvania, and others began to work on the 400-GeV data. These had been obtained later and so were not included in the first muonless-event analysis. Ford began later than the Harvard group and so, when the paper was finally ready in late July, only about half as much data existed at 400 GeV as at 300 GeV, but they seemed statistically in accord with the lower-energy results. Sulak then brought the manuscript to Mann (who was sick in bed with back problems), and Mann, Ford, and Cline agreed that this paper should be submitted for publication.

All of this work in the late spring of 1973 was done knowing that CERN was accumulating evidence on the weak neutral currents, since Rubbia was commuting regularly between CERN and the U.S., and others from the CERN group occasionally visited FNAL. In mid-July, Rubbia (1973), independently, wrote a letter to Lagarrigue telling him of the recent HWPF work:

> I have heard from several people at CERN that your neutrino experiment in Gargamelle in addition to the beautiful electron event has now a growing evidence for neutral currents. We have observed at NAL approximately one hundred unambiguous events of this type and we are in the phase of final write-up of the results. In view of the significance of the result I am addressing to you this note in order to know if announcing our result we should mention the existence of your work on the hadronic processes (and if so in which form). In this case I hope you will take a similar attitude toward our work.

Lagarrigue declined Rubbia's offer the next day (Lagarrigue, 1973b), suggesting that the announcements be made independently without mentioning the other's results, adding that the CERN announcement would be made in twenty-four hours, on 19 July.
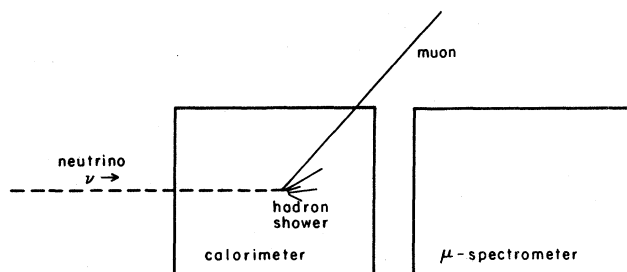
Upon returning to the United States, Rubbia helped

FIG. 17. Wide-angle muon escapes detection in the muon spectrometer, thereby making a charged-current event look like a neutral-current event.

---

[6]This value of R is from the first version of "Observation of Muonless Neutrino-Induced Inelastic Interactions," typescript (Benvenuti et al., 1974a) delivered by hand by L. Sulak to George Trigg, Editor of *Physical Review of Letters*, on 3 August 1973. A slightly revised version (Benvenuti et al., 1974b) was submitted on 14 September 1973.

make final revisions of the FNAL paper, which was wide-
ly distributed as a preprint in late July and August. After
Rubbia's departure from the U.S. on 27 July, Sulak fin-
ished the draft of the paper and brought it by hand on 3
August to George Trigg, the editor of *Physical Review
Letters*. This draft, meanwhile, had been seen by a fair
number of theorists and experimentalists, and based on
their comments the group made some corrections. First,
the theoretical angular distribution that had been used to
generate events in the Monte Carlo program was replaced
by an empirical one, based on the muon distribution in
the last few chambers of the calorimeter. Second, more
data were included from analyses at Madison and Phi-
ladelphia.[7] However, when Ford's new data were com-
piled, they showed a significantly lower value of R, espe-
cially in the first six segments of the calorimeter, where
they now found R equal to 0.06 ± 0.16, thus only half of
the standard deviation from zero. The average value of R
from all the 300 and 400 GeV data was therefore revised
from 0.42 ± 0.08 to 0.20 ± 0.09.

A technical digression is necessary here. The authors
wrote in their Table 1 of the revised paper that they had a
5.2 standard deviation effect, a remark which caused a
great deal of controversy and confusion. The justification
for this number was based on the following statistical dis-
tinction. There are two ways to measure the statistical
significance of the value of R determined by the group.
(1) The question can be asked, "How well is the value of
R known?" for which the answer depends on the uncer-
tainty of R, that is, on the ± 0.09. (2) The question can be
asked, "Given the assumption that the pre-Weinberg-
Salam theory of weak interactions is valid (i.e., that there
are only higher-order effects simulating neutral currents),
what is the probability that one would find a value of
$R = 0.29$?" The answer to this second question depends
not at all on the uncertainty in R, but only on the distri-
bution of R's to be expected from the old theory of weak
interaction. In other words, method (2) gave the probabil-
ity of the effect not being a random fluctuation from the
predictions of the old physics. This latter approach
characterized the overall point of the Harvard paper.
They did not want to stress the particular value of R, but
only that neutral currents *existed*.[8]

On the basis of their statistical evidence for the effect,
Rubbia and Sulak began to prepare for the summer
conferences at Aix-en-Provence and Bonn, where they
would announce their findings. In late August, Sulak
brought the data over to Europe (where Rubbia had
remained since leaving the U.S.), and they, along with Jim
Pilcher and Don Reeder, went to Bonn for the Interna-

tional Conference on Electron and Photon Interactions at
High Energies. (Their papers arrived too late for them to
present their data at a plenary session. However, George
Myatt had already been scheduled to speak on neutral
currents (Myatt, 1974), and he agreed to read a brief
handwritten report that had been handed to him by
members of the HWPF collaboration.

After the talk, Myatt was asked how these results of
CERN and FNAL could be reconciled with the low limits
on strangeness-changing neutral currents in K and Σ de-
cays. "That," he responded, "is a major obstacle to the
Weinberg-type theories." This exchange is important be-
cause it makes it clear that even *after* the existence of neu-
tral currents was being established, the charm hypothesis
was not widely accepted, even among the participants in
the neutral-current search.

In Aix-en-Provence the representatives of Gargamelle
and E1A reassembled during the week of 6—12 September
1973 to discuss their results. Again Musset insisted that
the evidence from the compatibility of v and $\bar{v}$ events,
constancy of NC/CC over a range of energy, and the gen-
eral similarity in the hadronic showers in NC and CC
events all conspired to suggest neutral currents were
present (Musset, 1974). Weinberg (1973) cautiously en-
dorsed the neutrino experimenters' conclusions. "It is
perhaps premature to conclude from all this that neutral
currents have really at last been observed. There may be
some mysterious source of background contaminating all
these experiments. It is certainly too early to conclude
that the old model of leptons is really correct. However,
there is now at last the shadow of a suspicion that some-
thing like an $SU(2) \otimes U(1)$ model, with $\sin^2\theta$ of order 0.3,
may not be so far from the truth" (Weinberg, 1973, p. 47).
Thus encouraged, by late summer after the conferences it
seemed to the Harvard group that the experiment had ac-
complished its primary goal.

V. THE SECOND HWPF EXPERIMENT 1A

At FNAL, however, it was just beginning. Four cir-
cumstances contributed to a certain distrust Cline and
Mann felt about the paper submitted to *Physcal Review
Letters*. First, the 400-GeV data reduced at Madison indi-
cated a very low ratio of neutral to charged currents.
Second, Cline at least came to the experiment having re-
peatedly set extremely low limits on neutral-current pro-
cesses in the kaon decays. Not unreasonably, he expected
in the summer of 1973 to place yet another low upper
bound on the neutral currents. Given the uncertainty in
the use of the new apparatus, in addition to the wide-
angle muon problem, it was natural that he sought a fur-
ther check on the new results. Finally, Mann felt that the
whole experiment could be redone rapidly in a much im-
proved way. As a result, the full attention of Cline,
Mann, and the others at FNAL was devoted to the rear-
rangement of the detector. For the moment, believing the
conference reports to be a sufficient description of their
work, they put the paper on the back burner.

---

[7]Compare the histogram on 400 GeV data in Benvenuti *et al.*,
1974a with the one in Benvenuti *et al.*, 1974b.

[8]See Rubbia's comment after G. Myatt's talk at the 1973 Bonn
Conference (Myatt, 1974): "The important question in my
opinion is whether neutral currents exist or not, not so much the
value of the branching ratio." Discussion after Myatt's talk, p.
405.

