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Proceedings of the Fifteenth LAMPF Users Group Meeting

Held at Los Alamos National Laboratory
Los Alamos, New Mexico
November 2—3, 1981

Compiled by

Donald R. F. Cochran

Los Alamos

Los Alamos National Laboratory
Los Alamos, New Mexico 87545

ABSTRACT

The Fifteenth *LAMPF Users Group Meeting* was held November 2-3, 1981 at the Clinton P. Anderson Meson Physics Facility.

The program of papers scheduled to be presented was amended to include a "Report from Washington" by Clarence R. Richardson, U.S. Department of Energy.

The general meeting ended with a round-table working group discussion concerning the "Planning for a Kaon Factory."

PROCEEDINGS

OF THE

FIFTEENTH LAMPF USERS GROUP MEETING

Los Alamos National Laboratory
November 2-3, 1981

1982 WORKING GROUP CHAIRMEN

Stopped Muon Channel (SMC)	Gary Sanders	Los Alamos
Solid-State Physics and Materials Science	Robert Brown	Los Alamos
Neutrino Facilities	Herbert Chen	University of California, Irvine
Muon Spin Rotation	Richard Hutson	Los Alamos
Nuclear Chemistry	Bruce Dropesky	Los Alamos
Biomed	James Bradbury	Los Alamos
Nucleon Physics Laboratory (NPL)	Lawrence Pinsky	University of Houston
Polarized Facilities	Michael McNaughton	Los Alamos
Energetic Pion Channel and Spectrometer (EPICS)	Donald Geesaman	Argonne
Computer Facilities	Michael McNaughton	Los Alamos
High-Energy Pion (P^3) Channel	Hans Plendl	Florida State University
Low-Energy Pion (LEP) Channel	Barry Ritchie	University of South Carolina
High-Resolution Spectrometer (HRS)	John McGill	Rutgers University
Graduate Student/Postdoc	John Faucett	University of Oregon
π^0 Spectrometer	Helmut Baer	Los Alamos

ANNOUNCEMENT

WORKING GROUP WORKSHOPS FOR PROPOSED LAMPF II

A series of working group meetings, intended to begin defining a target experimental program leading to a realistic set of specifications for machine and experimental facilities for the proposed LAMPF II accelerator, was held in Los Alamos, February 1-4, 1982. Long-range plans include another informal workshop in April, followed by a larger formal meeting, July 19-22, 1982, to be held in the Los Alamos Study Center. For information on these meetings Users should contact the LAMPF Visitors Center, MS 830, Los Alamos National Laboratory, Los Alamos, NM 87545 (telephone 505-667-5759, FTS 843-5759).

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PROGRAM

FIFTEENTH LAMPF USERS GROUP MEETING

Los Alamos National Laboratory — November 2-3, 1981

Chairman:	Felix Boehm, California Institute of Technology
Chairman-Elect:	Harold E. Jackson, Argonne National Laboratory

Monday, November 2 — **LAMPF Auditorium, Laboratory-Office Building (MPF-1, TA-53)**

MORNING SESSION

Felix Boehm, Presiding

8:00 - 9:00	Registration
9:00 - 9:15	Welcome - Donald M. Kerr, Director of the Los Alamos National Laboratory
9:15 - 10:00	LAMPF Status Report - Louis Rosen, Director of LAMPF
10:00 - 10:30	LAMPF Operations Report - Donald C. Hagerman, Chief of Operations at LAMPF
10:30 - 11:00	COFFEE BREAK
11:00 - 12:30	*NEW DIRECTIONS* Gerard J. Stephenson (Los Alamos National Laboratory) *"Progress Report on a Proposal for a Los Alamos Neutrino Facility"* Darragh E. Nagle (Los Alamos National Laboratory) *"Progress Report on a Kaon Factory"* Henry A. Thiessen (Los Alamos National Laboratory) *"Embryonic Plans for Kaon Factory Experimental Areas"*
12:30	Buses to the Laboratory Support Complex Cafeteria, Los Alamos Inn, and Hilltop House Motel

1:30 p.m. — **LAMPF Auditorium, Laboratory-Office Building (MPF-1, TA-53)**

AFTERNOON SESSION

Harold Jackson, Presiding

1:30 - 2:00	Clarence Richardson (Department of Energy) *"Report from Washington"*
2:00 - 2:45	Annual Users Group Report - Felix Boehm, Chairman of the Board of Directors General Business Session Election Results New Business
2:45 - 3:15	Jean-Pierre Blaser (Director, Swiss Institute for Nuclear Research) *"SIN: Status, Future Scientific and Technological Plans"*
3:15 - 3:30	COFFEE BREAK
3:30 - 4:30	Milla Baldo-Ceolin (Univ. of Padova-CERN) *"Neutron Oscillations"*
4:30 - 5:10	Alex Zehnder (ETH-Zürich) *"The Hunting for the Axion"*
5:10	Buses to the Los Alamos Inn and Hilltop House Motel
6:30	BANQUET at BOCCACCIO'S (Tickets to this event must be purchased in advance.)

Tuesday, November 3	LAMPF Auditorium, Laboratory-Office Building (MPF-1, TA-53)
8:00 - 9:00	Computer Facilities Working Group — Dennis G. Perry (Los Alamos National Laboratory), Chairman
	MORNING SESSION
	Robert Eisenstein, Presiding
9:00 - 9:45	Robert Redwine (Massachusetts Institute of Technology) *"Searches for Violation of Muon Number Conservation"*
9:45 - 10:15	COFFEE BREAK
10:15 - 11:00	George A. Rinker (Los Alamos National Laboratory) *"Current Problems in Muonic Atom Physics"*
11:00 - 11:45	David J. Ernst (Texas A&M Univ.) *"Recent Results in the Pion-Nucleus Interaction"*
11:45	Buses to the Laboratory Support Complex Cafeteria, Los Alamos Inn, and Hilltop House Motel

1:00 - 3:00 p.m. WORKING GROUP MEETINGS

LEP (Low-Energy Pion Channel) and π^0 Spectrometer	Felix E. Obenshain (ORNL), Chairman; Helmut Baer (Los Alamos National Laboratory), Chairman	LAMPF Auditorium
EPICS	David B. Holtkamp (Univ. of Minnesota), Chairman	LAMPF Cafeteria
Polarized Facilities	Michael McNaughton (Los Alamos National Laboratory), Chairman	LAMPF, Room A-234
SMC (Stopped Muon Channel)	Howard Matis (Los Alamos National Laboratory), Chairman	LAMPF, Room A-114
Biomedical Facilities	James N. Bradbury (Los Alamos National Laboratory), Chairman	LAMPF, Room A-218
Solid-State Physics and Materials Science	Robert D. Brown (Los Alamos Natonal Laboratory), Chairman	LAMPF, Room A-214
Neutrino Facilities	Herbert H. Chen (Univ. of California, Irvine), Chairman	LAMPF, Room D-105

3:15 - 5:15 p.m. WORKING GROUP MEETINGS

P^3 (High-Energy Pion Channel)	William J. Briscoe (UCLA), Chairman	LAMPF Cafeteria
HRS (High-Resolution Spectrometer)	Gary S. Blanpied (Univ. of South Carolina), Chairman	LAMPF Auditorium
NPL (Nucleon Physics Laboratory)	Lawrence S. Pinsky (Univ. of Houston), Chairman	LAMPF, Room A-234
*Muon Spin Rotation	Douglas E. MacLaughlin (Univ. of California, Riverside), Chairman	LAMPF, Room A-114
Nuclear Chemistry	Lon-Chang Liu (Los Alamos National Laboratory), Chairman	LAMPF, Room D-105
Graduate Student/Postdoc	John Faucett (Univ. of Oregon), Chairman	LAMPF, Room A-218

*"New Muon Channel Possibilities" will be discussed in the μSR Group from 4:30-6:00.

WORKING GROUPS - ROUND TABLE DISCUSSION

7:30 p.m.	*"Planning for a Kaon Factory"*	LAMPF Auditorium

REPORT FROM WASHINGTON

Clarence R. Richardson
Division of Nuclear Physics, Office of Energy Research
Department of Energy

Jim Leiss sends his apologies for not being able to be here today to talk to you. He tried very hard to keep his schedule open for this, but pressing matters finally made it impossible for him to be away from Washington at this time. When I found out that I was going to pinch-hit for him, I asked him what he thought I should talk about. What he said might be summed up, "Talk to them about what they want to hear about, but don't feel you have to tell them what they want to hear." Well, what I think you want to hear about is shown in Fig. 1. I will tell you in some detail where matters stand on the budgets that we are presently considering, I will then mention briefly some activities related to the dismantling of the Department of Energy (DOE), and to finish I will give you the view from Germantown about the outlook for the nuclear physics research program in the second half of this decade.

Before getting into those questions, a little orientation about how our part of DOE is organized may be helpful. As shown in Fig. 2, the presidential appointee to whom we report is the Director of Energy Research, Al Trivelpiece. He follows John Deutch and Ed Frieman in that job, and also Doug Pewitt, who filled in between Ed Frieman and Al Trivelpiece before going to work with

WHAT I THINK YOU WOULD LIKE TO HEAR ABOUT

WHAT'S HAPPENING WITH:

-- THE BUDGET?

-- THE DISMANTLING OF DOE?

WHAT IS THE "MEDIUM TERM" OUTLOOK FOR THE NUCLEAR PHYSICS PROGRAM?

Fig. 1.

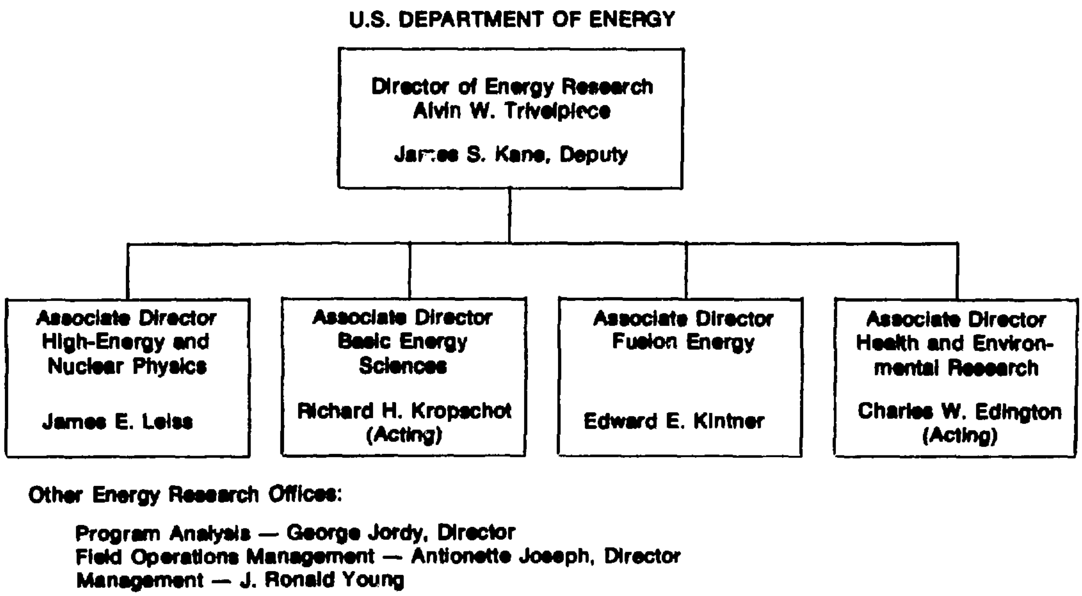

Fig. 2.

Jay Keyworth. The Director of Energy Research is on the same level as five line-program assistant secretaries, who all report to the Secretary of Energy through the Undersecretary.

Under the Director of Energy Research are four program associate directors: Jim Leiss for High-Energy and Nuclear Physics; Dick Kropschot for Basic Energy Sciences (acting in that capacity since Jim Kane moved up to Deputy Director of Energy Research); Ed Kintner for Fusion Energy (actually, only magnetic-fusion energy activities, because the so-called inertial-fusion activities are under the Assistant Secretary for Defense Programs, elsewhere in the department); and Charlie Edington for Health and Environmental Research, which was recently shifted to the Energy Research organization from another part of DOE. In addition, there are three other offices, primarily for staff functions, that are under the Director of Energy Research. Field Operations Management, under Toni Joseph, does have some outlay programs. Those of you who have followed the organizational evolution of our programs may note that the High-Energy and Nuclear Physics, Basic Energy Sciences, and Fusion programs are direct descendants of the old Atomic Energy Commission Research Division.

In Fig. 3 we focus on Jim Leiss and what comes under him — the High-Energy and the Nuclear Physics Research programs. The numbers you see in parentheses are the authorized levels of permanent staff but do not include temporary staff, such as people detailed to our programs from DOE labs. Let me show you more structure of both the High-Energy and Nuclear Physics Divisions, since both have constituents present here. The High-Energy Physics program is headed by Bill Wallenmeyer; the breakdown is shown in Fig. 4.

The Nuclear Physics program, Fig. 5, is headed by Enloe Ritter and is not nearly as broken down as the High-Energy Program. I mean by that that the Nuclear Physics program does not have a formal branch structure like High-Energy Physics, so this chart shows the responsible people by program area. Two detailees presently in the division are Dick Silbar (Los Alamos) in Nuclear Theory and Dave Hendrie (Lawrence Berkeley Laboratory) in Heavy Ions. Of the permanent staff, Stan Whetstone handles Low Energy, John Erskine concentrates on Facilities and Instrumentation, and I do the Medium-Energy program.

Let me now get into the issues I promised to discuss. The two budgets at issue presently are for FY 1982 and FY 1983. You realize, of course, that FY 1982 began October 1. You also know that the Congress never completes action on the budget before the fiscal year actually begins. Figure 6(a) shows combined National Science Foundation (NSF) and DOE funding for nuclear physics research since FY 1977. The DOE budgets are shown separately for the operating, equipment, and construction categories, with NSF money added on top. The

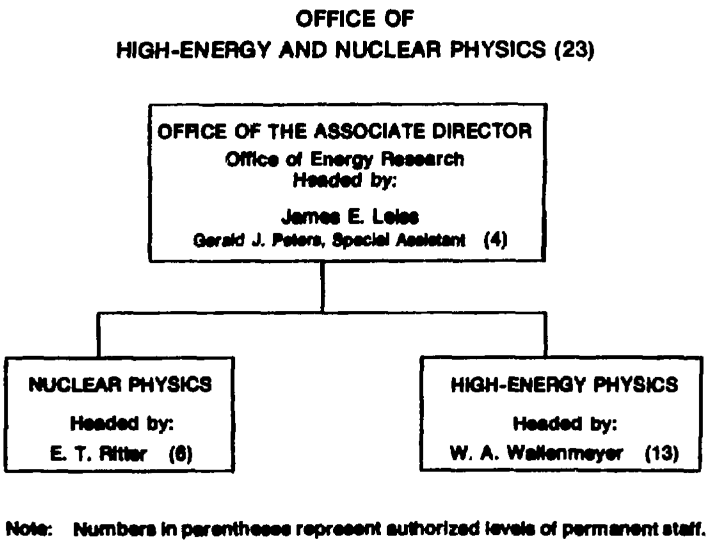

Fig. 3.

OFFICE OF HIGH-ENERGY AND NUCLEAR PHYSICS
DIVISION OF HIGH-ENERGY PHYSICS

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William A. Wallenmeyer
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S. Derflinger, Secretary

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E. Coleman, Physicist
*R. Thews, Theorist
**P. K. Williams, Theorist
***D. Peaslee, Theorist
S. Lewis, Secretary

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ADVANCED TECHNOLOGY R&D DEVELOPMENT
D. Sutter, Chief (Acting)
**M. Month, Physicist
J. Gollan, Secretary

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R. Woods, Physicist
L. Ray, Prog. Mgmt. Spec.
D. Sler, Computer Tech.
B. Priebe, Secretary

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Fermilab	G. Charlton
SLAC	*E. Fowler
LBL	*E. Fowler
Project Officers	
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Energy Saver/ Bevatron	**M. Month
HEPAP	**P. K. Williams
ADP	R. Woods

*IPA
**Detailee
***Consultant

9/81

Fig. 4.

OFFICE OF HIGH-ENERGY AND NUCLEAR PHYSICS
DIVISION OF NUCLEAR PHYSICS

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C. Porter, Secretary
J. Fink, Secretary

PROGRAM MANAGER MEDIUM-ENERGY NUCLEAR PHYSICS
C. RICHARDSON

PROGRAM MANAGER HEAVY-ION NUCLEAR PHYSICS
E. RITTER, ACTING
D. HENDRIE*

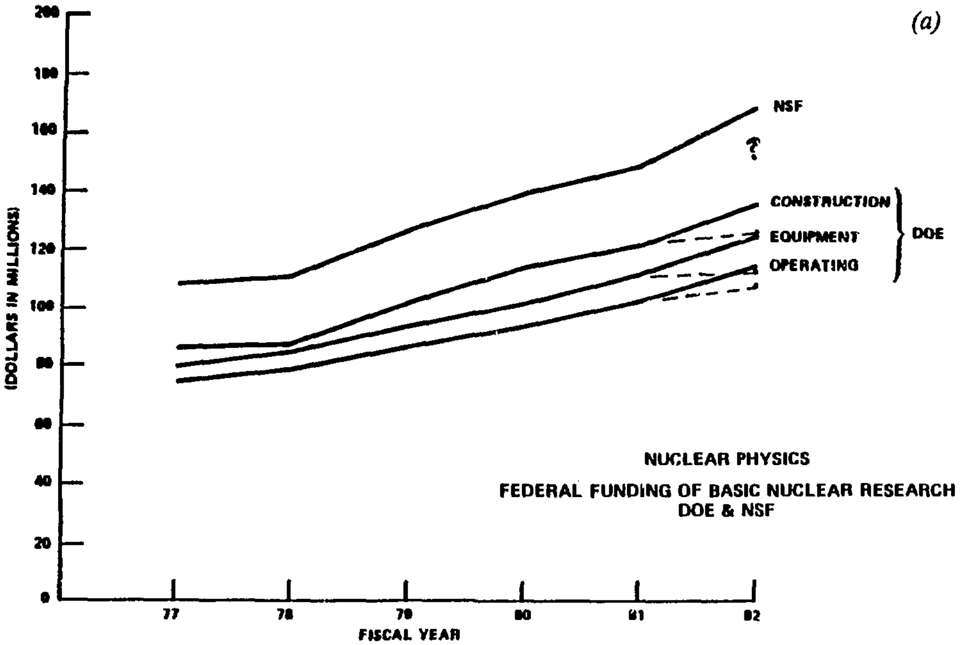
PROGRAM MANAGER LOW-ENERGY NUCLEAR PHYSICS
STANLEY WHETSTONE

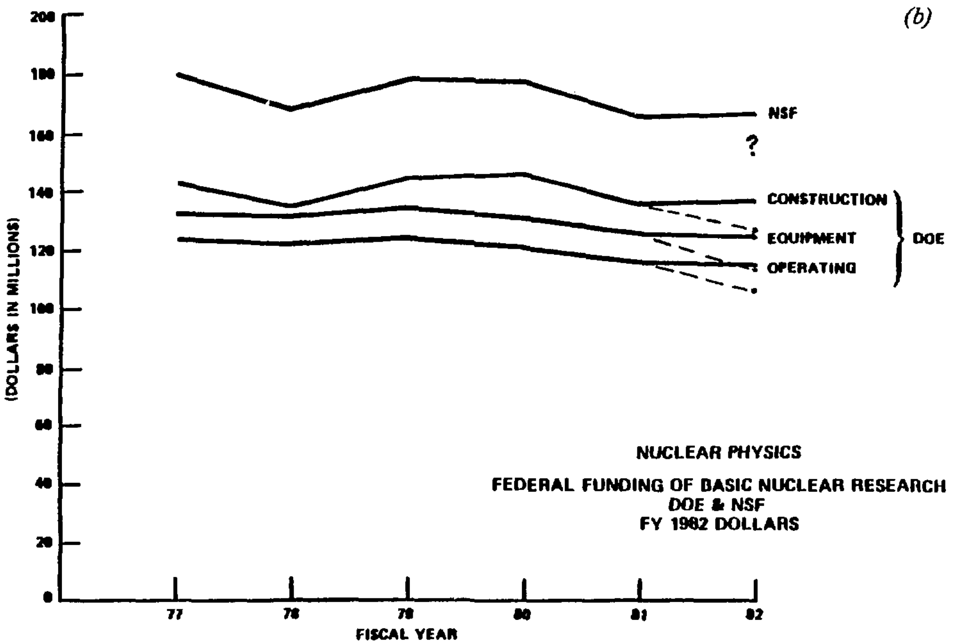
PROGRAM MANAGER NUCLEAR THEORY
E. RITTER, ACTING
R. SILBAR**

PROGRAM MANAGER FACILITIES AND INSTRUMENTATION
JOHN ERSKINE

*DETAILEE FROM LAWRENCE BERKELEY LABORATORY (7/81 - 7/83).
**DETAILEE FROM LOS ALAMOS (9/81 - 9/82).

Fig. 5.





Figs. 6(a) and (b).

(a) The combined NSF and DOE funding for nuclear physics research since FY 1977. The DOE budgets are shown separately for the operating, equipment, and construction categories, with NSF money added on top.

(b) The same budget history as (a) expressed in constant value (FY 1982) dollars, illustrating trends in actual buying power or level of effort.

curves are cumulative. You will note that there is a fork in the end of each of the DOE curves. The top branch goes to the budget figure provided in the original (March 1981) Reagan budget. Those figures remained the same throughout the entire budget reconciliation process when many other programs were getting significantly reduced. But, when the recent crunch came in the form of 12% across-the-board (well, almost across-the-board) reductions, the DOE nuclear physics numbers in the revised request to Congress were reduced to those on the lower branch of each curve. Harvey Willard told me this morning that the NSF nuclear physics budget for this case is reduced by 19%, so the lower branch there would hit the question mark somewhere.

The ground rules on this budget case called for reducing each of the three categories by somewhere near the same percent. Figure 6(b) shows the same budget history expressed in constant value (FY 1982) dollars, which illustrates the trends in actual buying power or level of effort. In this picture it is clear that such a 12% reduced budget for FY 1982 would necessitate making some very difficult decisions.

Now let's look at the actual numbers and the breakdown into the various parts of nuclear physics for the FY 1981 case and the two FY 1982 cases (Table I).

TABLE I

NUCLEAR PHYSICS
(Budget/Authorization in Thousands)

		FY 1982	
	Appropriation FY 1981	March 1981 President's Budget	Revised 12%
Operating Expenses			
Medium-Energy Nuclear Physics	$ 46 000	$ 50 900	$ 46 500
Heavy-Ion Nuclear Physics[a]	39 060	42 714	35 796
Low-Energy Nuclear Physics[b]	11 582	12 757	11 226
Nuclear Theory	7 000	8 200	7 300
	$103 642	$114 571	$100 822
Capital Equipment[c]	$ 9 870	$ 11 010	$ 8 954
Construction			
Accelerator Improvement Projects	$ 1 600	$ 2 000	$ 2 000
General Plant Projects	2 800	2 800	2 000
Argonne Tandem/Linac (82-ER-223)	0	4 000	4 000
National Superconducting Cyclotron Laboratory (80-GS-5)	6 900	4 500	4 500
	$ 11 300	$ 13 300	$ 12 500
TOTAL, Nuclear Physics	$124 812	$138 881	$122 276
[a]Transfer from Life Science and Nuclear Medicine Applications (LS&NMA)	$ 2 560	$ 2 714	$ 2 388
[b]Transfer from Basic Energy Sciences	11 582	12 757	11 226
[c]Transfer from Basic Energy Sciences	470	510	510

Here, you see the possible implications for the Medium-Energy, Heavy-Ion, Low-Energy, and Nuclear Theory programs. Among other things, this distribution reflects an attempt to preserve unique capabilities when possible, and an attempt to maintain LAMPF and Bates at viable levels, though they would not be spared all pain. The Heavy-Ion program would be hit harder, one reason being that the heavy-ion program has more facilities, with some overlapping capabilities.

Table II shows annual operating hours for the larger nuclear physics accelerators. These figures assume only nuclear physics program funding. That is to say, they do not include the defense program funds mentioned by Louis Rosen this morning. The last two columns are obviously estimates; the last column especially should be taken as an illustration of possible implications if we end up with the 12% reduced budget following Congressional action on the FY 1982 budget. As you can see, though the hours indicated for LAMPF are significantly reduced, some other facilities would fare much worse. I must add that we believe the nuclear physics community would regard this level of operation for LAMPF as not at all satisfactory and, in fact, unacceptable for more than 1 year. It's just too wasteful.

Table III is a further budget breakdown, showing the distribution of funds within the Medium-Energy program. It should come as no surprise that LAMPF and LAMPF Users are by far the largest component of medium-energy funding and that Bates also has a significant chunk. The "All Other" category is made up of work at other laboratories (such as Fermilab, CERN, and the Indiana University Cyclotron Facility), reactors, and a relatively small solar neutrino effort. The accelerator research and development line is one that will be protected — it represents the future of the program. The advice we are getting is that this type of effort in the past has not been given high enough priority.

As for the FY 1983 budget, things are naturally much more uncertain. Superficially the situation does not differ from the usual one at this point in the budget cycle, in that we are going through various exercises, including ones at the same dollar level as FY 1981 and FY 1982. The disturbing difference this year is that we are being told that the low case must be taken seriously rather than being included as a throwaway. We will be getting more information on the FY 1983 budget in the next few weeks, and what we learn about that budget will constrain very strongly what choices should be made in FY 1982.

TABLE II

NUCLEAR PHYSICS

Accelerator Operations
Beam Hours for Research

	Actual FY 1980	Appropriation FY 1981	FY 1982 March 1981 President's Budget	FY 1982 Revised 12%
LAMPF	3520	3104	2500	1900
Bevalac	3279	2934	2000 [a]	2000 [a]
SuperHILAC	3114	3061	3000	1000
Bates	2992	3200	2000 [b]	2000 [b]
Holifield	1980	2100	3000	2800 [c]
88-in. Cyclotron	4510	4059	4100	3000
BNL double MP	6312	4200	6500	<1000
ANL Tandem/Linac	5100	5150	5200	4000

[a] Shutdown in FY 1982 due to U Beams construction project.

[b] Shutdown in FY 1982 due to Recirculator construction project.

[c] Reduced Oak Ridge Isochronous Cyclotron operations; 25-MV tandem alone much of the time.

TABLE III

MEDIUM-ENERGY NUCLEAR PHYSICS

(Budget/Authorization in Thousands)

	Appropriation FY 1981	FY 1982 March 1981 President's Budget	FY 1982 Revised 12%
Operating Expenses			
LAMPF			
Operations	$24 020	$25 630	$23 380
In-house research	5 265	5 780	5 420
Outside users research			
University	3 610	4 310	3 430
National Lab	1 320	1 520	1 350
Bates Electron Accelerator			
Operations	4 020	4 475	4 390
In-house research	1 265	1 400	1 375
Outside users research			
University	700	840	650
National Lab	100	115	110
Los Alamos/National Bureau of Standards Accelerator Research and Development	2 300	3 000	3 000
BNL Kaon Experiments			
In-house	630	800	650
Outside users	420	490	370
All Other	2 350	2 540	2 375
	$46 000	$50 900	$46 500

Again, on the FY 1982 budget, we have all seen in the news (and heard Don Kerr and Louis Rosen mention) that Congress is showing resistance to going for the full 12% reduction and that, while we can't yet know for sure, it seems fairly probable there will only be a reduction of about half what I have shown in these tables. That is close to what is reflected in the recently issued Senate Appropriations Committee report on our part of the budget. Of course, the budget still has other steps to go through in Congress. But, as I said, things look reasonably promising that there will be some significant restoration from the low case. With that information it is tempting to assume that we have dodged the bullet and can breathe a collective sigh of relief. But the more prudent approach, in my opinion, is to recognize that there remains a serious problem for the next few years and that we should look at this as *temporary* relief, with an opportunity to make difficult decisions carefully rather than in haste.

I will take only a few minutes on the plan to dismantle DOE. The wheels are definitely in motion. The President recently publicly reaffirmed his intention to make good on that campaign promise. In so doing he also acknowledged that many of the components — together with a large part of the budget — would be preserved somewhere in the Executive Branch. That touched off a great flurry of activity, with many experts being called to Washington to give advice and many others offering suggestions, solicited or otherwise.

Now, in the legislation that formed DOE originally (way back in 1977), DOE was given the responsibility of reviewing its performance after a few years to see if the case was strong for its continuance. DOE was called on to send a report (often referred to as the sunset report) to Congress by January 15, 1982. After some recent encouragement, Secretary Edwards has agreed to submit to the Office of Management and Budget (OMB) a draft of that report by November 15. Pending receipt of the draft report, OMB has put together a plan and sent it as a decision paper to the President. I'm sure many of you saw that reported in the news. That plan (and other possible scenarios in the wind) calls for abolishing some parts, attaching some to other existing departments or agencies, and forming two new federal bodies. One would embody the regulatory functions and the other would embody the weapons program, high-energy and nuclear physics, basic energy sciences, fission and fusion energy, and a few other pieces. Again, the actual result is unpredictable, but it is very likely that the Energy Research programs will end up pretty much intact and will remain with the weapons program. From our point of view, that is a desirable outcome.

The other thing I promised to talk about was the outlook for nuclear physics research. I believe the outlook is reasonably favorable. It is a time for concern, but not a time for panic. The present budget strictures are part of a commitment to a certain approach to solving general economic difficulties. We are convinced that both the Administration and Congress are supportive of the kinds of things we are doing. However, given the economic situation, the importance of realistic planning is paramount. We must be willing to set priorities.

Central to this process for us is the Nuclear Science Advisory Committee (NSAC). It represents, for DOE and NSF, the means to get the best collective judgment of the nuclear science community. The advice is passed on sometimes informally and often formally, as in the annual NSAC review of facility proposals. The outstanding example of formal advice is the NSAC long-range plan, a comprehensive and responsible plan for nuclear science issued in 1979 and since endorsed by DOE, NSF, OMB, and the office of Science and Technology Policy.

Recognizing, however, that in the present situation constant scientific effort is a sounder planning principle than steady growth, we have modified the numbers in the plan to omit the 3% annual growth in operating funds called for originally. Figure 7 shows how well we have been able to track this modified plan. We are tracking reasonably well except in the construction category. Some of that shortcoming has resulted from our own decisions in response to reductions; we can avoid immediate pain to some extent by sacrificing construction for operating, but reduced construction indicates that we have already been borrowing against our future.

To make this planning process work, it is essential that people in the community make their input through NSAC members. At least two NSAC members are here today, Felix Boehm and Don Hagerman. Talk to them. Bear in mind that in this planning process it may be necessary to give up some things in order to have the best. That applies across the board. Here, you should pick the best science for LAMPF and go for it.

Often the decisions are out of our hands, but when latitude exists, decisions *must* have the backing of the community or we will self-destruct.

Now I want to mention another bothersome part of our process of making decisions. Although it consumes time and uses people who ideally should be doing other productive things, it is necessary occasionally to have fairly thorough reviews of ongoing programs and activities. Before the crunch, we had already started on the current round of reviews. During the last year, the entire Nuclear Theory program was reviewed. Ben Gibson, from Los Alamos, spent a year with us and organized the review. It was very well done and the results are guiding decisions now. A review of the nonmajor facilities was also carried out last year, and, sadly for some, very significant decisions are being made on the basis of that review. Obviously, though, it is better to have the results of the review to guide the decisions when choices are necessary. At present a process is being set up to review the heavy-ion users, and we are committed to a review of the major facilities (LAMPF, Bates, and Bevalac) next spring. The medium-energy users will be reviewed later next year.

In conclusion, I believe it is important to accentuate the positive aspects of our programs. And, again, this applies across the board. Take advantage of opportunities to publicize the physics program of LAMPF with the rest of the community. Don Kerr also stressed the importance of such advocacy. I believe it is important not to criticize other people's programs; that rarely benefits the critic. Concentrate on the achievements of the program and the importance of nuclear physics, and also on the importance of basic research to programs and policies that are currently in the ascendancy.

I'm like the optimistic brother, so I look at the situation this way: There's got to be a horse in there somewhere.

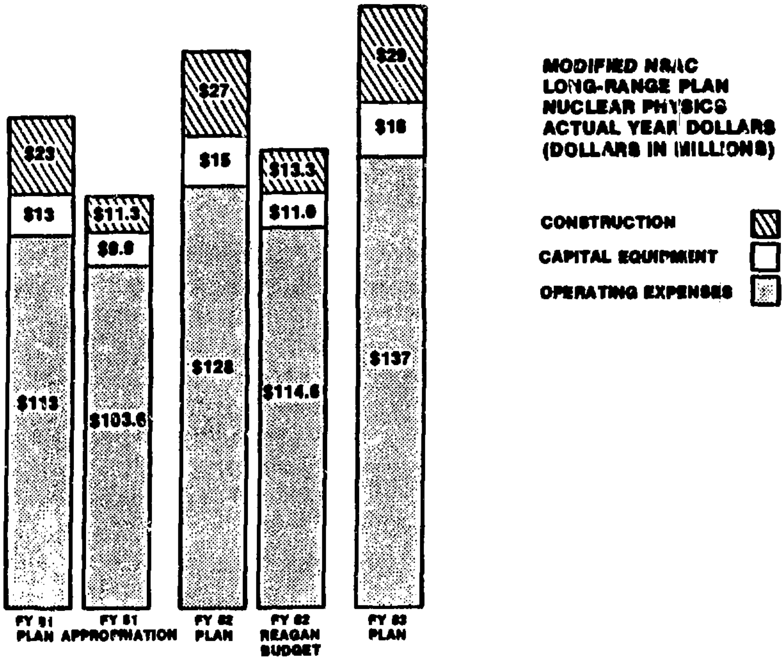


Fig. 7.

LAMPF STATUS REPORT

Louis Rosen, Director
Los Alamos Meson Physics Facility
Los Alamos National Laboratory

Good morning, Ladies and Gentlemen. This morning I would like to first spend a few minutes saying something about an old friend of LAMPF who recently passed away. Then I would like to tell you my perceptions of what the LAMPF budgetary situation looks like now. I must warn you that the picture is an oscillation, something like a sine wave. How things look at any point depends on the phase at which you query that wave. So things change, sometimes within 24 hours. I suspect that when Clarence Richardson talks he will correct what I will say, possibly as the result of a telephone call he will make in the meantime.

After I tell you the budgetary situation I want to walk you very quickly through the various activities in the beam channels that took place last fiscal year. Why I do that will become obvious. You remember, I hope, that last year I spent essentially my entire talk discussing the philosophy, or rationale, for the LAMPF research program, present and future. During the past 12 months the program has proceeded extraordinarily well. What I want to show you, by listing abbreviated titles of experiments, is how we have proceeded and the scope of the program. I am simply going to assume that you remember everything I said last year; that's not an unreasonable assumption, I hope. But, finally and most importantly, what I want to do today is talk about the future of LAMPF.

In view of what Dr. Kerr just told us you may think that perhaps the old man is losing his marbles. At this particular juncture he wants to talk about what is going to happen in the 1990s! But science and this country and civilization will, I hope, be around in the 1990s and we have to plan for it. If we stop planning we are surely saying that at least this particular activity is going to atrophy.

I am reminded of my first trip to the Soviet Union after World War II. We were at a luncheon given at the American Embassy for our committee (this was the first nuclear science committee to visit the Soviet Union after the war) and our Soviet hosts. At this reception, with many of the high officials of the Academy of Science and of the nuclear science establishment in the Soviet Union, we had numerous conversations. If we had more time and I thought my voice would hold out I would tell you some of the stories, for they are precious, but what I want to tell you has to do with something I found amazing. The director of one of the laboratories told me that he was in Leningrad during the seige of Leningrad, and I asked him, "Well, what were you doing?" He said, "Oh, you will find this hard to believe but I was a member of a group that was designing a cyclotron, which later became the one at Gatchina." During the seige of Leningrad! I have never forgotten that. You know, it takes people in a country with thousands of years of history to have a sense that there is going to be a future, and if they lose that sense, they have lost about everything. So if you think me a little odd for highlighting in this talk the future of LAMPF, please reflect on that story.

According to Stan Livingston's "History of LAMPF" (which I hope most of you have read, and if you haven't read it, it is worth reading), it was in 1962 that the first memorandum was written proposing that a meson factory be built at Los Alamos, almost 20 years ago. That memo was written to J. M. B. Kellogg, who was the head of P Division at the time. Jerry Kellogg passed away a few weeks ago. Probably not many of you know that Jerry Kellogg played a vital role in helping us start the planning for the development of LAMPF. He was a person of exquisite intellectual honesty. He was also one of the most conscientious people I have ever known, and very bright. It is probably these characteristics that endeared him to I. I. Rabi, under whom he did his Ph.D. thesis on the quadrupole moment of the deuteron. Jerry became Rabi's lifelong friend.

Jerry taught all of us a great deal. I think it was mainly from him that I came to appreciate that in creative areas management must be a tool of leadership and not an end in itself, that an ounce of persuasion is worth a pound of coercion, and that imperfect management by consensus can be more effective than perfect management by decree. But perhaps most importantly, I learned from Jerry that, at least where research is concerned, one should strive for minimal management. For better or for worse, for richer or for poorer, Jerry Kellogg has had a very significant impact on how LAMPF has been managed and how you have interacted with LAMPF. I don't know how many of you will miss Jerry, I don't know how many of you knew him, but I shall certainly miss him.

Well, what about the LAMPF budgetary situation? Once again science in this country is facing perilous times. Paradoxically, this comes at a time when we need science and its offspring, technology, perhaps more than at any time during this century — a century that included two World Wars. Mary and I have just returned from China where we had a meeting lasting well over an hour with Vice-Premier Fang Yi. From this meeting and meetings with the directors of several large laboratories I gained an appreciation of the devastation that was wrought in China by the interruption of science and education during the cultural revolution. It probably set them back a quarter of a century. For the United States a much less severe interruption could have an even more serious impact. The point is that in China they need science and technology to improve their standard of life; in this country we need it to maintain ours. Severe cutbacks in science could very adversely affect both our economic and military viability, and I just hope that doesn't happen.

Last week I attended the Technology Showcase in Albuquerque. Senator Harrison Schmitt was there, and I can tell you that you can't tell Senator Schmitt anything about the importance of science that he is not already convinced of. He is completely aware of the peril this country will face if it pursues a program of cutting back and deemphasizing research. Whether Senator Schmitt has enough influence to make understandable to the Senate his concerns and what he thinks ought to be done only time will tell.

I also talked to the President's Science Adviser, Jay Keyworth. Jay told me quite directly that it is not the intent of the present administration to reduce funding for research. When I hinted to him, "But, look, what's going on!", he said that we would just have to wait until we could look at the complete package. He said, "I promise that when you look at the complete package — all the research — you will find that there has not been a decrease in research support." I just tell you what I have been told and what we are planning for in various contingencies. But before I discuss budgetary matters, I'd like to review for you some history.

Figure 1 shows, plotted in a cumulative way as a function of time starting way back, the total number of proposals received, the number approved, and the completed experiments. To begin with, I want you to see that the numbers are very large. Even for the approved experiments the numbers continue to rise. However, in this past year the curve is sort of bent over in terms of completed experiments. This is partly a fluke because we

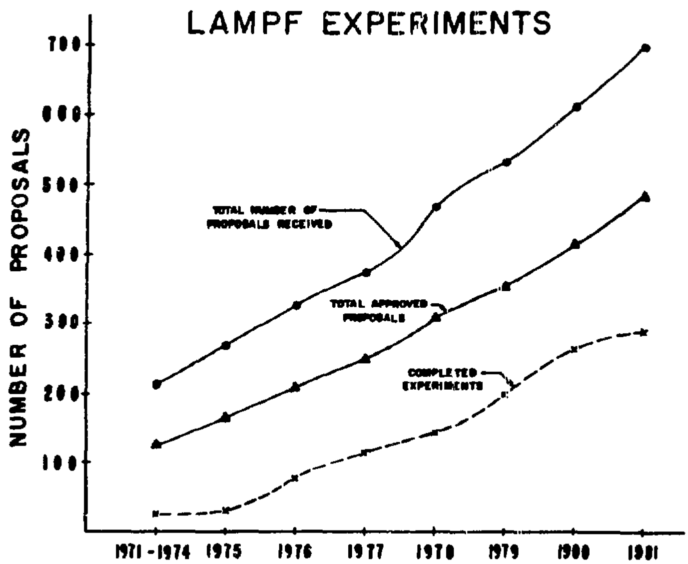


Fig. 1.

have spent a good part of our efforts tooling up for some very large experiments (a lot of resources have gone into them), and also because a number of experiments are just on the verge of being completed. But, in addition, I believe that this curve reflects the fact that experiments are becoming more complex, more sophisticated, and more consuming of beam time; at the same time, we have less beam time to provide to experimenters.

So if things become a worst-case situation, we are going to have to address this question and ask whether we are approving too many experiments and whether we are running more beam channels than we should run to maximize the physics output for a given amount of resources. The reason I am going to walk you through the beam channels is so you will see and understand very quickly what a tremendous problem it will pose if we have to start choosing between one approach and another. We have a very good balance right now, in my opinion, but we may have to talk about unbalancing it. This will be very painful and I want to show you just how painful it will be by reviewing some of those experiments.