The main improvement Cline and Mann sought to make was to move a counter in the muon spectrometer closer to the calorimeter to catch more of the wide-angle muons. (See Fig. 18.) In addition, they replaced the spark chambers with larger ones, which also improved the angular acceptance of the muon spectrometer. The price they had to pay for these changes at the time did not seem high; they were forced to introduce a new, 13-inch-thick steel shield to separate the calorimeter from spark chamber 4, which then could serve as a wide-angle muon detector. This shield, plus the downstream sections of the calorimeter, would presumably stop the hadrons formed in the upstream part of the calorimeter from penetrating into the spectrometer and thus impersonating muons. Previously, this function had been served by a much thicker (4-foot) iron slab that had come before the first counter in the spectrometer. But now with the steel slab wedged before the last spark chamber, the slab needed to be thinner to allow the last spark chamber to be close enough to the calorimeter to catch the wide-angle muons. Cline (1973a) commented on the change in a memorandum shortly after the first test run of the new apparatus on 28 September, 1973:

> The new iron placed behind the calorimeter is very effective in reducing the hadron penetration to ... [spark chamber 4]. Some small number of events do show penetration, but the fraction is very likely less than 20%. ... More study of the data is needed to make this a reliable conclusion.

Unfortunately, though it was not to be understood for several months in a quantitative way, the shield was *not* thick enough to be very effective in reducing hadron penetration. This was a crucial problem. For if the hadrons penetrated through the iron, even if no muon emerged from the vertex, the event would be recorded as a charged-current event. (See Fig. 19.) Because the experimenters had not compensated adequately for the punchthrough, the neutral-current signal seemed to vanish. The reason precise predictions could not be calculated for the hadron punchthrough is related to the reason the Gargamelle group was having such a hard time calculating the neutron interaction length: both problems involved the passage through matter of strongly interacting particles. Strong interactions presented a much more difficult problem than the well-understood electromagnetic interactions involved, for instance, in a muon's passage through matter. Compounding the problem was the absence of good data on the energy and momentum distribution of the hadrons being produced. This was the first observation of high-energy neutrino reactions; and the composition of the reaction products had not been studied at all. Since punchthrough had not been a dominant problem during the earlier experiment, it was not at first realized that the thinner shield made it a serious one now.

In part, this was because the FNAL group at this point was still looking for single unambiguous events, the kind of "gold-plated events" that Cline had successfully used before in his bubble-chamber work to set very low limits on neutral-current processes in kaon decay. In this respect his approach was similar to that of the electron group at CERN. It was therefore natural for him to continue to look in E1A for the same type of argument. In the same memorandum, Cline took the vertex reconstruction and other information from the data tapes to examine a single event, dead center in the fiducial volume, which had survived both position and energy cuts. (See Fig. 20.) "It is amusing," Cline (1973a) wrote,

> to investigate how improbable the central (x,y) event is ... (The other two events are too close to the edge of the fiducial region to be gold plated.) ... we expect to find ... 1 events. Thus, unfortunately this event is not improbable and we have not found a gold-plated event.

One corollary of this style of work (in which one searched for "shining examples") was that Cline was not especially confident in the statistical approach on which the initial paper was based (Cline, 1981). Such computer simulations seemed to him vulnerable to errors in fixing various
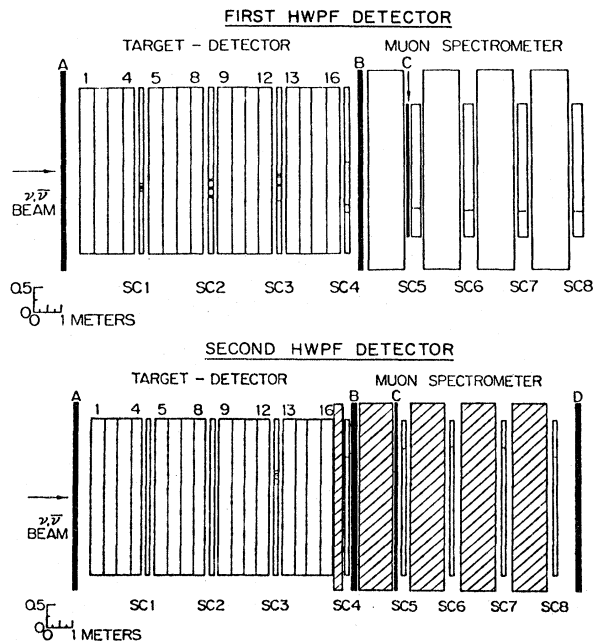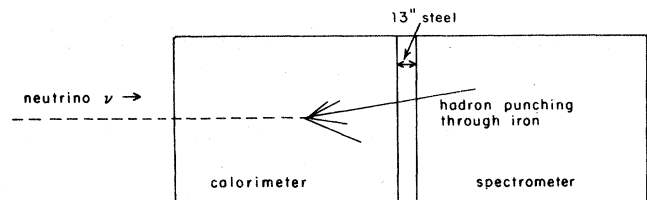
FIG. 18. Comparison of two versions of experiment 1A. Top: Old apparatus described in Fig. 16. Bottom: New arrangement using spark chamber 4 (previously part of the first stage) to capture muons at wider angles than was previously possible. To filter out hadrons a 13-inch thick steel plate was placed in front of SC4. The other iron filter plates in the muon detector are 4 feet thick. Bottom from Aubert et al., 1974a, p. 1455.

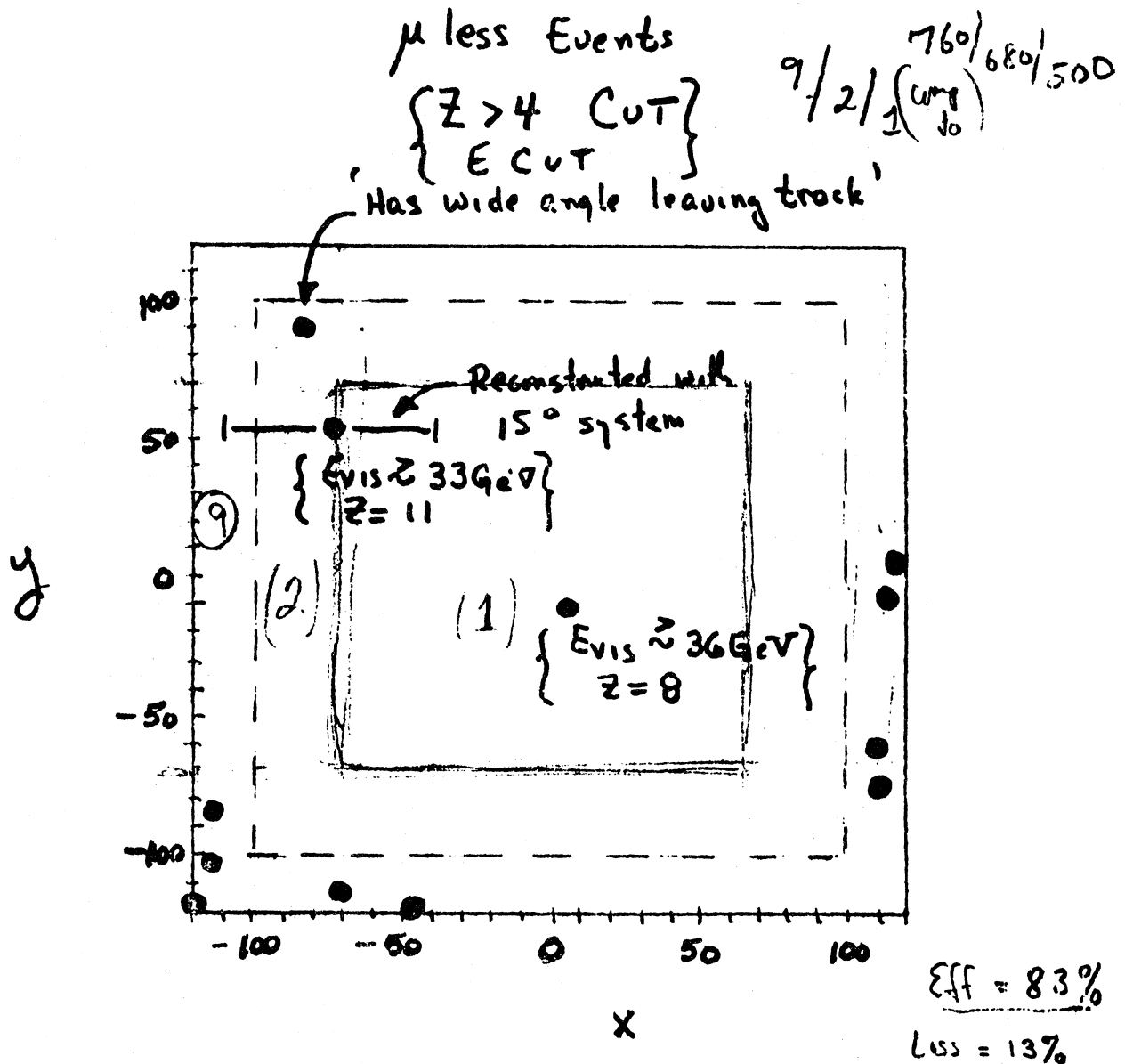FIG. 19. Punchthrough. Hadrons penetrate into muon spectrometer.