I should also say that during this past fiscal year we have provided beam to 106 experiments, involving investigators from 77 institutions from this country and abroad, so we have had a very active program this past year. Some beautiful results have emerged, and you will hear about some of them during this meeting.

If we take a worst-case situation as presently presented to us, and I will be delighted if Clarence Richardson corrects me when he talks, we will be faced with cutting back our beam time from about 23 weeks this past fiscal year to something like 18 weeks next fiscal year. In addition to doing that we will have to make economies that will reduce in a very serious way the support we can provide to the experimenters. We will also be reducing the maintenance activities, which means that the beam availability will almost undoubtedly decrease during the year. But whatever resources we have, we will try to achieve the best possible physics from these resources.

Frankly, I do not believe that we are going to be faced with the worst-case situation. I believe that Congress will not go along with the 12% cut that the administration has proposed. However, right now it is only a belief and we have to take prudent action to ward off disaster if our hopes are not realized. But even if I believe it, there is still a danger, even assuming that Congress will pass a bill superior to the one that the President has requested. I will tell you what that danger is: that there will be no appropriations bill for much of this fiscal year, that we will operate almost the entire year on continuing resolution, and that under those circumstances we are completely at the mercy of the Office of Management and Budget (OMB) because they can tell us, as they just have, what funds we may have for the next 50 days.

We are going to overrun the first 50 days. Our program managers in Washington can't tell us this is the right thing to do, but neither have they said they will shoot us if we do it. So barring that, I think we are okay. But if it happens another 50 days and then another 50 days, we have a real problem. We can only hope that Congress will pass an appropriations bill, because if they do we are going to be better off than under present circumstances.

To give you the good side of the picture, I must tell you that Congress has a real soft spot in its heart for this facility. I can even prove it to you. Very seldom can one prove such statements.

Figure 2 is taken from the House Armed Services Committee bill, and it has to do with operating expenses for Naval Reactors, Weapons Activities, Research and Development, and Weapons Neutron Research. Just notice, the committee recommendation includes an additional $3M for increased operating costs at LAMPF.

OPERATING EXPENSES

NAVAL REACTORS

WEAPONS ACTIVITIES

RESEARCH AND DEVELOPMENT

WEAPONS NEUTRON RESEARCH.

— The Committee recommendation includes an additional $3,000,000 for increased operating costs at the Los Alamos Meson Physics Facility to increase the number of weeks of operating time available for experimental activities. The Committee's intention is that additional funding should be directly applied for LAMPF operations, not laboratory overhead.

(Taken from the House Armed Services Committee Bill)

Fig. 2.

These funds are in addition to nuclear science funding to increase the number of weeks of operating time available for experimental activities. Now that isn't just for neutrons, but we do provide beam to the Weapons Neutron Research Facility (WNR), and that is, of course, the rationale behind the additional funds. In my entire two or more decades of experience with the Congress, I have never seen them identify a facility in this way and mandate an increase in operating time. What it tells you is that somebody high up really loves you, so don't lose heart.

Because I want to get to the important part of my talk, I am going to go very quickly through Figs. 3-19; it is almost going to be like a movie.

Some of you who work on one channel are not aware, I suspect, of what is going on across the aisle. I think you have to be because we will want your input to your Board of Directors.

Incidentally, I meant to say at the very outset that I don't think I have ever had more pleasure or more profit working with a Board of Directors, and especially a Chairman of the Board of Directors, than in the past fiscal year. It was a marvelous experience for me. Your Chairman spent fantastic amounts of time, considering his other duties, commitments, and travels, worrying about your problems and LAMPF problems. I want to commend you for your good judgment in electing the kind of chairmen and the kind of members on the Board of Directors that you have; they have been a great help.

The External Proton Beam (Fig. 3) is mainly concerned with the nucleon-nucleon problem, but we have a very nice program on photodetachment and photodissociation for H^-. Experiment 449 is a problem of quantum mechanics, a problem that has not been addressable with other means. Hans Bethe is fascinated with this; it is the classic three-body problem and real progress is being made. If you ever have some spare moments for educational amusement, get some of the reports on the spectroscopy of H^- ions using the methods developed here for studying, with millielectron-volt resolution, the dissociation spectrum. These reports are marvelous.

Figure 4 shows experiments on the AB channel. You see again the dominance of the nucleon-nucleon problem. As we talk here this afternoon, things have changed in a good way in our research on the nucleon-nucleon system. We now have two sets of complete data. That is, more than nine experiments have been done that unambiguously determine the scattering amplitudes for the nucleon-nucleon between 500 and 800 MeV. I am looking forward to Hans Bethe's visit here next week because he has been our harshest critic that we haven't been putting enough effort into this very fundamental problem.

Figure 5 shows more experiments on the nucleon-nucleon problem. What has made the research so productive here is that we have been able to use polarized targets and polarized beams with polarization directions adjustable for any given experiment. Once we

EXTERNAL PROTON BEAM (EPB)

Exps.

184 **Measurement of D, R, and A in (p,p) Scattering**

449 **Single and Double Photodetachment Cross Sections of the H^- Ion from 14 to 21.8 eV**

591 **Investigation of Inclusive One-Pion Production in Proton-Nucleus Collisions**

634 **Measurement of Parity Violation in p-Nucleon Total Cross Sections at 800 MeV**

635 **Spin Measurements in pd Elastic Scattering**

(These are abbreviated titles.)

Fig. 3.

AB

Exps.

457 **Measurement of Quasi-Free pn and pp and Free pp Analyzing Powers, 500-800 MeV**

492 **Polarimeter Calibrations and Search for Energy-Dependent Structure in pp Elastic Scattering**

498 **Measurements of $\Delta\sigma_L$ for Longitudinal Polarized Beam and Target, 1. $\Delta\sigma_L(pp)$**

504 **Measurement of $\Delta\sigma_T$ for pp and pn Scattering in Pure Transverse Initial Spin States, 400-800 MeV**

505 **Measurement of Transverse Spin-Spin Asymmetry in $pp \rightarrow d\pi^+$, 500-800 MeV**

512 **Proton-Proton Elastic-Scattering Measurements of the $A_{SS}(\theta)$, $A_{LL}(\theta)$, and $A_{SL}(\theta)$ at 500, 650, and 800 MeV**

(These are abbreviated titles.)

Fig. 4.

AB (Continued)

Exps.

517 Polarized Beam and Target Experiments in the p-p System: Phase I. A_N and A_{NN} for the dπ⁺ Channel and A_{NN} for the Elastic Channel, 500-800 MeV

518 Polarized Beam and Target Experiments in the p-p System: Phase II. Measurements of A_{LL} and A_{SL} for the dπ⁺ Channel and for the Elastic Channel, 500-800 MeV

(These are abbreviated titles.)

Fig. 5.

have done that we have done everything, and if we can't solve the problem with that capability, forget it — it is not solvable.

We are proud of the fact that we love nuclear chemistry. We give nuclear chemistry usually separate, but equal, facilities. Figure 6 shows some nuclear chemistry experiments in Area B. We don't have pions in this channel, so Exps. 349 and 416 used protons for calibration purposes. The nuclear chemists are studying helium-jet techniques for the future — radiochemical studies of fission, etc.

The thin target area is the radiochemists' area (Fig. 7). They study the production and properties of spallation products, and it is in this area that we hope to install a time-of-flight mass spectrometer for our radiochemistry colleagues. Don Kerr has promised to provide, from in-

AB NUCLEAR CHEMISTRY

Exps.

106 Proton-Induced Spallation Reactions

294 High-Energy Nuclear Reactions

349* Nuclear Reactions of ^{127}I with Pions

416* Pion Fission of Uranium

579 A Radiochemical Study of ^{238}U(p,f)X at 500 MeV

629 Helium-Jet Techniques for Studying Short-Lived Nuclei

***Also ran on P³ and LEP.**

(These are abbreviated titles.)

Fig. 6.

THIN TARGET AREA

Exp. 308 An Attempt to Make Direct Atomic Mass Measurements in the Thin Target Area

BEAM STOP A RADIATION

Exps.

161 The Microdistribution of Thorium in Geologic Samples

542 Feasibility Study: Using an Existing Neutron Beam Pipe at LAMPF Beam Stop for Crystal Diffraction Spectrometer Experiments

Fig. 7.

direct funds, $100K of capital equipment so we can start work on this channel and spectrometer.

Also shown in Fig. 7 are some experiments that were undertaken at Beam Stop A.

Figure 8 lists experiments for one of our most productive instruments, the High-Resolution Spectrometer (HRS). As we talk about shutting down channels, we must ask, Where do we save the most with minimal penalty? HRS is also one of the most costly ones to operate, partly because of the power requirements and partly because many experiments use cryogenic targets. which are also costly. But here some beautiful results have been obtained on the nucleon-nucleon problem and, perhaps even more so, on the proton-nucleon problem using polarized protons. George Igo was telling me last night about some experiments he is just now running, looking for asymmetries in proton-in and pion-out on light nuclei, the results of which are fantastic. When you see asymmetries of 90%, you know that has to be an enormous lever for understanding the reaction mechanisms involved.

Remember, all these experiments received beam time during the last fiscal year — some nucleon-nucleon experiments and some nucleon-nucleus experiments. From the nucleon-nucleus and the proton-nucleus, as well as the π^+/π^- nucleus scattering, we hope eventually to obtain neutron form factors in nuclei, one major goal of nuclear physics ever since I have been in the field.

A few more of the experiments that have been under way in the HRS are shown in Figs. 9 and 10.

HIGH-RESOLUTION SPECTROMETER (HRS)

Exps.

- 10 (p,π) Reactions with HRS
- 233 Search for Δ Configurations in Nuclei
- 356 Analyzing Power and Cross-Section Measurements for Inelastic Proton Excitation
- 388 Total Reaction Cross Sections
- 392 Measurement of the Triple-Scattering Parameters for p-p and p-n Scattering
- 399 Excitation of Giant Multipole Resonances by 200- to 400-MeV Protons
- 411 Survey of Spin-Flip Probabilities
- 432 Unnatural Parity States in ^{12}C
- 438 (p,d) Reactions on $^{12,13}C$, ^{7}Li, ^{16}O, ^{25}Mg, ^{28}Si, and ^{40}Ca

(These are abbreviated titles.)

Fig. 8.

HRS (Continued)

Exps.

- 451 Inelastic Proton Scattering at 300 to 500 MeV
- 462 Analyzing Power and Differential Cross Sections for $p + p \rightarrow d + \pi^+$ and $p + d \rightarrow t + \pi^+$
- 473 Giant Multipole Resonances with 800-MeV Protons
- 476 Analyzing Power for $\vec{p} + {}^{24,26}Mg$ at 500 and 800 MeV
- 479 Measurement of R, A, R′, and A′ in Elastic and Inelastic Scattering of 800-MeV Protons
- 508 Dibaryon Resonances in Pion Production
- 556 (p,p′) Process Leading to π-Atomic States
- 563 p + p Elastic Scattering at 800 and 500 MeV
- 580 Cross Sections and Analyzing Powers for Elastic and Inelastic Scattering of 515-MeV Protons from ^{13}C

(These are abbreviated titles.)

Fig. 9.

HRS (Continued)

Exps.

- 585 Measurement of $\bar{p}$-p and $\bar{p}$-d Elastic Scattering in the Coulomb Interference Region
- 616 Spin Rotation and Depolarization Parameters in the $^{12}C(p,p')$ Reaction
- 642 Reactive Content of the Optical Potential
- 648 Asymmetry Measurements of the $(p,\pi^{\pm})$ Reactions on ^{6}Li and ^{9}Be at 650 MeV

(These are abbreviated titles.)

Fig. 10.

Figure 11 shows what is going on in the Low-Energy Pion (LEP) channel. Here some incredibly beautiful results have emerged using the π^0 spectrometer — results that look at giant resonances, including the monopole, in ways that no one has been able to do previously. In fact,

LOW-ENERGY PION (LEP)

Exps.

- 123 Nuclear Structure Effects in Pion-Induced Nuclear Reactions
- 299 $^{12}C(\pi^+,\pi^+ + p)^{11}B$
- 315 High-Resolution Study of $(\pi^+,2p)$ Reaction
- 316 π^--Nucleus Elastic Scattering between 20 and 50 MeV
- 349* Nuclear Reactions of ^{127}I with Pions
- 401 Isobaric Analog Charge Exchange in $^{15}N(\pi^+,\pi^0)^{15}O$
- 416** Study of Fast Pion-Induced Fission of Uranium
- 465 Radiochemical Study of Pion Single Charge Exchange
- 487 Nuclear Resonance Effect in Pionic Atoms

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*Also ran on AB-Nucchem.

**Also ran on P³ and AB-Nucchem.

(These are abbreviated titles.)

Fig. 11.

LEP (Continued)

Exps.

- **523** **Study of $^{14}C(\pi^+,\pi^0)^{14}N$**
- **524** **Isovector Terms in π-Nucleus Interactions with (π^+,π^0) Reactions**
- **541** **Nuclear Critical Opalescence in $^{40}Ca(\pi^+,2\gamma)$**
- **543*** **Product Recoil in the $(\pi^\pm,\pi^\pm N)$ Reaction**
- **544** **Search for a Fast Fission Process**
- **553** **Target Thickness Effects in $^{13}C(\pi^+,\pi^0)^{13}N(g.s.)$**
- **607** **Study of Isovector Giant Resonances with Pion Charge Exchange**
- **650** **Neutrino Mixing via Nonexponential $\pi \to \mu\nu$ Decay**

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*Also ran on P^3.

(These are abbreviated titles.)

Fig. 12.

EPICS

Exps.

- **369** **Inelastic Pion Scattering by ^{17}O, ^{18}O, and ^{19}F**
- **389** **Inelastic Pion Scattering from Light Nuclei: ^{10}B, ^{11}B, ^{14}N, and ^{15}N**
- **484** **Inelastic Pion Scattering from ^{148}Sm and ^{152}Sm**
- **539** **Search for Pure Neutron/Proton Transitions in ^{14}C**
- **549** **The $^{42}Ca(\pi^+,\pi^-)^{42}Ti$ Reaction**
- **570** **Pion Inelastic Scattering on ^{18}O**
- **581** **$\pi^\pm$ Elastic Scattering from Deuterium at 237 MeV**
- **597** **Excitation of the Giant Monopole Resonance by Pions**
- **604** **An Investigation of the Near Stability of ^5H**
- **617** **A Study of the (3/2,3/2) Resonance in Light Nuclei**
- **619** **Inelastic Pion Scattering to 0^+ and 2^+ States in ^{40}Ca and ^{42}Ca**
- **622** **Investigation of the Strong Cancellation of Neutron/Proton Transition Amplitudes in ^{14}C**

(These are abbreviated titles.)

Fig. 13.

I am hopeful that the next CERN Courier will carry an article on π^+ and π^-, involving not only LEP but also the Energetic Pion Channel and Spectrometer (EPICS). This article could show, to a very strong extent, how these probes can be used in essentially unique ways to study and sort out the different transition probabilities, to sort out neutrons from protons, to look in some cases at pure neutron states and in some cases at pure proton states, and also to look at the spin structure of the excited states of nuclei. All these things are made possible by these new probes, which are so specific.

In Fig. 12 we see some more work with the π^0 spectrometer. Experiment 607, on an isovector giant resonance with pion charge exchange, has given marvelous results.

Figure 13 shows experiments using EPICS — another of the big contributors to the nuclear structure and nuclear reaction mechanism work that is going on with π^+ and π^- probes on a large variety of targets. Investigation of the stability of ^5H is an example.

I should also mention, in Fig. 13, work on the possibility of pion condensates. Precursors to nuclear pion condensates have been looked for, using the facilities available in the EPICS channel.

The P^3 channel has been very busy and will be more so (Fig. 14). Pion-beta decay experiments are very important experiments, and one of the large experiments is now being mounted on P^3 that will look with exquisite precision at the Michel parameters from μ decay. This is a classic experiment that is worth doing with utmost precision; we will be doing it as accurately as possible with modern technology and modern techniques. Experiments 400/445 will be done at the Stopped Muon channel (SMC) but used P^3 for calibration studies.

Figure 14, which lists muon-induced fission studies, and Figs. 15 and 16 show more experiments on P^3.

Now, let's go to the SMC (Fig. 17). Here, some very nice things have happened. Remember, a year ago I told you about some experiments we had great hopes for. One of those experiments was to discover fast muonium in vacuum. That discovery was programmed and it has been made. It is a marvelous discovery because it opens up some very exciting possibilities. (I am just picking a few things to talk about; I can't talk about 100 experiments.) For example, if we could measure the Lamb shift in the $n = 2$ state of muonium by using muonium in vacuum, we would probably have the most sensitive test of quantum electrodynamics that we can now devise. In addition, once we have fast muonium in vacuum we can

PION PARTICLE PHYSICS (P³)

Exps.

- **32** $\pi^{\pm} \rightarrow \pi^0 + e^{\pm} + \nu$
- **120** **Measurement of the Polarization Asymmetry and the Differential Cross Section of Pion-Nucleon Charge Exchange from 160 to 500 MeV**
- **123*** **Nuclear Structure Effects in Pion-Induced Nuclear Reactions**
- **154** **Elastic Scattering of $\pi^{\pm}$ from the Helium Isotopes**
- **309** **$\pi^{\pm}$ Double-Charge-Exchange Disintegration of ^{16}O, ^{40}Ca, and ^{208}Pb**
- **349**** **Nuclear Reactions of ^{127}I with Pions**
- **400/445†** **Search for the Rare μ Decays**
- **416**** ***Study of the Fast Pion-Induced Fission of Uranium***

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*Also ran on LEP.

**Also ran on All-Nucchem.

†Also ran on SMC.

(These are abbreviated titles.)

Fig. 14.

P³ (Continued)

Exps.

- **455** **High-Precision Study of μ^+ Decay**
- **465** **Radiochemical Study of Pion Single Charge Exchange**
- **480** **Discrete States from Pion Double Charge Exchange on Heavy Nuclei**
- **500** **$^{237}U(\pi,f)X$**
- **513** **$\pi^{\pm}$ from Helium Isotopes**
- **543*** **A Product Recoil Study of the $(\pi^{\pm},\pi^{\pm}N)$ Reaction**
- **553*** **Study of Target Thickness Effects in the Cross-Section Measurement of the Pion Single-Charge-Exchange Reaction $^{13}C(\pi^+,\pi^0)^{13}N(g.s.)$ from 50 to 350 MeV**

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*Also ran on LEP.

(These are abbreviated titles.)

Fig. 15.

P³ (Continued)

Exps.

- **582** **Pion Absorption Mechanism in the $A(\pi,p)X$ Reaction at $T_{\pi} = 500$ MeV**
- **584** **Small Angle $^4He(\pi^-,\pi^+)$ Reaction**
- **595** **An On-Line γ-Ray Study of Pion-Induced Single Nucleon Removal Reactions on ^{13}C and ^{40}Ca**
- **611** **Excitation Functions of the Four Reactions $^{130}Te(\pi^{\pm},\pi^{\pm}N)$**

(These are abbreviated titles.)

Fig. 16.

contemplate an experiment to look for conversion of muonium to antimuonium. These things are going to be tried.

The SMC is also used for μSR studies; substantial time will be used for rare-decay studies with the Crystal Box, such as $\mu \rightarrow 3e$ and $\mu \rightarrow e\gamma\gamma$. That is another of our very large experiments that is just now beginning to come to life; it is one of our great hopes.

STOPPED MUON CHANNEL (SMC)

Exps.

- **334** **Nuclear Charge Parameters of Cadmium and Tellurium**
- **382** **μSR: Impurity Trapping and Diffusion in bcc Metals**
- **400** **Search for the Rare Decay $\mu^+ \rightarrow e^+e^+e^-$**
- **421** **Search for $\mu^- \rightarrow e$ Conversion**
- **427** **μSR: μ^+-e^- Complexes in Nonmetals**
- **445** ***Search for the Rare Decay $\mu^+ \rightarrow e^+\gamma\gamma$***
- **491** **Strong Interaction Shift in 2p-1s in Hydrogen and Deuterium**
- **494** **Nuclear Charge Parameters of Ruthenium and Palladium**
- **499** **Muon Relaxation in Spin-Glass Systems**

(These are abbreviated titles.)

Fig. 17.

SMC (Continued)

Exps.

547 Search for Fast Muonium in Vacuum

571 μSR Studies of Dilute Magnetic Alloys

594 μ^- Coulomb Capture Ratios in Oxides

639 μSR: Muon Bonding and Motion in Magnetic Oxides

640 Transverse and Longitudinal Field μSR Measurements in Metallic Compounds

646 Hyperfine Structure of Muonic ^{3}He and ^{4}He

653 Muonic X-Ray Study of ^{241}Am and ^{243}Am

(These are abbreviated titles.)

Fig. 18.

ISOTOPE PRODUCTION AND RADIATION DAMAGE

Exp. 267 Preparation of Radioisotopes for Medicine and the Physical Sciences Using LAMPF Isotope Production Facility

RADIATION DAMAGE A-1

Exp. 545 Fusion Materials Neutron Irradiations: A Parasite Experiment

Fig. 19.

Figure 18 shows the μSR experiments being worked on. Of course, muonic atom studies are still an important part of that channel's work.

With isotope production and radiation damage (Fig. 19), we get into the practical application aspects of LAMPF. We are preparing radioisotopes now. I will just show two slides (Figs. 20 and 21) to demonstrate how far we have come in 1 year.

In Fig. 20, as a function of time, is shown millicuries of radionuclides, purified and shipped by Group CNC-3. It is almost a step function here in 1981 in terms of the shipments being made.

Figure 21 shows the number of isotope shipments as a function of years. The number of these shipments is going up very rapidly.

In Biomed (Fig. 22) we count as one experiment the treatment of patients; otherwise we would have a lot more than 106 experiments. In FY 1981, 64 patients were treated, and I am going to tell you that from the standpoint of a physicist (I just heard a talk in Albuquerque by Steve Bush, the principal investigator of the patient treatment project) it looks like this clinical trial is going extremely well. I have rarely seen such unambiguous evidence that a new treatment for severe disease can potentially have such dramatic impact.

Figure 23 shows experiments for WNR. Again we count them as one experiment, but the experimenters at WNR are doing many things; they have a number of independent beam lines. Recently, I heard that they have been looking, with great precision, at water. You would think that water would not give a diffraction pattern — after all, the molecules are moving around in random

MILLICURIES OF RADIONUCLIDES PURIFIED AND SHIPPED BY CNC-3

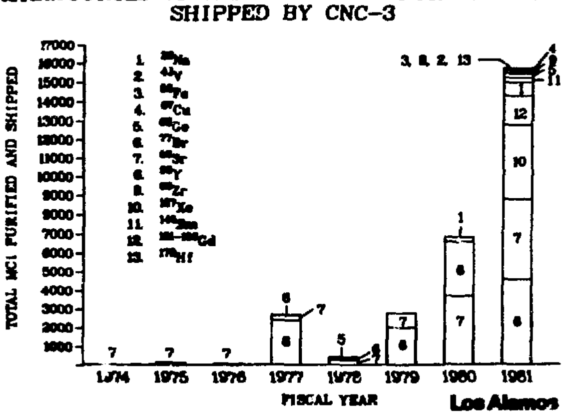

Fig. 20.

TOTAL NUMBER OF ISOTOPE SHIPMENTS

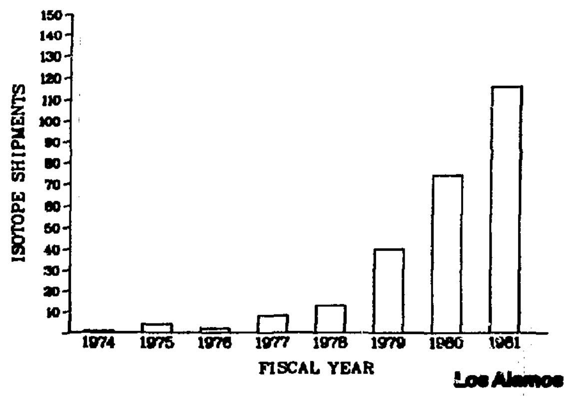

Fig. 21.

BIOMED

(64 Patients Treated FY 1981)

Patient Treatment

Exp. 275 Pion Clinical Trials

Radiobiology

Expa. 236 Biological Effects of Negative Pions

274 Pion Radiobiology

Therapy Beam Development

Expa. 270 Biomedical Channel Tuning

271 Dosimetry

272 Microdosimetry

(These are abbreviated titles.)

Fig. 22.

WNR

(Runs ~6 Experiments Simultaneously)
[Experiments by P-8 (Condensed Matter Physics)]

Inelastic Neutron Scattering

Instruments:

Crystal Analyzer Spectrometer
Filter Detector Spectrometer

(1) Hydrogen-Metal Interaction in Metal Hydrides
(2) Molecular Vibrational Spectroscopy

Elastic Neutron Scattering

Instruments:

General Purpose Diffractometer
Single Crystal Diffractometer
Special Environments Diffractometer

(1) Liquids
(2) Amorphous Materials
(3) Powder Diffraction
(4) Single Crystal Diffraction

(These are abbreviated titles.)

Fig. 23.

ways — but that is wrong. In some respects water has some of the attributes of a crystal. In other words, there are correlations there that have just not been anticipated — a quite amazing result. I know you are perhaps not interested in that branch of physics but you ought to know a little about it.

Figure 24 shows some other experiments being done at WNR; activities there are really moving in a good way. Dick Silver and his colleagues have, in the past year to a year and a half, done a marvelous job of bringing that facility to life; it is a very powerful one.

Now I come to the important part of my talk. In a way the theme of this particular meeting is, "What's in the Future?" Now, there is the immediate future and there is the long-term future. The immediate future I don't have too much problem with. First, I looked at what lies ahead in the experimental program. It ought to be possible to maintain a very exciting program for the next 5-10 years — certainly for 5 years, and it will be an amazing accident if something doesn't emerge in those 5 years to lead us to even more exciting things. I just believe these exciting things will happen. But we aren't banking on them happening. Five years from now will come on line the world's best facility for solid-state physics with pulsed neutrons and perhaps some kind of neutrino experiments. That's the Proton Storage Ring (PSR). The PSR seems to be funded and is reasonably on schedule. In addition, we are now building a very fine

WNR (Continued)

(Experiments by P-3)

1. **Parity Violation in Thermal Neutron Capture by Hydrogen (Feasibility Study)**
2. **High-Resolution Fission Cross Sections in ^{237}Np**
3. **High-Resolution n,p Total Cross-Section Measurement**
4. **Measurement of Magnetic Moments in Compound Nuclei ($n + {}^{161}Dy$ and $n + {}^{163}Dy$)**
5. **Bismuth-Germanate (BGO) Detector Characterization for use in Neutron Capture Gamma-Ray Studies**

(Experiments by P-9)

1. **Fertile-to-Fissile and Fission Measurements in Thorium and Depleted Uranium Bombarded by 800-MeV Protons**

Fig. 24.

addition to Area A, which will be finished in about a year. We are completing the data-analysis center, based mainly on VAX computers; that center also will be a very powerful facility. So these are the short-term activities that I feel confident will ensure the viability of LAMPF through this decade.

But what happens after that? To discuss that with you I must remind you why LAMPF was built in the first place. Some of you will remember that the argument that persuaded the then Atomic Energy Commission (AEC) and two Presidents and the Congress to build LAMPF was that it was necessary to provide assurance of a high level of accomplishment in nuclear science and nuclear technology in this country. It was necessary because the Congress at that time — and most of the Congress still feel — that a nuclear economy is unavoidable. Now, I know that irrationality on the part of segments of the public and irresponsibility on the part of a segment of private industry have combined to make the building of a nuclear economy extremely and excrutiatingly difficult. And if there were alternatives I would myself be inclined to say, "Gee, let's give it up for awhile." But there are no alternatives. There just aren't any. Fossil fuels in general and coal in particular have fundamental difficulties — most of them long-term difficulties — that make it essential that we look as hard as we can, as quickly as we can, at reducing the burning of fossil fuels, and especially the burning of coal.

So as I see this picture, there is no choice if we are to have any hope of surviving in a reasonable fashion. There is no choice to a nuclear economy, and if we are going to have a nuclear economy, we'd better have nuclear science, we'd better have nuclear technology, we'd better have the people who can safely monitor, manage, operate, and maintain that nuclear economy. That was the argument that prevailed 20 years ago.

In addition, there were subsidiary arguments to the effect that this laboratory is a great national resource; it is a critical element of the national security of this country, and we must take pains to maintain the viability of this laboratory so that it can address severe national problems whenever they arise. That argument was very persuasive, especially with the AEC General Advisory Board. I remember Wigner being on that committee and I remember testifying to that committee. His reaction was that he personally couldn't get so excited about medium-energy nuclear physics, but he was concerned about the vitality of Los Alamos, and for that reason he was inclined to support the philosophy that we should have as good a nuclear science facility as the world could provide. So the arguments revolved around improving the knowledge base in nuclear science and increasing the people base. Those arguments are still valid today, and if we believe there is going to be a future those arguments will be even more valid in the 1990s. You know, we all have our limits. But it is my perception that it is our duty to look ahead and ask what should the country be doing the next decade.

For some years we've had a workshop about every year about the future of LAMPF or LAMPF options. Some of these have been internal. There are reports on these workshops. Invariably, these workshops have identified — and so have the National Academy Committees, on one of which I served for a year and a half — that as a sequel to LAMPF we ought to consider injecting the LAMPF beam into a higher energy accelerator but with very high intensity to make the kind of beams for the kind of purposes you'll hear about today. That's one possibility. The Academy reports and other reports have identified that we must also look at electron machines and at heavy-ion machines.

But now I want to make a very important point. I went through very quickly, one after another, the various channels we operate at LAMPF, essentially simultaneously and essentially independently. We cannot do that with an electron facility; we cannot do that with a heavy-ion facility. If we want to accommodate a large program involving many investigators and many graduate students, I see no alternative to using a proton beam under some conditions. Very high energy, it's a possibility; very high intensity, that's the way we have gone. I once remember meeting with some OMB people, after which Schultz (some of you know him, a very high figure in the OMB) summarized the meeting. He said, "Well, if we believe Rosen, we have come away from this meeting as follows: energy — no!, intensity — si!" That was his interpretation of the results of that meeting.

Well, there are two frontiers — there is the energy frontier and there is the intensity frontier. I think each has its merits, each has its disadvantages. We chose intensity because we felt it had more direct application to the nation's immediate problems, and I think it does. And I think that is, to some extent, what is giving us the very good image we now enjoy in the Congress.

Well, what should we do? You know, there's only a certain amount we can do from Los Alamos and we're going to try to do it. We're going to try to provide the climate and the leadership for you to get involved in determining the future of nuclear science in this country from the standpoint of where LAMPF ought to be going.

I would urge you to consider very strongly involving yourself to the extent of determining what the options are and then determining whether you want to participate in one or more of these options. No matter what they do to our budget this fiscal year, we're going to make available some resources for bringing people here for workshops, for meetings, and for visits, to discuss in depth what we should be thinking of doing the next decade.

I should also tell you that about a year and a half ago I put in an item in the Laboratory long-range plans just to test the water. For want of a better word I said "kaon" factory. I think that's a bad word; I think we should somehow tie our next initiative to LAMPF because Congress likes LAMPF. Call it LAMPF Prime, LAMPF II, Super LAMPF — I don't care, you decide. But keep it tied to LAMPF for political reasons. We think, those of us here, that the time is now. On this item of the kaon factory I put a price tag of something like $150M. I advocated that they start providing funds this fiscal year to plan for such a facility. This particular element of the long-range plan hit Washington like a high-explosive missile and we got a reaction. And the reaction was "premature" and "under no circumstances can you expect funding for such an initiative before FY 1986." Well, that's okay. They didn't say, "Don't ever come back; we don't want to see you." Since then I have had conversations with our program managers in Washington — Richardson, Ritter, Liess — and they all say, "Look, we don't have any money for you but we think it's proper to do some planning, and we will not object if some of the funds we allocate for operations are used for planning and developing a proposal, which then we would certainly like to consider." So, the reaction is one I would characterize as, not negative. I won't go any further than that.

You will hear about some of the activities that are going on right now. My message to you is that now is the time to get involved if you intend to get involved, because if you wait 2 years it could be too late. A facility such as LAMPF has to get better and better with time. Once it stops improving it starts dying. We're going to be getting better and better through this decade, but after that I have serious worry and I think we need to plan for that decade.

Well, I think I've overextended my time and certainly my voice, but if the chairman permits and feels there is some time for questions, I will try to answer.

LAMPF OPERATIONS REPORT

Donald C. Hagerman, Chief of Operations
Los Alamos Meson Physics Facility
Los Alamos National Laboratory

It's a pleasure to report on the operation of our facility for the past year. We have serious problems, but if we face them in a realistic fashion we'll come through the next few years in reasonable style.

First, let's look at a summary of what happened at our facility last year (Fig. 1). For the four cycles, note that we reduced the cycle length about halfway through the year. This came about because of a change in the requirements of the biomedical program; the biomedical group is now satisfied with cycles about two-thirds the length of the ones we have used for the past few years. I think this has improved the usefulness of the facility because it gives us a lot more flexibility in switching between experiments.

The availability of the H^+ beam has been very good, above 85% on the average for the past year. That's about as good as it's ever going to be and is satisfactory for most of our Users.

The polarized source continues to improve; we frequently reduce the source intensity, which produces slightly better polarization. You will note that with the unpolarized H^- beam, however, there have been a few periods of bad availability, occurring at the start of the cycles. These statistical fluctuations come about because the use of the H^- beam is tending toward zero. The nuclear chemists are a bit unhappy about that, but that problem is the Program Advisory Committee's concern.

There are two major disappointments that we should mention. The first is the reduction in the total amount of beam time that we were able to produce during the past year and the second is the continued slip in increasing the intensity of the H^+ beam. Two or three years ago we decided that we should emphasize production even at the expense of raising the beam current. We had hoped to be running at ~750 μA by the time of this meeting but that has not been possible.

Figure 2 displays one aspect of the continuing emphasis on production, listing just a few of the engineering-support activities essential in the experimental program. Experiments 455 and 400 required relatively large amounts of support, Exp. 546 required careful attention to safety problems, and Exp. 539 presented a novel problem involving a special ^{14}C target.

We must keep on improving the facility. Figure 3 shows a list of some of the improvements we have recently made or are considering.

FY 1981 OPERATION

Cycle	Cycle Length (h)	Availability H^+ (%)	Availability $\vec{H}^-$ (%)	Availability H^- (%)
28*	1304	87	70	68
29	1264	84	84	51
30	898	86	79	84
31**	815	89	82	----

*Cycle 28 spanned FY 1980 and 1981.

**Cycle 31 spanned FY 1981 and 1982.

Fig. 1.

ENGINEERING SUPPORT OF EXPERIMENTS SPECIAL PROBLEMS

Exp. No.	Activity
455	150-ton Perle-magnet modification and installation, pole-handling fixture, TPC* engineering support
400	Crystal Box — NaI detectors, drift-chamber development, support system
546	Sealed spherical tritium gas target
539	^{14}C targets with sintered breathing plug

*Time-projection chamber.

Fig. 2.

1. The new transition region, which will improve the availability and control of the H^- beams, will be installed during a shutdown about a year from now.
2. The new control computer is appearing — the hardware is here — and major system decisions have been made. Within another month or two we will be able to measure some of the machine parameters through the new computer, and by next summer we will be able to use it for at least some aspects of facility control.
3. The Proton Storage Ring (PSR) is worth emphasizing because it really is a synergistic effort, the joint activity of P, AT, and MP Divisions. Not only are we assured of another major user for our beam, but we also are learning more about our facility as a result of the new requirements. As an example, we have expended significant effort on machine steering during the last six months because of the PSR requirement to handle simultaneously high-intensity H^+ and H^- beams. One result of these steering studies is that a year ago we were willing to run H^- beams only as low as 300 MeV; now we can say with confidence that we can run our dual-energy operation as low as 212 MeV.
4. We keep working on the on-going problem of reducing the operating costs of the facility. For Cycle 32 we are using a scheme to reduce the phase acceptance for a portion of the machine, thereby cutting the rf power required. It's only a few-percent reduction in rf power, but that reduction is significant because the power bill is now at the $4-million level per year.

Operation continues to improve in the experimental areas (items 5-8, Fig. 3).

5. During the past several years there has been a lot of trouble with the High-Resolution Spectrometer (HRS) scattering chamber because of many vacuum leaks. As a joint project between the accelerator maintenance group and the spectrometer group, a new scattering chamber was installed during the past year that completely cured the vacuum problems.
6. A focal-plane polarimeter, which was just coming into being at the last Uers Meeting, is in routine use at HRS.
7. Satisfying the continuing need for remote handling in Area A has been made easier by the installation of a second remote-handling device, which gives us the capability to work on two jobs simultaneouly and increases shutdown efficiency.
8. Improved flexibility of the EPICS program has resulted from the installation of a cooled-gas target.

FACILITY

UPGRADE/DEVELOPMENT

1. New transition region
2. VAX control computer
3. PSR-related activities
   a. High-intensity H^- source
   b. H^- low-energy transport
   c. Revised machine steering
   d. Revised switchyard
4. Power reduction via alternate tuning schemes
5. New HRS scattering chamber
6. HRS focal-plane polarimeter in routine use
7. Facilities available for two simultaneous remote-handling jobs
8. Cooled-gas target in routine use at EPICS

Fig. 3.

CONVENTIONAL CONSTRUCTION PROJECTS

Project	Status
Staging area/Area A addition	In construction
SMC Counting House	Complete
MP-7/ -13 Shop-Lab Building	In construction
Thin Target Counting House	Complete
Remote-handling addition	Design complete
CCR addition	Design complete

Fig. 4.

Improvements such as these must continue for LAMPF to remain a viable facility.

Another aspect of keeping a viable facility is continuing a fair amount of conventional construction (Fig. 4). Louis Rosen mentioned our staging area. It's worth reminding you how long it takes for one of these projects to come into being. We first asked for that staging area in either 1972 or 1973. It's been a long time coming, but Area A will be greatly improved. Completed is a new counting house for the Stopped Muon Channel (SMC), which will release valuable floor space in Area A, and a new counting house for the thin target area. Groups MP-7 and MP-13 need more shop space; that construction is under way. Planned for the future is a remote-handling addition and more space at the Computer Control Room (CCR) to fully implement the control computer conversion.

Now, let's go back to the question of production hours. Figure 5 shows a history of production at LAMPF for the past few years. It is very clear that we reached our peak production hours in FY 79. We don't know how many hours we can run in the next year, or even the next few months, because we do not have firm budget information. A production plan for the remainder of the year will be made as soon as that information is available. We should not extrapolate this figure to predict the future because it is far too easy to predict zero beam hours about three years from now.

LAMPF — H^+ BEAM ON TARGET

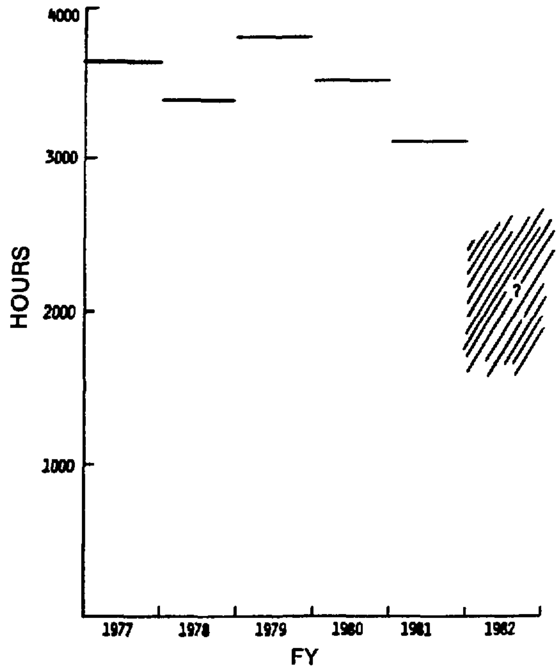

Fig. 5.

We have all expended an enormous amount of effort on this facility and we should not be giving up now. In the past we had to learn how to handle high-intensity beams and to satisfy many Users. The challenge of the future is more complex. We must continue to maximize the output of high-quality physics (it would not do physics nor LAMPF any good if we would simply run and turn out routine and mundane data), and we also must keep the facility in a viable operation. We must do these things even in the face of severely constrained budgets. If we succeed we will have an adequate reward from the investment we have made in the facility and we will have laid the foundation for some of the very nice things to be discussed next in this meeting by Gerry Stephenson, Darragh Nagle, and Arch Thiessen.

PROGRESS REPORT ON A PROPOSAL FOR A LOS ALAMOS NEUTRINO FACILITY

Gerard J. Stephenson
P Division
Los Alamos National Laboratory

As will become apparent in the course of the next three talks, there are a number of interesting possibilities under consideration here at the Laboratory for those things that we should now be planning for the future activities of this overall facility. In that regard, you must understand that I am talking about not a final version of a proposal that the Laboratory is certain to make, but rather a progress report from a working group charged with producing a neutrino facility proposal suitable for submission to the Department of Energy (DOE).