FIG. 20. Candidate for a "gold-plated event." Cline, 1973d/October 1973.

parameters such as the characteristics of the neutrino beam and the muon angular distribution. Mann, too, felt doubtful about the earlier Monte Carlo results, and sought to recheck the angular distribution of muons (for charged events) to large angles. This would ensure, he felt, that the corrections for wide-angle muons were being made properly for the muonless events (Mann, 1980).

Cline's doubts about the existence of neutral currents were expressed a few days later in a technical memorandum suggesting it would be interesting to look for muonless events that might arise from the production and decay of intermediate vector bosons—a possibility completely incompatible with the Weinberg-Salam theory at the energies they were using. The following day, 11 October, Cline (1973b) sent out the first preliminary indications

that experiment 1A no longer was giving results compatible with CERN's publication. The calculations were crude, using two crucial numbers: Reeder had calculated an 83% muon detection efficiency, and Ling had estimated a 13% hadron punchthrough. This last number was less than half of what it was eventually found to be, and had the effect of lowering radically the number of calculated excess muonless events. Since more pions were penetrating through the steel than they thought, many real muonless events were being counted as charged-current events.

For a variety of reasons, this error persisted for some time before a rigorous analysis was undertaken. First, in the old experiment, hadron punchthrough had not been a problem because of the thicker iron shield. Second, the

physics of hadron interactions in iron at high energies was not especially well understood or measured at the time. Third, the energy distribution of the pions was not well known. Fourth, the group was under enormous pressure to present a result. Finally, the Madison group were now finding what they thought they would find: that the muonless events had simply been an artifact of the apparatus' geometry.

Cline's 11 October memorandum also placed a 90% confidence limit on R of 0.07, and a 99% confidence upper limit of 0.21. "Taken at face value," he concluded, "these results are inconsistent with the CERN measurement of $R = 0.28 \pm 0.03$ for a mixed beam [of neutrinos and antineutrinos]. Clearly, it could still be that we did that one in 100 experiments or something else is wrong." Something else was wrong, but it would take the group two more months to be sure what it was.

The pressure, meanwhile, was building up. Cline recalls getting less and less sleep as the project was stepped up to provide a definite answer to the neutral-current question. On 16 October 1973, Cline (1973c) distributed a new memorandum:

(i) Because of the importance of the neutral-current question, the fact that we have extended our necks previously on the subject, and that other groups around the world are moving fast to check our results and the CERN result, I propose that a rapid, unified analysis of the muonless events be carried out early in November at NAL . . . .

(ii) The schedule of our run has changed, with the laboratory now inserting running time for E21 at the end of November. I suspect that this time will be used for a muonless search, since they are likely submitting a proposal for this experiment in the next week or two. Again this proves the need for us to move fast in our analysis and to settle the question before others get to it.

That same day, 16 October 1973, the referee reports (Anonymous, 1973) from the Harvard paper were sent back from *Physical Review Letters* to Sulak. Both referees agreed that corrections were necessary to clarify the wide-angle muon problem. Both also criticized the way the statistics had been handled, claiming that Sulak and Rubbia's technique for assessing the statistical significance of the data were not sufficiently conservative. Essentially, both referees wanted the authors to base their conclusions on the uncertainty in R rather than on the probability that E1A's value of R (0.29) was compatible with pre-Weinberg-Salam physics. Indeed, both referees recommended against publication of the paper until their objections were satisfied. However, Rubbia was out of the country, Sulak was at Harvard, and Cline and Mann were preoccupied with the revised experiment. The referees' comments as expressed in the report were therefore not answered until several months later.

The referees were not the only ones with doubts about the FNAL procedure. From Europe, Bernard Aubert, who had worked with the Gargamelle group until August and then transferred to the FNAL group, reported that he was spending his time defending the FNAL experiment to the neutrino physicists there. According to Aubert (1973), the FNAL's "lack of credibility . . . comes mainly from the fact that [the European physicists] do not know how well we measure E_μ and E_h and [they] believe that we guess more than measure the $[E_\mu/E_h]$ uncertainty."

By mid-November, Mann and Cline were convinced that the newer results definitely failed to give evidence of neutral currents; Rubbia concurred. Mann then drafted a letter to this effect for the *Physical Review Letters*, which was intended to replace the earlier paper that had been allowed to sit at the offices of *Physical Review Letters* pending the outcome of the revised experiment. Though the "No Neutral Currents" article was never actually submitted, it represented a good summary of the state of opinion at that time. (See Fig. 21.)[9] The abstract read in part as follows:

The ratio of muonless events to events with muons is observed to be 0.05 ± 0.05 for the specific case of an enriched antineutrino beam. This appears to be in disagreement with recent observations made at CERN and with the predictions of the Weinberg model.

There was some division in the FNAL group over the question of how and when the new results should be released. Mann (1980) felt the group should wait before discussing them. Rubbia and Cline at different times discussed the current situation with people outside E1A. When Rubbia went back to CERN in December 1973, he spoke with a variety of people, including Musset, Lagarrigue, A. Rousset, Jentschke, and others (Musset, 1980; Rousset, 1980). By this time, the Gargamelle group had, of course, already published their result that neutral currents did exist; naturally, they were somewhat distressed.

Jentschke, then Director General of CERN, convoked a meeting of the Gargamelle group to cross-examine them on the experiment; he was afraid that CERN would be publicly embarrassed by the forthcoming American announcement. The Gargamelle group, however, would not back down. Still, they were shaken (Rousset, 1980). Musset circulated a memorandum advising the groups to deemphasize the Weinberg theory and to redouble effort on the study of the associated events. The memorandum began:

Dear Friends,

After our last neutral-current meeting, all of you have probably heard rumours about new results in the WB beam at Batavia with a slightly changed apparatus (muon counter after 1 foot of iron) and a focusing horn for an $\bar{\nu}$ run. The efficiency for μ detection is better than previously and the result is an apparent lack of neutral-current—type events.

In the near future, we can expect to be heavily questioned about the reliability of our experiment.

---

[9]Typescript draft entitled, "Search for Neutrino Induced Events without a Muon in the Final State." The manuscript is undated, but is referred to in a letter dated 13 November (discussed below), and was therefore probably written during the second week of November 1973.

Search for Neutrino Induced Events

Without a Muon in the Final State*

B. Aubert, A. Benvenuti, D. Cline, W. T. Ford, R. Imlay

T. Y. Ling A. K. Mann, F. Messing, R. L. Piccioni,

J. Pilcher*, D. D. Reeder, C. Rubbia andStefanski and L. Sulak

Department of Physics
Harvard University
Cambridge, Massachusetts 02138

Department of Physics
University of Pennsylvania
Philadelphia, Pennsylvania 19104

Department of Physics
University of Wisconsin
Madison, Wisconsin 53706

National Accelerator Laboratory
Batavia, Illinois 60510

Abstract

A comprehensive search for neutrino induced muonless events has been carried out using a liquid scintillator calorimeter - magnetic spectrometer exposed to various neutrino beams produced at the National Accelerator Laboratory. The ratio of muonless events to events with muons is observed to be 0.05 ± 0.05 for the specific case of an enriched antineutrino beam. This appears to be in disagreement with recent observations made at CERN and with the predictions of the Weinberg model.

FIG. 21. Draft of letter to *Physical Review Letters* asserting that E1A showed no evidence for neutral currents at the Weinberg-Salam level. This paper was never published.

Independently from these new rumours, it is much more important to know if neutral-current—type events can be simulated by a trivial background such as neutrino-induced neutrons, than to measure accurately a $\sin^2\theta_W$ (Musset and Vialle, 1973).

At the National Accelerator Laboratory, not only Imlay, but Aubert, Ling, and Sulak were working on the punchthrough problem nearly full time. Preliminary results indicated that the punchthrough was higher than at first thought (Imlay, 1973a). The estimates would rise still higher, but not for several weeks.

When the "No Neutral Currents" paper was completed

in draft form, Mann composed a letter[10] to the CERN group informing them of the result. Mann, Cline, and Rubbia signed it. But before sending it, the authors (Mann, Cline, Rubbia, and Reeder) consulted with Robert

---

[10]Letter from D. Cline, A. K. Mann, D. D. Reeder, and C. Rubbia to A. Lagarrigue, 13 November 1973, signed by Cline, Mann, and Rubbia. Signed version in Mann's files, unsigned copy in Lagarrigue's scientific papers at Orsay. I would like to thank Mme. Lagarrigue and Professor Morellet for permission to see these papers.

November 13, 1973

Professor A. Lagarrigue, Director
Linear Accelerator Laboratory
University of Paris - SUD
Centre D'Orsay
Batiment 200
91405 Orsay
France

Dear Professor Lagarrigue:

We write to inform you of the preliminary result of our recent ex-
periment to search for neutrino interactions without final state muons.
As you know, our apparatus was modified to provide a much larger detection
efficiency for muons relative to the apparatus that was used in our earlier
search for muonless events. We also improved our ability to locate accu-
rately vertices of observed neutrino interactions, and lowered the
threshold on the total energy of the hadrons in the final state.

From about one half of the data obtained in our recent run, we find
the raw ratio R_{raw} = 0.18 ± 0.03. We estimate the muon detection
efficiency of the apparatus for the enriched antineutrino beam that was
used in this experiment to be approximately 0.85. Taking into account
small backgrounds produced by incident neutrons and by ν_e in the incident
beam, the corrected ratio is R_{corr} = $0.02 ^{+ 0.05}_{- 0.03}$, where the error includes
an estimate of the uncertainty in the calculated detection efficiency. We
are continuing to process the remainder of the data and to improve our
understanding of the experiment.

We have written a paper intended for Physical Review Letters which
will soon be submitted. A copy will, of course, be sent to you but for
obvious reasons we wanted to convey our result informally to you before
its publication.

With kindest regards

                              Yours sincerely,

                              D. Cline

                              A. K. Mann

                              D. D. Reeder

AKM/rs   C. Rubbia

FIG. 22. Letter from Cline, Mann, Reeder and Rubbia to Lagarrigue. The letter was never sent, but an unsigned duplicate was
brought by Rubbia to Lagarrigue and was subsequently seen by many members of the CERN collaboration. See footnote 10.

Wilson, then Director of Fermilab. Wilson advised them
to wait somewhat longer until the experiment was com-
plete before announcing the result. Though the letter was,
as a result, never sent, Rubbia brought an unsigned copy
to its intended recipient, Lagarrigue. From his office it
was duplicated and several members of the Gargamelle
collaboration were familiar with it. (See Fig. 22.) The au-
thors concluded in their letter that the corrected ratio of
muonless to charged-current events was

$$R = 0.02^{+0.05}_{-0.03} ,$$

which, like the 0.05±0.05, is *statistically indistinguishable
from zero*. The trade of punchthrough for a better angu-
lar acceptance had exacted a higher price than they knew.

Until early November only the simplest attempt had
been made to measure punchthrough. A pencilled com-

ment by Cline in the margin of the "No Neutral Currents" draft said, "R. I. Imlay, do this [punch-through] calculation." Adding to the uncertainty was the fact that different approaches to measuring the punchthrough probability at first yielded different results. For instance, Sulak (1973) measured the ratio of the number of events where many sparks appeared in the first spark chamber after the thin iron plate, to the number of events where only one spark was found there. Nominally, such a ratio should approximate the percentage of hadrons penetrating through the iron plate along with the muons. One problem with this method was that individual sparks often did not show up very well; another was that the two stereo cameras gave divergent results:

Multiple Sparks/Single Sparks	
x view	y view
15%	30%

Another approach Sulak mentioned was to plot the number of muonless events as a function of longitudinal distance along the machine. This plot indicated a rapid decrease, while the number of charged plus muonless events remained constant. (The total number presumably was the total number of neutrino events which *should* remain constant throughout the detector.)

Here too the interpretation remained ambiguous. On the one hand, one could say that there were few muonless events downstream because pions were punching through. On the other hand, one could say there were more muonless events upstream only because the wide-angle muons were escaping. Sulak left "the conclusions to the reader!" Since he remained an advocate of the original Monte Carlo method, he hoped to convince the others that the punchthrough was causing the rapid decrease.

Ford and Mann (1973) interpreted the data differently because their main concern was the elimination of the wide-angle muon losses. By extrapolating the ratio of R to the last segments of the calorimeter they hoped to obtain a value of NC/CC independent of muon losses. Instead, they inadvertently were choosing events which by proximity to the muon detector were most likely to engender punchthrough hadrons. With hindsight it is understandable that they found:

$$R(\text{corrected}) = 0.057 \pm 0.053 ,$$

in good accord with the value of R they had found in the "No Muonless Events" letter to *Physical Review Letters*.

Summing up these various punchthrough studies and a further one by Imlay (1973b), based on pion penetration measured by earlier experiments, Cline (1973d) gave a talk at NAL on E1A's latest results. When the values for the geometric efficiency (ε) and punchthrough (ε_p) were inserted, Cline arrived at a corrected R between 0.05 and 0.15 which was below both the CERN data and the Weinberg prediction. Cline concluded (1973d) (see Fig. 23):

(1) R' very likely too small to be consistent with Weinberg model and lower bounds deduced by Paschos and Wolfenstein for this model—also CERN data, if due to Weinberg model—energy dependence is still a loophole.

This last remark referred to the possibility then being entertained by some members of the collaboration that the discrepancy between CERN and FNAL might be attributed to the difference in their neutrino energies. (In retrospect, we know this not to be the case.) The conclusion continued:

(2) $R' = 0.29 \pm 0.09$ suggested by first E1A experiment is not confirmed in the present experiment—uncertainty in the (x,y) vertex reconstruction in that experiment was perhaps the trouble—there are still loopholes however!

With the addition of a new camera, the stereo photographs now yielded a more accurate location of the vertex; this was not, as it turned out, of great importance, but at the time was thought to be a possible explanation for the old results.

It is important to remember that throughout this time in September, October, November, and December, the group was under a great deal of pressure to announce their findings. This pressure came not just from the other experimentalists, but also from the theorists who were getting informal progress reports from various participants. As Mann (1980) later recounted:

> As the results began to emerge, we were being pressed harder and harder for some kind of decisive answer from people. It is very hard to communicate to you how [things were], when you are in the center of the stage at a time like that, particularly in high-energy physics where you do not quite have control over your own destiny. You have to work with collaborators, with the lab, with the director, with the program committee, and with all the people who do the chores that allow the experiment to be done. You're being leaned on over and over again to produce, whether you're ready to produce or not.

During this period, each of the participants was struggling to integrate the various calculations and measurements; each had to convince himself of the reality or artificiality of the effect. Every measurement and calculation had its own weaknesses and strengths, best known by the individual or subgroup involved. As a result both of pressure from outside the collaboration and of new evidence from within the group, opinions were changing. On 13 December, Cline (1973c) distributed a memorandum with a new tone:

> Three pieces of evidence now in hand point to the distinct possibility that a μless signal of order 10% is showing up in the data. At present I don't see how to make these effects go away.

The three pieces of evidence were, first, the Monte Carlo model now yielded an $R = 0.1$; second, the spatial distribution of the events looked as if it had been caused by true neutrino events. But what I take to be the most convincing for Cline was the third reason he offered: among twenty neutral-current candidates, five "had no hint of wide-angle tracks."

TENTATIVE Conclusions

1. R' Very likely too small to be consistent with Weinberg model and lower bounds deduced by Paschos and Wolfenstein for this model — also CERN data, if due to Weinberg model — Energy dependence is still a loophole

$R' \sim 0.28$ CERN at $E_\nu \sim \frac{1}{2}$ GeV

2. R' = 0.29 ± 0.09 Suggested by first E1A experiment is ~~not~~ not confirmed in the present experiment — uncertainty in the (x,y) vertex reconstruction in that experiment was perhaps the trouble — there are still loopholes however!

3. There are events in the present experiment that don't appear to have a visible μ track — could be due to spark chamber ineff. — but seems unlikely — needs further study

4. Some μ less events appear to have showers that are suggestive of predominate electromagnetic effects — perhaps due to ν_e's

FIG. 23. Concluding transparency from Cline's talk at FNAL 6 December 1973. From Cline, 1973d.

These events were in the center of the detector, and the μ angles would have to be at least 200—300 mr and with the result that the μ track will be *well separated from the rest of the shower*. The separation should help increase the sparking efficiency. It seems unlikely that the chamber efficiency goes to 25% for such modest angles .... This is certainly consistent with a true μless signal of R'... =0.08.

This was the kind of argument Cline liked: a small selec-tion of events, clean of possible edge effects, and with an analysis that did not require resorting to Monte Carlo techniques.