I would like to remind you of how this interest has come about and grown over the years. It has always been known that an intense beam of protons on a target, producing pions, can produce sufficient fluxes of neutrinos to do many experiments, hopefully precision experiments, that can't be done otherwise. Neutrino cross sections grow with the neutrino energy, so there has always been a question of the interest in neutrino physics at low energy even if there is a very large neutrino flux. These questions have been addressed in several workshops, beginning, I believe, with the LAMPF Program Options Workshop a little over two years ago. They were addressed again last winter in the discussion on physics up to 31 GeV and, more specifically, at a workshop* held this June.

It was concluded at all of those workshops that there are certain features of neutrino physics that are very interesting if one can do them with high intensities and at low energies. Summarizing the physics results of the workshop in June, I can say that the experiment that was chosen as most interesting to be done in the near future was the study of possible oscillations of muon neutrinos — specifically, the disappearance of muon neutrinos into any other channel. It is of interest to do these experiments at low energies because one is translating a time dependence into a distance dependence. As the energy increases, the Lorentz time dilation also increases, and one needs to go to greater and greater distances to get the same sensitivity to the basic time parameter desired.

A second set of experiments considered extremely important has to do with precision measurements of the Weinberg angle, which can be translated to precision measurements of the structure of the neutral currents (the bosons that mediate the neutral currents). Those measurements would correspond to, preferably, a purely neutral leptonic interaction, namely, ν_μ-e elastic scattering. If that could not be done because of the small cross section, then the next best thing is ν_μ-p elastic scattering.

Beyond that, a number of experiments were recommended to try with various nuclear targets. These experiments would allow one to study the isotopic structure of the weak currents, both charged and neutral. To do tne elastic scattering of ν_μ on electrons, or to do the various nuclear experiments, it is necessary to suppress cosmic backgrounds, and to suppress cosmic backgrounds it is necessary to compress the neutrino pulse. That means, if possible, to generate the neutrino pulse with protons that are the output of a ring taken with a very short spill mode. Darragh Nagle and Arch Thiessen discuss in their talks various possibilities of some such rings.

The workshop this summer was charged first with identifying such physics and then identifying those characteristics of a facility that would be needed to do such experiments. Also, a proposal-writing group was appointed by the Laboratory. There are seven of us from inside the Laboratory: Dick Cooper (AT Division); Bob Macek, Bob Burman, and Lew Agnew (MP Division); Tom Bowles and Tom Dombeck (P Division); and myself as chairman. We are joined by Felix Boehm and Bob McKeown (California Institute of Technology) and Peter Nemethy (Lawrence Berkeley Laboratory). Our group is charged with producing a proposal that can be sent to DOE this winter. The results will be discussed within the Laboratory with input that we have from the Users in light of all the proposals coming in.

*A draft report of the Los Alamos Neutrino Workshop, held June 8-12, 1981, with F. Boehm (California Institute of Technology) as Chairman and G. J. Stephenson (Los Alamos) as Assistant Chairman, was available to attendees of the LAMPF Users Group, Inc., meeting.

Starting from the workshop, we have been thinking of a facility at the end of Line D that would use the Proton Storage Ring (PSR) to provide the capability to compress the neutrino pulse. Most of what we are addressing is not tied to that as an option and it should be realized that, beyond the PSR, any future upgrade of LAMPF will have to provide for neutrino physics for part of its justification. In our working group we are trying to keep all of those options as open as possible at this point. Nonetheless, because neutrino physics has been designated as one of the things that can be done and should be done at LAMPF, we are moving toward such a proposal with actual cost estimates.

The workshop discussed certain features of a facility design. Things that were mentioned were the need for the ability to look at neutrinos going forward from a decay volume, therefore providing ν_μ neutrinos that have enough energy to make a muon and to give a clear signal, as well as the possibility of doing beam-stop experiments, where one views the beam stop at 90°. The above immediately calls for at least two detector halls, one at 90° to whatever beam is being used, and one in the forward direction. The discussions began with the notion that the oscillation experiment is one of the most important that we can do at this time, hence the desire to be able to vary the detector-target distance.

There was an existing design that called for moving a target through a beam tunnel; however, the workshop deliberations quickly pointed out that that would not be economically feasible to transport the PSR beam. A design was developed that had about a 260-m tunnel, through which a 100-ton scintillation detector could be moved, and tnat included the necessary beam transport to carry the output of the PSR south on the mesa from the end of Line D. The beam would then be transported around a 40° bend, which was put there so the detector tunnel would fit on the mesa without falling off the end. This design had a second detector house.

Now the DOE has asked us to submit a facility proposal that includes a detector. That isn't always done, but in these times I know that the DOE wants to hear the total numbers up front, which means including a detector. To do ν_μ proton elastic scattering it is necessary to be able to track the recoiling proton. For 150-MeV neutrinos, which is a reasonable energy to take, about 33- to 35-MeV recoiling protons are produced. They do not track very far in a scintillator. That leads to very fine granularity for a detector and also to a rather expensive design for a detector.

(I should comment here about some differences in the designs of the facility from that described in the draft report from the workshop. In that draft report a set of design parameters was picked to compare different flux calculations. Those parameters, which involved a 30-m decay path and 20 m of tuff shielding before the detector, are optimized for a proton beam that is considerably higher in energy than 800 MeV. The numbers we use now are for a 12-m decay path and 9 m of steel shielding, so it is only about 20 m to the beginning of the detector, which is much more nearly optimized for 800 MeV.)

The Laboratory has to respond to the DOE in a timely fashion to have things move on its calendar. Hence, an information form, called a short form 44, was sent to Washington about the beginning of September, estimating the cost of a design that has all the features called for in the workshop, including a 50-ton detector. As best as we could estimate in terms of 1981 dollars, it came to approximately $17 million. We then subjected it to the necessary massaging by people who know how construction projects are delayed and expanded upon, and by the time it left the budgetary and engineering sections of the Laboratory the number was $40 million for FY 1984 funding.

You may hear that number often, which was the result of our work over the summer, trying to absorb what we had heard from the workshop and estimating what it would take to do all the things that were suggested in the workshop. Since then, driven in an obvious direction by that number, we have been studying some of the possible changes that we might want to make.

The first point I should make was already inherent in the work by the nuclear physicists who discussed various cross sections at the workshop. One of the two experiments that we really want to design into an initial capability is ν_μ elastic scattering, either off electrons or off protons. We began with protons because the cross section is bigger. However, if we try to make a scintillator that is loaded with protons to do the elastic scattering, and it is made out of plastic, which seems natural, we also load it with carbon. The quasi-elastic scattering of neutrinos on protons in carbon gives a cross section that is essentially equal to the cross section for the elastic scattering on the protons. Therefore, although we could in fact see protons from a sum of elastic and quasi-elastic scattering, we could not do the kind of analysis with respect to momentum transfer that is required to extract precise information about the Weinberg angle.

Given that, the requirements of granularity are relaxed. As a matter of fact, the observation was then made that for about the same cost — $10 million or so — we could build a 1000-ton aluminum detector that has wire chambers.

During the workshop, concern was expressed that, *when we try to make a muon from a* μ *neutrino with a* charged-current interaction, we pay a fairly large price in Q value. We obviously have to make the *muon*, but worse, we have to convert ^{12}C into ^{12}N. As a matter of fact, that means going to the quasi-elastic region and therefore quite a ways up in excitation of ^{12}N. Hence, it costs somewhere between 20 and 30 or maybe even 40 MeV to get to the region of the final-state nucleus where there is a large amount of phase space. When slightly above threshold, theorists are unable to uniquely specify what the particular Q value is, which leads to a large uncertainty in the expected cross section. A real advantage in going to some higher Z material, like aluminum, is that the nuclear Q value drops by a good 15 to 20 MeV, the cross sections then are much better known, and it is possible to calculate with a great deal more confidence what the actual event rate will be in such a detector.

Now, a 1000-ton detector makes it possible to do several things quite differently. We can now look at the elastic scattering on the electron with muon neutrinos and make a precise measurement of the Weinberg angle. We also can now contemplate doing oscillation experiments at a somewhat greater distance. Remember that I am comparing a 50-ton detector with a 1000-ton detector, so that means there is somewhere between a factor of 4 and 5 that I can find in the maximum distance. Once we think about that, we realize that rather than moving this thing in a tunnel we might want to locate it on another mesa.

Those considerations are now in progress. They would allow us to shorten the beam transport and make it much simpler. Our best estimates, keeping most of the rest of the facility features in place, are that the facility would drop to somewhere in the range of $13-17 million in FY 1984 from the $30 million included in the September estimate. This is still assuming approximately $10 million in a detector. That leads to something more like $25 million instead of $40 million.

I am very vague here and the reason I have not put up a Vu-Graph is that these numbers are being worked out at the moment. I am delivering them to you for your information so you can have some sense of where we are going, but they are not firm enough yet, I believe, that they can be written up here starkly to be copied down as if we had already done all the engineering. As a matter of fact, while I was at Asilomar last week, I understood that various possibilities have been investigated that can in fact drop the cost of the detector as well. Of course, we can always go to a mere 500-ton detector and presumably save about half the cost of the detector.

One of the features that comes from breaking away from a concrete tunnel, which fixes where a detector must move, is that we have removed the requirement that the neutrino flux go down a particular line. This makes it much more possible to integrate such a neutrino facility with some of the plans discussed by Darragh and Arch.

So, I would say that a great deal of progress has been made. The internal working group meets at least once a week; the external members have stayed abreast of what we have been doing and have made very useful and cogent suggestions as we have been developing these plans.

We will continue to try to refine a proposal for a facility that can be used in conjunction with Line D and in conjunction with the PSR when it comes on, but that can also be considered as a piece of whatever higher energy proposal we are trying to generate for the future of LAMPF. There are reasons to want to do some of the physics soon, that is, the part of the physics that we can do and ought to do during this decade. It is also true that neutrino physics is part and parcel of the more long-range program Arch and Darragh discuss in their talks. We have to continue working with them very carefully to make sure that we do not preclude any sensible options to do neutrino physics later, including the possibility that it may in fact prove better in the long run to locate the facility elsewhere to match whatever future machine is generated.

PROGRESS REPORT ON A KAON FACTORY

Darragh E. Nagle
Los Alamos National Laboratory

I would like to add a few historical remarks to what Louis Rosen told you about the influence of Jerry Kellogg on accelerators at Los Alamos. Most of what I want to say relates to I. I. Rabi, who was Kellogg's teacher and mentor. Although Rabi did his Nobel Prize-winning research with particle beams of energy 1/40 of an electron volt, he nevertheless understood extremely clearly the need for very advanced accelerators for physical science in the postwar years. Rabi essentially told Jerry Kellogg that accelerators at Los Alamos were necessary if this was to be a real physics laboratory. Rabi also was very influential in persuading Europeans to create the CERN laboratory and so his influence should be remembered.

The history of proposals for intense proton accelerators in the range of 10 to 20 GeV goes back about 25 years. Fermi, shortly after the war, was one of the first to make charts to show the energy of accelerators vs time, and he showed the energy of accelerators going up exponentially with time — reaching cosmic-ray energies, perhaps in this decade. He also showed the diameter of these accelerators increasing with time; the limiting point on his chart was the diameter of the earth. How perspicacious a prophet Fermi was should strike us when we think about the LEP project now being discussed and perhaps activated at CERN. There would be a tunnel which would start near the French-Swiss border and go completely under the Jura Mountains and come out on the other side. Figure 1 shows the relative sizes of LEP, SPS, LAMPF, and our proposed synchrotron. Fermi also said that the intensity of accelerators would gradually decrease with time until, when we reach cosmic-ray accelerators, we would be down to perhaps one particle per hour. In that prediction he was, of course, completely wrong. Accelerators have increased in intensity and in power, but I don't have time to go into all that history with you. Let's recall that 20 years ago there was the old MURA project, which was a 10-GeV, strong-focusing, fixed-field, alternating-gradient accelerator with proposed capability of about 100 μA. Accelerators we are now contemplating here have about two times the power of the old MURA machine, and the cost in any real terms is about 1/30 or 1/50 the cost of building that old MURA machine. There was another project 20 years ago to build a superconducting proton linac. That project was abandoned because the technology of those days was inadequate for the task, and I think probably today is still inadequate for the task. Now we have a proven accelerator concept which will lead to very high intensities in the 10- to 20-GeV region. We can have up to 4 MW of beam power — power in the beam as required. The concept, of course, is the rapid-cycling synchrotron, and our confidence in success stems from the proven success of the Fermilab booster synchrotron. It was Bob Wilson, in 1975, who pointed out to me the extreme suitability of that type of design for a machine that could make intense beams of kaons, neutrinos, and all the other particles that we know and love. The other thing I wish to remind you of is the unequaled capability of the LAMPF linac as an injector for such a machine.

The excellence of our machine as an injector stems from several factors. Note that the Fermilab booster runs at about 8-μA average current. The higher energy of LAMPF compared to the Fermilab linac turns out to imply a factor of 8 times the intensity in protons per pulse accepted by the synchrotron. So if you ran the Fermilab booster with our injector, you could get 64 μA. If you simply double the rep rate to 30 Hz instead of 15 Hz, you'd be well above 100 μA, which is a reference design goal. Physically, we can understand this very easily. The current limitation in a big proton synchrotron is mostly at injection; when the injected beam is a very high current beam, it simply starts to perturb and destroy or change the strong focusing forces that keep the beam together in the transverse directions. The accelerator scientist describes it as a shift in the tune or frequency of the betatron oscillations of the beam caused by the perturbing fields, and you know that in synchrotrons you must avoid the numerous resonances that occur when the betatron oscillation frequencies go through certain numerical values. It's that requirement that makes us interested in tune shifts. Because the beam is stiffer as you raise the injection energy, the tune shift becomes smaller. Or you can say the injection magnetic fields are larger if you have a higher injection energy, and so the perturbing fields are smaller as a fraction and the tune shift is smaller. That's the physical origin of the factor of 8. In

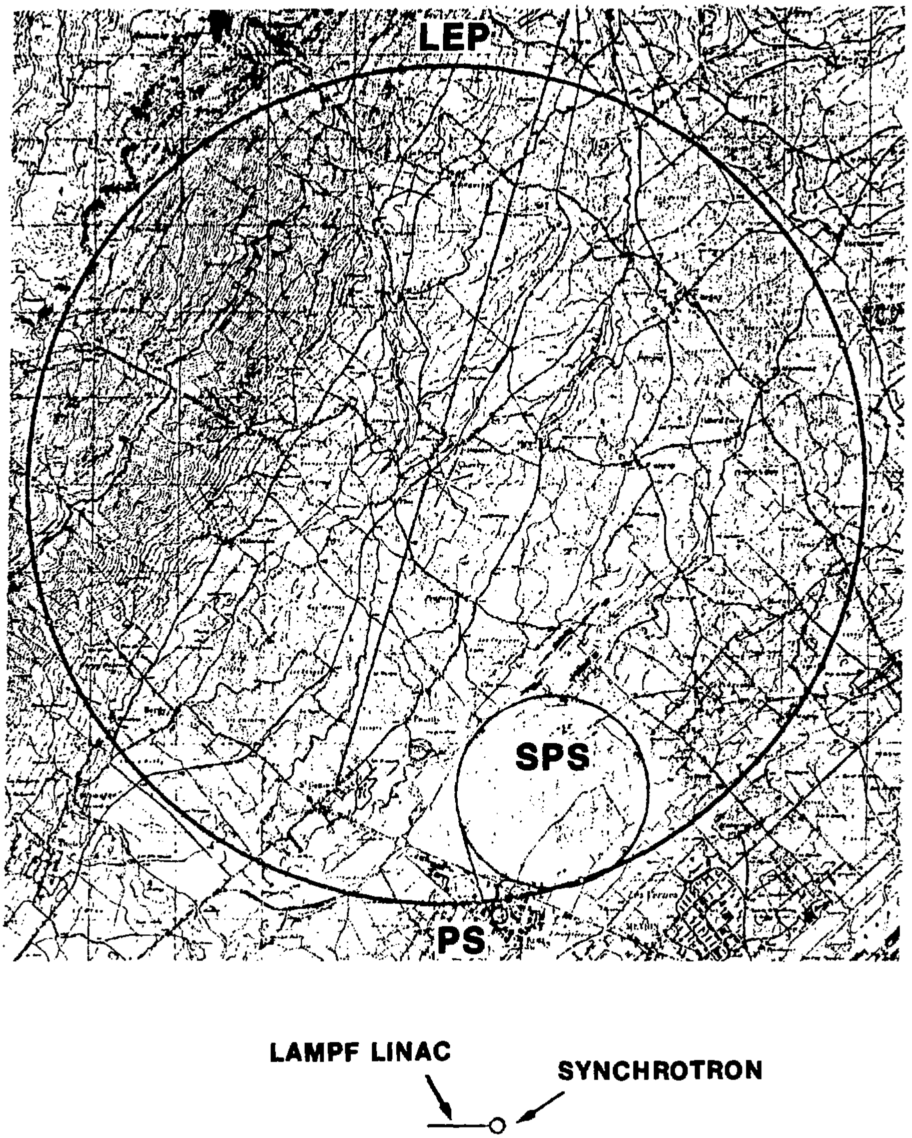

Fig. 1.
A possible location of the underground LEP ring superimposed on a map of the CERN Laboratory region on the Franco-Swiss border on the outskirts of Geneva. The large size of the machine, 30 km in circumference, is apparent in comparison with the existing Super Proton Synchrotron (SPS) which is also drawn. On the left, LEP passes under the Jura mountains. In comparison, at the bottom of the figure, the LAMPF linac and the proposed synchrotron are shown to the same scale.

addition, LAMPF has an excellent transverse beam quality. That means that the beam is almost a pencil beam. It has a small divergence, and that implies that you can either have a very intense beam or you can have a small aperture for the synchrotron ring. Either one of these has its advantages. The higher energy of LAMPF also means that the frequency excursion needed in the rf cavity for the synchrotron is five times less than at the Fermilab booster, and that results in a large saving in the cost of the rf cavities and in the power losses. We've known all this for some time. What we have come to realize in the last year, thanks to the efforts of several people (among them Thiessen, Macek, Agnew and others), is that we can use virtually all of the existing LAMPF experimental buildings, areas, shielding, and in many cases, secondary beam magnets to eliminate the need for constructing a whole new experimental area. Arch Thiessen will give you further details on this. We can imagine in ten years' time the physical appearance of the LAMPF site will not have changed much, but the kinds of activities will have changed, and that's what I would consider very healthy growth.

I would like to touch on some organizational and planning activities that went on during this year. In January of this year we held a workshop on "Nuclear and Particle Physics at Energies Up to 31 GeV." In February a committee was formed to study the future of LAMPF. Peter Carruthers is the chairman of the committee. The committee deliberated all spring and came out with a report that an intense proton facility at 16 GeV was needed to secure the future vitality of LAMPF. In March, Jim Potter and I made a visit to Fermilab to discuss the Fermilab booster and its problems with their staff. Their staff was enthusiastic about the design concepts that we had. They thought it was a sweet machine. During the spring, planning went on for mounting an experiment at CERN to measure the yields of pions, kaons, and antiprotons as a function of proton energy. The experiment was a collaborative effort among TRIUMF personnel, people from LAMPF, and people from CERN, and it was successfully mounted in June of this year. In May we formed a LAMPF II steering committee, chaired by Ed Knapp of AT Division and myself. Late in the summer there was a TRIUMF workshop on kaon factories to which some of our people went. Last month we formed a Synchrotron Working Group with Arch Thiessen as chairman.

Figure 2 presents the results of the experiment performed at CERN this summer. The main feature of the graph that I would like to have you appreciate is a very

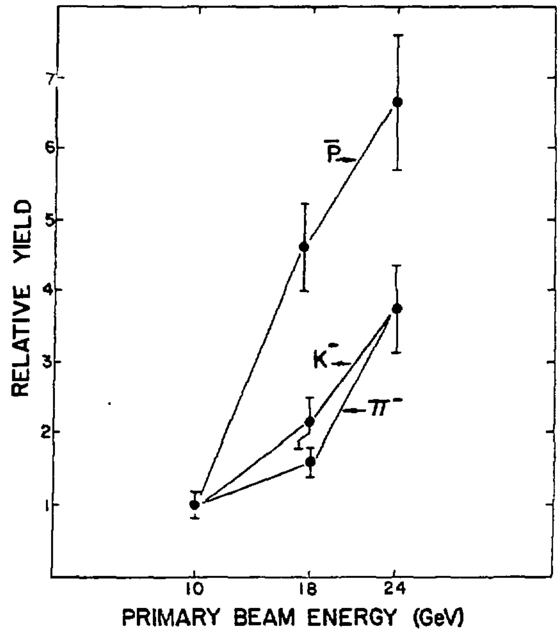

Fig. 2.
Preliminary results of the Los Alamos-TRIUMF-CERN measurement of yields of π, e, K, and $\bar{p}$ vs primary beam momentum.

rapid rise in the production cross sections of kaons and pions vs energy. This is a definitive experiment, just as an early experiment of Cochran et al. served for many years as the definitive experiment for pion yields at much lower energies.

I now show some results of a recent study by Dick Cooper of AT Division of a possible 16-GeV rapid cycling synchrotron. The machine has a 94-m average radius of curvature. The radius of curvature in the bender magnets is just half that. The rest of the circumference of the machine is devoted to straight sections for the rf cavities and diagnostic equipment and focusing magnets. Figure 3 shows the same lattice. It is a lattice that has 36 periods. It means the whole magnet system has a symmetry of 36. The viewgraphs show the envelopes in the transverse direction, X being the radial direction and Y being the vertical direction. In the machine you see that the beam envelope in the Y direction is less than 2 cm and in the X direction less than 3 cm. It is a quite tightly compressed beam. The D means defocusing quad, the B is a bender, the F is a focusing quad, B is another bender, D is a defocusing quad, and there is a straight section with a focusing quad in the middle. Figure 4 represents

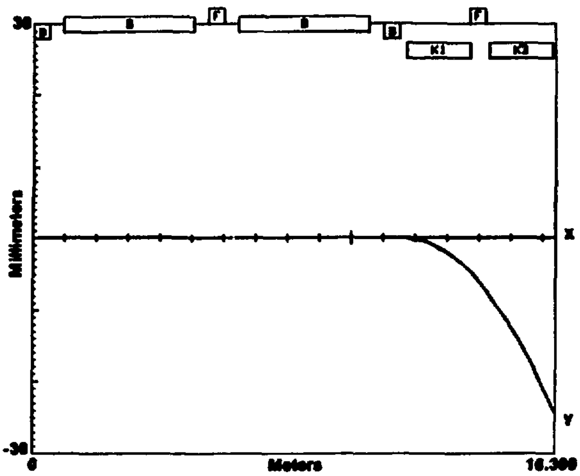

Fig. 3.

Results of a computer study of a possible magnet lattice. The X and Y envelopes are plotted for one period. The lattice is shown at the top of the figure (D = defocusing quad, F = focusing quad, B = bender, K1 and K3 are kicker magnets). Average radius of the ring is 94 m. There are 36 periods around the ring.

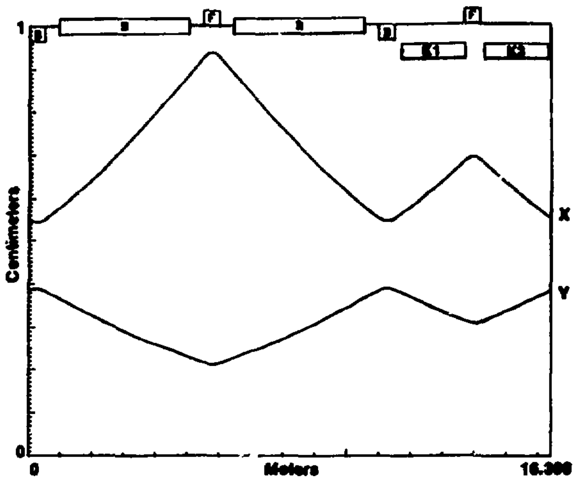

Fig. 4.

Output of a computer study showing fast extraction of the 16-GeV beam. Y = vertical displacement of beam, X = distance along equilibrium orbit.

Cooper's first look at a fast kicker system to bring beam out; with reasonable kicker fields, about 1200 G, it looks like the beam would come out nicely in the vertical direction. The kicker system is similar to one that is now being installed at Fermilab. This particular synchrotron design is again rather like the Fermilab booster, except the Fermilab booster has 24 periods and the Fermilab booster is a combined function magnet machine; that is, the focusing is built into the benders and is not separate as it is in this machine. It is thought that to separate the two functions will give us much better control over the beam, which we will dearly want to have when we try to deal with unprecedentedly high currents. Figure 5 shows some conceptual guidelines given to an architect-engineer a month ago to make a study for us concerning the relative cost of siting the ring in two different locations. The site on the left intersects the linac in two places; it intersects HRS and it intersects Ed Knapp's accelerator technology building in several places, and the central control building of LAMPF. That, of course, is only possible by having the whole thing deep in the ground, and it would be about 7.3 m (24 ft) under the existing structures. We asked the architect-engineer to cost that structure and also cost an alternative structure located out beyond the parking lot in Area A's northeast location. The alternate would be an above-ground structure; it would be constructed by doing some trenching, installing a culvert, and then backfilling over that. The dimensions of the two tunnels were 3.35 m by 3.66 m (11 ft by 12 ft) for the surface ring tunnel and essentially a 3.66-m- (12-ft-) diam tube for the underground tunnel. In both cases they were 183 m (600 ft) in circumference, which is a little smaller than the ring that Cooper presented to you, but it is not out of the question at all. Now, included in the instructions to the architect were to excavate, to restore and relocate roads and utilities, to repair any impact to existing facilities, to provide an access ramp into the underground ring, and to stabilize the walls, in the case of the underground ring, with concrete. That's basically it; the surface tunnel — the dotted-straight line which is the extractor for the injector tunnel and also an extraction tunnel — is included. Estimated costs for these two came out $3.6 million for the surface tunnel and $6.2 million for the underground tunnel estimate. These are very rough figures, but what I think they point out is that on the basis of cost you cannot rule out either possibility. Both of them would have to be considered. The great advantages of the underground tunnel are that it is self-shielding and that it is located upstream of all the experimental facilities at LAMPF. That means that injection into the existing experimental areas should be relatively simple, whereas for

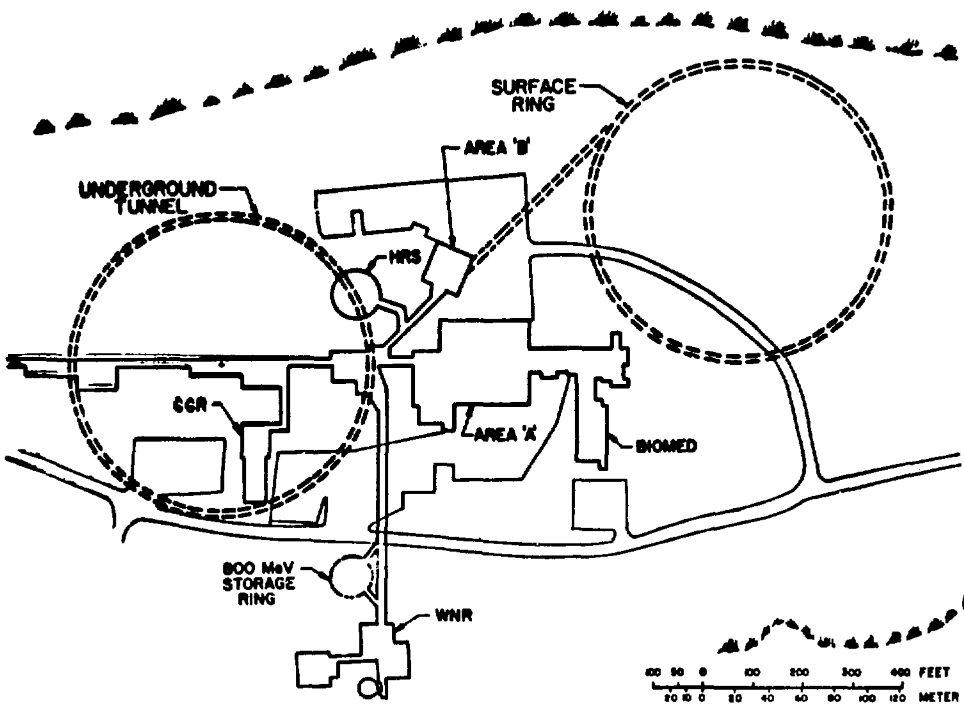

Fig. 5.

Two possible locations of the synchrotron on the existing LAMPF site.

the one on the east end it is more difficult but not out of the question.

Concerning the use of such a proton beam to make neutrinos, Herb Chen sent in some figures last week in which he had estimated the ν_μ flux for 30-m decay path and the detector 50 m from the target, assuming various proton energies from 0.8 to 10 GeV. And in going from 0.8 to 10 GeV, the neutrino flux goes up a factor of 100. In addition to this, the average energy of the neutrino goes up from about 150 to 700 MeV. The experiments that depend on interactions of the neutrinos with the nuclei will also take advantage of an increasing energy — cross section with energy — so that the event rate for a large class of neutrino experiments will go up extremely *rapidly with energy.*

I do not consider that we have in any way optimized the design of the accelerator or the experimental areas yet. This is only a beginning and I think you are going to see great progress during the coming year. We are just starting to move on improving the accelerator, making the design of the accelerator more realistic and firm, and coming up with realistic and innovative ideas for experimental areas; I want to leave you with the thought that, "You ain't seen nothing yet!"

EMBRYONIC PLANS FOR LAMPF II EXPERIMENTAL AREAS

H. A. Thiessen
Los Alamos National Laboratory

Introduction

I would like to share with you today some ideas that we at Los Alamos have been developing for several years. In particular, I will discuss ideas for an accelerator which have resulted from previous ideas of Nagle,[1] Teng,[2] Wilson,[3] and Macek.[4] Similarly, I will show a physics program developed from proposals made in a series of seminars organized by Bowman and Silbar,[5] two TRIUMF Kaon Factory Workshops,[6] and two LAMPF Workshops.[7] What I will show is still a concept not completely supported by the required technical justification. I hope that I can demonstrate the value of our ideas and inspire you to work with us to develop a full proposal with complete documentation in time for the 1982 annual meeting of the LAMPF Users' Group and for submission to the funding agencies at the end of 1982.

In the past we have discussed a "Kaon Factory." This name is clearly inadequate and misleading because what we propose is simultaneously a neutrino factory, a muon factory, a pion factory, and a kaon factory. We already have constructed a WNR complex, which is a neutron factory. Future extensions could create an antiproton factory. For the purposes of this talk, I will use the name LAMPF II — a name will be chosen during this meeting, as Dr. Rosen has already indicated.

The point of departure for this discussion will be LAMPF as it will be in 1985. This includes Areas A, B, and C, WNR, and PSR. The H^- source will have been upgraded to provide a capability on the order of 1 mA. A polarized H^- source capable of 10-100 μA might be nearing completion. During any conversion of LAMPF into LAMPF II, we would be constrained to operate the 800-MeV H^- beam on the order of 6 months per year, as we do now, in order to keep our commitments to WNR/PSR. We should also minimize any possible interruption of H^+ beam. In order to make a cost effective proposal, we should use as much as possible the existing buildings, utilities, magnets, spectrometers, and shielding located in experimental areas A, B, and C.

Accelerator

In order to make a definite proposal, I shall assume a three-stage accelerator injected by the LAMPF H^- beam. (D. Nagle has already indicated that there are many options under discussion.) The characteristics of the three stages are as follows:

Ring 1	Energy	4 GeV
	Rep rate	120 Hz
	Beam current	400 μA (2×10^{13}/pulse)
	Beam power	1.6×10^6 W
	Fast extraction	(Slow extraction at reduced current)
Ring 2	Energy	16 GeV
	Rep rate	30 Hz
	Beam current	100 μA (2×10^{13}/pulse)
	Beam power	1.6×10^6 W
	Fast extraction	
Ring 3	Energy	16 GeV
	Direct current	(Superconducting?)
	Beam current	100 μA (2×10^{13}/pulse)
	Beam power	1.6×10^6 W
	Slow extraction	(~100% macroscopic duty factor)
	Microstructure	50 MHz, better than 1-ns bunching

A possible location for the rings is shown in Fig. 1. Ring 1 is located in the smaller diameter tunnel; Rings 2 and 3 are located in the larger tunnel. Both tunnels are assumed to be sufficiently far underground that shielding of people on the surface is not a problem. The 16-GeV rings will be further underground in order that the tunneling operation does not interfere with the linac.

In addition to injecting the larger accelerator, the 4-GeV ring could be used to provide beam for a neutrino and pulsed muon facility and a polarized beam to Area B. The beam provided to the neutrino area could be

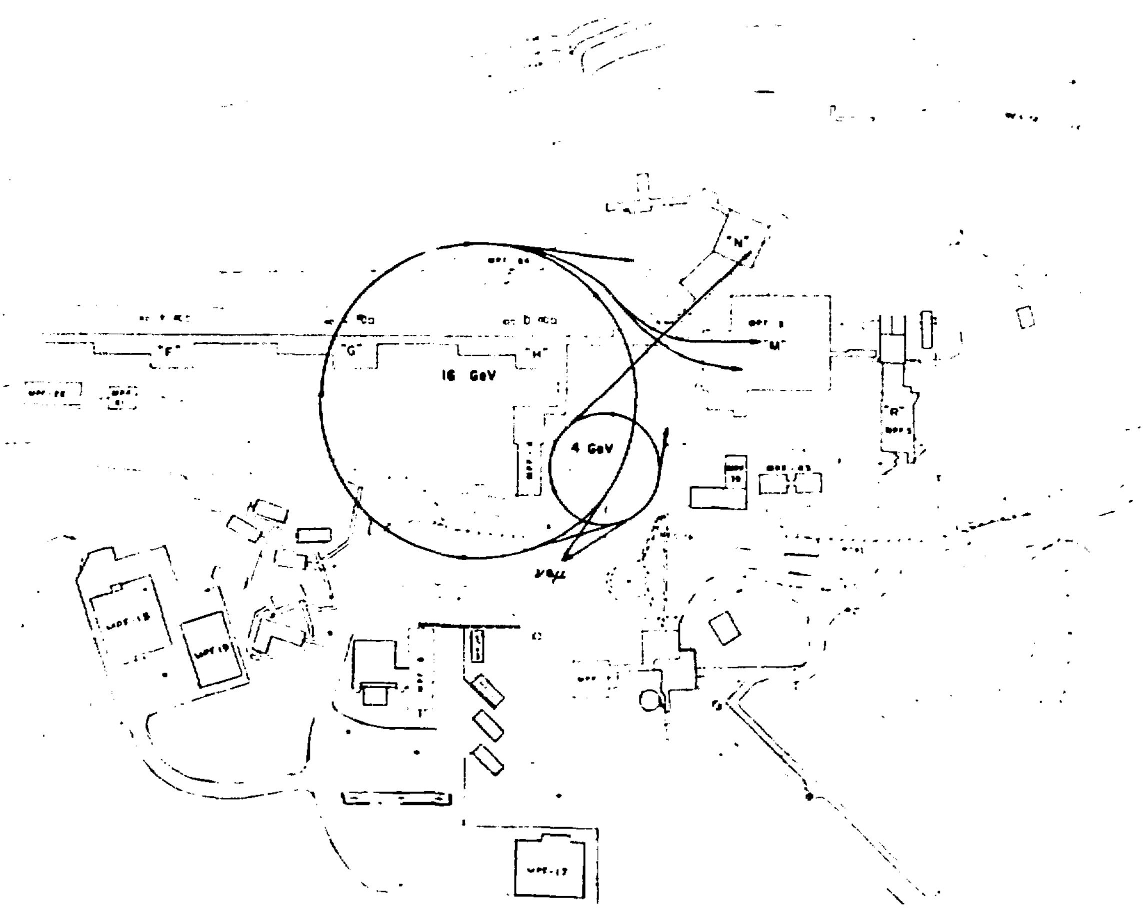

Fig. 1.

Possible location for 4- and 16-GeV accelerators at LAMPF site.

fast extracted in a single turn (~1/2 μs) at any energy between 0.8 and 4 GeV. A stretcher ring would be required if a large duty factor is to be provided for Area B. A reasonable plan for this accelerator might be to provide 200 μA to the neutrino area and up to 100 μA to Area B.

The 16-GeV rapid cycling ring would inject its beam into the d.c. stretcher ring. The slow extracted beam from the stretcher is assumed to be divided three ways: two beams are directed towards Area A, the third is sent to a target cell close to Area C. A fast extracted beam at any energy between 4-16 GeV could also be provided to a high-energy neutrino area. If the high-energy neutrino area is used, then the 100-μA beam will be divided among the slow extracted beams and the neutrino area.

16-GeV Experimental Area

A more detailed drawing of a proposed layout of beam lines in experimental areas A, B, and C is shown in Fig. 2. The primary beam is split in thirds by a system of electrostatic and magnetic septa. One line, which is shown on the line of the present H^- beam, passes through a thin target near the present A-1 target and is refocused on a thick target at the location of A-2. A new, dispersed, high-resolution π and K beam is produced at the thin target. A new high-momentum π and K beam is located at 0° from the thick target. This long beam line makes excellent use of the new staging area as an experimental area. If the thick target were to be located at A-2, then the present P^3 and SMC would still work — the fluxes would be comparable to those available today.

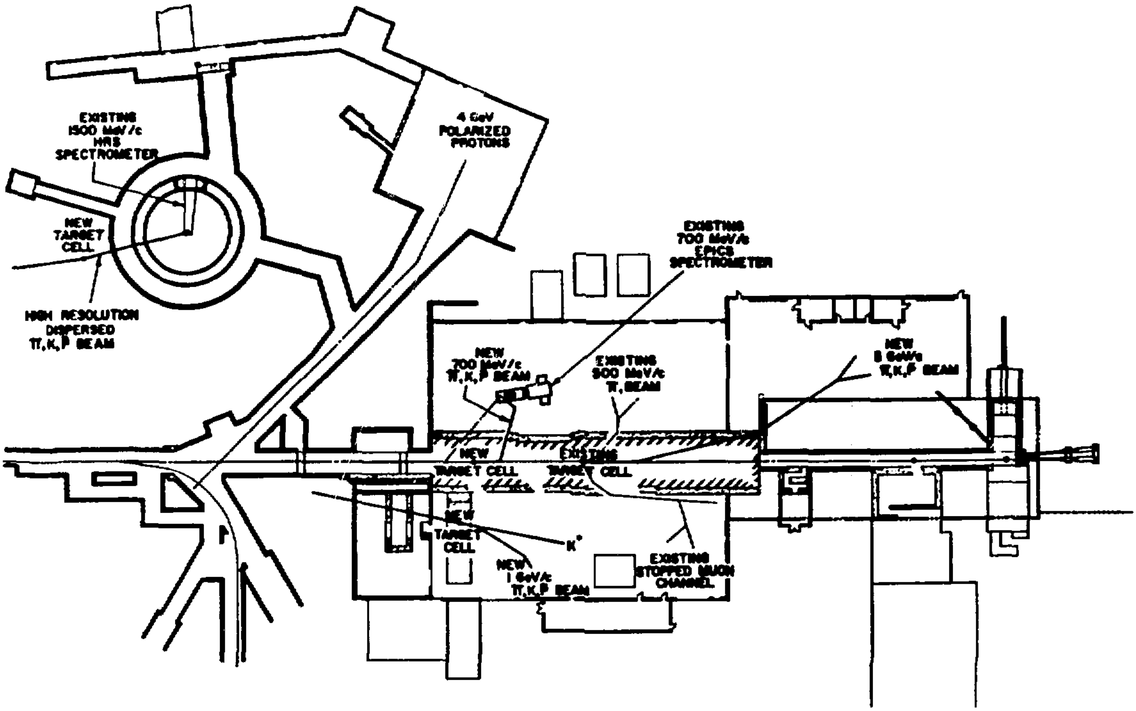

Fig. 2.
LAMPF II experimental areas.

It is likely that electron contamination will be higher and that a separator to remove electrons from SMC will be required.

The proton beam line to the south serves a single target cell. Two beams are serviced by the single thick target. A low-momentum separated K beam is shown at 0°. The neutral beam is brought through 20 m of shielding into Area A. This neutral beam is intended as a K^0 beam and may also be useful as an antineutron beam.

The northernmost proton beam is taken to a new target cell. The purpose of this line is to produce a π and K beam for use at HRS. It is possible that a vertically dispersed beam can be provided at the HRS pivot point that matches the HRS dispersion, providing a high resolution energy loss system for pions and kaons. Because the target cannot be very thick and still give high resolution, the beam dump for this line will have a substantial flux. Perhaps an isotope production facility can be located at this beam dump.

Maintenance is a major concern for any high-intensity facility. It is important to separate the target cells from each other by large enough distances so that it will be possible to work in one cell while operating most of the others. The 16-GeV beam is intentionally split into as many independent lines as possible — only one case of two targets in series on a single proton line is shown. This should make emergency maintenance a more tractable problem than it is at LAMPF today.

Request for Help from Users Groups

At this point I would like to review the program of experiments that might be carried out at LAMPF II. The purpose of this review is to bring to your attention the questions that affect the design of the accelerator or the experimental areas. I would like to request that the working groups help to formulate plans for the beam lines and experimental areas. I have prepared a list of working groups and matched it to the physics programs that would be addressed at LAMPF II. I would like each of the mentioned working groups to form a subcommittee of enthusiastic volunteers who will meet several times before next summer to prepare a physics justification and specifications for the required facilities. If properly done, the working group reports can be combined to make a major portion of a proposal. A summer study in the early part of the summer of 1982 would be an excellent way to complete the writing of the proposal.