About the same time, Mann concurred: the signal would not go away (Mann, 1980). Over the course of December and January 1974, Mann examined the data and photographs again, applying various selection criteria to be sure no simple error would account for most of the muonless events. Just as in the CERN meetings, the

events were scanned and rescanned, energy was remeasured, fiducial regions were redefined, pictures were rechecked for through muons, and so on. In a final, internal report of 26 January, Mann (1974) argued his new position:

> ... it appears that our scanning criteria and fiducial region cuts, (x,y) 120 and 5 Z 12, do in fact elmininate most questionable events. Of 13 "N" [neutral current candidate] events in runs 328 to 332 in the final sample, 8 of them are "good to look at," as the attached reproductions indicate.

In addition, a new Monte Carlo calculation was almost ready. Five days after Cline's "ten percent" memorandum, Aubert, Ling, and Imlay (1973) completed a detailed and rigorous study of the punchthrough, which could then be used to generate an accurate assessment of the background. When this was done, the NC/CC ratio appeared to be as high as 12–15 %. The second version of the 1A experiment thus neared completion, and after several meetings during January and February, it was decided to publish the original Harvard paper with the comment that additional work had confirmed the earlier findings.

By the end of February, the 1A group had essentially finished two separate experiments. Not only was the beam changed, the geometry shifted, the spark chambers replaced, the background different, but the participants in the two experiments were not all the same. The style of experimentation of the two subgroups was different, their expectations were not the same, and the evidence that finally convined them the neutral currents were real was different.

In mid-March of 1974, the collaboration finished a paper on the revised experiment entitled "Further Observation of Muonless Neutrino-Induced Inelastic Interactions," which they sent to *Physical Review Letters* (Aubert *et al.*, 1974). By then the new evidence was presented in its most convincing form, concisely summarized in the following nine figures. (See Figs. 24 and 25.) Most of the symbols used in the figure captions have been defined earlier. The others are $\overline{A}EBC$=anticoincidence of counter A with energy trigger and coincidence of counters B and C; SC4=spark chamber 4, which is now serving as the first muon detector; E_μ=geometric efficiency of muon detector; R=ratio NC/CC; (on diagrams of Fig. 25 below) μ_1=muon detection by SC4 and counter B; μ_1'=muon detection by SC4 alone; μ_2=muon detection by SC5 (the first muon detector of the old experiment).

In the future, it will undoubtedly be for these and similar diagrams that the work of E1A will be remembered. Indeed, with this paper, the first chapter of the discovery of weak neutral currents drew to an end. Further experiments were performed at many laboratories all over the world to determine the space-time and isotopic spin structure of the currents, but the existence of the currents themselves seemed to be assured. Twice over, the FNAL collaboration had had to struggle through the slow, frustrating task of separating artifact from reality.

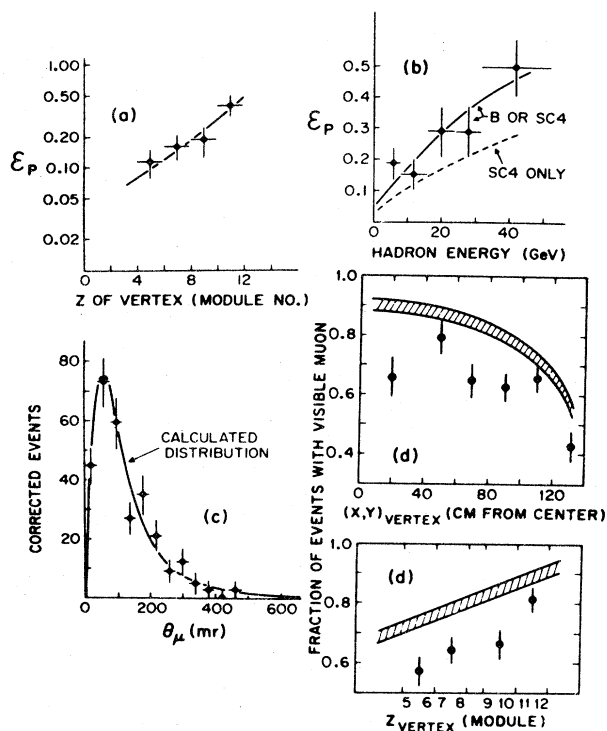Within a few weeks of the FNAL group's first publica-

FIG. 24. Published evidence for neutral currents from E1A's second neutral-current publication, Aubert *et al.*, 1974a. (a) The measured punchthrough probability of hadrons accompanying $\overline{A}EBC$ events (for all hadron energies) as a function of z, and the expected shape of the distribution. (b) The measured punchthrough probability (for z between 5 and 12) as a function of E_H, compared with the expected variation. (c) The corrected muon angular distribution measured in SC4 compared with the predicted distribution. (d) Comparison of the observed fraction of events with a muon for the μ_1' identifier (SC4 alone) and ε_μ as functions of transverse position and z position. The cross-hatching indicates the uncertainty in ε_μ arising from the statistics of the data in (c).

tion in April 1974, a conference was held in Philadelphia (26–28 April) on the topic of neutrino physics. Naturally neutral currents figured prominently in the discussions. One participant reflected the general trend of thought in his opening remarks: "The existence of a hadronic neutral current in high-energy neutrino experiments is, by this time, reasonably well established by four 'independent' experiments. Clearly our next major goal is to determine the symmetry properties" (Sakurai, 1974, p. 57). This was also R. P. Feynman's opinion in his summary talk. He argued that the successful prediction of neutral currents was a credit to the Weinberg-Salam theory. "But," he added, "I should like to follow the advice of Mr. Mann. Neutral currents should be studied in their own right. That means the experimentalists should say, all right, we have neutral currents, let's find out what their properties are. (Rather than just comparing them to the theory of Salam and Weinberg)" (Feynman, 1974, p. 299).
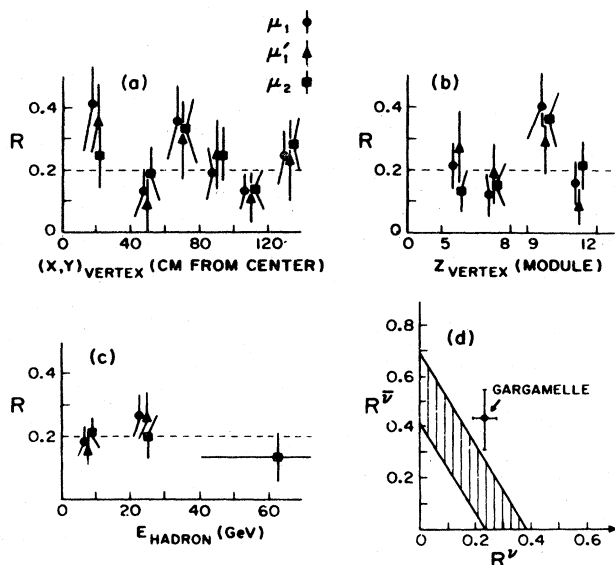
FIG. 25. Additional evidence for neutral currents from E1A's second neutral-current publication, Aubert *et al.*, 1974a. (a) R obtained from three different muon identifiers as a function of the transverse distance from the center of the calorimeter. (b) The z variation of R obtained using three different muon identifiers. (c) The E_H variaion of R from three muon identifiers. (d) The allowed region of R^ν and $R^{\bar\nu}$ from this experiment compared with R^ν and $R^{\bar\nu}$ obtained by the Gargamelle collaboration.

In June of 1974 the case for neutral currents was strengthened yet further when the London conference on high-energy physics convened. B. C. Barish reported on the CalTech FNAL neutrino experiment: "It is concluded. . .that neutral-current phenomena indeed exist in interaction of both neutrinos and antineutrinos" (Barish, 1974, p. IV-113). Other preliminary, but positive results came from the Argonne-Concordia-Purdue 12-foot bubble-chamber experiment (Schreiner, 1974) and the Columbia-Illinois-Rockefeller-Brookhaven collaboration (Lee, 1974). In the early days of the HWPF collaboration's trouble with punchthrough, the group was bemusedly said to have discovered "alternating neutral currents." By the spring of 1974 the physics community's consensus was that neutral currents would alternate no longer.