Neutrino Working Group

The work already in progress for the neutrino proposal is an excellent starting point for discussion. The list of experiments discussed should include:

1. ν_μ oscillation experiments
2. $\nu_\mu e$ scattering to measure $\sin^2\theta_w$
3. $\nu_e e$ scattering
4. ν_μ nucleus scattering
5. $\nu_\mu N \rightarrow \nu_\mu N\pi$
6. $\nu_\mu N \rightarrow \nu_\mu \Lambda K$
7. Search for $\nu_\mu \rightarrow \nu_\tau$ oscillations.

There are several issues to be resolved. First, what would be included in a 10-year program of experiments with a new accelerator? Then, if a variable energy beam is provided, how does the neutrino flux and spectrum vary with the proton energy when the setup is varied to optimize yield for each proton energy? Is there an important class of experiments that can benefit from a proton beam on the order of 4 GeV and 200 μA, and which cannot be done if they must share the beam from a 100-μA 16-GeV accelerator? Can we build a single facility which is useful with variable energy proton beams from 0.8-16 GeV? Will it be reasonable to use some of the increase in neutrino flux from a high-energy accelerator to reduce the required detector size? Should we build a special device to enhance the ν_e flux, such as a magnetic bottle? How important is the pulse length, i.e., would 2.5 μs be sufficient?

K^0 Working Group

A K^0 beam will be required for charge parity (CP) violation studies. No existing working group closely overlaps this area of physics. I suggest that we form a new working group to discuss the next generation of K^0 experiments to be performed, the design of a beam line, and the space required for experiments. The working group could also address the possibilities for antineutron experiments. The working group should consider the possibility of locating the K^0 area near the present Line A beam dump.

High-Momentum K Working Group (P^3)

It is clear that a high-momentum beam line utilizing the maximum available flux from the 16-GeV beam will be an element of any plan for an experimental area. Experiments that might profit most from such a beam include rare kaon decays in flight, K-nucleon interactions, and searches for strange dibaryons. Because the philosophy of the beam line closely overlaps with that of P^3, I would like to ask the P^3 working group to study such a beam line. In addition to the program of physics to be performed with such a beam line, this group should consider carefully the maximum momentum for which this beam should be designed. They should also consider the amount of space required for experiments. Finally, the group should consider the question of the usefulness of keeping the present P^3 beam line operational.

Low-Energy Kaon Working Group (LEP)

One of the most interesting physics programs can be performed with a low-energy (or stopping) kaon beam. This includes rare K decays at rest, low-energy kaon nucleon scattering, and K mesic atoms. A beam line with very low pion contamination is required in order that experiments can be designed to take advantage of the increase in intensity provided by LAMPF II. A beam line design that includes a crossover and slit upstream of the separator should make possible a much improved pion-kaon separation. The existing Low-Energy Pion (LEP) Working Group would be an excellent body to consider the justification and design of such a facility. The group should consider very carefully the maximum momentum of the beam line, and should consider the possible uses of the proposed new low-energy pion spectrometer at LAMPF II.

Muon Working Group (Pulsed Muon)

Two muon beams are envisaged for LAMPF II: a stopped muon channel taking advantage of the 100% duty factor available from the 16-GeV accelerator, and a pulsed muon beam. The pulsed beam could provide a 0.5-μs pulse if it shared a target with the proposed neutrino facility. To obtain a shorter pulse, it would be necessary to locate the pulsed muon line at PSR. It is possible that the existing stopped muon channel will be adequate for a d.c. beam. The muon working group should discuss the question of whether a new d.c. muon beam is required and if so, what improvements should be considered. There are presently three muon working groups. It appears that all users interested in muon beams should be involved in any decision. I suggest that the pulsed muon working group, which has been actively considering most of these issues recently, should be the basis of a new working group to discuss all the muon beams at LAMPF II.

Nucleon-Nucleon Working Group

A significant program in high-energy nucleon-nucleon scattering with polarized beams is possible, especially if a high-intensity polarized H^- ion source is developed. The working group should consider the possibilities for a program of physics in this area, and should consider the competition already underway at SATURNE II and the plans for polarized beam at the National Laboratory for High-Energy Physics, Japan (KEK) and Brookhaven. In order to provide a variable energy beam at high duty factor, a separate storage ring is required. This group should consider whether a slow spill with continuously varying energy would be sufficient. The nucleon-nucleon working group should consider the question of whether a polarized beam should be provided at full energy, or whether a lower energy would be sufficient. This working group should also discuss the question of antiproton facilities and, if possible, come up with the best possible plan for producing a polarized antiproton beam. Finally, the question of hyperon beams should be addressed by this group.

HRS Working Group

A high-resolution dispersed pion and kaon beam would allow an exciting program of experiments in the following areas:

(π,π')	in the energy region of the 2nd and 3rd π-N resonances,
(π,K)	to study hypernuclei,
(K,K')	see note in text,
(K^-,K^+)	to study Ξ and $\Lambda\Lambda$ hypernuclei.

It may be possible to design a small solid angle beam line that matches the dispersion of the existing HRS spectrometer and which could have resolution of 10^{-4} or better. The momentum range of HRS is ideally suited for these reactions. The (K^+,K^+) reaction should be particularly interesting because the G parity of the K^+ does not permit scattering from the pion cloud surrounding the nucleon in certain bag models. The HRS committee should pay particular attention to the beam optics design. In addition, the performance of a new spectrometer should be compared with that expected from HRS.

EPICS Working Group

Because of the short lifetime of the charged kaon, it is extremely important to choose the maximum momentum of a beam design to match the physics program. The 700-MeV/c maximum momentum of the EPICS spectrometer matches very well to the needs of field of hypernuclei studied with the (K^-,π^-) reaction, and also of K^+ elastic and inelastic scattering. A preliminary dispersed beam design suitable for these purposes is shown in the January 1981 Workshop proceedings. The expected kaon flux is $\sim 10^7$ per second. The EPICS spectrometer would be perfect for this application, because detectors would normally be used near the scattering target. The EPICS committee should consider means of using the 10^9 pion flux that would be available, either for double charge exchange or for pion inelastic scattering. The possibility of studying Y* resonances in nuclei by multiparticle coincidence experiments should also be considered.

π^0 Spectrometer Working Group

Many opportunities requiring detecting π^0's with high resolution exist at LAMPF II. The π^0 spectrometer working group should consider the possible utilization of the π^0 spectrometer for kaon induced reactions and reactions involving production of η^0.

Nuclear Chemistry Working Group

The 1980 nuclear chemistry summer study considered the question of nuclear chemistry at a kaon factory. The nuclear chemistry working group should sharpen up this discussion and especially consider the program that would be possible with K beams containing on the order of one pion for each kaon. This group should also consider the possibility for use of the proposed time-of-flight spectrometer at LAMPF II.

Costs

The funding agencies will be particularly interested in the cost of this proposal. Because no new detailed work has been done, it is not possible to give an estimate more accurate than that of Teng. It should be clear from this talk that major portions of the existing experimental facilities can be reused and that very significant savings have been achieved. The power requirements are comparable to those of LAMPF. If the bulk of the experimental area magnets are replaced with superconducting magnets, perhaps we can keep the total power usage at about today's level. If not, perhaps 10×10^6 watts of additional power will be required. The operating budget, exclusive of power costs, would be comparable to that of LAMPF — the number of beam lines and experiments operating simultaneously would be about the same as at LAMPF. Operating and maintenance costs of the new rings might require a 20-30% increase in operating budget.

Summary

A very broad, exciting program of physics can be addressed with a high-intensity, 16-GeV facility such as LAMPF II. This program is nicely matched to the interests of the present user community. A sample layout of the experimental areas shows that the bulk of the required facilities can be accommodated within the existing LAMPF experimental areas. We have requested the help of the LAMPF working groups in preparation of a proposal. The individual working groups have been asked to prepare reports that can be combined into a full proposal during a summer study to be held in 1982. A completed proposal should be ready for distribution at the 1982 annual meeting of the users group. I hope you agree that we have a most exciting project with opportunities for all of you at LAMPF II.

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USERS GROUP REPORT

Felix H. Boehm
California Institute of Technology

The Users Group, as you well know, is conducting its functions through the Technical Advisory Panel (TAP) and the Board of Directors (BOD). These bodies met in January, March, and July 1981, and are meeting again November 4 following this Users Meeting. The ongoing projects discussed and reviewed at these meetings are the following:

Low-Energy Pion Spectrometer. Design studies were presented and a final design is in progress.
Time-of-Flight Spectrometer. This instrument, which will serve in isotope mass identification, is now on the drawing board.
Polarized Ion Source. The development of an optically pumped polarized ion source will have to wait for parallel developments at the National Laboratory for High-Energy Physics, Japan (KEK).
Neutrino Experiments. Experiment 225 is in progress while the large oscillation experiments are awaiting funding.
Neutrino Facility on Line D. This Los Alamos Laboratory facility has been discussed extensively over the past year. We shall hear more about it in G. Stephenson's lecture. A formal proposal to the Agency for funding is being drafted.
Kaon Factory. A two-year study grant has been approved and good progress will be reported by D. Nagel and A. Thiessen.

Other items of concern were:

SMC Beam Splitter. The splitter will allow simultaneous operation of two μ^+ beams.
LEEP Pool New Acquisitions. Here, more users input is needed! Line-item funds are now available.
Staff Shop. This facility has been approved and is being implemented.

Finally, the term of office of the BOD chairman was discussed with the intent to insure better continuity for long-range plans at LAMPF, such as neutrino or kaon facilities. It may seem that a 2- or 3-year term would be more appropriate. There was little response favoring such a change. The chairman stressed that the BOD should play a more dynamic role in supporting the users and their experiments. Clearly, concern over most of the problems sketched above will have to continue well into next year.

Because much effort went into studies of neutrino experimental proposals and their implementation, I'll review briefly the chronology of these deliberations.

January '81. Five neutrino oscillation proposals were brought before the PAC. The recommendations of the PAC were: to approve one low-energy (beam stop) experiment (645) and one high-energy (beam line D) experiment (638).
March '81. A BOD *ad hoc* committee reviews some technical aspects of these proposals. It recommends a more global view on neutrino physics and recommends a workshop.
June '81. A Los Alamos Neutrino Workshop is held. Proceedings are available in draft form now.
November '81. Despite formal approval of 645 and 638, a struggle for funding for both experiments goes on.

Next I shall briefly report on the Los Alamos Neutrino Workshop, presenting to you some highlights. This workshop, organized by your Chairman and G. Stephenson, was conducted by working groups chaired by the individuals mentioned: (1) Particle Physics — Ramond, (2) Nuclear Physics — Donnelly, (3) Cross Sections — O'Connell, (4) Flux Calculations — Slansky, (5) Cost Estimates — Agnew, (6) Detector — Barish, and (7) Pulsed π,μ — Macek.

The particle physics group identified the following topics as principal motivation for a facility.

- Neutrino oscillations $\nu_\mu \rightarrow \nu_e$, $\nu_\mu \rightarrow$ all.
- Precision determination of $\sin^2\theta_W$ at low energies from neutrino-electron scattering.
- Electron-neutrino physics. Neutral and charged current interference. Test of universality in neutral current processes.

A principal topic in nuclear physics is the determination of neutral coupling constants of neutrino nucleus interactions. The isoscalar and isovector pieces will allow test of gauge models of weak interaction. The neutrino-proton and neutrino-deuteron scatterings deserve high priority, while the neutrino-^{12}C, etc., scatterings are considered less fundamental. Coherent scattering, such as neutrino-^{4}He, is an interesting and unique process governed by the isoscalar-vector coupling.

Neutrino flux calculations have been conducted by several independent groups. With a 100-μA proton beam in line D, a 40-cm carbon target, a 12-m pion decay section, and a 9-m iron shield, the flux comes out to be 2×10^6 ν_μ/cm^2 s. This number can be augmented if a pion focusing device is used. A muon storage device ("bottle") is needed to produce a high-energy ν_e beam. These magnetic devices will be studied now. Finally, it is clear that an increase in energy from 800 MeV would enhance the pion production as well as the detector yield.

A detector based on present-day technology would consist of active slabs of target material interspersed with tracking chambers. A minimum size of 100 tons is envisaged and matched to the required granularity. Other designs, notably the liquid argon time projection chambers, look promising but need prior development. Counting rates will typically be 1-100 events per day. Clearly a tight veto counter as well as plenty of passive shielding will be needed to suppress cosmic-ray background.

As a final word, your chairman wants to stress that important decisions on the future of LAMPF will have to be made now. We need your response! What kind of physics do you want to see in the future? Do you want a kaon factory, a neutrino facility? It is not the Director of the Lab who decides on the future of physics here, it is you, the users!

SIN: STATUS, FUTURE SCIENTIFIC AND TECHNOLOGICAL PLANS

Jean-Pierre Blaser, Director
Swiss Institute for Nuclear Research

In the last year the research program proceeded satisfactorily. I shall not report on it, as a SIN Newsletter will be published soon.

Accelerators

The combination of Philips injector and ring cyclotron has been operating well. The operation of the third-harmonic fifth cavity for flattopping has brought a considerable improvement in beam quality, leading to virtually complete extraction and lower losses in the proton channel. Currents up to 190 μA could be produced.

The Philips injector has been further improved towards high extraction rates (93%) and high-intensity polarized beams (1.5 μA p↑ at 70 MeV).

Injector II (Fig. 1) is proceeding very well according to plan; however, it is somewhat delayed because of financial reasons. The ion-source test stand has delivered successfully a 40-keV, 10-mA dc proton beam with a normalized emittance of $<\pi \cdot 0.4$ mm-mrad, which is well within the requirements for injector II. Work is under way to accelerate this beam to 300 keV, using a prototype accelerating tube.

The 860-kV Cockcroft-Walton has operated successfully. The first beam tests of the new injector are planned

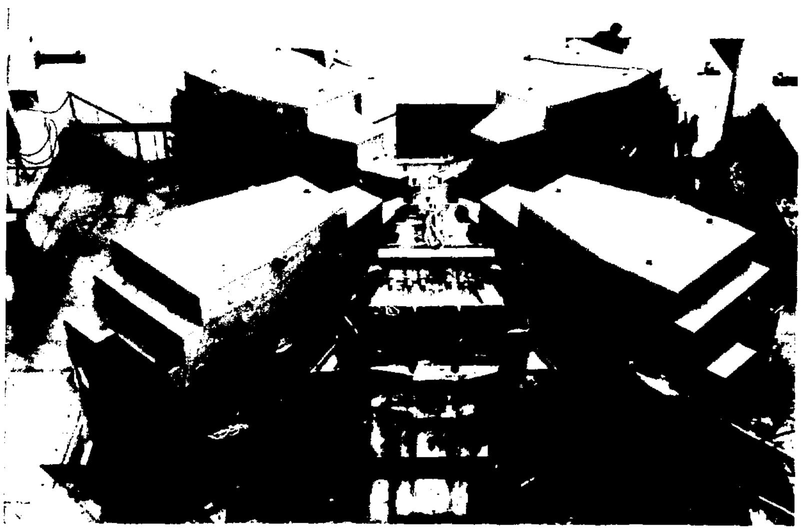

Fig. 1.
The 72-MeV ring cyclotron under construction in May 1981. The new injector is a fixed-energy proton accelerator designed for currents above 1 mA (average). It combines an 860-keV electro-cyclotron. The main components of the 72-MeV ring are four small-gap sector magnets, two main accelerating structures operating at 50 MHz, and two 150-MHz flattopping cavities. (The above picture shows the first 50-MHz resonator installed between the sector magnets.) The turn separation at extraction radius will be 2 cm, big enough to use a magnetic septum for the extraction of the beam. The extraction efficiency will be very close to 100%.

for the end of 1983, with a current goal of 2 mA. Starting in 1984, it is hoped that injector II can be used for normal operation (some 200 μA) to increase beam time for the high-energy beams and to free the Philips injector for its own research program.

The 600-MeV ring shall then be gradually developed towards higher currents, which will mean an increase of rf power from the present 600 kW to ~2 MW for beams of 2 mA. Limiting factors are expected to be the longitudinal space charge, the beam loading of the cavities, and the beam losses caused by beam halo.

The beam statistics in 1980 were reduced somewhat by the target shutdown. An approximation of the final figures follows.

	Hours
600-MeV proton target (including polarized 600-MeV protons)	3730
Injector low-energy operation (including isotope production)	1450
Shutdown, services	2210
Setup, beam development, training	1400

The total electricity consumption of SIN is ~41 000 MWh/year (normal operation is 7-7.5 MW), so no reduction of beam time is required at present for financial reasons.

Beam Lines and Targets

After some initial difficulties, the beam splitter, peeling off 10-20 μA from the main beam for the medical facility, is now operating very reliably. It is an electrostatic separator that can provide variable beam-splitting ratios. The losses are ~0.5% of the total beam.

Difficulties have been encountered with the target stations after operation at currents close to 200 μA. A leak caused by thermal stresses developed in the E target vacuum chamber and required a 3-month shutdown in the spring of 1981. The rotating target developed bearing troubles, and a stationary replacement target showed thermal strain damage, which forced us to reduce beam currents to 100-120 μA. The πE3 channel was badly contaminated by beryllium that had evaporated from a blocked target wheel.

Experimental Facilities

Only four are mentioned here; for the others see SIN "Jahresberichte" and SIN "Newsletters."

- SUSI is being gradually converted to a two-arm spectrometer to make use of the large solid angle and high duty cycle advantage.
- The pionic atom crystal spectrometer has started operation (a collaboration of ETH Zürich with Leningrad). It uses a "Gatchina"-type target on the 20-μA proton beam of the medical facility.
- A low-energy pion spectrometer (LEPS) is being built by Karlsrühe in collaboration with the University of Neuchâtel. It shall be used first on πM3, later at the modified πE3 YOYO beam.
- SINDRUM will be built in a first stage, optimized for the $\mu \rightarrow 3e$ decay. It has a cylindrical box magnet, five concentric low-mass cylindrical wire chambers, and a scintillator trigger hodoscope; 16% efficiency and 5% invariant mass resolution are expected. The project is a collaboration of ETH Zürich, the University of Zürich, and SIN.

Medical Pion Therapy

This project proceeds well, though the implementation of the superconducting piotron and of the elaborate dynamic therapy system has been a demanding task. Dosimetry on phantoms has proved that true three-dimensional dose shaping with optimum distribution is indeed achieved in patient irradiation. After more than 1 year on a small number of phase I patients (superficial tumors), phase II (with a curative goal for deep-seated large primary tumors) has begun. The continued active coordination of the LAMPF and SIN therapy projects, which are complementary to a large extent, is felt to be essential.

Medium-Range Planning

To adapt the facility to high-current operation, we foresee carrying out modifications to the primary proton channel during one or two shutdowns totaling 6 to 9 months in 1984 and 1985. The main task is to

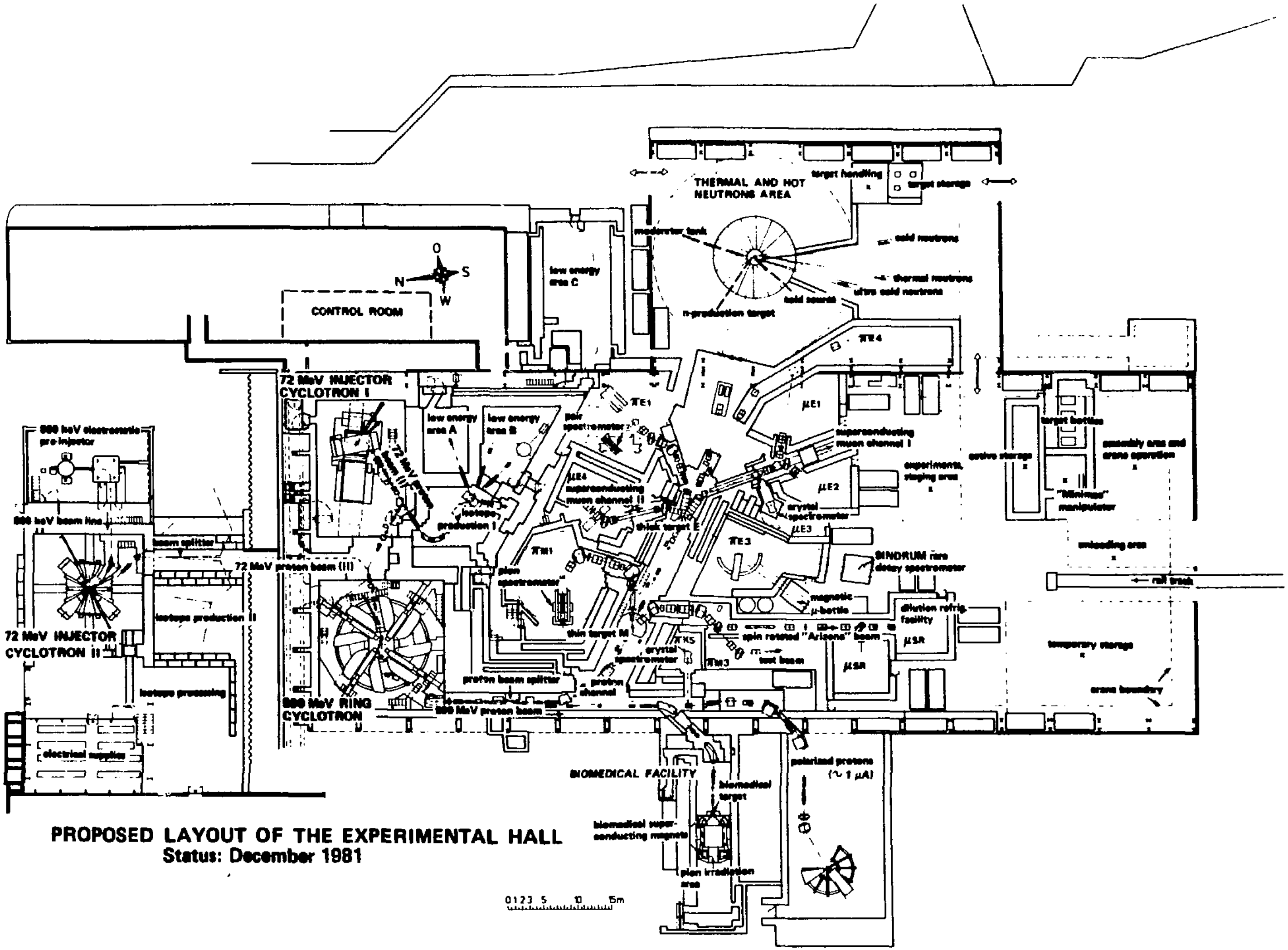

Fig. 2.
Proposed layout of the experimental hall as of December 1981.

reconstruct the thick E target and beam-dump region. Instead of forward pion extraction, two separate and independent legs at ±12° for πE1 and μE1 are presently foreseen, using half quadrupoles (Fig. 2). This should facilitate recombination of the proton beam for the neutron spallation source. However, it means a certain loss in beam intensity and beam resolution. The special problems expected at currents in the 1-2-mA range, such as collimators, shielding, remote handling, and fast interlocking, are being studied.

The originally planned construction of a new proton channel with target stations optimized for high beam currents had to be abandoned because of financial reasons. Basically, therefore, we will continue to use the present system, but the following modifications and additions are foreseen (Fig. 2):

- as mentioned above, the separation of πE1 and μE1;
- a new πE3 beam, using YOYO geometry to provide a high-intensity and good-resolution, low-energy pion beam as well as a surface muon beam. This beam will ultimately be used for the LEPS as well as with SINDRUM;
- the conversion of the πE4 (relocated) superconducting μ channel to allow surface muon operation;
- the transfer of the pM1 polarized proton area to the medical beam line;
- the reconstruction of the πM3 area into a beam, mainly dedicated to μSR, that includes two areas, one equipped with a spin rotator; and
- the consideration of maintaining a time-of-flight area for high-energy neutron work in spite of the spallation neutron source.

For the improvement of the proton channel and the installation of the new secondary beams to be carried out in a reasonably short time, space has to be made available by extending the main hall by 43 m (Fig. 2). The budget has to be authorized by parliament, which unfortunately will not be considering it until the end of 1983.

Neutron Spallation Source (Fig. 3)

Plans are being made to transport the high-intensity proton beam past the two pion production targets to a spallation target made of a lead-bismuth liquid eutectic, which is able to dissipate about 1 MW by natural convection. The beam would hit the target from below, allowing almost 360° access to neutron beam tubes. The rationale behind the project is to develop technology and gather experience with spallation devices in the >1-MW range, and at the same time provide neutron beams for

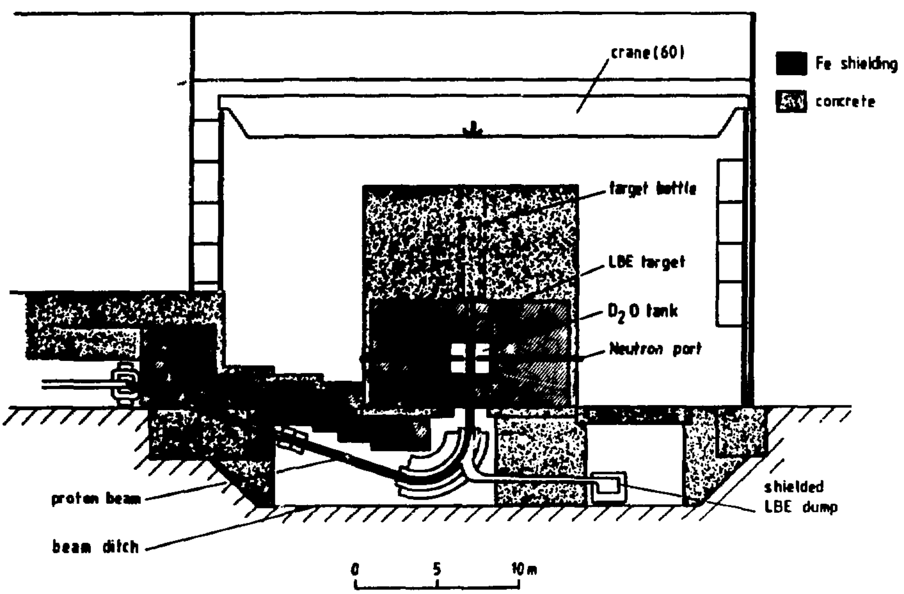

Fig. 3.
Proposed layout of the neutron spallation source.

research. The advantage of a continuous spallation source for producing cold neutrons shall be particularly emphasized.

The fluxes obtainable can justify the expense only if currents of >1 mA are achieved on the spallation target. Therefore, the final decision to go ahead with the project will be subject to first experiments on high-intensity operation of accelerators. The earliest operation of the spallation source is 1986.

Long-Range Policy

The main goal is to provide the best possible facilities for physics and applications with pion and muon beams. Thought is given, however, to possible extensions that could open new fields like neutrino physics, pulsed muon beams, kaon beams, and high-power pulsed or stationary spallation devices. Ideas have emerged in this direction for a further stage to be added to the present 600-MeV ring cyclotron. In using the type of magnets and cavities now operating, a ring 2-2.5 GeV could be constructed practically, using present technology, that should be able to handle ultimately some 2 mA (5 MW). It was discovered that two operating modes would be possible, acceleration and storage, thus the name ASTOR. The storage mode makes use of the phase expansion occurring in the isochronous cyclotron when the amplitude of the rf accelerating voltage diminishes with increasing radius. In this mode, 25% of the 600-MeV beam (75% would be used for experiments as at present) is injected into ASTOR and ejected by a fast kicker once 500 turns have collected into a stored beam at the extraction radius. The result would be something like 300-ns pulses at 1600-Hz repetition rate with an average current of 0.5 mA at 2.5 GeV. These very attractive ideas are, of course, in a very preliminary state and a number of difficult problems still need to be solved. Anyway, such dreams are good for the morale of laboratories, which have to keep fit while aging.

THE HUNTING OF THE AXION

A. Zehnder
Swiss Institute for Nuclear Research

Introduction

In a unified description of the strong and electroweak forces, we face the problem of parity and charge-parity violation in the strong-interaction part of the quantum chromodynamics (QCD) theory. This problem led R. Peccei and H. Quinn[1] to postulate a global U(1) symmetry for the total Lagrangian. Wilczek and Weinberg[2] pointed out that this assumption leads, by way of the Higgs mechanism, to the existence of a light pseudoscalar particle, the axion a_0. In the standard model the axion interacts weakly, like an isovector (weak π^0) and/or like an isoscalar (weak η) particle. If its mass is $<2m_e$ it decays chiefly into two gammas. Axion production proceeds in competition with the production of particles such as gammas, pions, etc. The branching ratios can crudely be estimated from the ratios of coupling constants,[3]

$$\omega_a/\omega_\gamma \simeq 2.3\ 10^{-4}$$

and

$$\omega_a/\omega_{\pi^0} \simeq 1.5\ 10^{-7}\quad .$$

These values may change significantly because of specific dynamics. The two-gamma decay width is[3]

$$\Gamma_{a\to\gamma\gamma} = 1.4 \quad \left[\frac{m_a(\mathrm{keV})}{100}\right]^5 (s^{-1}) \qquad (1)$$

and the mass is[3]

$$m_a = 50\ N/\sin(2\lambda)$$
$$= 25\ N(X + X^{-1})\ (\mathrm{keV})\quad , \qquad (2)$$

where $X = \tan(\lambda)$, N is the number of quark doublets (assuming $N = 3$), and λ is the yet unknown mixing angle of the Higgs fields. For the present discussion, λ (or X) is the only free parameter and allows for the comparison of the different experiments.

Axion Experiments

The setup for axion experiments consists of a well-shielded axion source, a decay region (length ℓ), and a 2γ-detector system (efficiency ε_{tot}). The 2γ count rate $R_{\gamma\gamma}$ is

$$R_{\gamma\gamma} = R_x\ \frac{\omega_a}{\omega_x}\frac{\ell}{v}\frac{\Gamma_{a\to\gamma\gamma}}{\gamma}\ \varepsilon_{tot}\quad , \qquad (3)$$

where $v \simeq c$ is the axion velocity, $\gamma = E/m_a$ is the Lorentz factor, and R_x is the source strength for the competing process. The branching ratio ω_a/ω_x must be calculated [see Eq. (4), below]. Using Eq. (1) we see that $R_{\gamma\gamma}$ is proportional to m_a^6.

Axion searches have been performed by different groups.[4-6,8] We briefly summarize the results as follows.

Reactor Experiments

The axions are supposed to be emitted from excited fission products in competition with γ decay. The branching ratio was estimated to be $\sim 10^{-8}$ because of a suppression of M1 transitions in fission products, but this is a severe underestimate (see below). The best limit is reported by the Caltech-Munich group,[4] who found $m_a \leq 280$ keV.

Electron Beam-Dump Experiments

Because the axion-lepton coupling is proportional to $\mathrm{ctn}\,\lambda = X^{-1}$ (Ref. 3), these experiments are essentially sensitive to small mixing angles λ. Bechis et al.[5] rule out $X \geq 1.2$. Nevertheless, because of theoretical ambiguities, X could be as small as 0.4.

K Decay

The ratio[6] $(K^+ \rightarrow \pi^+ a/K^+ \rightarrow \text{all}) = 3.7\ 10^{-8}$ rules out isoscalar axions because there should not be any hindrance for the $\Delta I = 3/2$ channel.[7]

Proton Beam-Dump Experiments

There is positive evidence (published by Faissner et al.[8]) for axions with a mass of (250 ± 25) keV, as calculated from $(X + X^{-1}) = 3.4 \pm 0.4$ [Eq. (2)]. In Ref. 8, X is assumed to be 3.3 ± 0.3 (mostly isoscalar axions), but $X \simeq 0.3$ cannot be ruled out *a priori.*

Axion Search in Specific Nuclear Transitions

To obtain further information on axions we have performed an experiment designed to meet the following criteria.

- The axion rate should be reliably calculable.
- The 2γ decay should produce a characteristic signature.
- The background should be independent of the presence of the source.

For the following reasons these requirements are best met by searching for axions in magnetic transitions.

- The axion is a pseudoscalar.[3]
- The ω_a/ω_γ can be calculated for specific single-particle transitions quite reliably, primarily because of cancellation of the reduced nuclear matrix elements.[3,9]
- The sum of the energy of the two gammas yields a monoenergetic peak at the known transition energy.
- The low-energy gammas of a source are easy to shield against.

Detailed calculations for such experiments are given in Refs. 3 and 9. The branching ratio for proton (i = p) or neutron (i = n) single-particle transitions of multipolarity L is given by

$$\omega_a/\omega_\gamma = (a_a/a_\gamma)\cdot[L/(L+1)] \cdot (k_a/k_\gamma)^{2L+1}(\rho_i/2\mu_i)^2 \quad , \qquad (4)$$

where k_a and k_γ are the axion and gamma momenta, $\mu_n = -1.9$ ($\mu_p = 2.8$) is the neutron (proton) magnetic moment, and ρ_i is the axionic analog. The ρ_i's are plotted vs λ (X) in Fig. 1. Note that for a single-particle neutron transition, ρ_n may be zero for $\lambda \simeq 35°$ ($X = 0.7$). For a proton transition no such cancellation exists.

The first search for axions in a specific nuclear transition was carried out with a 950-Ci ^{137}Cs source.[10] The ^{137}Cs proceeds by way of β decay to the excited state of

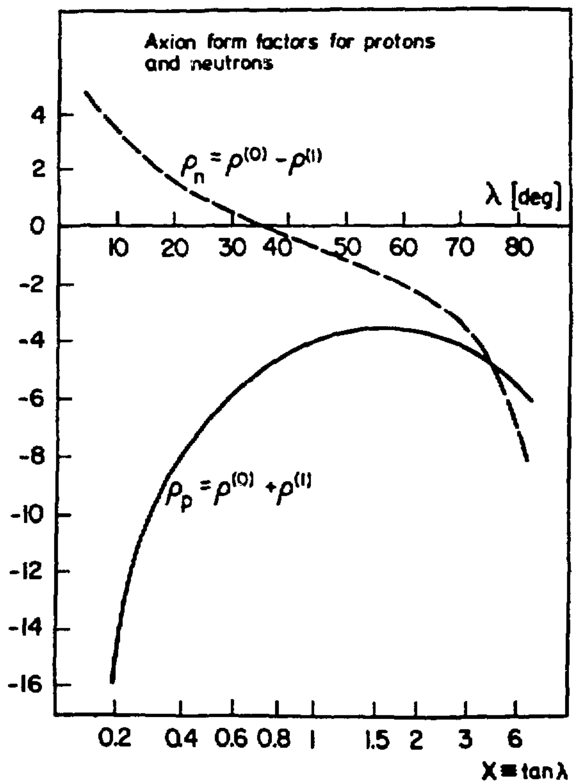

Fig. 1.
Axionic form factors for single proton and neutron transitions vs mixing angle λ (X = tan λ). The $\rho^{(0)}/\rho^{(1)}$ are the isoscalar (isovector) form factors of Ref. 3.

$^{137}Ba^*$ $[I_i = (\frac{11}{2})^-]$, followed by a 662-keV transition to the ground state (g.s.) $[I_f = (\frac{3}{2})^+]$. The transition is primarily a single-particle neutron transition[9] (^{137}Ba has 81 neutrons). The experimental setup (described in detail in Ref. 10) mainly consists of two well-shielded NaI counters (127 mm in diameter and 102 mm in length). In this experiment axions with $m_a = (250 \pm 25)$ keV, as stated in Ref. 8, are ruled out if one assumes $X = 3.0 \pm 0.3$. Because $m_a = 75\ (X + X^{-1})$ keV [Eq. (2)], no clear discrepancy remains between the experiments of Refs. 8 and 10, provided one chooses $X \simeq 0.3$. The axion form factor ρ_n, and therefore the axion emission from $^{137}Ba^*$, is strongly suppressed for this value of X (see Fig. 1).

Complementary information can be obtained by studying proton transitions that have a different X dependence. The most promising is the neutron capture by protons $n + p \rightarrow d + 2.2$ MeV. This isovector transition reduces to a proton transition for small X,

$$|\rho_d| = |\rho_p - \rho_n| \simeq |\rho_p| \quad .$$

At the Institute Laue-Langevin (ILL) reactor in Grenoble, France where the Caltech-Munich experiment[4] was performed, 18% of all fission neutrons are captured by the protons in the pool water, resulting in $\omega_a/\omega_\gamma \simeq 10^{-5}$ instead of 10^{-8} as assumed in Ref. 3. The ILL reactor experiment was reanalyzed, taking this particular reaction into account.* The result is shown in Fig. 2. There is no indication of axions, which also implies that $\rho_p \simeq 0$, ruling out axions.

In terms of the standard axion model one can exclude the following X values,

$X \leq 0.45$, $X \geq 1.4$ ($^{137}Ba^*$ experiment, Ref. 10)

and

$X \leq 3$, $X \geq 3.8$ (ILL reactor experiment, Ref. 4**) .

Combining the two experiments we see that all X values are excluded and that the axions, as predicted by the standard model, are ruled out.

*V. L. Telegdi, private communication.

**J. L. Vuilleumier, private communication.

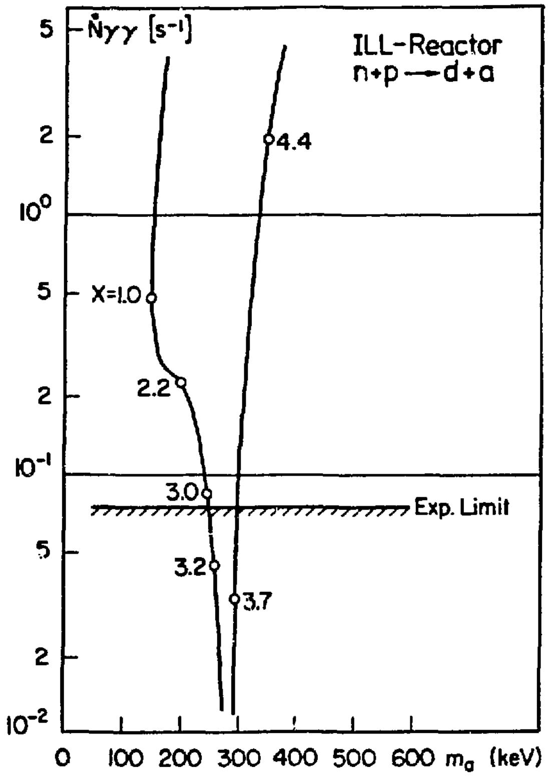

Fig. 2.
The reanalyzed Caltech-Münich experiment (Ref. 4) for the $n + p \rightarrow d + a$ *reaction at the ILL reactor.*

This conclusion, however, is in clear contradiction with the findings of Ref. 8. Therefore, we performed a further experiment with the apparatus of Ref. 10 at the 2830 MW_{th} light-water power reactor at Gösgen, Switzerland. Only in a reactor are single-particle proton transitions produced in sufficient strength. The most interesting case is the $^7Li^* \rightarrow Li(g.s.) + 477$-keV M1 single-proton transition.[11] The $^7Li^*$ is produced by thermal neutron capture in ^{10}B($^{10}B + n \rightarrow {}^7Li^* + \alpha$). With a 700-ppm average concentration of boric acid in the cooling water of the reactor, one has $1.02 \cdot 10^{19}$ lithium transitions per second.* Other prominent transitions in the reactor are

*G. Meyer, Gösgen, Switzerland, private communication.

$$^{91}Sr \rightarrow {}^{91}Y^* + 551 \text{ keV} : 5 \cdot 10^{18}/s \quad ,$$

$$^{97}Zr \rightarrow {}^{97}Nb^* + 747 \text{ keV} : 5 \cdot 10^{18}/s \quad , \text{and}$$

$$^{137}Cs \rightarrow {}^{137}Ba^* + 662 \text{ keV} : 2.4 \cdot 10^{17}/s \quad .$$

Furthermore, neutrons are captured by the protons of the cooling water,

$$n + p \rightarrow d + 2.2 \text{ MeV} : 7.8 \cdot 10^{18}/s \quad .$$

The experiment was located 42 m from the reactor core with a decay region of 3.2 m. The typical total efficiencies for detecting the two gammas from an axion decay were $6 \cdot 10^{-10}$ for 747 keV and $4 \cdot 10^{-10}$ for 2.2 MeV (Monte Carlo calculation). The preliminary results[12] are plotted in Fig. 3. One sees again that the standard axions are ruled out.

If, neglecting the standard axion model, one accepts an axion mass smaller than 150 keV [the lower limit for standard axions, Eq. (2)], then for $|\rho_i| = 4$, as an example, our results give an upper limit on the axion mass of 55 keV.

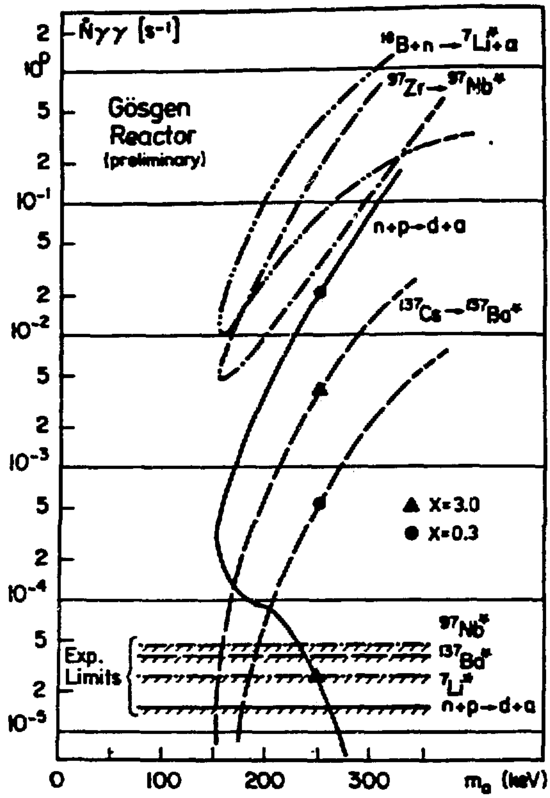

Fig. 3.
Preliminary results (Ref. 12) of the Gösgen power reactor experiment. The standard axion can be excluded either from the $^7Li^*$ or from the $^{97}Nb^*$ transitions or from the combined Ba and n-capture reaction because the lower branch of the Ba* decay corresponds to the upper branch of the n-capture reaction and* vice versa. *The experimental limits are 2 std dev.*

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SEARCHES FOR VIOLATION OF MUON NUMBER CONSERVATION

Robert P. Redwine
Massachusetts Institute of Technology

Introduction

The question of violation of muon number conservation is one that has occupied considerable attention and resources in recent years. The first generation of experiments at the medium-energy accelerators has now been completed and the next generation of experiments is ready to begin. In this talk I will review the history of muon number conservation, including the reasons for our present belief that the conservation law may not be exact. After that I will examine the experiments that have been completed in the last few years, and then look carefully at the new experiments that are being mounted and planned at several laboratories. Finally, the relationship of these types of experiments to other studies, such as searches for neutrino oscillations, will be considered.

As many of you know, although I have participated in these experiments in recent years, I am no longer directly involved. Therefore, to some extent I should qualify as a neutral observer. I shall try to give a point of view that, if not entirely unbiased, is at least disinterested!

Muon Number Conservation

As is well known, the discovery of the muon[1] was unexpected. Its properties appeared to be identical to those of the electron, except for having much greater mass. To some extent we are still trying to answer the question of why the muon exists, only the question is now usually posed differently. We speak now of understanding the different generations of lepton flavors.

By the late 1950s there was growing evidence that at least one property did distinguish the muon from the electron. There was no evidence for the following decays and reactions:

$$\mu^+ \to e^+\gamma \quad , \tag{1}$$

$$\mu^+ \to e^+e^+e^- \quad , \tag{2}$$

and

$$\mu^- + Z \to e^- + Z \text{ (muon-electron conversion).} \tag{3}$$

At that time the branching-ratio limits were $\sim 10^{-4}$ or a little better, but the evidence for a new conservation law was not conclusive. This was because of the difficulty of calculating Eqs. (1), (2), and (3) with the nonrenormalizable weak-interaction theories then in use. Integral cutoff parameters introduced sufficient uncertainty into the calculations as to make the comparison with experiment inconclusive.

The definitive experiment that led to the introduction of a new conservation law was done at Brookhaven National Laboratory in 1962.[2] It was found that the neutrinos emitted in π^+ decay could produce muons, but not electrons.

$$\pi^+ \to \mu^+ + \nu_\mu$$

$$\hookrightarrow \nu_\mu + Z \not\to e^- + \ldots$$

$$\hookrightarrow \nu_\mu + Z \to \mu^- + \ldots \quad .$$

One was thus led to the idea of muon number conservation; that is, in addition to conservation of lepton number, a separate conservation of lepton flavors was required. All evidence so far[3] is consistent with an additive muon number conservation law, although it is useful to remember that a multiplicative law has not been strictly ruled out. Certainly one generally now assumes that the conservation law is additive.

There has been continuing improvement in the branching-ratio limits of Eqs. (1), (2), and (3), as well as in other tests of muon number conservation. I will discuss here the tests that have been performed at the medium-energy laboratories but will not, for example, discuss strangeness-changing reactions and decays. Actually, from approximately the mid-1960s until the late 1970s there was relatively little improvement in the branching-ratio limits. This situation changed drastically in 1977 for two reasons.

1. The commissioning of the three meson factories (LAMPF, SIN, and TRIUMF) made it possible to investigate muon number violating processes with much greater precision. In fact, a very preliminary report (actually a few rumors!) of a possible positive result from a $\mu^+ \rightarrow e^+\gamma$ search at SIN, although later shown to be false, served the very useful purpose of getting the attention of the physics world.
2. More importantly, it was recognized about the same time that there are sound theoretical reasons for believing that muon number should not be a strictly conserved quantity.

The current theoretical situation as regards muon number conservation deserves some comment. (I emphasize the word "comment" because this is not the place and I am not the person for a complete review of the subject.) The so-called Standard Model of weak and electromagnetic interactions, from Weinberg, Salam, and Glashow,[4] contains no violation of lepton flavor conservation. However, when one tries to include the strong interaction in the unification [the unfortunately named Grand Unified Theories (GUTs)], violation of several previously conserved quantities becomes possible and perhaps even probable. These are

- lepton flavor violation,
- lepton number violation,
- baryon number violation, and
- nonzero neutrino masses.

All of these are presently under active investigation. Lepton flavor violation is, of course, the topic of this talk. Lepton number violation is being investigated through the search for neutrinoless double-beta decay.[5] Searches for baryon number violation, through proton decay or neutron-antineutron oscillations, are presently beginning in earnest.[6] Nonzero neutrino masses are being investigated through direct searches as well as through searches for neutrino oscillations.[7]

To test the current GUTs, an obvious question is, Which of these processes should one search for? The answer turns out to be extremely model dependent. That is, in some models a conservation law may not strictly hold, but the predicted violation is so small as to be unobservable in practice. The same model may predict a quite observable rate for another "forbidden" process, but for a different model the situation may be reversed.

As an example of these ideas, consider the standard SO(10) GUT.[8] This model predicts baryon number violation that may indeed be observable in proton decay experiments in the next several years. It also predicts a negligibly small rate of lepton flavor violation. However, SO(10) models can break down to a model involving $SU(2)_L \times SU(2)_R \times U(1)$, which would allow lepton flavor violation at rates comparable to present limits.[8]

In the absence of clear guidelines from theory (and probably also for somewhat philosophical reasons), the approach of experimenters has been to view all these searches as important. They are fundamental experiments, and the clear observation of any of the forbidden processes would be of enormous value for the current generation of theories.

Even if a theory does predict that lepton flavor violation may occur at observable levels, the relative rates of the processes [Eqs. (1), (2), and (3)] are quite model dependent. There are, of course, unitarity-type limits on the relative rates of the processes. For example, if $\mu^+ \rightarrow e^+\gamma$ were observed at a certain level, one could treat the $\mu^+ \rightarrow e^+\gamma$ vertex as phenomenological and predict the $\mu^+ \rightarrow e^+e^+e^-$ rate using a virtual photon producing a real pair. This is illustrated in Fig. 1. Thus the $\mu^+ \rightarrow e^+e^+e^-$

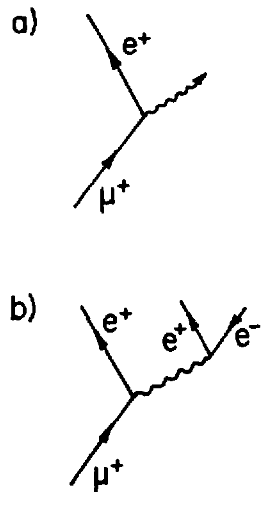

Fig. 1.
If $\mu^+ \rightarrow e^+\gamma$ decay were observed (a), it would imply a lower limit on $\mu^+ \rightarrow e^+e^+e^-$ decay, treating the $\mu^+ \rightarrow e^+\gamma$ vertex as phenomenological (b). However, other mechanisms for muon number violation could give higher rates for $\mu^+ \rightarrow e^+e^+e^-$ decay than for $\mu^+ \rightarrow e^+\gamma$ (Ref. 9).

rate would be a lot ($\sim\alpha$) smaller than the $\mu^+ \to e^+\gamma$ rate, but finite. However, there are models[9] that predict a $\mu^+ \to e^+e^+e^-$ rate even greater than the $\mu^+ \to e^+\gamma$ rate. Once again, the approach taken by experimentalists has been to search for all muon number violating processes as sensitively as possible.

Recent Experimental Results

Present limits on several muon number violating processes are shown in Table I (Refs. 10-17). The list is somewhat selective, but does include the best current limits as well as a few other recent measurements. With the exception of the $\mu^+ \to e^+e^+e^-$ limit, all the measurements have been completed since the renewed interest in this subject started in 1977. Thus the $\mu^+ \to e^+e^+e^-$ limit probably has the greatest room for improvement; as we shall see, this is reflected in plans for upcoming experiments. The listed limit for $\mu^+ \to e^+\gamma\gamma$ comes from a separate analysis of a search for $\mu^+ \to e^+\gamma$ at TRIUMF.

The last entries in the table, results of searches for $\mu^-Z \to e^+Z'$, deserve some comment because the physics issues are somewhat different than for the other listed processes. For some time, $\mu^-Z \to e^+Z'$ has been searched for as a test of the Konopinski-Mahmoud muon number conservation scheme,[18] in which μ^- and e^+ have the same quantum number. Today we realize that this reaction tests issues similar to those addressed by searches for neutrinoless double-beta decay.

The first of the two limits on $\mu^-Z \to e^+Z'$ is the result of a counter experiment, the same measurement that produced the limit on $\mu^-Z \to e^-Z$. The second entry is the result of a radiochemical search for $\mu^-Z \to e^+Z'$. Although the quoted limit ($\sim 3 \times 10^{-10}$) appears to be more stringent than the counter experiment limit, we shall see that it is actually considerably less stringent.

TABLE I

PRESENT BRANCHING-RATIO LIMITS[a] FOR VARIOUS MUON NUMBER VIOLATING PROCESSES

$\frac{\mu^+ \to e^+\gamma}{\mu^+ \to \text{all}}$	$<1.7 \times 10^{-10}$	LAMPF	Ref. 10
	$<1.1 \times 10^{-9}$	SIN	Ref. 11
	$<3.6 \times 10^{-9}$	TRIUMF	Ref. 12
$\frac{\mu^+ \to e^+\gamma\gamma}{\mu^+ \to \text{all}}$	$<5 \times 10^{-8}$	TRIUMF[b]	Ref. 13
$\frac{\mu^+ \to e^+e^+e^-}{\mu^+ \to \text{all}}$	$<1.9 \times 10^{-9}$	Dubna	Ref. 14
$\frac{\mu^-Z \to e^-Z}{\mu^-Z \to \nu_\mu + \ldots}$	$<7 \times 10^{-11}$ ($Z = {}^{32}S$)	SIN	Ref. 15
$\frac{\mu^-Z \to e^+Z'}{\mu^-Z \to \nu_\mu + \ldots}$	$<9 \times 10^{-10}$ ($Z = {}^{32}S$)	SIN	Ref. 16
	$<3 \times 10^{-10}$[c] ($Z = {}^{127}I$)	SIN	Ref. 17

[a]All listed limits are at the 90% confidence level.

[b]This limit is the result of a separate analysis of the TRIUMF $\mu^+ \to e^+\gamma$ search (Ref. 12) by the authors of Ref. 13.

[c]This limit comes from a radiochemical experiment, which has severe limitations in its real sensitivity (see text).

Before proceeding to an examination of the next generation of experiments, it is useful to look at the parameters of the experiments already completed. We will look at the two experiments I consider to be the most sophisticated of those listed in Table I, the LAMPF $\mu^+ \rightarrow e^+\gamma$ search[10] and the SIN $\mu^-Z \rightarrow e^-Z$ search.[15] Not surprisingly, these have also provided the two lowest branching-ratio limits.

Figure 2 shows the LAMPF $\mu^+ \rightarrow e^+\gamma$ apparatus [hereafter referred to as LAMPF $\mu^+ \rightarrow e^+\gamma$ (I)]. A surface μ^+ beam, coming from stopped π^+ decay at the surface of the production target, was stopped in a polyethylene target. The e^+ spectrometer consisted of a bending magnet with multiwire proportional chambers (MWPCs) before and after the field region. The γ spectrometer consisted of a wall of NaI(Tℓ) crystals, with a sweeping magnet to remove charged particles coming from the target. The Van de Graaff accelerator shown in Fig. 2 was used for calibration purposes.

The approach taken in this experiment, as well as in others we will discuss, was to reduce possible backgrounds to zero over the running time of the experiment. Some of these backgrounds come from accidental coincidences; to reduce these, good timing resolution as well as lower beam peak current are used. Other backgrounds are of a more intrinsic nature. For example, a background for $\mu^+ \rightarrow e^+\gamma$ is radiative μ^+ decay,

$$\mu^+ \rightarrow e^+\nu\bar{\nu}\gamma \quad ,$$

where the neutrinos can in principle take away negligible energy and look like a $\mu^+ \rightarrow e^+\gamma$ event. Fortunately, such radiative decay events occupy a small part of phase space and are thus relatively unlikely. To reduce such intrinsic backgrounds, good energy and angular resolution are especially important.

Relevant parameters of the $\mu^+ \rightarrow e^+\gamma$ (I) experiment are shown in Table II. It will be useful to compare these parameters with those expected for upcoming experiments. We will note an especially large variation in the important factor of solid angle times efficiency $(\Omega/4\pi)\varepsilon$.

The apparatus used for the SIN $\mu^-Z \rightarrow e^-Z$ search is shown in Fig. 3. Muons from π^- decay in a superconducting solenoid were stopped in a sulfur target. The sulfur target was at the center of a small streamer chamber, which itself was in the (roughly) constant axial magnetic field produced by a superconducting Helmholtz coil. Electrons and positrons, emerging from the target at 90°

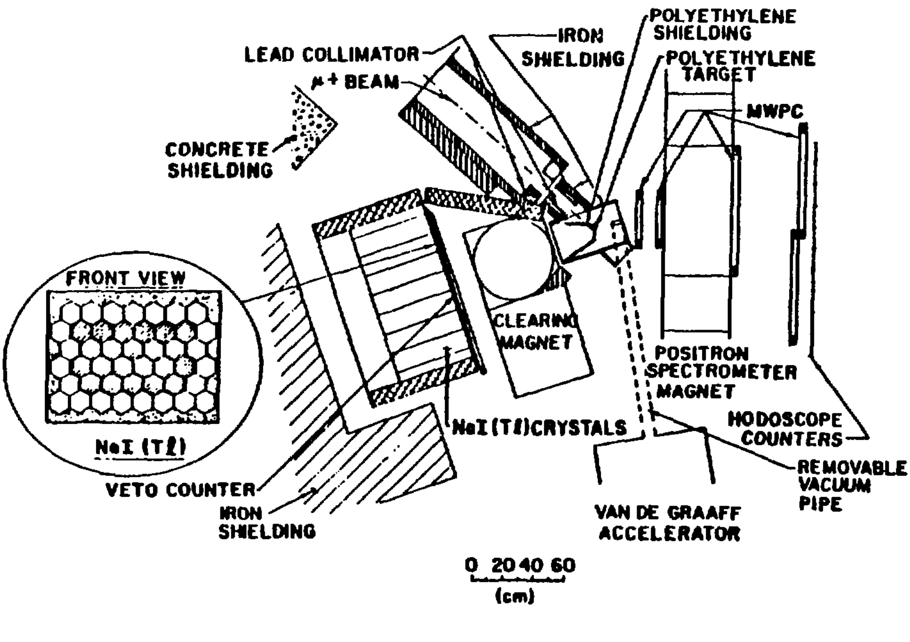

Fig. 2.
Experimental arrangement used for the LAMPF $\mu^+ \rightarrow e^+\gamma$ (I) experiment (Ref. 10).

TABLE II

PARAMETERS OF THE LAMPF $\mu^+ \rightarrow e^+\gamma$ (I) EXPERIMENT

$\frac{\Delta E_\gamma}{E_\gamma}$	8%
$\frac{\Delta E_e}{E_e}$	9%
Δt	2 ns
$(\Omega/4\pi)\varepsilon$	1.8%
Average μ^+ rate	2.5×10^6/s

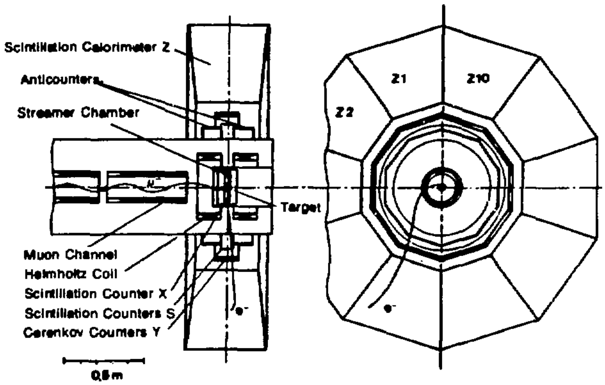

Fig. 3.
Detector system used for the SIN muon-electron conversion search (Ref. 15).

to the beam, triggered concentric rings of scintillation and Čerenkov counters; such a trigger was used to fire the high voltage and cameras of the streamer chamber. The electron/positron energy was then determined from the curvature of the track as measured in the streamer chamber.

Because this was a singles experiment (a good event involved detecting only one particle), special care had to be taken to control not only singles backgrounds but also the raw trigger rate. The latter was accomplished by containing the Michel electrons ($E_e < 53$ MeV) from the target in the magnetic-field region inside the first trigger counters. The most serious potential source of background events was from radiative π^- capture with subsequent production of an asymmetric electron-positron pair. Because there were many π^-'s in the beam, this was a potentially fatal source of background. It was suppressed at SIN[19] by pulsing the main proton beam. The pulsing frequency was 400 kHz, with a beam-off suppression of about 10^{-7}. One then looked for electrons emitted during the beam-off period, long after the pions had decayed or been captured.

There are also intrinsic backgrounds in a $\mu^-Z \rightarrow e^-Z$ experiment, from two main sources,

(1) muon decay in orbit, $\mu^- + Z \rightarrow Z + e^- + \nu + \bar{\nu}$, and

2. radiative muon capture with production of an asymmetric pair.

Both intrinsic backgrounds can yield electrons of energy ~105 MeV, which is the signal one is looking for, but both rates are greatly suppressed by phase-space factors. The key to using the phase-space suppression is, of course, good energy resolution for the electron/positron. The resolution and acceptance of the SIN $\mu^-Z \rightarrow e^-Z$ search are shown in Table III. The chief limitation on the μ^- rate was the ability of the beam pulser to maintain a good suppression factor.

It is interesting to look at the electron and positron spectra obtained in this experiment. The electron energy spectrum is shown in Fig. 4. Also shown is a Monte Carlo calculation of the intrinsic backgrounds and a Monte Carlo simulation of the events that should have been observed if the $\mu^-Z \rightarrow e^-Z$ branching ratio were 1×10^{-9}. It is clear that the measured spectrum is consistent with the intrinsic background from muon decay in orbit and from radiative muon capture.

Figure 5 shows the measured positron spectrum, as well as the Monte Carlo simulation of the radiative muon capture background. Though the two are certainly consistent, the interpretation of the data in terms of a limit on $\mu^-Z \rightarrow e^+Z'$ is somewhat complicated. This is

TABLE III

PARAMETERS OF THE SIN $\mu^-Z \rightarrow e^-Z$ SEARCH

$\frac{\Delta E_e}{E_e}$	7%
$(\Omega/4\pi)\varepsilon$	5%
Average μ^- rate	3×10^5/s

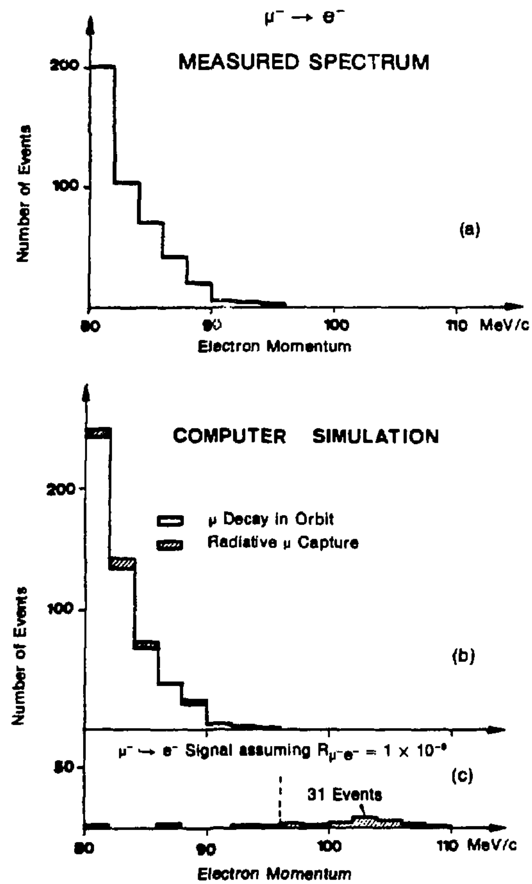

Fig. 4.

Results of the SIN $\mu^-Z \rightarrow e^-Z$ search. The measured e^- energy spectrum is shown in (a); (b) is a calculated spectrum from the expected events from μ decay in orbit and from radiative μ capture. If the searched-for process occurred at a rate of 1×10^{-9}, the additional spectrum shown in (c) should have been observed.

because one does not expect the positron energy spectrum from $\mu^-Z \rightarrow e^+Z'$ to display a line at the maximum positron energy.[20] Because the initial and final nuclear states cannot be the same, the average excitation of the residual nucleus can be quite significant (for normal μ^- capture, $\mu^-Z \rightarrow \nu_\mu Z'$, it is ~15 MeV[21]). The authors of Ref. 16 assumed an average excitation of 20 MeV and a width also equal to 20 MeV to quote their limit of 9×10^{-10}. This is illustrated in Fig. 5.

It is for this reason (the fairly high excitation of the residual nuclear system) that radiochemical searches for $\mu^-Z \rightarrow e^+Z'$ are likely to be of limited usefulness. Such searches are obviously sensitive only to the particle-stable states of the residual nucleus. Because the β emitters in the radiochemical searches are bound by only a few MeV, one is sensitive to only a small portion of the cross section, probably $\ll 10\%$ (Ref. 20). The limit shown in Table I from Ref. 17 assumed that *all* the cross section went to particle-stable states. Therefore, it is in reality a much less stringent limit on $\mu^-Z \rightarrow e^+Z'$ than is the counter experiment.

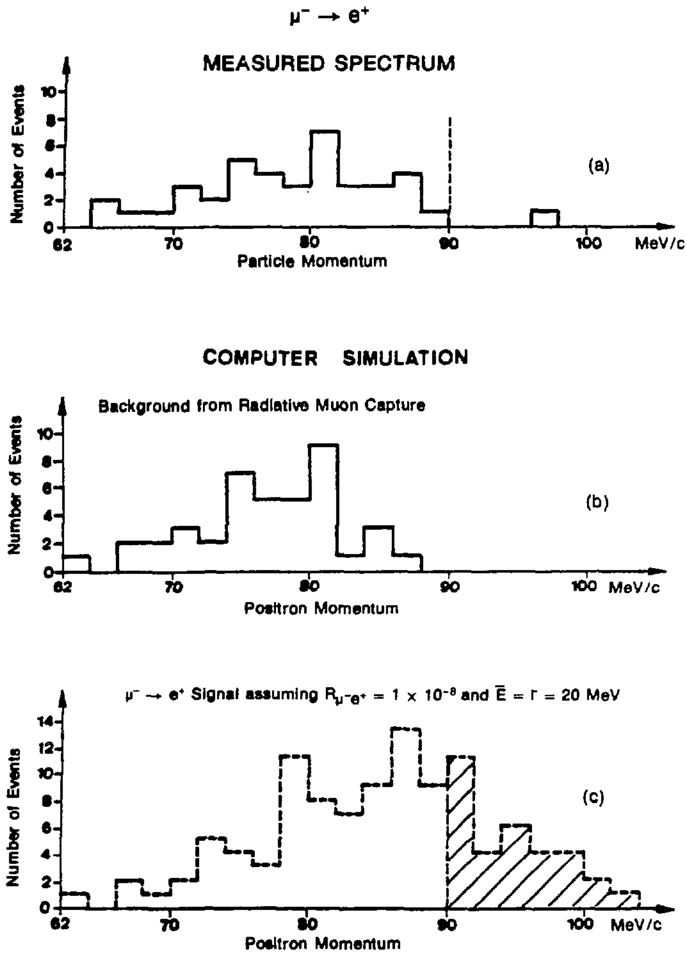

Fig. 5.

Results of the SIN $\mu^-Z \to e^+Z'$ search. The measured e^+ spectrum is shown in (a); expected background from radiative μ capture is shown in (b); and the total expected spectrum from background events and real events, assuming a branching ratio of 10^{-8} and a spectrum shape of $\bar{E} = \Gamma = 20$ MeV (see text) is given in (c).

The history of experimental branching-ratio limits for muon number violating processes is shown in Fig. 6.* Besides showing the relative lack of interest in the subject between 1965 and 1976, it indicates an exponential improvement with time. It has been said[22] that this means the longer one waits, the better experiment one does! I prefer to think that it means the longer one waits, the better experiment one *has* to do. We will see in the next section that this exponential improvement in the branching-ratio limits is likely to continue for at least several more years.

*Figure 6 is from C. M. Hoffman, Los Alamos National Laboratory, Group MP-4.

Upcoming Experiments

There are a number of experiments to study muon number violation that are being installed or planned at the various medium-energy accelerators. In this section I will discuss the aims and characteristics of each of them.

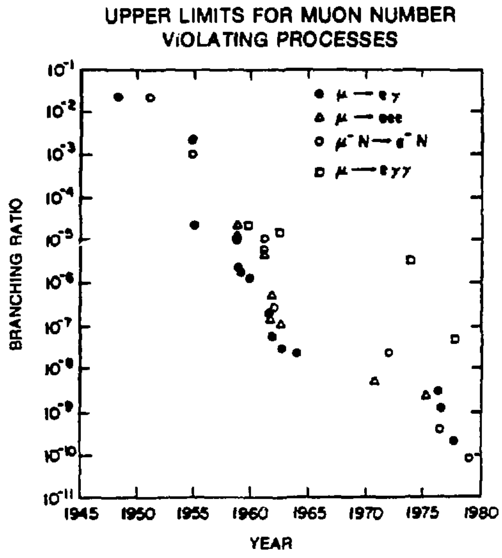

Fig. 6.
History of upper limits for muon number violating processes (Ref. 22).

Because some of the detector systems will be used to look for more than one muon number violating process, I will not try to group the experiments according to the reaction or decay to be investigated. Instead, it seems most sensible to discuss them in the chronological order in which they are expected to begin taking data. Obviously, this ordering is only a guess for those experiments that are a few years away.

TRIUMF Time Projection Chamber (Ref. 23)*

The aim of the TRIUMF Time Projection Chamber (TPC) project is to look for the reactions

$$\mu^- Z \rightarrow e^- Z$$

and

$$\mu^- Z \rightarrow e^+ Z' \quad .$$

*Information was also obtained by private communications from M. Blecher and D. Bryman.

The apparatus is schematically shown in Fig. 7. The μ^- beam enters along the axis of the detector, parallel to a magnetic field in which the TPC is located. Tracks from e^+'s and e^-'s emerging from the target located at the center of the TPC are measured to determine the momentum and origin of the event. The principle of the operation of the TPC has been described in Ref. 24. Roughly, an applied electric field is parallel to the magnetic field. Therefore, the ionization electrons drift toward the end regions where they are multiplied and collected by x-y wire proportional planes. The position in the x-y plane is then given by the centroid of the collected charge on the x-y wires. The z position (along the axis) is given by the drift time of the electrons to the end plane. A number of (x,y,z) determinations are available for each track. thus making it possible to reconstruct the trajectory of the e^+ or e^-.

The TRIUMF experiment is now set up and will begin a long data run in November. In fact, a preliminary branching-ratio upper limit for manganese has already been achieved,[23]

$$\frac{\mu^- + \mathrm{Mn} \rightarrow e^- + \mathrm{Mn}}{\mu^- + \mathrm{Mn} \rightarrow \nu_\mu + \ldots} < 2 \times 10^{-9} \quad .$$

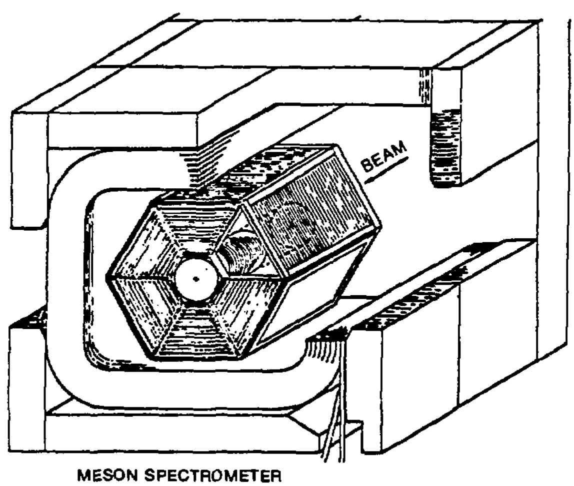

Fig. 7.
Diagram of the TRIUMF TPC. It will be used to search for $\mu^- Z \rightarrow e^- Z$ and $\mu^- Z \rightarrow e^+ Z'$.

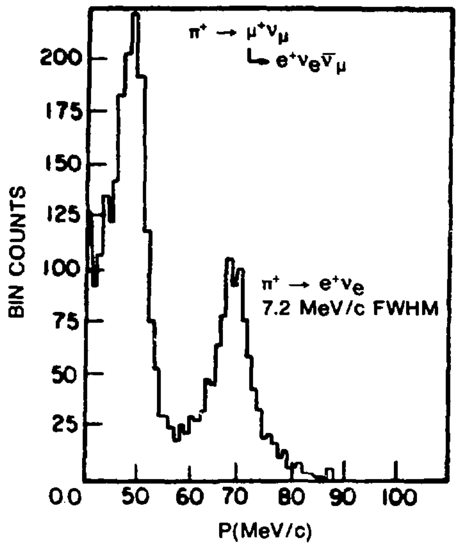

Fig. 8.
Positron momentum spectrum from π^+'s stopped in the TRIUMF TPC.

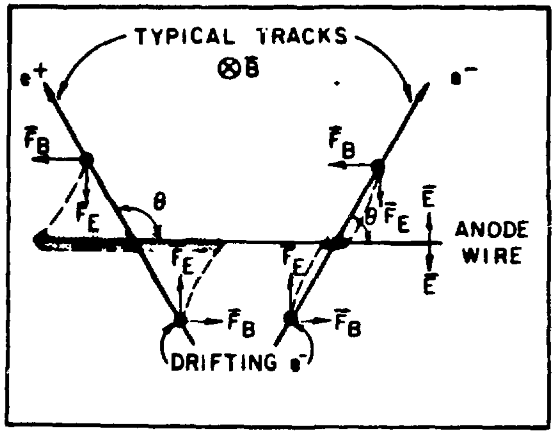

Fig. 9.
$\bar{E} \times \bar{B} \neq 0$ effects near an anode wire of the TRIUMF TPC (see text). The charge distribution is smaller for electron tracks than for positron tracks.

Although this is not as stringent as the previous limit for ^{32}S (Ref. 15), it is the best limit for an isospin nonzero nucleus.

As part of the testing of the TPC, the TRIUMF group saw evidence for an interesting effect near the end wires. To test the resolution of the system,they stopped positive pions in the target and looked for positrons from $\pi^+ \to e^+\nu_e$ decay. The resulting energy spectrum is shown in Fig. 8. Besides a clear peak from the $\pi^+ \to e^+\nu_e$ decay, one sees the beginning of the Michel e^+ spectrum, cut off at the low-energy end by the acceptance of the TPC. Unfortunately, the measured resolution (7.2 MeV/c FWHM) is significantly worse than anticipated. This phenomenon was traced to $\vec{E} \times \vec{B}$ effects in the region of the x-y anode wires. Figure 9 shows a view along the axis of the TPC. Ionization electrons drift toward the end wires along the direction of the applied electric field — that is, into the page. However, because the field lines must end on the wires, $\vec{E} \times \vec{B}$ can be nonzero in the vicinity of the wire. This leads to a smearing of the position of the charge arriving at the anode wire. Because it is the centroid of the charge that is detected, this affects the position and therefore the energy resolution. The smearing is very much a function of the angle the track makes with the anode wire. The measured dependence is shown in Fig. 10. Because electrons and positrons are bent in opposite directions by the axial magnetic field, their tracks make different angles with the anode wires. As shown in Fig. 9, the result is that the positron resolution is affected significantly whereas the electron resolution is relatively unaffected.

The TRIUMF group believes they understand the effect and that it will not compromise the expected electron energy resolution of 4%. Energy resolution is less important for the positron, as we have already discussed. It never hurts to be lucky!

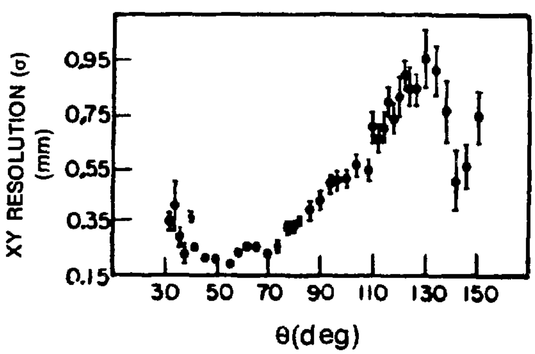

Fig. 10.
Measured x-y resolution in the TRIUMF TPC as a function of the angle of the track with respect to the anode wire (see Fig. 9 for the definition of θ).

TABLE IV

PARAMETERS OF THE
TRIUMF TPC $\mu^- Z \rightarrow e^- Z'$ SEARCH

$\frac{\Delta E_e}{E_e}$	4%
$(\Omega/4\pi)\varepsilon$	40%
Average μ^- rate	10^6/s

Important parameters for the TRIUMF $\mu^- Z \rightarrow e^- Z'$ experiment are shown in Table IV. There are large improvements over the SIN experiment (Table III), especially in the acceptance of the apparatus. The only serious uncertainty remaining is whether an rf separator can be used to suppress fast backgrounds from π^--induced events.* As mentioned above, data taking with the full system is beginning at this time; a run of ~1 year is planned. This should allow the group to set an upper limit to $\mu^- Z \rightarrow e^- Z$ of $<10^{-11}$.

LAMPF Crystal Box**

The LAMPF Crystal Box project is an attempt to make major improvements in the branching-ratio limits of three muon number violating processes,

(1) $\mu^+ \rightarrow e^+ e^+ e^-$,
(2) $\mu^+ \rightarrow e^+ \gamma\gamma$, and
(3) $\mu^+ \rightarrow e^+ \gamma$.

A diagram of the apparatus is shown in Fig. 11. Positive muons will be brought in along the axis of the detector and stopped in a target at the center. There are eight cylindrical drift-chamber planes to measure the trajectory of emerging charged particles. The drift chambers are surrounded by plastic scintillator hodoscope counters and by 396 NaI(Tl) detectors (hence the name) to measure the energies of electrons, positrons, and γ's. There is no magnetic field in this experiment, so reconstructing the energy of each event depends on the resolution obtained from the NaI(Tl) crystals. These crystals were purchased for optimal energy and position resolution for γ's in this energy range.

TABLE V

PARAMETERS OF THE
LAMPF CRYSTAL BOX EXPERIMENT

$\frac{\Delta E_\gamma}{E_\gamma}$	6%
$\frac{\Delta E_e}{E_e}$	6%
Δt	0.7 ns
$(\Omega/4\pi)\varepsilon$	20%
Average μ^+ rate	5×10^5/s

Reduction of backgrounds to negligible levels depends on reconstructing as well as possible the time structure, energy, vector momentum, and vertex of an event. The experiment has been optimized for these requirements. Relevant parameters are shown in Table V. It can be seen that there are large improvements over the LAMPF $\mu^+ \rightarrow e^+\gamma$ (I) parameters (Table II). The limitation in

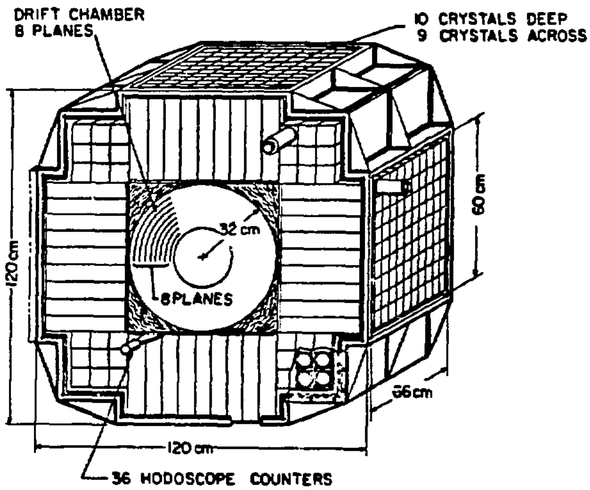

Fig. 11.
Schematic view of the LAMPF Crystal Box. The experiment will look for $\mu^+ \rightarrow e^+e^+e^-$, $\mu^+ \rightarrow e^+\gamma\gamma$, and $\mu^+ \rightarrow e^+\gamma$.

*D. Bryman, private communication.

**LAMPF Exps. 400/445, M. Duong-van, C. M. Hoffman, H. S. Matis, and J. D. Bowman, Spokesmen; also, C. M. Hoffman, private communication.

average μ^+ rate is due to the necessity to reduce accidental coincidences.

As almost all the hardware for the experiment is in hand, data taking is expected to begin in 1982. Figure 12 shows the branching-ratio limits that should be reached by the Crystal B~x experiment. The $\mu^+ \rightarrow e^+\gamma$ (II) line refers to the $\mu^+ \rightarrow e^+\gamma$ data, which will be taken simultaneously with the search for $\mu^+ \rightarrow e^+e^+e^-$ and $\mu^+ \rightarrow e^+\gamma\gamma$. The $\mu^+ \rightarrow e^+\gamma$ (III) line refers to a future reconfiguration of the NaI(Tℓ) crystals, described below.

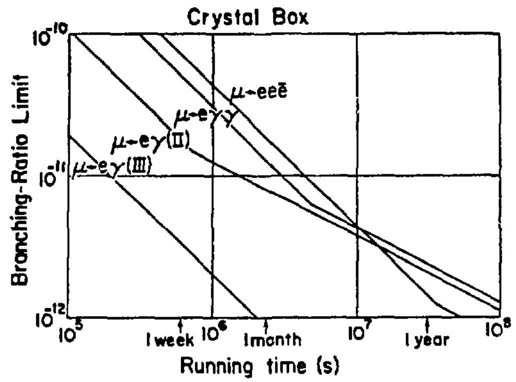

Fig. 12.
Projected branching-ratio limits for the LAMPF Crystal Box as a function of running time. The line labeled $\mu^+ \rightarrow e^+\gamma$ (III) refers to a later experiment, as discussed in the text.

Yale-Pennsylvania-LAMPF $\mu^-Z \rightarrow e^{\pm}Z'$ Search*

An experiment to search for $\mu^-Z \rightarrow e^-Z$ and $\mu^-Z \rightarrow e^+Z'$ at LAMPF is well along in preparation. The experimental apparatus is shown in Fig. 13. The principle of the detector is similar to that of a TPC. A cylindrical drift chamber is enclosed in a constant magnetic field whose direction is along the axis of the drift chamber. The applied electric field, and thus the drift direction, is parallel to the magnetic field. A μ^- beam is brought in along the axis to stop in a target at the center of the drift chamber. Electron and positron trajectories emerging from the target are determined by locating the drift electron charge centroid on the end cap wires and by measuring the drift time to the end cap. In this case the end cap wires will be radial.

The expected parameters for the experiment are shown in Table VI. It is hoped to be able to set a branching-ratio limit for $\mu^-Z \rightarrow e^-Z$ of around 10^{-12}. It should be noted that this experiment (muon-electron conversion) in principle is well suited for LAMPF because it

*LAMPF Exp. 421. P. A. Souder, Spokesman; also, P. A. Souder, private communication.

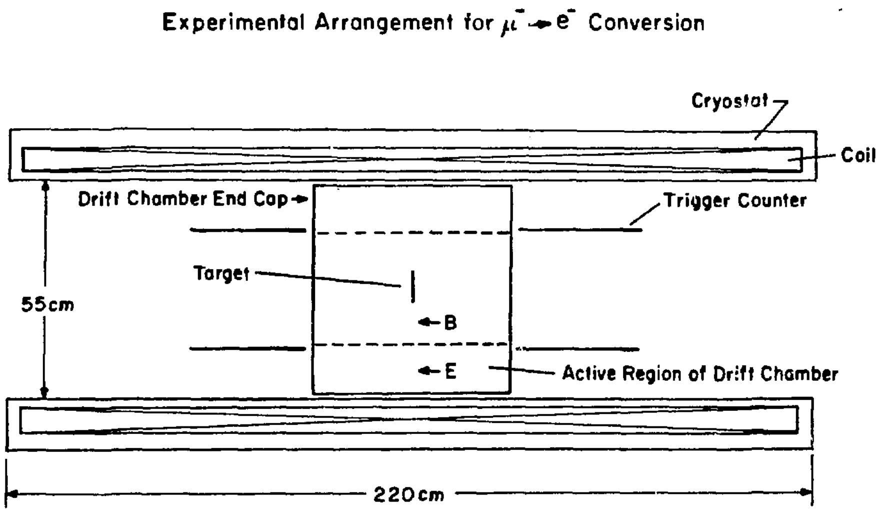

Fig. 13.
Detector system for the $\mu^-Z \rightarrow e^{\pm}Z'$ search at LAMPF. The principle of operation of the detector is similar to that of a TPC (see text).