VI. CONCLUSION: THE END OF EXPERIMENTS

Experiment 1A and Gargamelle evolved from searches for the intermediate vector boson to studies of the parton model, and only gradually to an investigation of weak neutral currents. There are similarities, as well, between the two groups as each sifted through the evidence for and against neutral currents. Within the broad framework of the participants' interest in weak-interaction physics and neutral currents, each collaboration fell into

two subgroups. One group within each experiment came to the experiment with the experience of having looked for and not found neutral currents. On both sides of the Atlantic this experience involved establishing extremely low upper bounds for the strangeness-changing decays; in Europe, it also included setting some (at first incorrectly low) limits on strangeness-conserving processes. Furthermore, at the time no one had a strong theoretical reason to expect different results in strangeness-changing and strangeness-conserving events, even though the future explanation of the difference (in terms of charm) had appeared in print. In Gargamelle, there was the additional factor that all those having participated in the earlier CERN bubble-chamber work knew very well how difficult it was to extract a signal from the neutral background.

Similarly, in both E1A and Gargamelle, there was another group that, from 1971 onwards, put priority on the hadronic neutral-current search. In E1A, part of this interest came from Weinberg, who encouraged some of the participants to look for the hadronic neutral currents at the 20% level. For the Gargamelle collaboration, Zumino, Paschos, Wolfenstein, Prentki, and Gaillard played the analogous role.

By thus persuading some members of the two collaborations that a search at the 20% level was worthwhile—in fact, urgent from the theorists' perspective—the Weinberg-Salam theory exerted the first stage of its influence. The immediate result at E1A was the installation of a trigger that would fire when a certain amount of hadron energy was deposited, even if no muon emerged, thus providing at least the possibility of a neutral-weak-current search. At CERN, the main consequence was the establishment of an informal group of physicists (later a formal subgroup of the collaboration) to scan, measure, and select the hadronic neutral-current events.

In a certain limited sense, the neutral currents were "there" from the start: both FNAL and CERN had photographs they would eventually present as evidence for weak neutral currents. The real work of the experiments, however, was for the collaborators to convince themselves that the photographs were significant and not an artifact induced by the apparatus or environment. What followed was almost a year and a half of a seemingly endless list of internal debates over the tracks and sparks, the acceptance, the efficiency, the neutral background, the muon spectrum, the neutrino flux, the beam purity, the through muons, the fiducial volumes, the cosmic rays, the neutral kaons, and the statistical significance of the results. Many of these subexperiments required a commitment of weeks, sometimes months; each helped to expand the circle of participants convinced that the effect existed above the background. On the whole, these subexperiments took place in the domain of established physics, meaning within established experimental techniques and theoretical ideas. Delineating the background thus formed the second interaction of theory and experiment.

These two conceptual moments, the design of the ex-

periment (both of goals and apparatus) and the decision of when to *stop* the experiment both need to be studied to understand the history of an experiment. Traditionally, textbook and even most historical accounts have left out the latter, limiting themselves to a description of what the experiment was intended to determine, and then discussing the results eventually found. In doing so, they leave out the flesh and blood of the experiment.

One might call this second stage of experiment-theory interaction a process of reinforcement. In the experiments studied here, this process of gathering support for the contentious thesis that neutral currents existed at high levels took place in several different ways. During the original E1A experiment, with the analysis based at Harvard, the reinforcement was accomplished primarily by a combination of variation of the fiducial volume to show that the percentage of neutral currents remained relatively constant and by comparing the experimental number of neutral-current events to the number predicted by the Monte Carlo model. When the experiment was redone by the Wisconsin group, the reinforcement process focused more on the punchthrough and other machine characteristics than on the Monte Carlo results. In Cline's work, especially, one sees the reflection of a style of work developed earlier in the bubble-chamber study of rare events. During the early stages of the revised experiment Cline searched for "golden events," while Mann sought events that were "good to look at." The presence or absence of these events would determine whether or not they could support the neutral-current hypothesis. In the Gargamelle collaboration there was also a group that was looking for a "Bilderbuch" example that would stand on its own as evidence for neutral currents. By contrast, at least in the early months of the experiment, the hadron groups struggled to buttress the evidence principally through statistical analyses of the neutron background. Even among the hadron group's participants, different types of evidence were given differing weight. For example, some participants were persuaded by the relative number and spatial distribution of associated events; some other collaborators were persuaded by the thermodynamic analysis, yet others by the Monte Carlo simulations. Still others remained skeptical until the problems of the neutron cascade and kaon regeneration were fully understood.

Only gradually were the various individual arguments transformed into the kind of evidence finally assembled for publication. Little by little, the conclusion was reinforced by the many studies necessary to assess the background. Certainly no one moment can be pointed to either in E1A or in Gargamelle that could be called the instant of discovery.

Finally, I should like to suggest that it is by studying how the process of reinforcement has changed over time that we can understand the nature of the change in experiment since the turn of the century. Before the time of high-energy physics, experiments were conducted primarily either by one or two physicists. The change in the scale of the apparatus has necessitated much larger groups. This has had several effects. First, reinforcement is ac-

complished by small subgroups working on specific problems instead of one or two people conducting variations on the main experiment. Second, since the apparatus itself is very likely to have no close duplicate, many of its properties have to be understood as part of the experiment. In earlier, smaller-scale experiments, such investigations would have been conducted as separate experiments, as was possible with some of the more standardized equipment of cloud-chamber experiments in the mid-thirties.

Consequently, in many ways the large experiment now subsumes into its own internal dynamics the processes which previously look place in the scientific community as a whole. This is visible not only in the large-scale repetition of E1A within the same experiment, but also in the multiplicity of subgroups working separately and partly independently to determine the punchthrough in E1A or the neutron and inverse beta-decay backgrounds in Gargamelle. It is visible too in the role of the internal publication of reports and memoranda.

If I could finish with one suggestion: In the history of experiment, past and contemporary, we must focus attention on the process by which the experiment was ended as well as by how it began. For the decision that an effect is real brings together the social dynamics, the theoretical assumptions, the experimental technique, and the individual styles of research. When looked at in this way, contemporary experiments suggest that the processes of discovery and justification lose much of their distinct identities.

We need a richer descriptive vocabulary to describe experimentation in a way that will account for the many intermediate steps between the often very subjective working hypotheses of various participants and the logically or empirically based argument that eventually finds its way to publication. Such a vocabulary would be able to depict the degrees of persuasive force that evidence has as it begins to accumulate from diverse considerations. In the process of developing an account like this, we shall come to understand how data is gradually transformed (as in the case of the first muonless-event photograph) from a collection of curiosities to the foundation of a compelling demonstration.

ACKNOWLEDGMENTS

REFERENCES

Abers, E. S., and B. W. Lee, 1973, Phys. Rep. 9, 1.

Allard, J. F., B. Aubert, C. Baglin, L. Behr, P. Beilliere, A. Bezaguet, V. Brisson, L. Carlino, L. M. Chounet, R. Florent, R. Hecart, J. Hennesy, F. Jacquet, L. Jauneau, A. Lagarrigue, G. Lemesle, A. Lloret, J. P. Lowys, D. Morelet, P. Musset, V. Nguyen-Khac, M. Ohayon, A. Orkin-Lecourtois, C. Pascaud, P. Petiau, P. Queru, P. Rançon, M. Reposeur, A. Rousset, J. Six, and J. J. Veillet, 1964, Bubble-Chamber Proposal, Ecole Polytechnique, Summer 1964.

Anonymous, 1973, referee reports for Phys. Rev. Lett., October.

Aubert, B., 1970, letter to D. H. Perkins, 27 February.

Aubert, B., 1973, letter to Neutrino Collaboration at NAL, 16 October.

Aubert, B., et al., 1970, amended draft to the Gargamelle Proposal, 25 February.

Aubert, B., et al., 1974a (see footnote 1), Phys. Rev. Lett. 32, 1454.

Aubert, B., A. Benvenuti, D. Cline, W. T. Ford, R. Imlay, J. Y. Ling, A. K. Mann, F. Messing, J. Pilcher, D. D. Reeder, C. Rubbia, R. Stefanski, and L. Sulak, 1974b, Phys. Rev. Lett. 32 1457.

Aubert, B., F. Ling, and R. Imlay, 1973, technical memorandum, 18 December.

Baltay, C., U. Camerini, W. Fry, P. Musset, B. Osculati, and A. Pullia, 1972a CERN-TCL, technical memorandum, 14 July.

Baltay, C., U. Camerini, W. Fry, P. Musset, B. Osculati, and A. Pullia, 1972b, CERN-TCL, technical memorandum, 14 July.

Baltay, C., 1979, in Proceedings of the 19th International Conference on High Energy Physics, Tokyo, August 23—30, 1978, edited by S. Homma, M. Kawaguchi, and H. Miyazawa (Physical Society of Japan, Tokyo), p. 882.