TABLE VI

PARAMETERS OF THE
YALE-PENNSYLVANIA-LAMPF
$\mu^- Z \rightarrow e^{\pm} Z'$ SEARCH

$\frac{\Delta E_e}{E_e}$	~3%
$(\Omega/4\pi)\varepsilon$	~50%
Average μ^- rate	$5. \times 10^5$/s

is a singles experiment and the relatively low LAMPF duty factor is not a disadvantage. It is, of course, still necessary to suppress the background from π^- captures. Various methods could be used, but it is likely that simply using the long Stopped Muon channel (SMC) at LAMPF for ~30-MeV/c μ^-'s will reduce the π^-'s by decay sufficiently for the measurement. Equipment is under development now for this project.

SINDRUM

A detector system is planned at SIN to look for $\mu^+ \rightarrow e^+e^+e^-$ decays. The detector has been named SINDRUM and is presently being designed. A cylindrical magnetic detector, having large acceptance and good resolution, is contemplated. Rough parameters are shown in Table VII. The advantage of using the SIN high duty factor for coincidence experiments is evident. A branching-ratio limit for $\mu^+ \rightarrow e^+e^+e^-$ of $<10^{-12}$ should be attainable. The SINDRUM project is presently being considered at SIN, and a decision on its authorization is expected shortly.*

LAMPF $\mu^+ \rightarrow e^+\gamma$ (III)**

After the Crystal Box at LAMPF has completed data taking, it is planned to reconfigure the NaI(Tl) crystals (as well as about 400 more crystals) into the arrangement shown in Fig. 14. This will be the $\mu^+ \rightarrow e^+\gamma$ (III) experiment at LAMPF. Surface μ^+'s will be brought in through the pole face of a dipole magnet to stop in the target at the center. The γ energy and position will be measured in one of two walls of NaI(Tl) crystals. The energy of the positron will be determined by its trajectory in the magnetic field. Anticipated parameters of the experiment are given in Table VIII. The experiment should be quite powerful, with an ultimate sensitivity to $\mu^+ \rightarrow e^+\gamma$ decay of a few $\times 10^{-13}$. This is probably the rough limit of sensitivity for such experiments at present-day medium-energy accelerators. As there is not a lot of room for improvement in the acceptance of the experiments and the running times are already fairly long, major improvements in the branching ratios will come from using much more intense muon beams.

The $\mu^+ \rightarrow e^+\gamma$ (III) experimental schedule is fairly uncertain because of the necessity to wait until the Crystal Box runs are completed. This experiment clearly will not be able to begin data taking for a few years.

*H. K. Walter, private communication; and J. P. Blaser, private communication.

**LAMPF Exp. 444, J. D. Bowman and R. Hofstadter, Spokesmen; and J. D. Bowman, private communication.

TABLE VII

PARAMETERS OF THE
SINDRUM $\mu^+ \rightarrow e^+e^+e^-$ PROPOSAL

$\frac{\Delta m_\mu}{m_\mu}$	4%
$(\Omega/4\pi)\varepsilon$	8%
Average μ^+ rate	10^7/s

TABLE VIII

PARAMETERS OF THE
LAMPF $\mu^+ \rightarrow e^+\gamma$ (III) EXPERIMENT

$\frac{\Delta E_\gamma}{E_\gamma}$	4%
$\frac{\Delta E_e}{E_e}$	0.6%
Δt	0.7 ns
$(\Omega/4\pi)\varepsilon$	16%
Average μ^+ rate	2.5×10^7/s

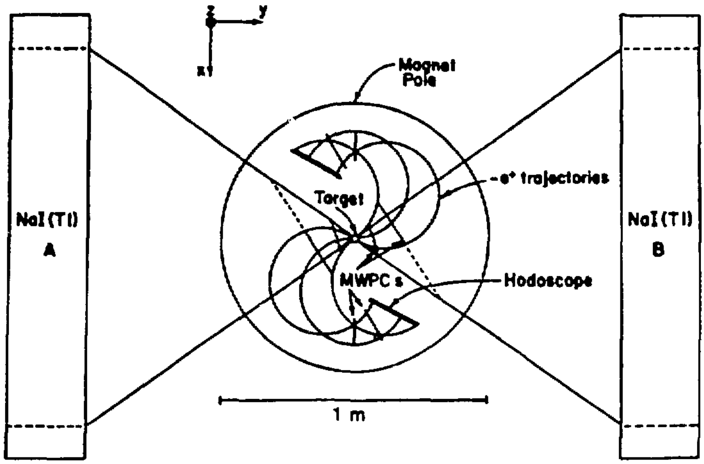

Fig. 14.
Design of the future LAMPF $\mu^+ \rightarrow e^+\gamma$ (III) experiment.

Conclusions

A significant portion of the resources at medium-energy laboratories have been and are being devoted to searches for muon number violation. It is certainly a legitimate question to ask whether the physics issues are sufficiently important to justify the expenditures. Such questions clearly can be answered only subjectively, but that does not mean the answers are unimportant. It is useful to keep in mind that these are, by any measure, fundamental experiments. Measurements were under way before present-day GUTs made muon number violation fashionable. Though much of the current interest comes from the possibility of testing present theories, one would in any event want to test the conservation law to the limit of the available beam fluxes.

I point this out because experiments can become unfashionable as quickly as they become fashionable. For example, there is a great deal of interest currently in neutrino oscillations. Muon number violation is related to neutrino oscillations: $\nu_\mu \leftrightarrow \nu_e$ implies muon number violation and *vice versa.** If neutrino oscillations were the only mechanism for muon number violation, current limits on neutrino masses and mixing would allow only an unobservably small muon number violation rate. Of course, neutrino oscillations are *not* the only possible mechanism for muon number violation, and one should view the two types of experiments as complementary. Searches for violation of a number of symmetries that are "unnatural" in the context of present theories[8] are a major task for experimentalists at many laboratories. The muon number violation searches at medium energy laboratories form an important part of this work.

*T. Goldman, private communication.

Acknowledgments

Because much of the work discussed here is not mine, I am indebted to many people for information, graphs, and discussions. Special thanks are owed to G. Backenstoss, J. P. Blaser, M. Blecher, J. D. Bowman, D. Bryman, M. D. Cooper, T. W. Donnelly, T. Goldman, B. Hahn, C. M. Hoffman, P. Souder, and H. K. Walter.

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WHY IS THERE AN ANOMALOUS SPIN- AND ISOSPIN-DEPENDENT MUON-NUCLEAR INTERACTION?

George A. Rinker
Los Alamos National Laboratory

When I originally agreed to give this talk I hoped to survey all the interesting things going on in muonic-atom physics these days. It soon became apparent that this was an unrealistic goal, so I decided to concentrate on open problems and unresolved discrepancies with the hope of stimulating some of you who might be looking for new fields to conquer. A great deal of interesting and useful work is being done, and the wealth of data being produced at meson factories invites ever more serious and complicated theoretical efforts. It turns out, however, that there are not many serious outstanding discrepancies in this field. Quantum electrodynamics (QED) is in good shape in spite of meticulous experimental efforts to find fault with it. Interest in chemical effects is growing, but there are no glaring anomalies that I know of, even though the experiments and calculations are subtle, complex, and excitingly new. Also, nuclear effects are very well understood in principle, even though the experiments and calculations are full of difficult technical problems that sometimes make even obvious conclusions impossible to reach.[*,1]

So what is there to talk about? In light of the above comments I decided to tease you with an old problem (over 10 years old, in fact) that clearly cannot exist because all possible explanations have been ruled out. First I will show that the discrepancies do exist and must be taken seriously and then I will prove that they cannot exist. I am sorry that the situation is this way. Please do not complain to me about it, as I am not responsible for nature.

The problem is that the measured muonic transition energies in ^{204,206,208}Pb are internally inconsistent. As you all know, lead is the classic simple heavy nucleus that serves as a test case for our understanding of the fundamental ingredients of the muonic-atom theory in heavy nuclei. If we can't understand this very simple system then we have little hope for the more complicated ones. The problems that arise here are problems that arise everywhere in muonic atoms, which is why it is especially interesting when something goes wrong in muonic lead.

What I'm talking about is the famous nuclear polarization problem that was formulated clearly for the first time back in 1969 or so by the Chicago Muonic Atom Group.[2] The idea was to measure transition energies in muonic atoms, fit nuclear charge distributions to the higher lying ones, and thus see if we could determine experimentally the nuclear polarization in the 1s state. This, like a lot of problems in physics, has turned out to be much more complicated than was originally thought. The main difficulty is that the nuclear charge distribution is not given beforehand. There is no independent experiment or theory that can tell accurately enough what the charge distribution is, so we have to fit it to the muonic data; we get involved in consistency checks rather than absolute measurements. What is sometimes forgotten is that through fooling with the charge distribution we are able to hide all kinds of other effects and sometimes fool ourselves into believing something that doesn't really exist. There have been a lot of ^{208}Pb experiments and analyses since then, all of which show similar problems.[*,3-8] The simplest description of the situation is that the muon binding energies are just internally inconsistent, and there is nothing to tell experimentally exactly where the problem lies.

There are also problems with the intensities in the 1969 experiment. The ratio of the $2p_{3/2} \rightarrow 1s_{1/2}$ to the $2p_{1/2} \rightarrow 1s_{1/2}$ intensity is wrong by something like 12%. This may be related to the energy-level problems. There are also problems in ^{206}Pb. Figure 1 gives an idea of what we're talking about. You can view this as a plot of experimental transition energies (the points) vs calculated energies (the line). Things don't look too bad on this scale, but in fact these are extremely precise experiments and there are big discrepancies, several standard deviations.

So that this makes some sense to those of you who may not have been exposed to it, I'll explain how we

*For a current in-depth discussion of these things, see Ref. 1.

*Y. Tanaka, H. D. Wohlfahrt, E. B. Shera, M. V. Hoehn, and R. M. Steffen, private communication.

transform the data to get a plot like this. It's called a radial moment analysis, also known as a model-independent analysis. This latter terminology is appropriate only in the sense that we believe we have reduced the model dependence to a negligible level, as there is no such thing as an absolutely model-independent analysis, either of muonic atoms or of electron-scattering data. Radial moment analysis was invented by Ford and Willis[9] and Barrett.[10] It amounts to doing a variational calculation that enables us to relate each experimental datum to a radial moment of the nuclear charge distribution. One starts with a trial charge distribution $\rho^{(0)}(r_N)$ and, using the Dirac equation and all known corrections, computes a set of trial energies $E_{n\kappa}^{(0)}$, $n = 1,2\ldots$, $\kappa = \pm 1, \pm 2\ldots$. Naturally, these will not be the same as the measured experimental energies. For simplicity, in this discussion we will speak of binding energies, as the generalization to transition energies is trivial. We investigate the effect on these energies of an arbitrary but small variation in the nuclear charge distribution,

$$\delta\rho(r_N) = \rho(r_N) - \rho^{(0)}(r_N) \quad , \qquad (\ )$$

and obtain in lowest order

$$E_{n\kappa} = E_{n\kappa}^{(0)} + Z \int dr_N \, r_N^2 \, \delta\rho(r_N) \int d\Omega_N \, V_{n\kappa}^{(0)}(\underset{\sim}{r}_N) \quad , \qquad (2)$$

where $V_{n\kappa}^{(0)}(\underset{\sim}{r}_N)$ is the electrostatic potential generated by the muon in state $n\kappa$. This provides a linear integral constraint on any acceptable $\rho(r_N) = \rho^{(0)}(r_N) + \delta\rho(r_N)$, in terms of measured energies $E_{n\kappa}$ and trial energies $E_{n\kappa}^{(0)}$. Each experimental datum provides one constraint.

One obtains the radial moment interpretation by parameterizing

$$\int d\Omega_N \, V_{n\kappa}^{(0)}(\underset{\sim}{r}_N) \simeq C + B r_N^k e^{-\alpha r_N} \quad , \qquad (3)$$

so that Eq. (2) may be written

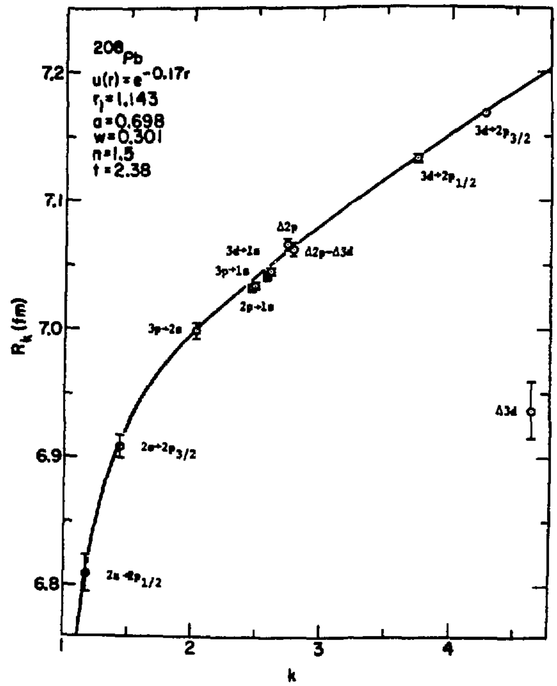

Fig. 1.
Equivalent radii for ^{208}Pb (Ref. 8).

$$\langle r_N^k \, e^{-\alpha r_N} \rangle \simeq \langle r_N^k \, e^{-\alpha r_N} \rangle^{(0)} + \frac{4\pi}{ZB} \left[E_{n\kappa} - E_{n\kappa}^{(0)} \right] \quad , \qquad (4a)$$

$$R_\alpha(k) = R_\alpha^{(0)}(k) - C_Z \left[E_{n\kappa} - E_{n\kappa}^{(0)} \right] \quad . \qquad (4b)$$

Equation (4b) was obtained from Eq. (4a) by defining $R_\alpha(k)$ as the radius of the uniform charge distribution that has the same expectation value defined in Eq. (4a). This is done to get units of length rather than length to the k^{th} power.

In general, we apply Eq. (3) by choosing an optimum value of α for a given nucleus and by numerically fitting C, B, and k to each transition or binding energy. These fits turn out to be accurate enough for our physical interpretation, which can always be verified by direct recalculation.

The crucial thing is the sensitivity of the energy to small variations in the nuclear charge distribution, given by the coefficient C_Z in Eq. (4b). We can be a little more transparent by writing

$$\delta R_\alpha(k) = -C_Z\, \delta E_{n\kappa} \ ,$$
$$\frac{\delta R_\alpha(k)}{R_\alpha(k)} = -\frac{C_Z E_{n\kappa}}{R_\alpha(k)} \frac{\delta E_{n\kappa}}{E_{n\kappa}} \ . \quad (5)$$

Values of k and the coefficient $C_Z E_{n\kappa}/R_\alpha(k)$ are approximately given for some intervals in lead by the following.

Interval	k	$C_Z E_{n\kappa}/R_\alpha(k)$
$2p_{3/2} \rightarrow 1s_{1/2}$	2.5	1.2
$2p_{1/2} \rightarrow 1s_{1/2}$	2.5	1.2
$2p_{3/2} - 2p_{1/2}$	2.8	0.8

What is important to note here is that the values of k and $C_Z E_{n\kappa}/R_\alpha(k)$ are very nearly the same for the $2p \rightarrow 1s$ transitions and the 2p splitting. This means that these energy intervals are strongly correlated, that is, if we try to adjust one interval with a small change in the nuclear charge distribution we will inevitably cause nearly the same fractional change in the other intervals. Thus measurement of the 2p splitting and the $2p \rightarrow 1s$ transitions provides a strong internal consistency check on the data, but only one absolute radius measurement. The reason for this is not too obscure. Relativistic effects cause the $2p_{1/2}$ wave function to have a large $s_{1/2}$ component at small radii, so that when the difference of $2p_{3/2} - 2p_{1/2}$ is taken, the $s_{1/2}$ component dominates the shape of the resulting transition charge. The same result is obtained in either of the $2p \rightarrow 1s$ transitions, so that all the transition potentials have similar shapes (that is, values of k). The similar values for the products $C_Z E_{n\kappa}$ can be understood by noting the rough proportionalities

$$E_{n\kappa} \propto \langle V(r_\mu)\rangle_{n\kappa} \propto \langle \rho(r)\rangle_{n\kappa} \propto C_Z^{-1} \ . \quad (6)$$

The relevance of this to Fig. 1 is that the experimental $2p \rightarrow 1s$ transition energies are inconsistent with the measured 2p splitting, and by the above discussion this inconsistency cannot be accommodated by an adjustment of the nuclear charge distribution. Note from the figure that all transitions that wind up in the 1s state are experimentally consistent with each other even though the n = 3 to n = 1 transitions have energies of about 8.5 MeV whereas the n = 2 to n = 1 transitions are about 5.9 MeV. The magnitude of the discrepancy is such that the 2p splitting is off by nearly 200 eV or, alternatively, the 1s binding energy is off by around 3 keV.

One question people always bring up is whether the residual model dependence in this analysis, or the approximations made, invalidate the above conclusion. A frequent suggested remedy is to use electron-scattering charge distributions. Figure 2 shows an analysis of the same data using charge distributions derived from electron-scattering experiments. (The curves look different from those in Fig. 1 because the weighting function $e^{-\alpha r}$ was changed. In Fig. 1, $\alpha = 0.17$, whereas in

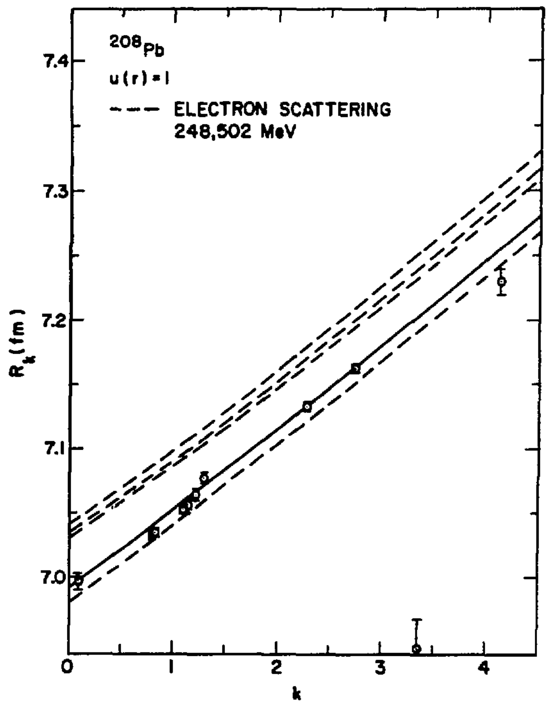

Fig. 2.
Equivalent radii for ^{208}Pb obtained from charge distributions fit to electron-scattering cross sections and from muonic-atom measurements (Ref. 8).

Fig. 2, $\alpha = 0$.) All the dashed lines represent charge distributions that fit the scattering data reasonably well. The two most notable conclusions to be drawn are (1) the radial normalization (that is, the vertical position in the figure) of the charge distribution is not well determined by electron-scattering experiments, but (2) the shape (that is, the curvature and slope) is well determined. Thus if we use electron-scattering charge distributions and normalize them approximately to fit the muonic measurements we get even tighter correlations among the experimental energies. But the electron measurements will not tell us in any useful way just what the radial scale of the charge distribution is.

Figure 3 shows that the same problem occurs in ^{206}Pb, but in this case it appears to be the $\Delta 2p - \Delta 3d$ interval that is inconsistent rather than the $\Delta 2p$ itself. Note the point in the lower right-hand corner of the graph that corresponds to the 3d splitting. This point is very badly inconsistent with the rest of the data, as it is also in ^{208}Pb (Fig. 1).

These difficulties also show up in the isotope shifts (Fig. 4). Here I've plotted changes in equivalent radii between ^{208}Pb and ^{206}Pb for the various transitions. These equivalent radius shifts are related, as in Eqs. (4) and (5), to the directly measured isotope energy shifts (mass corrections included). Even more clearly than before, the points involving the p states are inconsistent with those involving the 1s state. The solid curve represents the difference between the two charge distributions optimally fit to the individual isotope data, whereas the dashed curve represents an (unsuccessful) attempt to fit the apparently rising and then falling isotope shifts themselves.

An interesting sidelight in Fig. 4 is the electronic K x-ray shift, which is not entirely consistent with the muonic data. I expect that the discrepancy is due to inadequate theoretical knowledge of the electron wave functions at the nucleus. If we accept this as the source of the discrepancy, then a comparison of muonic and electronic

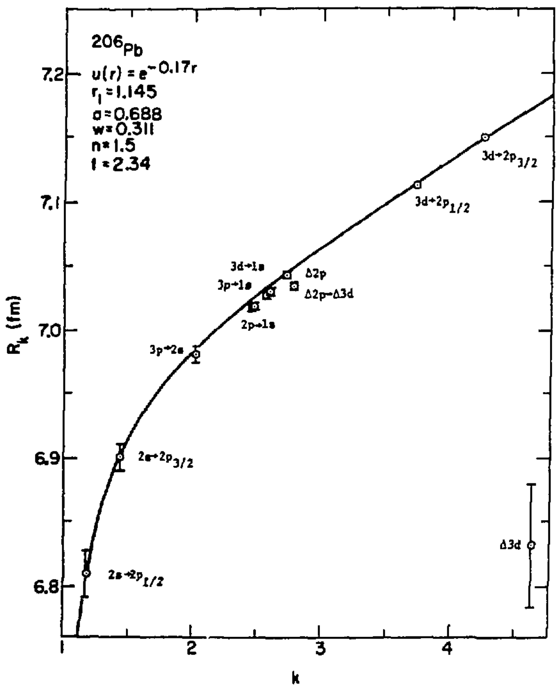

Fig. 3.
Equivalent radii for ^{206}Pb (Ref. 8).

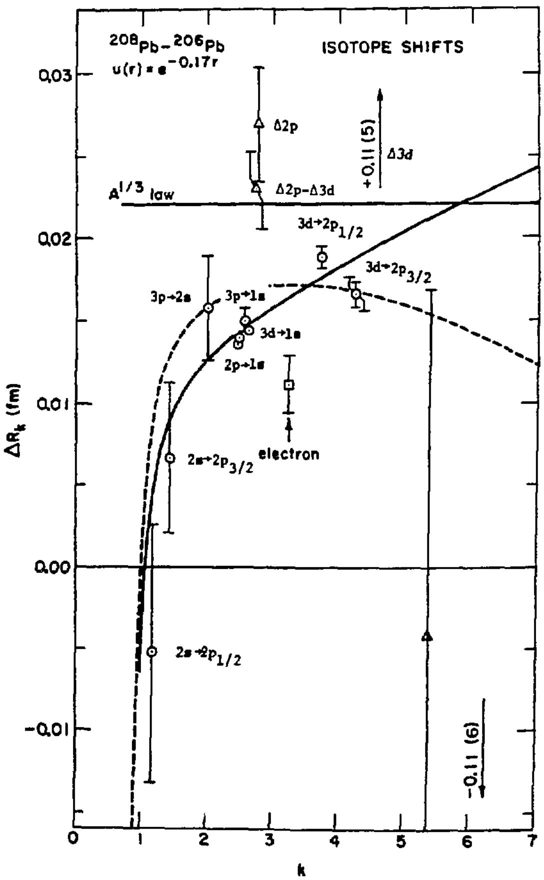

Fig. 4.
Equivalent radius shifts between the isotopes ^{208}Pb and ^{206}Pb (Ref. 8).

isotope shifts provides a test of atomic many-body theory or, alternatively, a normalization of the electronic isotope shifts. There is considerable interest in this subject, which demonstrates one of the many ways in which muonic-atom results can be applied to other fields.

You probably wonder why I'm concentrating on the $\Delta 2p$ and the 2p-1s intervals and not worrying much about the 3d splittings even though the 3d splittings are obviously much worse. The reason is that there is a good explanation for the 3d problems. The octupole state in lead resonates with the 3d states and causes big perturbation shifts, which can be accounted for without difficulty, shifting the 3d states into agreement. But the 2p splittings are still quite wrong.

Let's look for possible explanations for this puzzle. One thing people tend to bring up is that there is something wrong with the experiments. I'm not going to say anything about that. You can take it up with all the people in Refs. 2-7 if you want.

Another suggestion is that we have not been careful enough about using the available electron-scattering data, that a more complete analysis might lead to different conclusions. In fact, such an analysis has been made by a group here at Los Alamos.[7] Using a combined, model-independent analysis of electron-scattering cross sections and muonic transition energies, they made various fits and reached several interesting conclusions.

Their first conclusion is similar to the one shown in Fig. 2. They assume a 1% normalization uncertainty in the cross sections and fit the electron data alone. Then they take the resulting charge distributions and calculate muon binding energies, including all known corrections except for nuclear polarization. The difference between the calculated and measured values is the "experimentally determined" nuclear polarization. For the 1s state, they obtain -1 ± 19 keV for this correction. This is not very helpful. In fact, one needs a factor of 20 improvement in the experiments before useful results can be obtained. Another important point is that dispersion corrections in the electron-scattering data are needed because they have a very similar effect to the nuclear polarization. If we've made a mistake by leaving the dispersion corrections out we can account for that by changing the charge distribution a little bit, which will lead to the conclusion that there is little or no nuclear polarization effect. Unfortunately, the dispersion corrections have not been calculated except for some very crude estimates.

The interesting thing is the correlations these people got. They assumed various values for the nuclear polarization correction for a given state, fit the cross sections combined with the relevant muonic data point, and then looked at the corrections required to make the other experimental intervals consistent. Figure 5 shows some of their results. The solid lines with grey error bands represent the experimental correlations. The additional lines and points result from various theoretical nuclear polarizaton calculations. One sees from the bottom two graphs that there is no theory that successfully correlates the $1s_{1/2}$ and 2p energies.

The upper graphs show that most calculations produce results consistent among the s states but not between the 2s and $2p_{1/2}$ states. Again, the experiments seem to be telling us that the s and p energies are internally inconsistent.

An interesting sidelight arises from the graph in the upper right-hand corner of Fig. 5. The line is obtained by assuming that the isoscalar monopole strength is concentrated in a single resonant state whose energy is a free parameter anywhere between 10 MeV and infinity. Using a phenomenological form factor and keeping the energy-weighted sum rule satisfied, a range of nuclear polarization corrections is obtained. The graph shows that the 1s and 2s energies are consistent only if the monopole energy is somewhere above ~14 MeV. This is not the most compelling evidence one could imagine, but it supports the idea of a higher monopole energy. This has been a matter of heated debate in the last few years.

An obvious possible source of the discrepancy is that all these nuclear polarization calculations are wrong. People have usually tended to blame the 1s state because

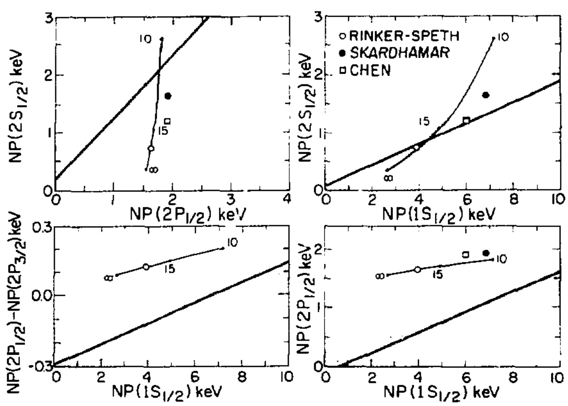

Fig. 5.

Correlations between muonic energies in ^{208}Pb obtained from analysis of electron-scattering cross sections (Ref. 7).

the correction is largest there. But as I pointed out awhile ago, the 2p splitting is as sensitive to such corrections as the 1s state is, just because it is much less sensitive to the nuclear radius. A given fractional error in the 1s binding energy propagates as roughly the same error in the interpreted nuclear radius as does the same fractional error in the 2p splitting. There is no reason to assume that the nuclear polarization calculations are more reliable for the higher lying states than for the 1s state.

There is a long history of these calculations, which were gradually improved with time. Cole[11] used closure approximations for the muon so that there were large numerical errors. Chen[12] and Skardhamar[13] were the first to treat the muon excitations accurately, through Green's function techniques. There is a very nice paper by Hüfner[14] that talks about resonances, which were not considered by Chen and Skardhamar. Then there was some further work[15] using Hartree-Fock models to calculate polarization; there were various problems with them because of the limitations of the Hartree-Fock approximation. There is a paper by Shakin and Weiss[16] that basically took care of the 3d resonances, which we talked about earlier. Then Speth and I[17] got involved in some calculations, which were extensions of all this previous work. We put together the best models we had and we came up with results that were possibly even more inconsistent with the data than the previous calculations. Our calculations included relativistic kinematics so that we could reasonably treat the 2p splitting. The earlier calculations were nonrelativistic, so the difference between the $2p_{1/2}$ and the $2p_{3/2}$ weight functions wasn't in. We put in all known nuclear states and we used accurate excitation models for the high-lying collective resonances. We did a very careful calculation and we still didn't get the "experimental" answer.

One possible explanation for this is that there exists an unknown 1^- state that's at something like 5.9 MeV in lead, which nobody has ever seen, that is disturbing the 2p splitting and not having any big effect on anything else. (Angular momentum and energetics make this the only possibility other than a 2^+ state at ~180 keV, which can safely be ruled out.) It doesn't have to be a very strong 1^- state; it just has to be at the right energy. Now it probably seems like a great coincidence that something like this could occur — it seems like a coincidence to me.

We can say a few things about the properties that this state would have to have. It has to be separated from the $2p \rightarrow 1s$ transitions by at least one line width (~5 keV), if it decays to the ground state, because it has to be resolved from the $2p \rightarrow 1s$ transitions or else it's not going to move the centroid of the line. This implies that the state have a radiative width of 1 eV or more. There have been a couple of very careful experiments using photon scattering[18] and neutron scattering[19] to excite such levels in lead and look for them in the cascade. No such level has been found, although many far weaker ones have. It is quite clear that there is no level decaying to the ground state at the right energy in these experiments.

But suppose this level is there; what could have happened to it? One obvious possibility is that maybe these experiments don't excite the level at all through some selection process. Another possibility is that the experiments excite the level and, rather than going directly to the ground state, it has a large branching ratio to an excited 0^+ or 2^+ state, which then cascades to the ground state. Of course, if this is the situation and we're looking for a 5.9-MeV gamma ray, it will be much weaker than we expect. I think a very crucial point in understanding this problem is to have good inelastic electron-scattering data because we really need to see if this state exists. The important point is to know whether the state can be excited (not whether it decays directly to the ground state), and as far as I know, current experiments are marginal at seeing a state like this.

There could be more exotic explanations. There can be neglected effects. For example, none of the nuclear polarization calculations has ever accurately included magnetic polarization because it's much more difficult to do. It takes more time to work out the algebra and there is not a whole lot known about the magnetic excitations. But we can make an estimate as to whether this is important at all. Because we work in the Coulomb gauge, the relevant parameter is the velocity of the protons in the nucleus, because if the protons were at rest they would generate no magnetic field* and there would be no magnetic polarization, no matter how fast the muon is moving. The velocity of the protons in the nucleus is very small. Twice the energy over the mass is ~0.05, which is $\simeq v_p^2/c^2$, the same as the magnetic perturbation energy shift. Thus we would expect magnetic polarization effects to be at most a few percent of the electrostatic polarization effects. In fact, various more careful estimates have been made, and the only estimate that gives a big answer is the one that Cole[11] made in his original paper. However, he made a number of very serious approximations in order to get an upper bound, and I don't think his result is realistic. The other estimates are generally even smaller than mine.

*Nucleon-spin effects neglected.

Another temptation is to think that if there are magnetic effects around they might affect the various 2p states differently. But in fact, we calculate the polarization effect in second order perturbation theory. The energy shift depends only on the energy differences, not the sign of the matrix element.

There may be QED corrections that are not included. As you all know there has been a tremendous amount of work in the last few years calculating these corrections, mainly for the high-lying states. Things are in very good agreement. With one exception people believe these are adequately taken care of. The only correction that seems to be in doubt is the high-order [relativistic, $(Z\alpha)^{n\geqslant 2}$] vertex. This is a real bag of worms because we really need to use the bound-state interaction picture to do the calculation. If we use scattering states, the typical kind of treatment that people use for hydrogen, we don't get the high orders in $Z\alpha$; we get an expansion in powers of α and $Z\alpha$, and $Z\alpha$ is big for lead. So we really need to do it to all orders, using a bound-state picture. The only calculation that I'm aware of was done by Cheng et al.,[20] which was restricted to the 1s state for numerical reasons. One of the interesting results of that calculation is that much simpler estimates that have been made are good to within 5% or so where comparison may be made. The classic paper on this was Barrett et al.,[21] where they estimated these higher order corrections. If we apply their estimate to the 2p states we get numbers about 100 eV, and in the 2p splitting it's only a few electron volts. (Let me remind you that we would like to adjust the 2p splitting by ~200 eV.) For the 1s state this estimate is around 150 eV, whereas ~3 keV is required. So it's extremely difficult to believe that neglect of this high-order correction can explain such big differences.

As far as high-order vacuum polarization is concerned I think the high-lying transition measurements have said the final word, at least for today. The limits set in those experiments are far too precise to allow such big changes in the 2p splitting or 1s binding energy.

Well, you know that here at Los Alamos when things go wrong it's the altitude. These experiments have been carried out in Switzerland and Virginia and places like that, so we can't blame the altitude. The only other standard excuse is anomalous interactions. So let's see what would be the effect of an anomalous interaction of some kind. There are many theories and there is tremendous interest, as everybody knows, in this subject. In the static limit the calculation is simple to do. We assume a Yukawa potential interacting between the muon and the nucleons

$$V_\phi(r) = g_\phi A \frac{e^{-m_\phi r}}{r} \quad . \tag{7}$$

The parameters are the muon-nucleon coupling constant g_ϕ and the mass m_ϕ of the exchanged particle. If we assume the value $g_\phi = 1.2 \times 10^{-7}$, as in Jackiw and Weinberg,[22] we get the following as a function of m_ϕ (Table I).

We can see that the effect is much too small. Furthermore, the ratio of the effect on the 2p splitting to the effect on the 2p → 1s transition does not vary much once the ϕ particle gets fairly massive. This is because the range of the interaction is very short, and the effect begins to look more and more like a simple small perturbation in the nuclear charge distribution. This is the central problem we have to face, namely, that we can have all kinds of effects, but if they look a lot like a change in the charge distribution, we'll never see them. And you see, anyway, that if we're going to explain a discrepancy like this we have to have coupling constants that are enormous compared to the things that are being talked about today, coupling constants that are ruled out completely by other experiments.

We could suppose that there is a spin-dependent interaction. This is really far fetched, of course, because ^{208}Pb is a doubly closed shell and it's hard to imagine how we could get a spin-dependent interaction between a muon and a ^{208}Pb nucleus. But, just to give you a feel for the order of magnitude, note that the Schwinger magnetic moment term contributes about 440 eV to the 2p splitting. We would thus need something half as big as the $\alpha/2\pi$ correction to explain the discrepancy. In principle, this could do it because it wouldn't have such a big

TABLE I

EFFECT OF AN ANOMALOUS INTERACTION WITH COUPLING CONSTANT $g_\phi = 1.2 \times 10^{-7}$ AS A FUNCTION OF THE MASS OF THE EXCHANGED PARTICLE

m_ϕ (MeV)	$\delta E(2p_{1/2} \rightarrow 1s_{1/2})$ (keV)	$\delta E(\Delta 2p)$ (keV)
10	0.243	0.018
20	0.202	0.014
40	0.132	0.0086
80	0.0614	0.0037
1000	0.0006	3.3×10^{-5}

effect on the 1s state. But again, to accommodate something like this we have to use a lot of imagination because muon g-2 tells about the coupling constant. Of course, if we make the nuclear coupling constant very large we can make the muon coupling constant very small so that we don't affect muon g-2, but it all becomes sillier and sillier as we think about it.

So this is what I mean when I say that all possible explanations have been ruled out. I've discussed this with a lot of people over the last 10 years or so. We've hoped that the problem would go away as the experiments got better, and as the calculations got better, but in fact the problem is still with us. If it exists in such a simple system as ^{208}Pb then it has to exist in more complicated systems. The whole analysis of muonic atoms, I think, is questionable at this level. At today's level of precision there is structure in the data that we don't know how to explain.

Acknowledgments

I would like to thank M. V. Hoehn, W. Johnson, E. B. Shera, R. M. Steffen, Y. Tanaka, and H. D. Wohlfahrt for interesting and educational discussions of the material used in this talk and for communicating results before publication.

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RECENT RESULTS IN THE PION-NUCLEUS INTERACTION

David J. Ernst
Texas A&M University

Introduction

It is not necessary to sell the importance of understanding the propagation of a pion in a nucleus to nuclear physicists, especially those associated with LAMPF. This interaction and its understanding lie at the base of much of the work done at LAMPF. The reliability of any nuclear structure we may learn with the pion is dependent on how quantitatively we are able to treat the pion propagating through the nucleus. Because pion exchange is the longest range piece (although not the dominant piece) of the nucleon-nucleon interaction, a complete understanding of nucleon-induced reactions and nuclear structure will eventually have to include the pionic degrees of freedom. The problem is surely a vital one for nuclear physics and I should like to take this opportunity to review, from a very personal point of view, progress that has been made in recent years and to report some of my latest thoughts on the subject.

The field of pion physics is much too broad to be covered in a single talk, so I'll have to narrow the topic substantially. The simplest of all the pion-induced reactions is elastic scattering. Inelastic scattering or pion absorption and emission, for example, provide much richer and varied phenomena. However, understanding the simpler process is the necessary precursor to understanding the more complicated. Even though the talk and the work to be discussed are limited to elastic scattering, the techniques, approach, and understanding are equally applicable to other reactions.

Very Quick Review

In order to set the background for the more recent results, let me review in a few minutes (as much as is possible) the first-order optical potential. Let me use as examples the work of Landau and Thomas,[1] Liu and Shakin,[2] and the isobar-hole model.[3] For all cases, the first-order optical potential is given by

$$(\vec{k}' \,|\, U(\omega) \,|\, \vec{k}) = \int d\vec{p}'\, d\vec{p}\; (\vec{k}',\vec{p}'|\, t(\omega)|\, \vec{k},\vec{p})\,(\vec{p}'|\,\rho\,|\,\vec{p}) \quad . \tag{1}$$

Each of these groups would, however, implement this formula somewhat differently.

Each treats the problem of relativistic kinematics in a unique way. Landau and Thomas use what I like to call "relativistic potential theory kinematics." Here, all particles are assumed to be on their mass shell and then they are boosted from frame to frame. Liu and Shakin use the kinematics appropriate to the off-mass shell particles that follow from a reduction of a Bethe-Saipeter equation. The matrix elements required to construct t and ρ are, however, taken from on-mass shell models. The isobar model uses kinematics that are derived by replacing masses with energies in nonrelativistic formulae. In addition, the recoiling pion-nucleon pair is given the delta mass. In the other approaches, this mass is essentially equal to the pion plus nucleon energies. Near resonance, where one may apply the isobar model, these two quantities are, by definition, nearly equal. We have recently investigated[4] these various treatments of kinematics and found that, although the differences are not negligible, they are also not large. The best way to describe the situation is to note that the differences between models is much smaller than the difference between any model in lowest order and the data.

Secondly, the groups use different models for the pion-nucleon amplitude. Landau and Thomas, and Liu and Shakin use a separable potential model while the isobar approach requires a delta model. The delta model used differs from the separable potential only in that the delta amplitude has a Castillejo-Dalitz-Dyson (CDD) pole at quite high energies. In the region where they are used, they are virtually identical; both produce the same on-shell data and have an off-shell cutoff of roughly 300 MeV/c.

The integral over the momenta of the nucleons in Eq. (1) is also treated differently. There is a momentum conserving delta function in the t-matrix in Eq. (1). This reduces the two three-dimensional integrals to a single three-dimensional integral. Liu and Shakin perform the integration completely. The other two approaches treat exactly the integral over the dependence of ω on the nucleon momenta (called "delta recoil" in the isobar model). The intrinsic nonlocality of the amplitude is, however, approximated. Again, we[5] have recently checked the validity of this approximation and found it to be quite good.

To lowest order, no density squared terms in the potential models and no delta-nucleus interaction in the isobar model, the models are for practical purposes quite similar. The differences in kinematics, two-body model, and treatment of the Fermi averaging are not large. In Fig. 1 we show a typical result which would be representative of any of these approaches. We have concentrated on the forward peak and first diffraction minimum. Because the scattering is strongly diffractive, this region is predominantly determined by the size of the target. These approaches are clearly not getting the size correct in first order. One can shift the value of ω downward to make the result at 160 MeV more diffractive, but then one finds[6] that the size of the target as seen by the pion is too large.