Barish, B. C., 1974, in Proceedings of the XVII International Conference on High Energy Physics, London, July, 1974, edited by J. R. Smith (Science Research Council, Rutherford Laboratory, Chilton), p. IV-III.

Beier, E. W., D. Cline, A. K. Mann, J. Pilcher, D. D. Reeder, and C. Rubbia, 1970, draft HWPF proposal.

Beier, E., D. A. Buchholz, A. K. Mann, and S. H. Parker, 1972, Phys. Rev. Lett. 29, 678.

Bell, J. S., J. Løvseth, and M. Veltman, 1963, in Proceedings of the Sienna International Conference on Elementary Particles 30 September—5 October, 1963, edited by G. Bernardini and G. Puppi (Società Italiana di Fisica, Bologna), Vol. 1, p. 584.

Benvenuti, A., D. Cline, R. L. Imlay, W. Ford, A. K. Mann, J. E. Pilcher, D. D. Reeder, C. Rubbia, and L. R. Sulak, 1972, letter to R. R. Wilson, 14 March.

Benvenuti, A., D. Cheng, D. Cline, W. T. Ford, R. Imlay, T. Y. Ling, A. K. Mann, F. Messing, J. Pilcher, D. D. Reeders, C. Rubbia, and L. Sulak, 1973, Phys. Rev. Lett. 30, 1084.

Benvenuti, A., et al., 1974a, 1974b (see footnote 1), Phys. Rev. Lett. 32, 800.

Bernardini, G., 1966 in Proceedings of the International School of Physics "Enrico Fermi" Course XXXII (Academic, New York), p. 1.

Bingham, H. H.. et al., in Proceedings of the Sienna International Conference on Elemenatry Particles 30 September—5 October, 1963, edited by G. Bernardini and G. Puppi (Società Italian di Fisica, Bologna), Vol. 1, p. 555.

Bjorken, J. D., and E. A. Paschos, 1969, Phys. Rev. 185, 1975.

Block, M. M., H. Burmeister, D. C. Cundy, B. Eiben, C. Franzinetti, J. Keren, R. Møllerud, G. Myatt, M. Nikolic, A. Orkin-Lecourtois, M. Paty, D. H. Perkins, C. A. Ramm, K. Schultze, H. Sletten, K. Soop, R. Stump, W. Venus, and H.

Yoshiki, 1964, Phys. Lett. 12, 281.

Breidenbach, M., J. I. Friedman, H. W. Kendall, E. D. Bloom, D. H. Coward, H. DeStaebler, J. Drees, L. W. Mo, and R. E. Taylor, 1969, Phys. Rev. Lett. 23, 935.

Camerini, U., D. Cline, W. F. Fry, and W. M. Powell, 1964, Phys. Rev. Lett. 13, 318.

Camerini, U., D. Ljung, M. Sheaff, and D. Cline, 1969, Phys. Rev. Lett. 23, 326.

Christenson, J. H., J. W. Cronin, V. L. Fitch, and R. Turlay, 1964, Phys. Rev. Lett. 13, 138.

Cline, D., 1967, "Experimental Search for Weak Neutral Currents." Preprinted from the school held in Heceg Novi, Yugoslavia.

Cline, D., 1973a, Wisconsin, technical memorandum, 1 October.

Cline, D., 1973b, Wisconsin, technical memorandum, 11 October.

Cline, D., 1973c, Wisconsin, technical memorandum, 16 October.

Cline, D., 1973d, Wisconsin, technical memorandum, 13 December with xeroxed transparencies of 6 December FNAL talk.

Cline, D., 1979, Preface to Unification of Elementary Forces and Gauge Theories: Proceedings Ben Lee Memorial International Conference on Parity Nonconservation, Weak Neutral Currents, and Gauge Theories, FNAL Oct. 20—22, 1977, edited by D. B. Cline and F. E. Mills (Harwood Academic, London), p. ix.

Cline, D., 1981, interview, 14 January.

Cline, D., and W. F. Fry, 1977, Annu. Rev. Nucl. Sci. 27, 209.

Cline, D., and A. K. Mann, 1969, "Preliminary Version of a Proposal for Neutrino Scattering Experiments at NAL," draft NAL proposal, 18 December.

Cline, D., A. K. Mann, and C. Rubbia, 1970, Phys. Rev. Lett. 25, 1309.

Commins, E. D., 1973, Weak Interactions (McGraw-Hill, New York).

Cronin, J. W., 1981, Rev. Mod. Phys. 53, 373.

Cundy, D. C., 1964, in XII International Conference on High Energy Physics, Dubna, 5—15 August, 1964 (Atomizdat, Moscow), p. 7.

Cundy, D. C., 1973, in CERN-TCL, minutes 21 March, 26 March.

Cundy, D. C., 1974, in Proceedings of the XVII International Conference on High Energy Physics, London, July, 1974, edited by J. R. Smith (Science Research Council, Rutherford Laboratory, Chilton), p. IV-131.

Cundy, D. C., 1980, interview, 27 November.

Cundy, D. C., and C. Baltay, 1972, CERN-TCL, technical memorandum, 11 July.

Cundy, D. C., G. Myatt, F. A. Nezrick, J. B. M. Pattison, D. H. Perkins, C. A. Ramm, W. Venus, and H. W. Wachsmuth, 1970, Phys. Lett. B 31, 478.

Danby, G., J-M. Gaillard, K. Goulianos, L. M. Lederman, N. Mistry, M. Schwartz, and J. Steinberger, 1962, Phys. Rev. Lett. 9, 36.

Faissner, H., 1973a, letter to A. Lagarrigue, 11 January.

Faissner, H., 1973b, letter to W. Jentschke, 9 February.

Faissner, H., 1979, in New Phemonea in Lepton-Hadron Physics, edited by D. Fries and J. Wess (Plenum, New York).

Faissner, H., 1981, letter to author, 7 December.

Feinberg, G., 1964, in Brandeis Summer Institute in Theoretical Physics. Lectures on Astrophysics and Weak Interactions, (Colony Offset, Boston), p. 278.

Fermi, E., 1934, Z. Phys. 88, 161.

Feynman, R., 1969, in High Energy Collisions, Third International Conference, State University of New York, Stony Brook,

September 5 and 6, 1969, edited by C. N. Yang, J. A. Cole, M. Good, R. Hwa, and J. Lee-Franzini (Gordon and Breach, New York), p. 237.

Feynman, R. P., 1974, in *Neutrinos-1974 (Philadelphia)* AIP Conference Proceedings No. 22, Particles and Fields Subseries No. 9, edited by C. Baltay (American Institute of Physics, New York), p. 299.

Fiorini, E., 1973, letter to Musset, 11 July.

Fitch, V. L., 1981, Rev. Mod. Phys. **53**, 367.

Ford, W. T., and A. K. Mann, 1973, Pennsylvania, technical memorandum, 28 November.

Frauenfelder, H., and E. Henley, 1974, *Subatomic Physics* (Prentice Hall, Englewood Cliffs).

Fry, W., and D. Haidt, 1973, CERN-TCL, technical memorandum, 22 May.

Fry, W. F., and D. Haidt, 1975, CERN yellow report 75-1.

Glashow, S. L., 1980, Rev. Mod. Phys. **52**, 539.

Glashow, S. L., and H. Georgi, 1972, Phys. Rev. Lett. **28**, 1494.

Glashow S. L., J. Illiopoulos, and L. Maiani, 1970, Phys. Rev. D **2**, 1285.

Gurr, H. S., F. Reines, and H. W. Sobel, 1972, Phys. Rev. Lett. **28**, 1406.

Hasert, F. J., *et al.,* 1973a, Phys. Lett. B **46**, 121.

Hasert, F. J., *et al.,* 1973b (see footnote 1) Phys. Lett. B **46**, 138.

Hasert, F. J., *et al.,* 1974 (see footnote 1) Nucl. Phys. B **73**, 1.

Imlay, R., 1973a, Wisconsin, technical memorandum, 29 November.

Imlay, R., 1973b, Wisconsin, technical memorandum, 29 November.

Kabir, P. K., 1963, in *The Development of Weak Interaction Theory,* edited by P. K. Kabir (Gordon and Breach, New York).

Kim, J. E., P. Langacker, M. Levine, and H. H. Williams, 1981, Rev. Mod. Phys. **53**, 211.

Lagarrigue, A., 1972, letter to W. Jentschke, 12 April.

Lagarrigue, A., 1973a, letter to H. Faissner, 16 January.

Lagarrigue, A., 1973b, letter to C. Rubbia, 19 July.