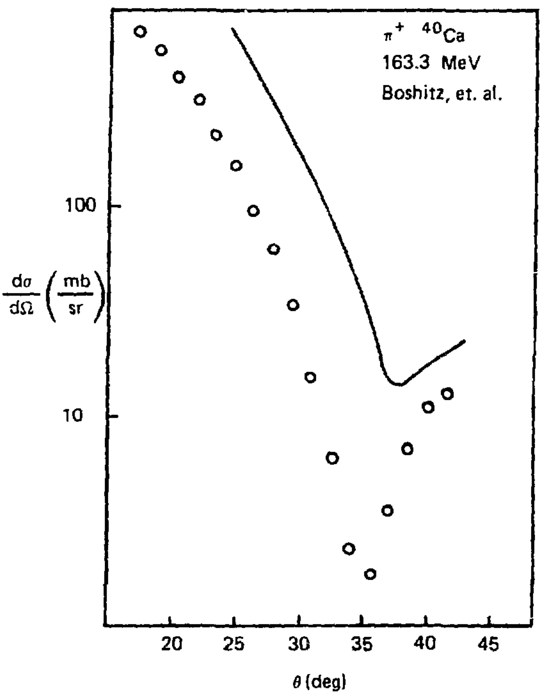

Fig. 1.
Elastic scattering of π^+ from ^{40}Ca. The curve is the result of an optical potential that uses a separable potential π-nucleon t-matrix, the three-body energy denominator, and the Fermi averaging integral is performed.

Even though this first order result is not satisfactory, much can be learned from it. The use of some form of relativistic kinematics is essential. In particular, the transformation from lab to pion-nucleon center-of-mass system and the proper treatment of the recoil of the pion-nucleon pair must be included. The Fermi averaging integral must be performed. This point is intimately related to the first because it is the rapid energy dependence of t and the shift in ω as a result of the recoil that inhibits an approximate treatment of the Fermi averaging. Finally, we see a need for higher corrections.

What is first order and what is second order is defined by how the theorist arranges his perturbation theory. A goal of the work that I'll discuss here is to move as much of the physics into the first order as is possible. This would then produce a more rapidly convergent perturbation theory. This has great practical significance because it is difficult but possible to calculate the first order cleanly and without approximation. The second order is enormously more difficult and generally impossible, or at least extremely costly on the computer, to calculate without approximation. Second order is thus generally treated phenomenologically. Microscopic theory is thus practically limited to first order, and it is imperative that the first order be as all encompassing as possible. The second and most significant goal of this work has been to incorporate the basic field theoretic character of the pion into a multiple scattering theory. If I may quote Roy Thaler, "A pion is not simply a lightweight nucleon and we must stop treating it as if it were."

Field Theoretic Approach

All of the approaches reviewed here overlook some of the basic physics of a pion. The pion is a boson; it can interact by being absorbed and emitted, much like a photon. This implies an additional symmetry in the problem, namely crossing symmetry. The symmetry is simply the result of the fact that for any process in which a pion enters an interaction at t and leaves at t′, there is an equally valid process where the pion first leaves at t and then the incident pion arrives at t′. The direct nucleon-pole diagram and its crossed counterpart are shown in Fig. 2. In addition, the propagation of a pion backward in time corresponds to the forward propagation of the antiparticle. How does one incorporate these aspects of the pion into the pion-nucleus problem?

In collaboration with Mikkel Johnson,[7] an approach has been developed that contains many desirable features

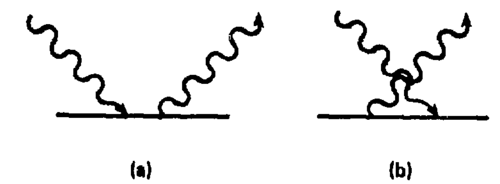

Fig. 2.
The direct (a) and crossed (b) pion-nucleon pole terms.

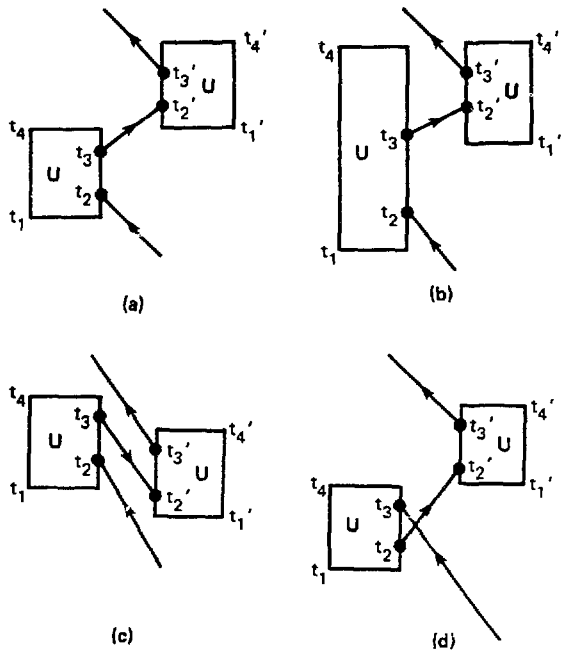

Fig. 3.
A few of the time orderings of the optical potential summed when the potential is inserted in the Klein-Gordon equation.

not found in other approaches to this problem. The first step in this approach is to define the pion-nucleus optical potential as the proper self energy of the pion-nucleus Green's function. The proper self energy is defined as those terms in a diagrammatic expansion that cannot be cut into two separate pieces by cutting a single (forward- or backward-going) pion line. Notice that this is very different from the alternate approach of defining a self energy by requiring that there always be more than one pion present at any given time in the self energy. This latter approach leads to an ordering of the perturbation theory according to the number of pions present at any given time, termed the fixed pion number expansion (FPN expansion).

The definition of the optical potential that we have chosen immediately leads to several important implications.

1. The optical potential appears linearly in a Klein-Gordon equation. We see no terms in a multiple scattering approach that could lead to the optical potential entering quadratically.
2. There are no restrictions on the time orderings of the interactions between iterations of the optical potential. This is clarified in Fig. 3. The iteration of the optical potential in the Klein-Gordon equation will include not only the orderings depicted in Fig. 3(a) but also those pictured in Fig. 3(b-d).
3. Although there is more than one pion present at several times in Fig. 3, there is no[8] implied pion production other than that which one might explicitly build into the optical potential itself. This comes about by an intricate, but automatic, cancellation of diagrams containing different numbers of pions. In any fixed pion number expansion, this cancellation would not occur. To any order there would be a spurious model of multiple pion production which would be canceled by pieces of the higher order terms.
4. Crossing symmetry for the pion-nucleus problem can be maintained without resorting to nonlinear equations.[9] For every contribution to the optical potential that has a pion entering at time t and a pion leaving at time t′ with t′ > t, we include the crossed contribution with the pion arriving at t′ and the pion leaving at the earlier time t. This guarantees that the optical potential satisfies crossing symmetry,

$$[\vec{k}'|\,U(\omega)|\,\vec{k}] = [\vec{k}|\,U(-\omega)|\,\vec{k}'] \quad . \tag{2}$$

When inserted into the Klein-Gordon equation,

$$[\vec{k}'|\,T(\omega)|\,\vec{k}] = [\vec{k}'|\,U(\omega)|\,\vec{k}] + \int [\vec{k}'|\,U(\omega)|\,\vec{k}''] \frac{1}{\omega^2 + i\eta - \omega^2(k'')} [\vec{k}''|\,T(\omega)|\,\vec{k}] \quad , \tag{3}$$

the t-matrix will automatically be crossing symmetric, due to the quadratic dependence on ω of the propagator.

To proceed further, we must decide on a perturbation theory for the optical potential. We choose to utilize a spectator expansion;[10,11] the ordering is done according to the number of active nucleons, with no reference to the number of pions. The lowest order contains a single active nucleon and leads to the impulse approximation, Eq. (1). The second order, as in Ref. 10, contains two active nucleons and leads to a three-body problem for the pion and the two nucleons. Although the definition of the optical potential is not at all related to that of Ref. 11, the technique used there to derive a spectator expansion can, to a great degree, be used here. The details can be found in Ref. 7. We shall from here on concentrate on the first order.

The first-order optical potential will satisfy Eq. (1) with the t-matrix given by a crossing symmetric field theory. We choose to use the extended Chew-Low model of Ref. 12. This model fits the pion-nucleon data very well, as can be seen in Fig. 4. The off-shell cutoff which occurs in Eq. (1) is then the short range (in coordinate space) cutoff of the Chew-Low model. This cutoff is characterized by a range in momentum space of about 900 MeV. This should help with the difficulty with the radius of the first-order optical potential. The effective radius of the optical potential can be shown to behave qualitatively like[13]

$$R^2_{opt} = R^2_{nucleus} + R^2_{\pi N} \qquad (4)$$

where R_{opt} is the radius of the optical potential, $R_{nucleus}$ is a radius for the nucleus (roughly the radius where the density drops to 15% maximum), and R_N is the radius of the pion-nucleon interaction. The relationship of a separable potential form factor to the Chew-Low form factor[14] when both theories produce the same phase shifts is

$$V_{sep\ pot}(k) = \frac{V_{CL}(k)}{\omega_k} \quad , \qquad (5)$$

where $\omega_k = \sqrt{k^2 + m^2}$. This additional cutoff of the potential model form factor produces a longer range interaction in coordinate space for the potential model. In Fig. 5 we compare the two models in coordinate space.

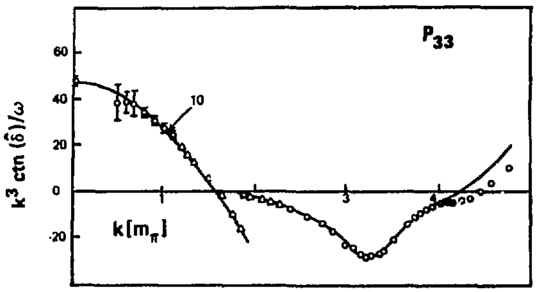

Fig. 4.
The quantity k^3 ctn δ/ω vs pion momentum. The curve is the result of an extended Chew-Low model. Notice that both the data and the curve have been multiplied by 10 at low energies.

Rather than compare the two form factors in Eq. (5), we must recall that the Chew-Low model is used in a Klein-Gordon equation while the separable potential is used in a Schrodinger equation. The propagators are related by

$$\frac{1}{\omega^2 - \omega_k^2} = \frac{1}{2\omega_k(\omega - \omega_k)} - \frac{1}{2\omega_k(\omega + \omega_k)} \quad . \qquad (6)$$

Near resonance, one may neglect[13] the backward-going propagator. The Schrodinger equation equivalent to the Klein-Gordon equation thus contains an extra factor of $(2\omega_k)^{-1}$. This factor is generally subsumed into the definition of $v^2_{sep\ pot}(k)$. The effective separable potential form factor which does not include this extra ω_k^{-1} is $\bar{v}_{sep\ pot}(k)$,

$$\bar{v}_{sep\ pot}(k) = \frac{v_{CL}(k)}{\sqrt{\omega_k}} \quad . \qquad (7)$$

It is the Fourier transforms of $v_{CL}(k)$ and $\bar{v}_{sep\ pot}(k)$ that are plotted in Fig. 5. We see that $\bar{v}_{sep\ pot}(r)$ has a long range tail absent in $v_{CL}(r)$. Because the form factors appear squared in any integration, this tail gives an artificially increased size to the optical potential.

This increased size cannot, however, account for the large differences between the results of the potential

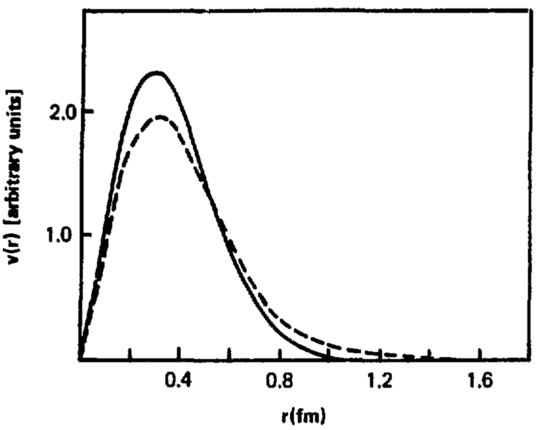

Fig. 5.
The separable potential form factor $\bar{v}_{sep\ pot}(r)$ [dashed curve] and the Chew-Low form factor $v_{CL}(r)$ in coordinate space.

model and the data in Fig. 1. We can infer from the isobar-hole model wherein the difficulty might lie. The three-body energy denominator treats the bound nucleon as bound by a potential. The excited nucleon is treated as a free particle. It has been shown[15] that the potential between the excited nucleon and the residual nucleus is important. Because the mass of the nucleon is much greater than the mass of the pion, this is equivalent to a potential acting on the center-of-mass of the pion-nucleon pair. This would then be the delta self energy in the isobar-hole model which is well known to be quite important. The question that then arises is can we include this potential, at least on the average, in the first-order potential?

The propagator that occurs in summing the individual interactions to arrive at the t-matrix in Eq. (1) is

$$g(\omega) = (\omega - \omega_k - t_p - U_p + t_h + U_h)^{-1} \qquad (8)$$

where t_p (t_h) is the nucleon particle (hole) kinetic energy, and U_p (U_h) is the particle (hole) potential. The three-body energy denominator neglects U_p. A better approximation[16] is to cancel U_p against U_h. This choice is called the "impulse approximation energy denominator."

There is finally one remaining consideration; how does the Chew-Low amplitude go off shell in the medium? There is a difficulty if one assumes that the amplitude is separable as was found by Mhyrer and Thomas.[17] The absorption of a pion proceeds through the direct channel pole, Fig. 2(a). If the amplitude is separable, then the crossed channel pole, Fig. 2(b) will also lead to the absorption of a pion. This time, however, the absorption leads to a spin 3/2 fictitious nucleon. Mhyrer and Thomas then use this spurious absorption to argue against the field theoretic model.

The resolution to this problem follows from a more careful examination of these pole terms. Following Ref. 7, but including propagators for finite mass nucleons, we find an energy denominator for the direct pole of the form,[17]

$$\frac{1}{\omega - t_p - U_p + t_n + U_h} \quad . \qquad (9)$$

When the particle-hole energy is sufficiently large, the denominator is singular and energy conservation permits the pion to be absorbed on a single nucleon.

For the crossed pole contribution, one finds[17]

$$\frac{1}{-\omega - t_p - U_p + t_h + U_h} \quad . \qquad (10)$$

The denominator is the sum of negative terms. The particle energy, $t_p + U_p$, is positive and enters with a minus sign; the hole energy, $t_h + U_h$, is negative — it is minus the binding energy, $-E_b$. The crossed term is thus never singular and yields no problems. The Chew-Low amplitude is thus separable, but it is rank two separable. The direct pieces go off shell differently from the crossed pieces.

The results for a first-order optical potential utilizing this approach are given in Figs. 6 and 7. Although not perfect, this is a dramatic improvement.

Conclusion

Let us review all of the pieces that have gone into the results of Figs. 6 and 7. The treatment of kinematics is the "relativistic potential theory" approach from Ref. 18. The recoil of the nucleus as a whole is included as derived in Ref. 4. The incorporation of a field theory in a

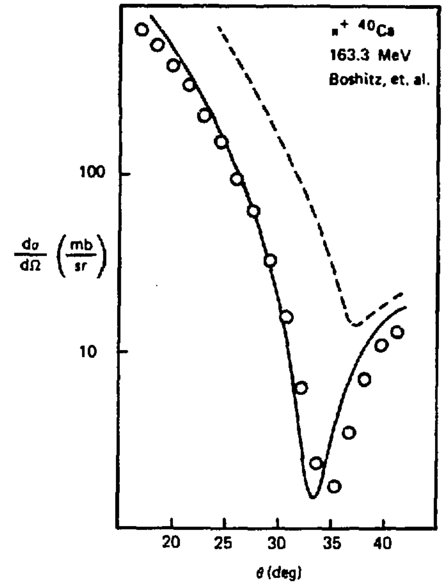

Fig. 6.
Elastic scattering of π^+ from ^{40}Ca. The solid curve is the result of the field theoretic approach presented here. The dashed curve is the same as was given in Fig. 1.

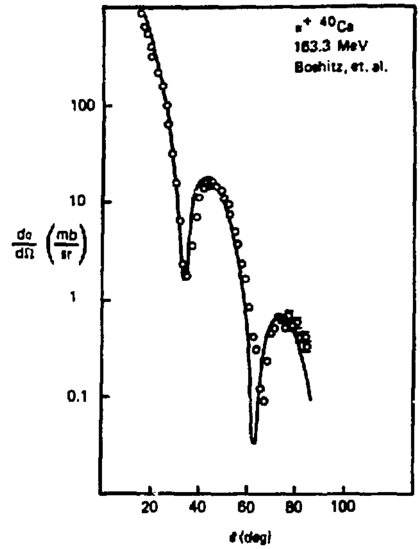

Fig. 7.
The same as Fig. 6 except the entire angular range is given.

multiple scattering approach is from Ref. 7. This introduces an optical potential into the Klein-Gordon equation in such a way that (i) crossing symmetry is maintained, (ii) unitarity is satisfied without the problem[8] of spurious pion production found in approaches that fix the number of pions, and (iii) the short range form factors of theoretical models control the off-shell behavior of the model. The Fermi integral in Eq. (1) is performed using the technique of Ref. 5. The field theoretic model is the extended Chew-Low model of Ref. 12. The energy at which the two-body amplitude is evaluated is the impulse energy denominator as argued in Ref. 17. The crossed pieces of the two-body amplitude are taken off-shell differently than the direct pieces according to Ref. 18.

There still remains much to be done, and several projects are presently under way. The large sensitivity of the results to the choice of the energy denominator has motivated a study to see if one can extract a form for this energy dependence from data or from models of the response function. A sufficiently accurate local density approximation, valid in the surface, would allow one to include all of the physics in a simple coordinate space approach. Such an approximation is being investigated. Finally, of course, a second-order term must be added, particularly at low energies where the present approach produces cross sections that are significantly too large. The extension to other interactions including kaon interactions is beginning.

Much progress has been made in understanding the propagation of a pion in a nucleus. The problem is one where a careful treatment of the details, i.e., the relativistic kinematics and the Fermi integration, is necessary. It is also a problem where new conceptual approaches have proved fruitful, and a problem that appears to be rich in opportunities for further advances.

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NEUTRON-ANTINEUTRON OSCILLATIONS

Milla Baldo-Ceolin

Istituto di Fisica dell'Università, Padova, Italy
Istituto Nazionale di Fisica Nucleare, Sezione di Padova, Padova, Italy

SUMMARY

In this lecture I will present and discuss the experiment being performed at the nuclear reactor in Grenoble to search for n-$\bar{n}$ oscillations.(1)

People and institutions involved are: G. Fidecaro, M. Fidecaro, L. Lanceri, A. Marchioro CERN; W. Mampe ILL; M. Baldo Ceolin, F. Mattioli, G. Puglierin Padova; C.J. Batty, K. Green, P. Sharman RHEL; J.M. Pendlebury, K. Smith Sussex.

After a general discussion on the phenomenology of neutron-antineutron oscillation I will first present the result of the first step of the experiment, which gave the lower limit

$$\tau_{n\bar{n}} \geqslant 10^5 \text{ sec} \qquad \text{with } 80\% \text{ C.L.}$$

Then I will illustrate the present set-up, which aims at reaching

$$\tau_{n\bar{n}} \geqslant 10^6 \text{ sec}$$

and finally I will briefly discuss the project to reach a sensitivity of the order of $10^8 \div 10^9$ sec.

INTRODUCTION

The stability of the nucleon and the correlated law of the baryon number conservation was considered till recently quite well established. The nucleon mean life has been determined to be(2)

$$\tau \geq 10^{30} \text{ years}$$

and the apparent stability of the nucleons has been attributed to the conservation of the baryon number B. However, it was soon clear that the validity of the baryon non conservation law must be considered as an experimental problem since the baryon number does not appear to be associated with a gauge symmetry as in the case for the electric charge[3].

Moreover, a small non conservation of the baryonic number was suggested[4] in order to explain the matter-antimatter asymmetry in the known universe.

Grand unification theories (GUT) have promoted renewed speculations on the conservation of fundamental quantum numbers as well as on their nature. In particular, since in these theories quarks and leptons belong to the same multiplet, the conversion of quarks into leptons becomes natural, and baryon number non-conserving processes become allowed.[5]

One of the most exciting implications of GUT is that they may explain the excess of matter over antimatter. If the baryon numbers were exactly conserved, the present baryon excess would have to be postulated as an asymmetric initial condition of the big-bang. On the contrary, if GUT are correct the baryon excess could have been generated dynamically through baryon number violating, CP non conserving, interactions in the first instants after the big-bang. The synthesis of unified theories with the theory of a hot universe gives thus a qualitative and quantitative explanation of the present baryon asymmetry[6].

Unfortunately, the new interactions predicted by most GUT are so weak that they are unobservable in the Laboratory. Possible exceptions are nucleon decay and neutrino oscillations.

The predicted rate for nucleon decay is

$$\tau_n \sim 10^{30} \div 10^{33} \text{ years}$$

Several experiments are in progress or are being planned in order to measure the nucleon lifetime.

The ideal experiment would consist, of course, in measuring the total rate and the branching ratios of the baryon decay processes; however, the limits of accuracy which must be reached make this experiment extremely difficult, if not impossible.

The point that I would like to stress here is that, when the baryon number non conservation is considered, the selection rules involved in the B-violating processes are very important for the design of any experiment.

Two main classes of selection rules can be considered, from a purely phenomenological point of view, in accordance with the fact that baryons are fermions:

1) the decay involves leptons, so that a lepton number non-conservation is associated with the baryon number non-conservation as is in the cases

a) $\Delta B = +\Delta L$; b) $\Delta B = -\Delta L$

2) nucleons annihilate in pairs, so that

$|\Delta B| = 2$

As each of the possible selection rules governing the B-violating processes has different implications for physics, and since a selection rule can only be an experimental result, it appears of great importance to search for the various possible B-violating processes.

Some predictions of the baryon and lepton non-conserving processes have already been presented[7]. Any of such processes must be extremely slow and hence have to be mediated by the exchange of very heavy particles.

There may exist superheavy and medium superheavy exotic particles, whose quantum numbers allow them to violate baryon and lepton number conservation.

Table 1 summarizes the implications for the different selection rules.

	$\Delta B=\Delta L$ $p\to e^+\pi^0$	$\Delta B=-\Delta L$ $p\to e^- +\pi\pi$	$\Delta B=2$	ν oscill.
standard SU(5)	yes	-	-	-
beyond standard SU(5)	yes	yes	yes	yes
SU(16)	yes	yes	yes	yes
$(SU_2)_L x (SU_2)_R x U(1)$	-	-	yes	yes
mass of the intermediate particle (GeV)	$\sim 10^{15}$	$\sim 10^{11}$	$\sim 10^5$	$\sim 10^5$

Table 1

The $\Delta B=2$ selection rule gives rise typically to baryon-number non conserving decay processes of the type[(8)]

$$(np) \to \text{pions}.$$

This interaction mediated by mesons with the appropriate couplings could be competitive with the conventional decay modes of nucleons, provided that the mass of the mediating bosons is $M^* \sim 10^5 \div 10^6$ GeV.

1. NEUTRON-ANTINEUTRON OSCILLATION PHENOMENOLOGY

The Grenoble experiment was designed to test the $\Delta B=2$ selection rule. What makes this selection rule attractive from an experimental point of view consists in the fact that according to the $\Delta B=2$ hypothesis there must be a $\Delta B=2$ neutron-antineutron mixing characterized by a transition energy

$$\Delta m = \langle \bar{n}|H|n\rangle \sim \sqrt{\Gamma(\Delta B=2)M} = 10^{-28} \div 10^{-30} \ M$$

with $\Gamma(\Delta B=2) \simeq \frac{1}{\tau} \simeq \frac{1}{10^{30}\div 10^{33}y}$ and M = nucleon mass

An initially pure neutron beam becomes a neutron-antineutron mixture after a finite time: the characteristic transition time for free neutrons being

$$\tau_{n\bar{n}} = \frac{1}{\Delta m} \sim (10^6 \div 10^9) \text{ sec}$$

Many estimates have been done for the expected value $\tau_{n\bar{n}}$(9). All the predicted values being in the range $10^5 \div 10^9$sec for $\tau_{\Delta B=2}$ decay in $\simeq 10^{30}\div 10^{33}$ years.

However, as we shall see, such an experiment is difficult because neutrons are never free in nature, and therefore their interaction with external electromagnetic or nuclear fields removes the deneracy between neutron and antineutron states; this results in an energy splitting ΔE, which suppresses the $n-\bar{n}$ mixing.(10)

The most general Hamiltonian describing a $\Delta B=2$, CP conserving neutron--antineutron interaction is:

$$H' = \begin{pmatrix} E_0+\Delta E & \Delta m \\ \Delta m & E_0-\Delta E \end{pmatrix}$$

where E_0 is the free neutron energy. If a $n \rightleftarrows \bar{n}$ mixing exists, neutrons and antineutrons are no longer eigenstates: the new eigenstates are:

$$n_1 = n\cos\theta + \bar{n}\,\mathrm{sen}\,\theta \quad ; \quad n_2 = -n\,\mathrm{sen}\,\theta + \bar{n}\cos\theta$$

with $\mathrm{tg}\,\theta = \frac{\Delta m}{\Delta E+\sqrt{\Delta E^2+\Delta m^2}}$

and the intensity of the antineutron component in a neutron beam after a propagation time t is given by

$$I(\bar{n},t) = I(n,0)\ \frac{\Delta m^2}{\Delta m^2+\Delta E^2}\ \text{sen}^2\left[\left(\Delta m^2+\Delta E^2\right)^{\frac{1}{2}}\ t\right]$$

It appears that two situations are particularly significant:

a) $\Delta E=0$, i.e. free neutrons: the probability for a neutron to be found in an antineutron state is maximum, and $P(\bar{n},t) = \left(\frac{t}{T}\right)^2$; the probability increases as t^2 for $(t\tilde{<}\tau)$;

b) $\Delta E\neq 0$ and $\Delta E>>\Delta m$: $P(\bar{n},t)$ increases as t^2 only for values of t such that $\Delta E\cdot t<1$ (quasi free neutron condition).

It is worth noting that if $\Delta E>>\Delta m$, as for neutrons in nuclei, the intensity of the antineutron component $I(\bar{n},t)$ goes to zero as $\left(\frac{\Delta m}{\Delta E}\right)^2\rightarrow 0$, so that $n\rightleftarrows\bar{n}$ are practically suppressed and neutrons appear stable.

2. NEUTRON OSCILLATIONS FROM ON EXPERIMENTAL POINT OF VIEW

Designing an experiment to search for neutron-antineutron transitions a very important condition is given by the relation

$$\Delta E\cdot t << 1\ ,$$

which defines the "quasi free" condition. It allows the optimization of the experimental conditions, defining, when the oscillation time t has been established, the level at which the external perturbation have to be reduced so that the oscillation can develop up to the corresponding maximum value.

Then if neutron-antineutron oscillations do exist they will show up in a beam which was initially a pure neutron beam. Hitting a target after a time of "quasi free" propagation, the antineutron component will annihilate releasing an energy of $\sim$2 GeV. This will be the typical signature in the experiment. The ratio of antineutron interactions to the total number of neutrons will provide a measurement of the neutron--antineutron transition amplitude.

The sensitivity of such an experiment is, by definition, the maximum value of the oscillation time $\tau_{n\bar{n}}$ the measurement can reach or equivalently the minimum detectable mass difference Δm:

$$(1) \qquad \tau_{osc} = \frac{1}{\Delta m} = \sqrt{\frac{N}{\bar{N}}}\, t \propto \sqrt{\frac{N}{\bar{N}} \frac{1}{E}}\, L$$

where L is the neutron propagation length and E the neutrons kinetic energy.

For high sensitivities one, therefore, needs a very intense source of neutrons, since the $\bar{n}$ component is expected to be very small, long neutron propagation lenghts and low energy neutrons.

In practice, however, the sensitivity of the experiment will not be limited only by the accessible available experimental values of the parameters of Eq. (1), but by the amount of background events which can simulate antineutron annihilation events in the target.

When background is present the maximum measurable oscillation time becomes

$$(2) \qquad \tau \leqslant \sqrt{\frac{N}{2\sqrt{2N_B/\varepsilon^2}}}\, t$$

$$N > \frac{\bar{N}_C - N_B}{\varepsilon} \sim \frac{2\sqrt{2N_B}}{\varepsilon}$$

$\bar{N}_C$ = candidates

N_B = background events

ε = detection efficiency

Two points have to be stressed on this argument:

a) an interesting peculiarity of the experiment consists in the fact that the background can be directly evaluated by changing ΔE along the propagation region, the neutron-antineutron transition probability being $P(\bar{n}) \propto \left(\frac{1}{\Delta E}\right)^2$. Moreover, should an effect be found, it would be possible to modulate its intensity that way.

b) The effective sensitivity depends on the background as

$$(3) \qquad \tau \propto \left(\frac{N_B}{\varepsilon^2}\right)^{-1/4} = (BT)^{-1/4} \qquad \text{where} \quad B = \frac{N_B}{\varepsilon^2 T}$$

and T is the duration of the experiment.

Just to give an example, an experiment with one background event per day, running for 100 days with an efficiency $\varepsilon=0.5$ would reach a sensitivity an order of magnitude smaller than that obtainable in the condition $B=0$, $\varepsilon=1$.

It is therefore crucial, planning an experiment, to search for the conditions for which the background is negligible. Only when $BT<1$ and $\varepsilon^2\simeq1$ it is possible to fully exploit the available neutron current and oscillation length.

3. THE GRENOBLE EXPERIMENT

The Grenoble experiment designed to detect neutron oscillations up to a sensitivity in $\tau_{n\bar{n}}\sim10^6$ sec (corresponding to $\Delta m\sim10^{-28}$ MeV and a $\Delta B=2$ decay processes lifetime approximately $\tau(\Delta B=2)\sim10^{30}$ years) makes use of the von Laue-Langevin Institute (ILL) nuclear reactor. The final goal of the experiment is to reach $\tau_{n\bar{n}}=10^8\div10^9$ sec.

With a power output of 57 MW, the ILL reactor provides a very high neutron flux. The reactor core is immersed in liquid D_2O, which brings the fission neutrons quickly to thermal equilibrium at room temperature ($\langle E_n\rangle\sim2.5\cdot10^{-2}$eV). A liquid deuterium moderator near the core cools the neutron down to a temperature of about 25°K ($\sim2\cdot10^{-4}$eV). Cold neutron beams are transported by means of total reflection in neutron guide tubes.

The experiment was conceived in two steps: the first one capable to reach $\tau\sim10^6$ sec with a rather cheap apparatus, as well as to study deeply the problem of the background.

The major sources of background are:

1) gamma rays and fast neutrons from the reactor travelling along with the neutron beam;
2) pile-up of gamma rays produced by neutrons in the target;
3) neutral cosmic ray (C.R.) interactions.

1) and 2) are proportional to the neutron current, 3) to the target mass.

The experiment started, profiting of the ILL facilities, using cold guided neutrons. As it was noted before, the sensitivity in $\tau_{n\bar{n}}$ is proportional to the neutron wave length so that with cold neutrons higher limits for $\tau_{n\bar{n}}$ can be reached. Furthermore, neutron beam guides, which are in practice constant section tubes with totally reflecting surfaces allow the transport at any distance from the reactor of neutron beams maintaining constant lateral dimensions and densities. The limiting angle θ_L for total reflection is $\theta_L = 0.0017\ \lambda\ (\text{Å})$ rad.

The experimental set up is shown in Fig. 1: cold neutrons from a beam guide (H 18) propagated in vacuum (10^{-5} torr) in a low magnetic field region ($B \ll 10^{-3}$ gauss). The neutron beam was then dumped on a 54 cm diameter B_4C target covered in the central part by an additional ^6LiF absorber, 20 cm in diameter.

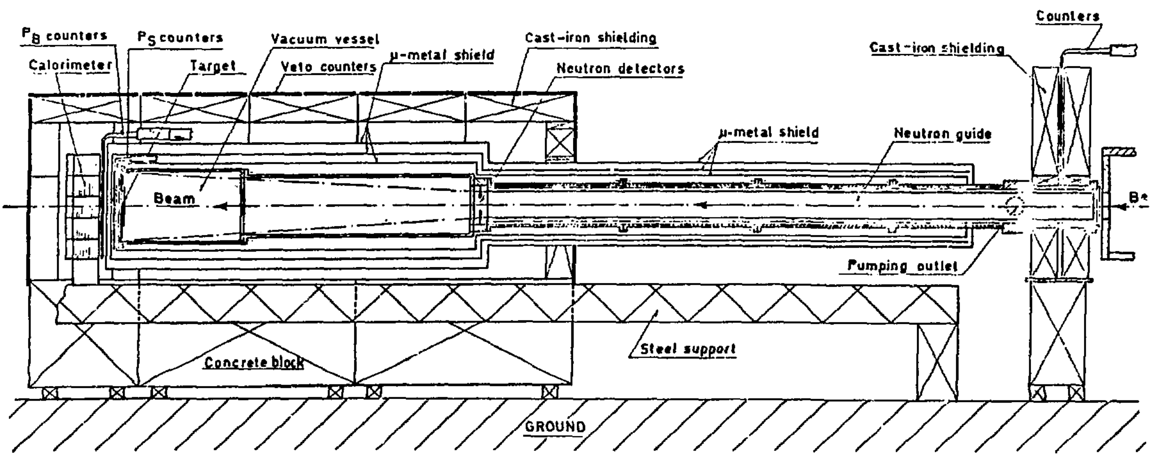


Fig. 1 Experimental set-up at the ILL in Grenoble.

The possible antineutron component would annihilate into the absorber releasing the characteristic 2 GeV energy of the antineutron-nucleon annihilation process, and would be detected by the calorimeter placed in front of the target, outside the magnetic shielding. On the average, an energy deposition of $\sim$350 MeV is expected since the calorimeter covered $\sim$1/4 of the solid angle.

Let us look at the apparatus in greater detail. The H18 beam guide is made of 10 elements 1 m long arranged so as to give a curvature radius of 25 m. In this way γ rays and fast neutrons coming from the reactor are eliminated and background 1) is made negligible.

Fig. 2 shows the intensity and the divergence of the neutron beam as a function of its energy. The energy of the transported neutrons ranges from $10^{-3} \div 10^{-5}$ eV.

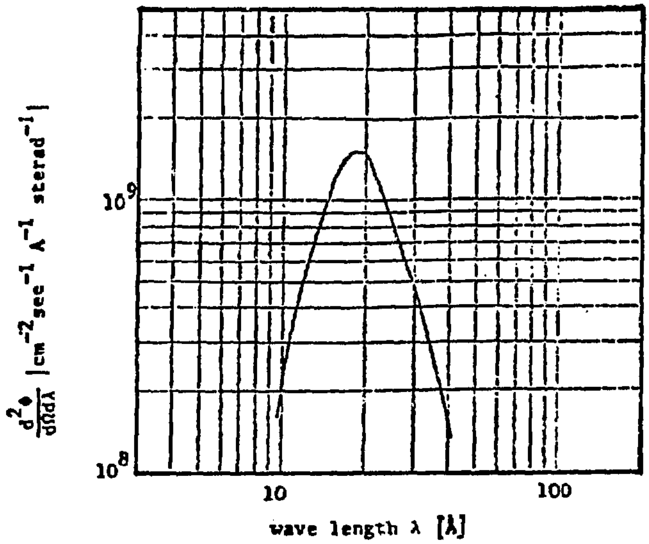
A 4.5 m long straight section guided the neutrons to the propagation region, consisting of a 2.7 m long drift vessel with increasing diameter in order to match the aperture of the initial beam, thus preventing any interaction with the nuclei of the walls. The average oscillation time for neutrons between their last reflection in the guide and their absorption was $3 \cdot 10^{-2}$ sec.

The neutron beam was monitored by four small neutron detectors placed at the exit of the neutron guide. The measured intensity was 10^9 n/sec.

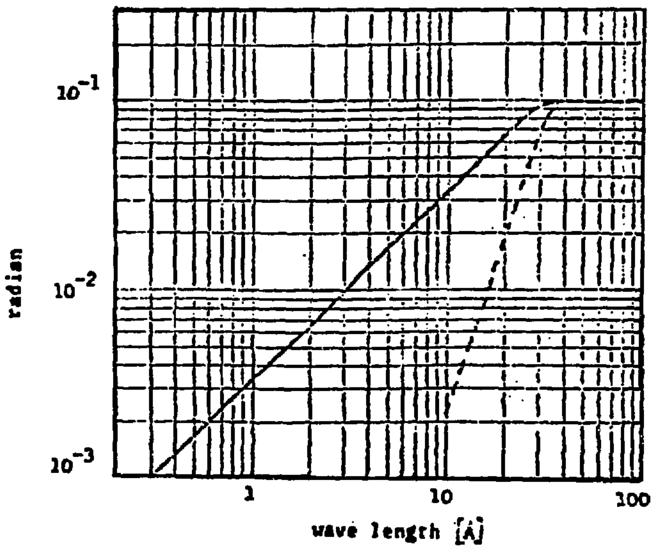
Neutrons ran for a time $t \sim 10^{-2} \div 10^{-1}$ sec across the oscillation region; to obtain the "quasi free condition" the Earth magnetic field was correspondingly reduced to a few tenths of milligauss by surrounding the drift vessel and the straight beam guide by a triple μ-metal shield.

Coils were used to demagnetize the μ-metal and to provide a magnetic field of a few tenths of gauss so that by alternatively switching the magnetic field off and on, the probability of neutron oscillation changed by a factor $\sim 10^6$. The magnetic field was also monitored all along the experiment and resulted to be stable around the fixed value.

The calorimeter C, placed in front of the neutron target, consisted of 5 modules 16 cm high, and covered an area of 80×80 cm^2. Each module was made of 20 alternated 5 mm thick layers of lead and scintillator and



Neutron mean flux per unit wavelength per unit of solid angle.



Vertical — and horizontal --- divergency of the neutron beam.

Fig. 2 Neutron flux and divergencies of the beams used in the experiment.

was viewed one side by a 15 in.XP2040 PM and by two smaller PM's on the opposite side looking separately at the front and at the back side of the calorimeter.

The calorimeter was calibrated at CERN PS, in a low energy μ^-, π^-, $\bar{p}$ beam. The antiproton annihilations were studied in particular, and their characteristics reproduced through a M.C.

The calorimeter was monitored continuously during the experiment through charged cosmic-rays crossing the apparatus. Its efficiency was constant over all its volume for energy deposition E≥150 MeV, and at least E≥75 MeV per module, when more than one module was triggered.

Two sets of two counters each are placed between the annihilation target and the calorimeter: one, P_S, just behind the target, between the two innermost magnetic shields, had the same area of the target and was 0.6 cm thick; the second, P_B, 1 cm thick, was placed just in front of the calorimeter and had the same area of the calorimeter.

The whole apparatus was protected from cosmic rays by anti-coincidence counters covering an area of about 30 m^2, only the very small solid angle around the neutron beam axis remaining uncovered. The overall efficiency of the anticoincidence system to reject cosmic ray events is ε≥99.95%.

The anticoincidence shield was protected from inside, in order to avoid rejection of annihilation events, by a 20 cm thick layer of cast iron.

Single rates of each counter as well as rates of vetoed and non-vetoed coincidence were recorded. Pulse height and timing were recorded for each PM not in the veto system.

When veto counters were used in coincidence for calibration their labels were also recorded. This information permits a rough reconstruction of single charged particles.

The coincidence $P_S \cdot P_B \cdot C \cdot \bar{V}$ corresponded to at least one charged particle from the target, which traversed P_S and P_B, reached the calorimeter C and was not accompanied by a signal in the anticoincidence counters V.

Data were taken alternating almost daily runs with the magnetic field off (quasi-free condition) and the magnetic field on (oscillation probability soppressed by a factor $\sim 10^6$), for a total effective time of 15 days in each condition. Data were taken also for the same effective time with the reactor off.

The three samples of experimental data were studied separately and then compared. The two sets of data taken with the reactor on look practically identical. Moreover when the conditions for the energy deposition in the calorimeter C are applied

a) $E_{Tot} \geq 150$ MeV b) E per module >75 MeV

and the three sets of data are compared, taking into account the rates, the energy distributions and the topologies, they result to be identical at 94% C.L.

It is then possible to conclude that:

a) there is no indication for neutron oscillations
b) at a level of $1.26 \cdot 10^{15}$ neutrons the background from γ rays from neutron capture is negligible
c) cosmic rays are the most important source of background.

In order to evaluate a limit for $\tau_{n\bar{n}}$, the upper limit for the ratio $\bar{N}/N$ was evaluated. To this purpose a M.C. calculation was performed, taking into account the experimental set-up and the properties of the antineutron-nucleus annihilations. The results of the M.C. were then used to compare the data obtained in the runs with the "magnetic field off" (quasi-free condition) with those taken with the "magnetic field on" and those with the "reactor off", where only background events are expected.

Only events with field off in which the energy deposited in the calorimeter was

$$150 < E \leq 525 \text{ MeV}$$

and at least two modules of the calorimeter were triggered, were considered as candidates.

These selection criteria make systematic errors negligible in comparison with the statistical ones.

The detection efficiency evaluated from M.C. is 36%. The total number of candidates in 15 days running is 687.