Lagarrigue, A., P. Musset, and A. Rousset, 1964, "Projet de Chambre á Liquides Lourds de 17m3," Draft Project Proposal for Bubble Chamber, Ecole Polytechnique, February.

Lee, B. W., 1972a, Phys. Lett. B **40**, 420.

Lee, B. W., 1972b, in *XVI International Conference on High Energy Physics* (NAL, Batavia) Vol. 4, p. 249.

Lee, W., 1972, Phys. Lett. B **40**, 423.

Lee, W., 1974, in *Proceedings of the XVII International Conference on High Energy Physics, London, July 1974,* edited by J. R. Smith (Science Research Council, Rutherford Laboratory, Chilton), p. IV-127.

Lee, T. D., and C. N. Yang, 1960a, Phys. Rev. Lett. **4**, 307.

Lee, T. D., and C. N. Yang, 1960b, Phys. Rev. **119**, 1410.

Mann, A. K., 1969, *1969 Summer Study, Volume 4 Particle Beams* (NAL, Batavia), p. 201.

Mann, A. K., 1974, Pennsylvania, technical memorandum, with spark-chamber photographs, 26 January.

Mann, A. K., 1977, *Unification of Elementary Forces and Gauge Theories* (Harwood Academic, London), p. 20.

Mann, A. K., 1980, interview, 29 September.

Marshak, R. E., Riazuddin, and C. P. Ryan, 1969, *Theory of Weak Interactions in Particle Physics* (Wiley-Interscience, New York).

Musset, P., 1972, transparencies from presentation to American Physical Society Meeting, New York, January 1973.

Musset, P., 1973a, CERN-TCL, technical memorandum, 19 March.

Musset, P., 1973b, CERN-TCL, technical memorandum, 26

Musset, P., 1973c, CERN-TCL, technical memorandum, 17 April.

Musset, P., 1973d, in *2nd International Conference on Elementary Particles, Aix-en Provence 6—12 September,* J. Phys. (Paris) **34**, Supplement to Number 10, Colloque C1, p. C1-45.

Musset, P., 1980, interview, 26 November.

Musset, P., 1982, letter to author, 2 April.

Musset, P., and J. P. Vialle, 1973, CERN-TCL, technical memorandum, 20 November.

Myatt, G., 1974, in *Proceedings of the 6th International Symposium on Electron and Photon Interactions at High Energies, Physikalisches Institut, University of Bonn, August 27—31, 1973,* edited by H. Rollnik and W. Pfeil (North-Holland, Amsterdam), p. 389.

Pais, A., and S. B. Treiman, 1972, Phys. Rev. D **6**, 2700.

Paschos, E. A., and L. Wolfenstein, 1973, Phys. Rev. D **7**, 91.

Perkins, D. H., 1970, "Draft Gargamelle Proposal," 24 February.

Perkins, D. H., 1972a, in *Proceedings of the XVI International Conference on High Energy Physics, 6 September—13 September, Volume 4* (NAL, Batavia), p. 189.

Perkins, D. H., 1972b, technical memorandum, addressed to Gargamelle collaboration, undated but before 28 April.

Perkins, D. H., 1972c, letter to C. Baltay, 28 April (copies to Gargamelle collaboration).

Pullia, A., 1972, in *Neutrino '72, Europhysics Conference, Balatonfüred, Hungary, 11—17 June, 1972,* edited by A. Frenkel and G. Marx (Omkdk-Technioinform, Budapest), Vol. 1, p. 229.

Reeder, D., 1973, "Report of Bonn Conference," Internal Report, 4 September.

Rousset, A., 1973a, memorandum to Professor Cresti, CERN-TCL, 19 February.

Rousset, A., 1973b, CERN-TCL, 22 May.

Rousset, A., 1974, in *Neutrinos-1974 (Philadelphia),* AIP Conference Proceedings No. 22, Particles and Fields Subseries No. 9, edited by C. Baltay (American Institute of Physics, New York), p. 141.

Rousset, A., 1980, interview, 30 November.

Rubbia, C., 1973, letter to A. Lagarrigue, 17 July.

Rubbia, C., 1980, interview, 3 October.

Rubbia, C., and L. Sulak, 1973, "Analysis of Neutrino Events with No Associated Muon," Harvard, technical memorandum, 18 August.

Sakurai, J. J., 1974, in *Neutrinos-1974, (Philadelphia)* AIP Conference Proceedings No. 22, Particles and Fields Subseries No. 9, edited by C. Baltay (American Institute of Physics, New York), p. 57.

Salam, A., 1968, in *Elementary Particle Physics,* edited by N. Svartholm (Almquist and Wiksells, Stockholm), p. 367.

Schreiner, P., 1974, in *Proceedings of the XVII International Conference on High Energy Physics, London, July 1974,* edited by J. R. Smith (Science Research Council, Rutherford Laboratory, Chilton), p. IV-123.

Schwartz, M., 1960, Phys. Rev. Lett. **4**, 306.

Sulak, L. R., 1973, technical memorandum, 17 November.

Sulak, L. R., 1980, interview, 8 September.

Sullivan, D., D. Koester, D. H. White, and R. Kern, 1980, Scientometrics **2**, 309.

Taylor, J. C., 1976, *Gauge Theories of Weak Interactions* (Cambridge University Press, Cambridge, England).

t'Hooft, G., 1971a, Nucl. Phys. B **33**, 173.

t'Hooft, G., 1971b, Phys. Lett. B **37**, 195.

Vialle, J. P., 1980, interview, 28 November.

Vialle, J. P., and D. Blum 1973, Orsay, technical memorandum, 15 May.

Wachsmuth, H., 1973, CERN-TCL, 24 May.

Weinberg, S., 1967, Phys. Rev. Lett. 19, 1264.

Weinberg, S., 1972, Phys Rev. D 5, 1412.

Weinberg, S., 1973, in 2nd International Conference on Elementary Particles Aix-en-Provence 6–12 September 1973, J. Phys. (Paris) 34, Supplement to Number 10, Colloque C1, p. C1-45.

Weinberg, S., 1980, Rev. Mod. Phys. 52, 515.

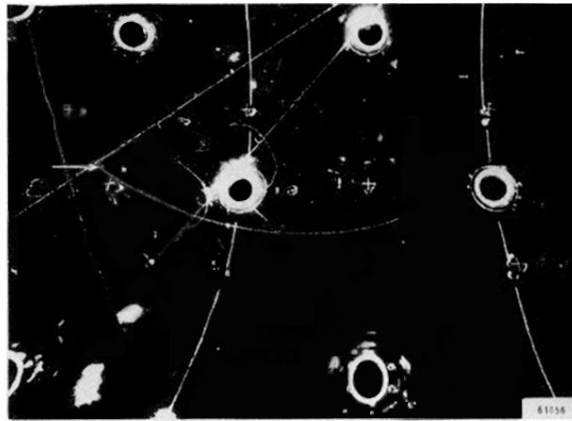Young, E. C. M., 1967 CERN yellow report 67-12.

FIG. 1. Neutral-current event. Bubble-chamber photographs from Gargamelle resembling and including this one were at first mistakenly classified as neutron stars. (These are events in which a neutron—putatively at the arrow's end—collided with a nucleus to create a right-moving shower of particles.) Later many of these events were understood to be neutral-current events in which an unseen right-moving neutrino scattered elastically from a quark, creating a right-moving hadronic shower.
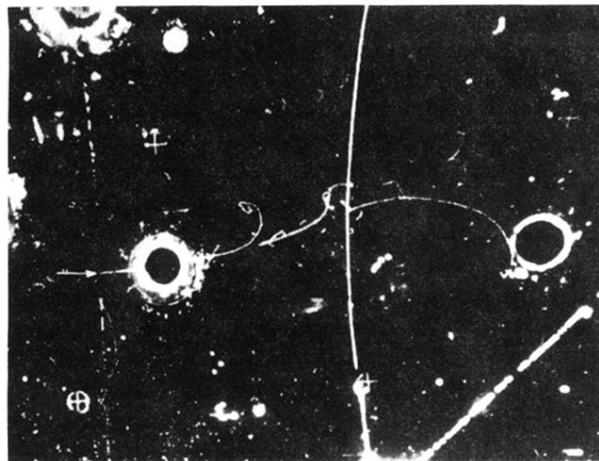
FIG. 10. First single-electron event from Gargamelle. Found at Aachen early January 1973. The electron's trajectory goes from left to right, beginning at the arrow's end, where it ostensibly was hit by a right-moving neutrino. The haloed black circles are lights to illuminate the bubble-chamber liquid.