During the same time the background events collected were 689 and 670 in the samples with "magnetic field on" and "reactor off" respectively. When also the topological and energetic distribution expected from M.C. for antineutron-nucleon annihilations are compared with those from the experimental events, the statistical error is slightly reduced and the result is:

$$\frac{\bar{N}}{N} = \frac{\bar{N}_C - N_B}{N\varepsilon} \leq 9 \cdot 10^{-14} \quad \text{at } 80\% \text{ CL}$$

Consequently,

$$\tau_{n\bar{n}} \geq \sqrt{\frac{1}{9 \cdot 10^{-14}}}\ 3 \cdot 10^{-2} \text{ sec} = 10^{5} \text{ sec} \quad \text{at } 80\% \text{ CL}$$

and the effective $B = \frac{N_B}{\varepsilon^2 T} \sim 3 \cdot 10^{-3}$/sec.

A further point to be discussed is the relation between efficiency and background in the evaluation of the lower limit on $\tau_{n\bar{n}}$ shown in formulae (2) and (3).

Since backgrounds are not expected to have the same distributions as one would expect for the real events, it might be that choosing events with particular topologies would reduce both the efficiency and the background events.

However, as can be seen from Eq. (3), only in the case that background events decrease more quickly than the efficiency square, the restriction at a smaller efficiency will enlarge the effective background.

Another way to evaluate the lower limit for $\tau_{n\bar{n}}$ is to consider a subsample of the previous events, namely those in which at least two particles reach the calorimeter from the target triggering two non-adjacent modules.

The expected fraction for this type of events is 35% while in the experimental data they represent only the 14%, practically satisfying the relation

$$\frac{N_{B_1}}{\varepsilon_1^{\,2}} \sim \frac{N_{B_2}}{\varepsilon_2^{\,2}}$$

yielding:

$$\frac{\bar{N}}{N} = \frac{\bar{N}_C - N_B}{\varepsilon N} < 7\cdot 10^{-14} \qquad \text{at} \quad 1 \text{ s.d.}$$

So that $\tau \geq 1.15\cdot 10^{5}$ sec

and the effective $B \simeq \frac{N_B}{\varepsilon^2 T} \sim 5\cdot 10^{-3}$.

4. THE PRESENT SET-UP FOR THE GRENOBLE EXPERIMENT

The second step of the experiment then, aiming at reducing as much as possible the C.R. background, was to build a detector with high spatial resolution, so to allow the reconstruction of the vertex of the particles coming from a neutral interaction.

The solid angle around the target covered by the new apparatus is much larger than before.

It is worthwhile to emphasize at this point that in the Grenoble experiment, in which the neutron beam is transported through a neutron beam guide, the area of the target illuminated by neutrons reflects the dimensions of the neutron beam guide (in the present experiment it is $\sim 30\text{x}50$ cm^2) and that, from the average divergence of neutrons in the beam one expects most of the possible antineutrons to annihilate in the very central part of the target. In contrast, C.R. neutral interactions are expected to be equally distributed on the target volume.

The new experimental set-up, shown in Fig. 3, will be briefly discussed.

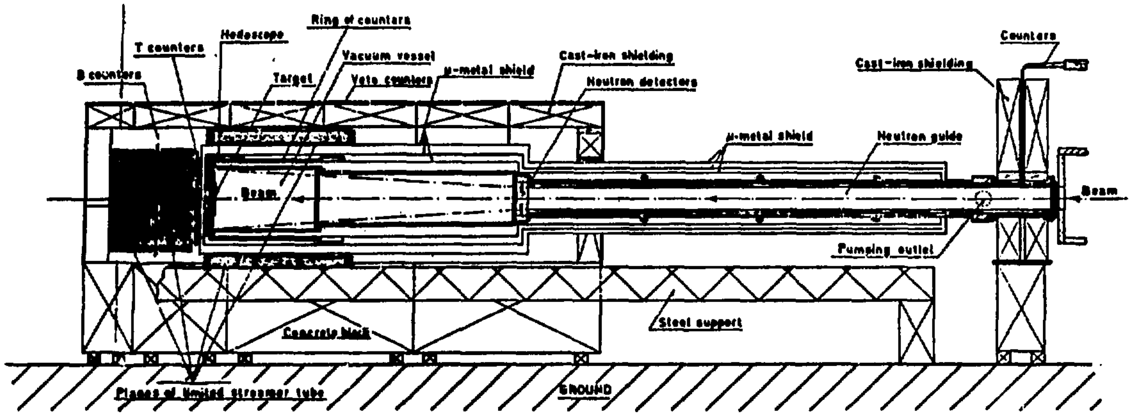


Fig. 3 New experimental set-up operating since November 1981.

Between the two innermost μ-metal shields there is now a hodoscope with a resolution of $4 \times 4\ cm^2$ and a ring of 56 counters, 4 cm wide, 100 cm long, 1 cm thick, covering almost completely the backward solid angle.

The P_B scintillation counters are replaced by a set of 4 counters with an area $80 \times 80\ cm^2$.

Instead of the old calorimeter we have now 18 planes of limited streamer tubes, the first 8 interleaved with 0.5 cm Al planes, the rest with 0.5 cm Fe, covering an area of $100 \times 150\ cm^2$.

A set of 4 scintillation counters with the same total area, 2.5 cm thick, are placed between the first 8 and the following 10 planes of limited streamer tube.

Furthermore, 4 sets of 4 planes of limited streamer tubes with the same area each interleaved with 0.5 cm Al plates, cover almost completely the backward solid angle.

Finally, the target is now practically totally made of ^6LiF, limiting that way the high energy gammas from nucleon absorption in the target.

The limited streamer tubes, a technique which has been recently developed in Frascati[11] and which are similar to those used in the

Mont Blanc proton decay experiment, are in practice Geiger type tubes 1x1 cm^2, 150 cm long, with a highly quenching mixture (75% isobutane, 25% Argon) and an anode wire of large diameter (∿100 μm) so as to limit the discharge in a few millimeter region inside the tubes.

The latter are plastic tubes vernished inside with graphite acting as cathodes. The pulse is then collected in a system of x-y bidimensional strips. A track going through the detector will give a signal in each x,y plane allowing a spatial resolution of 2 cm for a vertex of a two track event.

This new set-up will be operational in November 1981. Fig. 4 shows two typical events from C.R. in a part of the apparatus.

The goal of this second step of the Grenoble experiment is a strong reduction of the C.R. background which should allow from one side to reach value for $\tau_{n\bar{n}} \sim 10^6$ sec, and from the other to be able to safely plan an experiment sensitive to $\tau_{n\bar{n}} \sim 10^8 \div 10^9$.

5. FUTURE PROGRAMS

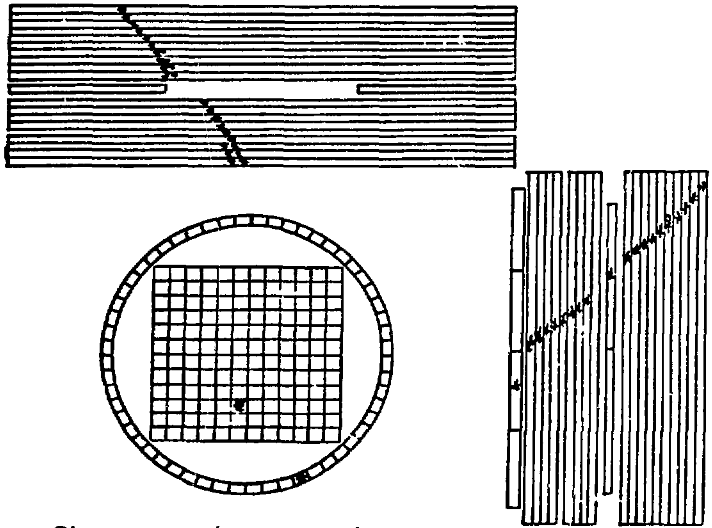
Recalling the formula for the probability of neutron-antineutron transition in quasi free neutron condition ($\Delta Et \ll 1$)

$$P(\bar{N},t) = N \frac{1}{\tau^2} \frac{L^2}{v^2}$$

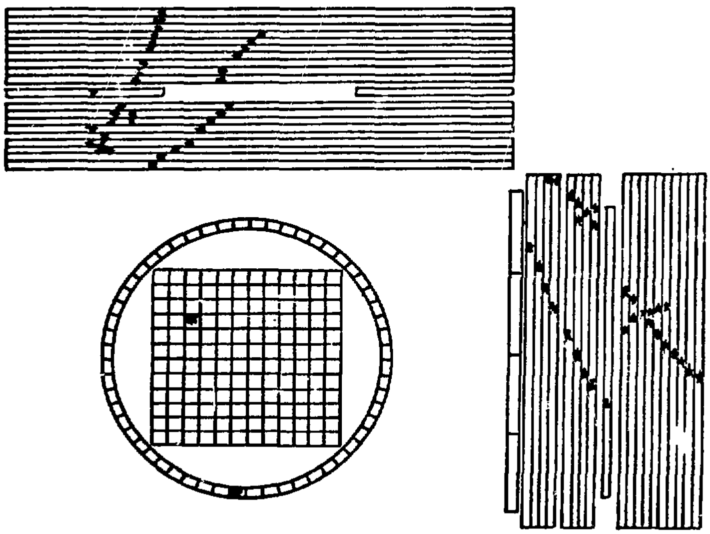
it is apparent that the sensitivity for an experiment studying neutron-antineutron trasitions is increased by the following procedures.

1) N , the number of neutrons examined in a unit time t, must be increased to as large a value as possible.
2) The energy E of oscillating neutrons must be as small as possible.
3) L, the oscillation length, must be as long as possible, the dependence of $P(\bar{N},t)$ being on L^2.

However, all these conditions have to be matched with the requirement of a negligible background.



Charge cosmic ray event



Neutral cosmic ray event

Fig. 4 On-line display of two cosmic ray events. Hits are recorded in the hodoscope (square), in the ring of counters around the target, in the limited streamer chambers (two views) and the B and T counters.

Formula (2) in Sec. 2 gives the sensitivity for an experiment in the presece of background. It can be rewritten as

$$(4) \qquad \tau \propto \sqrt[4]{\frac{I\ TL^2}{2B'}}\ \frac{1}{v} \qquad \text{where} \quad B' = \frac{N_p}{\varepsilon^2\ I\ TL} ,$$

I is the neutron current and the other quantities are the same as in Sec. 2.

In the following we will discuss the implications of (4) for two possible experiments to be performed at a nuclear reactor.

1) Neutrons from the reactor core which oscillate along beam tubes. In this case

$$\tau \sim \sqrt{I\ T}\ \frac{L}{v} = \sqrt{\frac{\phi_r\ S\ AT}{4\pi L^2}}\ \frac{L}{v} = \sqrt{I_o \left(\frac{\theta}{2}\right)^2 T}\ \frac{L}{v} = \sqrt{\frac{I_o\ A\ T}{4\pi}}\ \frac{1}{v} ,$$

where

ϕ_r is the neutron flux at the reactor

S the area of the neutron beam at the reactor

A the area of the neutron beam at the annihilation target

$I_o = \phi_r \cdot S$

2) Neutrons transported by means of neutron beam guides on the experimental area, and then oscillating along drift vessels whose section increases as a function of L in order to fully contain the beam current One can write:

$$\tau \sim \sqrt{IT}\ \frac{L}{v} = \sqrt{a\ \phi_r\ S \left(\frac{\theta_L}{2}\right)^2 T}\ \frac{L}{v} = \sqrt{a\ I \left(\frac{\theta_L}{2}\right)^2 T}\ \frac{L}{v} = \sqrt{\frac{a\ I\ \ AT}{4\pi}}\ \frac{1}{v}$$

where θ_L is the total reflection limiting angle, A, the beam area at the target is given by $2\pi(L/\theta_L)^2$ and a is a factor of the order of 0.5 taking into account that for a beam guide the full solid angle is available only for the fraction of the neutron in the guided beam which have the largest λ, the others being confined in a narrower solid angle proportionally to their λ.

Furthermore, the oscillation time in an experiment with guided neutrons is the oscillation time in the drift vessel increased by the average oscillation time in the beam guide between the last reflection in the guide and the entrance in the drift vessel.

In a beam guide with a section of $\sim 100\ cm^2$ neutrons with $\langle\lambda\rangle \sim 10$ Å will travel in average a distance $L_o \simeq 3$ m without any nuclear interaction for a corresponding oscillation time 0.8×10^{-2}s. The result being that the annihilation target can be reduced in proportion

$$A \propto (L-L_o)^2$$

Having in both experiments annihilation targets of the same material and thickness, when background is present

$$\tau \propto \sqrt[4]{\frac{I\ TA}{2B}}\ \frac{1}{v} \qquad\qquad B = \frac{N_B}{I\cdot T\cdot A\cdot\varepsilon^2}$$

Let us concentrate on some realistic experimental conditions:

a) cold neutrons are transported by means of guided beam tube: $\langle\lambda\rangle \simeq 10$Å corresponding to a $\langle v\rangle \sim 400$ m sec^{-1}.
b) neutrons propagating in beam tube from the reactor are in the thermal region $\langle v\rangle \sim 2200$ m sec^{-1}.

From the previous formula it follows that two experiments at the same reactor can reach the same sensitivity provided that: either

i) $A_T \sim 10^3\ A_C$

or

ii) $T_T \sim 10^3\ T_C$

or iii) $L_T \sim 30\ L_C$

or, more generally,

$$I_T \sim 10^3 \; I_C$$

The subscript C refers to cold neutrons and T to thermal neutrons.

On the basis of these considerations, it was planned to continue the experiment at the new cold source in Grenoble with a beam guide $\sim$40 m long, a section of $\sim$100 cm^2 and a flux $\sim 10^{10}$ n cm^{-2} sec^{-1}, the average λ being 10 Å.

With $\sim$35 m long drift vessel an annihilation area of $\sim$1 m^2 and a magnetic field $B<10^{-4}$ gauss it will be then possible to measure $\tau_{n\bar{n}}$ at a level of 3×10^8 sec, provided the present experiment will show that it is possible to make the cosmic rays background negligible.

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WORKING GROUP MEETINGS

MUON-SPIN-ROTATION WORKING GROUP

D. MacLaughlin, Chairman

Richard Hutson was elected Chairman for 1982.

Based on projections made by L. Agnew at the earlier Stopped Muon Channel (SMC) Working Group meeting, it was concluded that the combination of reduced operating schedule from impending budget cuts and the growing beam use of the Crystal Box group would probably result in a reduction of available beam time at SMC of at least 50% for both the Los Alamos Group MP-3/μSR collaboration and the Yale University group.

Discussion of long-range plans involving possible muon channels at LAMPF II or the Proton Storage Ring (PSR) was deferred to subsequent meetings about such facilities to be held later in the day.

Intermediate-range plans for increasing data rates with the MP-3/μSR spectrometer were discussed by R. H. Heffner. These included

- the use of thin-foil samples and surface-muon beams to reduce e^+ absorption and scattering within the sample, resulting in an expected gain of from 30% to a factor of 2 in data rates; and
- the use of wire-chamber detectors to identify decay e^+ with stopping μ^+, effectively overcoming the disadvantage of low duty cycle by segmenting the sample into several (4,9,...) subvolumes.

ATTENDEES

Los Alamos National Laboratory

Andreas Badertscher	Richard Hutson
Carolus Boekema	Melvin Leon
Robert H. Heffner	Mario E. Schillaci

Other Institutions

Patrick Egan, Yale University
Michael Gladisch, Yale University

* * *

ENERGETIC PION CHANNEL AND SPECTROMETER (EPICS) WORKING GROUP

David B. Holtkamp, Chairman

R. L. Boudrie reported on the status of the EPICS system. He discussed the current resolution of the spectrometer as well as efforts by the Group MP-10 staff to reduce backgrounds and increase the ease of operation of the system. Plans for a new muon rejection system were discussed and the operation of the EPICS cooled-gas target was described. During the discussion of the status of EPICS a suggestion was made to consult with the Low-Energy Pion (LEP) channel users to see if they would be amenable to increasing the A-1 target thickness from 3 to 4.5 or 6 cm. This change would be contingent on whether the A-1 target box could withstand the added heat loading.

A report was then given by D. B. Holtkamp on the success of having the working group chairman sit in as an observer on the EPICS Program Advisory Committee (PAC) deliberations. It was emphasized that it is the prerogative of the working group chairman to continue to observe the PAC deliberations if such a need exists.

Nelson Stein gave an interesting talk on the possible physics one could do with 700-MeV/c reaction products at a higher energy LAMPF. The (K,π) reaction was emphasized as a method of learning new nuclear structure. The current state of the art in (K,π) reactions was discussed, and it was felt that the field of nuclear physics as a whole would benefit from studies using kaons at much higher intensity and resolution.

H. A. Thiessen then presented a schematic of a kaon and high-energy pion channel that could conceivably use the existing EPICS spectrometer with great profit. After further discussion a subcommittee was set up with Nelson Stein as its chairman to explore further the nuclear physics one can do with high-intensity kaon and pion beams. The purpose of such a study would be a proposal to enhance LAMPF in the next 5-10 years by making it a facility to study kaon, antiproton, neutrino, etc. reactions at energies near 1 GeV.

After these discussions Don Geesaman was elected EPICS Working Group Chairman for 1981-82.

EPICS Working Group (continued)

ATTENDEES

Los Alamos National Laboratory

Richard L. Boudrie	Joel M. Moss
Andrew Browman	Susan Seestrom-Morris
Steven J. Greene	Nelson Stein
C. L. Morris	H. A. Thiessen

Other Institutions

Gary S. Blanpied, University of South Carolina
George R. Burleson, New Mexico State University
Robert E. Chrien, Brookhaven National Laboratory
H. Terry Fortune, University of Pennsylvania
Don Geesaman, Argonne National Laboratory
Carol J. Harvey, University of Texas
David B. Holtkamp, University of Minnesota
Roger Liljestrand, EG&G, Inc.
Benjamin Zeidman, Argonne National Laboratory

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STOPPED MUON CHANNEL (SMC) WORKING GROUP

Howard Matis, Chairman

Lew Agnew, in an overview about scheduling for SMC, said there had been sufficient beam time for approved experiments during the 1981 fiscal year. However, because of the limited availability of beam and the prospect of heavier requests for beam time, there could be scheduling conflicts in 1982.

Bob Macek and Gary Sanders discussed the status of the SMC beam line. The new SMC counting house is complete except for moving the magnet controls. Work is proceeding on an electrostatic separator, which should be ready soon, and an advertisement for customers of the separator was made. Progress on design of a beam splitter is slower because of priority assignments. The extension of Cave B has been completed. Also, a new degrader, jaws, and a gas barrier have been installed in the beam line.

Pat Egan requested that a cryogenic radioactive gas trap be installed in the beam line so that very low-momentum muon beams can be used.

The group noted that because many LAMPF Users have different research interests and because there are only two sessions for working group meetings, Users frequently cannot attend a working group meeting that is of interest to them. To reduce some of the scheduling conflicts it was suggested that the number of time slots for the working group sessions be increased to three. With an extra session there would be a decreased probability for conflict.

The working group unanimously nominated Richard Mischke to be its Technical Advisory Committee representative. It also selected Gary Sanders to chair its next meeting.

ATTENDEES

Los Alamos National Laboratory

Lewis E. Agnew	Richard E. Mischke
Robert H. Heffner	Gary H. Sanders
M. William Johnson	Mario E. Schillaci
Robert J. Macek	E. Brooks Shera
Howard Matis	Dieter Wohlfahrt

Other Institutions

Andreas Badertscher, Yale University
Gerald Dugan, Columbia University
Patrick O. Egan, Yale University
Kip Gardner, Yale University
Michael W. Gladisch, Yale University
Virgil L. Highland, Temple University
Fesseha G. Mariam, Yale University
Jean M. Oöstens, Oklahoma University
Richard J. Powers, California Institute of Technology
David H. Snow, Museum of New Mexico
John D. Zumbro, Notre Dame University/Princeton University

* * *

SOLID-STATE PHYSICS AND MATERIALS SCIENCE WORKING GROUP
Robert D. Brown, Chairman

R. Brown described a new facility, under the beam stop at Target A-6, that gives access to a neutron flux of $\sim 10^{13}/cm^2$-s. Samples can be placed into this flux by using a remotely movable cart (~5 by 7 by 25 cm). To date several irradiations have been completed on a variety of materials that have application for accelerator service. Experience has shown that materials placed in this facility will reach a temperature of 120-150°C when "chill block" cooling is provided.

Brown also reported that S. McEwen of the Atomic Energy Commission/Canada has used another stringer at the neutron irradiation effects facility to study dimensional changes in heavily cold-worked zirconium alloys. The initial results have indicated a decrease in linear dimension in these materials and have prompted efforts to improve the resolution of the measurements. The alloys are candidate materials for fission reactor fuel cladding.

J. Cost described recent progress on Exp. 554, "800-MeV Proton Irradiation of Technologically Important Materials," by noting that irradiations of 304 stainless steel and Inconel 718 (candidate accelerator window materials) and tantalum, tungsten, and molybdenum (candidate spallation neutron source materials) have been completed. The work is undertaken in an attempt to quantify the mechanical property changes of materials under 800-MeV proton irradiation. Postirradiation testing has indicated an increase in yield strength and a decrease in ductility; specific conclusions are now being drawn. Future plans for this experiment include the irradiation of depleted uranium, another possible material for spallation neutron sources. The work is carried out in an isotope production stringer at Target A-6.

W. Sommer described plans for gaining additional access to the direct proton beam in the A-6 area. Several experiments, which are basic and phenomenological and which are related to the interaction of high-energy protons and materials, will benefit from the proposed system that will allow vertical emplacement, temperature control, and *in situ* property measurements.

Also noted were a number of recent publications generated by work at LAMPF on radiation effects; they will be included in the publications section in *Progress at LAMPF*, July 1 - December 31, 1981.

D. M. Parkin was nominated by this group for representation on the Technical Advisory Panel.

R. Brown was reelected Chairman of this working group.

ATTENDEES

Los Alamos National Laboratory

James N. Bradbury	Robert P. Damjanovich
Robert D. Brown	J. Fowler
J. Cost	C. Hansen
K. Christensen	Walter F. Sommer

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NEUTRINO WORKING GROUP
Herbert H. Chen, Chairman

Four primary topics were covered during the Neutrino Working Group meeting.

1. Status of experiments
   - Exp. 225, R. Burman
   - Exp. 559, G. Phillips
   - Exp. 609, H. Kruse
   - Exp. 638, T. Dombeck
   - Exp. 645, L. Hyman
2. Neutrino fluxes
   - Line A beam stop, H. Chen
   - LAMPF II decay in flight, R. Allen
3. Liquid time projection chamber (TPC) developments
   - Overview, H. Chen
   - Status at the University of California at Irvine, H. Mahler
4. Elections, nominations, etc.

Status of Experiments

Installation of the Exp. 225 (University of California at Irvine/Los Alamos) detector system within the neutrino house is almost complete. The central detector and the multiwire-proportional-counter anticounters (except those on the door) are in place. Several layers of the central detector are operational. The immediate goal is to run in November with one-third the central detector to assess neutron backgrounds from the beam stop. After this the intention (funds permitting) is to complete the detector system by March 1982 to be ready for beam when it becomes available.

The Exp. 559 collaboration (Rice University/University of Houston/University of California at Los Angeles/Los Alamos/Lawrence Berkeley Laboratory/New Mexico State University) is continuing to develop the fast neutron coincidence technique for detection of the inverse beta reaction, $\bar{\nu}_e p \rightarrow e^+ n$. Background suppression with this technique may permit high sensitivity to $\bar{\nu}_\mu \rightarrow \bar{\nu}_e$ oscillations with a small detector.

Recently, the Exp. 609 effort (Los Alamos) has been turned off within the Laboratory. Its proponents are considering the use of the detector at a reactor on site.

Efforts are being put forward by the Exp. 638 collaboration (Los Alamos/University of Maryland) to measure the pion yield at 730 and 800 MeV. Evolution of the Line D neutrino facility and detector design continues. A proposal is scheduled to be sent to the Department of Energy in January 1982 for Nuclear Science Advisory Committee review.

Estimated cost of the adit for Exp. 645 (Ohio State University/Argonne/Caltech/Louisiana State University) is reported now to be in the \$500-\$600K range sought earlier. Substantial funds have been allocated for the detector so far, but additional funds are required to complete it within 3 years. Designs for moving the detector system within the adit, and studies of anticoincidence efficiencies and of neutron delayed coincidence by capture in a thin layer of gadolinium, are progressing.

Neutrino Fluxes

The addition of a 20-cm H_2O insert upstream of the beam stop is expected to increase the neutrino flux by about 40%. The uncertainty accompanying this change is substantial and it is felt that an experiment to remeasure the rate of stopped π^+ decays is essential for the neutrino program at the beam stop. A collaborative effort for such a measurement is being initiated.

In connection with LAMPF II (an upgrade of LAMPF to the 4- to 16-GeV range), the neutrino flux from a pion decay-in-flight facility has been estimated. Typically, an increase in the range of 30-100 in neutrino flux per proton and an increase of 3-5 in average neutrino energy is expected. The combination gives a larger event rate per unit detector mass than that available at any other accelerator.

Liquid TPC Developments

The continuing requirements for massive but fine-grained detectors at affordable cost have given impetus to the development of new detector technology. The most significant among these is the development of liquid TPCs. Efforts in this direction have been made in Japan, Europe, and the USSR, as well as in the United States. Such detectors are low-rate devices and appear well suited for neutrino physics at accelerators.

The major difficulty in the development of the liquid TPC is the ability to drift ionization electrons long distances in the liquid, that is, obtaining and maintaining very pure liquids. The purity problem appears to be solved for liquid argon, where attenuation lengths in excess of 1 m at 1 kV/cm have been consistently achieved. Progress on liquid xenon and liquid methane has also been impressive. Attempts are now under way to build several-ton test detectors.

Elections and Nominations

Herbert H. Chen was reelected working group chairman for 1982. Thomas A. Romanowski was nominated for the Technical Advisory Panel.

ATTENDEES

Los Alamos National Laboratory

Lewis E. Agnew	Terry Goldman
John C. Allred	Cyrus M. Hoffman
Thomas J. Bowles	H. Kruse
Robert L. Burman	Robert J. Macek
R. Carlini	Howard Matis
Donald R. F. Cochran	Takamitsu Oka
Thomas Dombeck	Gerard J. Stephenson, Jr.
Joey B. Donahue	Richard L. Talaga

Neutrino Working Group (continued)

Other Institutions

Bjarne Aas, University of California at Los Angeles
Richard C. Allen, University of California at Irvine
Felix H. Boehm, California Institute of Technology
Herbert H. Chen, University of California at Irvine
Nance L. Colbert, University of California at Irvine
Gerald T. Garvey, Argonne National Laboratory
A. Dayle Hancock, University of Houston
Lloyd G. Hyman, Argonne National Laboratory
George J. Igo, University of California at Los Angeles
M. William Johnson, University of California at Irvine
Hansjurg Mahler, University of California at Irvine
Bill W. Mayes II, University of Houston
Gerald C. Phillips, Rice University
Phillip H. Steinberg, University of Maryland
Eric A. Umland, Rice University
K.-C. Wang, University of California at Irvine
Alex F. Zehnder, SIN

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NUCLEAR CHEMISTRY WORKING GROUP
Lon-Chang Liu, Chairman

Bruce Dropesky was elected Working Group Chairman for 1982, after which he reported on the status of the ongoing chemistry experiments at LAMPF and on the budget outlook for next year.

Merle Bunker discussed in detail the scientific merits of a helium-jet transport system coupled to a mass separator to study spallation and fission products at LAMPF. He stressed that the proposed system will enable us to perform various measurements that the CERN ISOLDE is unable to do, for example, the determination of spin, parity, magnetic moments, and other ground-state properties of isotopes of nonvolatile elements. He also discussed various technical aspects of the project, such as the choice of a site and equipment arrangement. Finally, Dr. Bunker listed several remaining technical problems to be resolved in the near future. The preparation of a full-fledged proposal for a helium-jet-fed, on-line mass separator project would then follow.

Bruce Dropesky presented a survey on the recent results on pion-nucleus single-charge-exchange reactions on several medium- to heavy-mass nuclei. He compared experimental excitation functions to theoretical results obtained from a Fermi gas model (from W. Gibbs), which incorporates a phenomenological modification of pion-nucleus interaction.

Gil Butler reported the progress made at the Thin Target Area in relation to searches for new neutron-deficient light nuclei near the proton drip line.

Dave Vieira presented a status report on the proposed time-of-flight (TOF) spectrometer for the Thin Target Area. He projected the following time schedule: finalization of optics by February 1982, followed by preengineering work to be started in March 1982. He also encouraged and welcomed those in the nuclear chemistry users' groups who are interested in using the TOF spectrometer to contact him and start writing proposals for Program Advisory Committee approval.

Paul Karol gave a talk on his recent research of the possibility of using high-energy spallation to study high-temperature nuclei. He pointed out the usefulness of separating the evaporation and the fast intranuclear cascade processes in these studies.

The participants then turned to a round-table discussion on experiments at the proposed LAMPF II. Lon-Chang Liu gave an introduction to kaon-nucleus interactions and presented a list of potential research fields for consideration by the participants. The discussion was quite enthusiastic and the preliminary suggestions may be grouped as follows.

1. The Nuclear Chemistry Working Group expressed great interest in undertaking kaonic-atom and hypernuclear spectroscopy studies, and in developing, accordingly, new experimental techniques in this new frontier of nuclear sciences.
2. The working group noted the enhanced fragmentation cross sections for the production of neutron-rich nuclei that lie far from β stability at proton energies of $\sim$4 GeV and above. A high-intensity proton beam of such energy at LAMPF II would be of great value to the investigation of the nuclear properties of such exotic nuclei.
3. The availability of pure and high-intensity pion beams at energies substantially higher than those at LAMPF will provide excellent experimental conditions for studying higher πN-isobar resonances in the nucleus.

4. The antiproton beam will be very useful in studying energy deposition processes when an antiproton annihilates with a nucleon. The working group requested that the planning committee of LAMPF II make a careful evaluation of the scientific merits as well as the competitiveness with CERN-LEAR on the production of an antiproton beam.

The committee recommended that more frequent contacts should be established between the LAMPF II planning committee and the nuclear chemistry group at LAMPF. The format of such contact is to be discussed at an appropriate occasion in the future.

ATTENDEES

Los Alamos National Laboratory

Jean J. Harry Berlijn	Gregg C. Giesler
Merle E. Bunker	Michael Leitch
Gilbert W. Butler	Lon-Chang Liu
James Clark	J. Rayford Nix
Bruce J. Dropesky	Charles J. Orth
Peggy Dyer	Bob Reedy
Zev Frankel	David J. Vieira

Other Institutions

Paul Karol, Carnegie-Mellon University

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POLARIZED FACILITIES WORKING GROUP

Michael W. McNaughton, Chairman

Michael McNaughton was reelected Chairman and Bill Bonner was nominated to the Technical Advisory Panel.

The following "newsbriefs" were presented and discussed.

Polarized Beams

- Beam-line polarimeter calibrations.[1]
- The (P,Q)(N,R) cycle,[2] which is operating satisfactorily.
- Tentative plans for installing a second solenoid (to give approximate longitudinal spin at the liquid-deuterium target for neutron production) in the fall of 1982.
- Standard sign convention (since Cycle 30) for spin direction. Conventions are up, left, and parallel to momentum for NOR (there may be rare exceptions).[3] To be consistent, polarimeters should calculate (left-right) and (down-up), not (up-down).

Carbon Polarimeters

A fit has been made[4] to the world's data for proton-carbon inclusive analyzing power, A_c. The fit gives $A_c(\theta,E)(0 < \theta \lesssim 30°, 100 \lesssim E \lesssim 800$ MeV) with about ±2.5% uncertainty.

Olin van Dyck suggested that more thought be given to optimizing use of the carbon polarimeters (for example, carbon thickness, angle resolution).

Polarized Targets

- Plans for installing HERA at the High-Resolution Spectrometer (tentative).
- Experiment 512. These experimenters are presently using a frozen-spin target with a dilution refrigerator in HERA. Decay time is about 500 hours in a 3-kG field.

The system gives unlimited access in one quadrant (or quarter sphere) in the S-spin configuration.

John Jarmer outlined plans to build a frozen-spin target that would allow the beam to enter along the axis of the cryostat (for longitudinal spin).

Wayne Cornelius reported on the high-intensity (optically pumped) polarized source. Work at LAMPF is slow because of the lack of funds. The Japanese at the National Laboratory for High-Energy Physics (KEK) are committed to producing an optically pumped source by March 1983. Wayne recently returned from a trip to KEK where he worked with Y. Mori on the KEK source. The KEK goal is 20 μA, but Wayne believes that the latest techniques will produce >100 μA. TRIUMF also has a program to build such a source.

The group discussed the possible use of a polarized beam at LAMPF II (or the LAMPF Flavor Factory). A low-intensity polarized beam would yield no advantage over the competition [Zero Gradient Synchrotron (ZGS), 12 GeV (now closed); SATURNE II, 3 GeV; KEK, 12 GeV; Alternating Gradient Synchrotron (AGS), 24 GeV] and would arrive too late to do anything new and exciting. The only advantage of a high-intensity polarized beam would be to produce polarized secondary particles. Theoretical expectations are that spin transfer in antiparticle production is very small, but the group agreed that we should measure this at the AGS as soon as possible. Spin transfer from polarized protons to neutrons is not an attractive prospect in view of the competition from polarized deuteron stripping at SATURNE.

The group proceeded to discuss other possibilities for producing polarized antiparticles. Double scattering should work (first to produce the antiparticles, the second time to polarize them), but the group came up with no other promising ideas.

Bill Bonner advocated building a Super-LEAR (at higher energy) to study "bottomonium" spectroscopy.

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ATTENDEES

Los Alamos National Laboratory

Billy E. Bonner
Wayne D. Cornelius
J. J. Jarmer
Michael W. McNaughton
Joel M. Moss
Thomas M. Putnam
Olin B. van Dyck

Other Institutions

Charles L. Hollas, University of Texas
Lawrence S. Pinsky, University of Houston
Peter J. Riley, University of Texas
Stephen E. Turpin, Rice University
William R. Wharton, Carnegie-Mellon University
Stephen A. Wood, Massachusetts Institute of Technology

* * *

COMPUTER FACILITIES WORKING GROUP
Dennis G. Perry, Chairman

Jim Little introduced his plan for a computer temperature alarm. This device, based on a commercial burglar alarm, would dial a preselected telephone number (for example, the Central Control Room) if the computer became too hot or the power for the air conditioning shut off. The alarm could also be activated by any other equipment malfunction (for example, cryogenic targets).

Mark Kaletka reported on the progress of the Data Analysis Center (DAC) and the terminal network. All computers will soon be located at the DAC with terminals in the Laboratory Office Building connected to the computer of choice through a port selector.

Ted Spitzmiller reported on the LAMPF Electronics Equipment Pool (LEEP) and computer maintenance. This is now entirely a Group MP-1 operation under Bob Critchfield. Many of the technicians are new and inexperienced, so response time for lower priority work may be slow. The LEEP inventory is low, especially on the most popular items, but maintenance capabilities are expanding following the addition of computer automation.

Mike Oothoudt reported on the conversion of Q for RSX-11M. A document describing the new features is available. The first test will use the π^0 spectrometer system as a guinea pig during the next production period.

Several speakers regretted the poor participation of users in future planning. A recent Q questionnaire produced only 11 responses. Another question on which user feedback would be appropriate is the plan to replace the RK05 disks with Winchester-style drives. Although these sealed units should be more reliable and would provide 10-50 times the storage capacity of a single RK05, the lack of removable packs may cause problems in backup or intercomputer transfer.

Computer Facilities Working Group (continued)

Michael McNaughton asked how to improve user participation and was thereupon elected Chairman for the coming year. He eagerly awaits suggestions.

ATTENDEES

Los Alamos National Laboratory

James F. Amann	J. Little
Harold S. Butler	Kok Heong McNaughton
David Daniels	Michael M. McNaughton
Gregg C. Giesler	Takamitsu Oka
James F. Harrison	Michael A. Oothoudt
Martha Hoehn	Dennis G. Perry
Earl W. Hoffman	Dennis D. Simmonds
Mark Kaletka	Ted Spitzmiller
Thomas Kozlowski	Stephen A. Wood
Michael J. Leitch	

Other Institutions

Kenneth Butterfield, University of New Mexico
David Clark, University of New Mexico
David B. Holtkamp, University of Minnesota
B. Joseph Lieb, George Mason University
V. Gordon Lind, Utah State University
Donald R. Machen, Scientific Systems International
Hans S. Plendl, Florida State University
Stephen E. Turpin, Rice University
Stephen A. Wood, Massachusettts Institute of Technology

* * *

LOW-ENERGY PION (LEP) CHANNEL WORKING GROUP

Felix E. Obenshain, Chairman

The LEP Working Group elected Barry G. Ritchie Chairman for 1981-82.

A discussion of several problems present in the channel produced a resolution that the horizontal and vertical solid-angle jaws should be redesigned for greater reliability because of their importance to the optical quality of the pion beam. Martin Cooper reported that the existing disk drives in the counting house are to be replaced by a sealed, 134-Mb, dual-plotter disk-drive system, which may alleviate some of the LEP computer system problems. A consensus of those present indicated that the air conditioning system in the LEP counting house is inadequate and may be the source of some computer problems; this resulted in a resolution asking for engineering support to redesign the air conditioning system.

Martha Hoehn reported on the channel magnet control system installed at LEP. Helmut Baer suggested that the HP-85 minicomputer be set up to print out the magnet settings for experiment records. Several people suggested that a steel floor be placed in the LEP cave, and a resolution to that effect was passed unanimously.

Helmut Baer presented a progress report on the π^0 spectrometer. Developmental activities since the last meeting have produced needed cave modifications, a semipermanent setup, improved shielding, overhead boom installation, and generally more stable data-taking conditions. Setup time takes $\lesssim$1 week, and a higher flux may be used at 0°. The 1981 runs included isobaric-analog states, critical opalescence, and inclusive single-charge-exchange studies. Further improvements to the data-acquisition system and spectrometer hardware are planned.

Dick Boudrie discussed the initial design characteristics of the portable low-energy pion spectrometer. As contemplated the device would cover pion energy and lab-angle ranges of 20-80 MeV and 30-135°, respectively, with a typical energy resolution of 100-250 keV. The possibilities of operating in a dispersed-beam mode are being considered, which would cost about $700K in addition to the $500-$600K for the spectrometer. An optional smaller solid-angle setting of the support stand would permit studies beyond 135°.

Boudrie also brought up the possibility of installing a thicker (4.5-cm) production target in lieu of achieving 750 μA of proton beam. Users were urged to think about possible adverse effects accompanying the increased pion flux.

LEP Working Group (continued)

Two speakers, Thomas Sanford and Jen-Chieh Peng, discussed physics possible at LEP with the contemplated LAMPF II kaon beams. Sanford suggested three possible areas of research using a stopping K^- beam: (1)kaon-nucleus scattering, (2) kaonic x rays, and (3) rare kaon decays. Peng discussed the use of the π^0 spectrometer in kaon single-charge-exchange experiments that would investigate hypernuclei and the so-called strange dibaryon H(2129).

ATTENDEES

Los Alamos National Laboratory

Helmut Baer	Michael V. Hynes
Richard L. Boudrie	Michael J. Leitch
J. David Bowman	Richard E. Mischke
Martin D. Cooper	Jen-Chieh Peng
Peter Herczeg	Thomas Sanford
Martha Hoehn	Richard D. Werbeck
Cyrus M. Hoffman	

Other Institutions

Larry E. Antonuk, University of South Carolina
Peter D. Barnes, Carnegie-Mellon University
Ewart W. Blackmore, University of British Columbia, TRIUMF
S. Dytman, Massachusetts Institute of Technology
Ed V. Hungerford III, University of Houston
Harold E. Jackson, Jr., Argonne National Laboratory
Felix E. Obenshain, Oak Ridge National Laboratory
Robert P. Redwine, Massachusetts Institute of Technology
Barry G. Ritchie, University of South Carolina
William R. Wharton, Carnegie-Mellon University

* * *

HIGH-ENERGY PION (P^3) CHANNEL WORKING GROUP

William J. Briscoe, Chairman

Hans S. Plendl was elected Chairman for 1981-82 and Daniel H. Fitzgerald was reelected representative to the Technical Advisory Panel.

Cy Hoffman gave a talk, "A K^0 Beam for CP Violation Experiments and a 5-GeV/c Separated $K^{\pm}\bar{p}$ Beam," in which he discussed the production of such beams and some of their possible uses in particle and nuclear physics experiments. He requested all users who are interested in such beams to communicate with him (phone 505-667-5876) so that their ideas and suggestions can be used in the preparation of a proposal. A copy of his talk is available on request.

Chris Morris presented a proposal to use the Large-Aperture Spectrometer (LAS) as a moderately high-resolution spectrometer in conjunction with a dispersed P^3 beam that would ideally achieve resolution of 0.5%. Such a system would be capable of doing Energetic Pion Channel Spectrometer (EPICS)-type experiments in the 250- to 550-MeV range. One such experiment (Exp. 674) has been approved by the Program Advisory Committee subject to a demonstration that the system is feasible; Chris Morris estimated that 200 hours of beam development time would be needed to do this. After a spirited discussion of the feasibility of converting LAS and P^3 into a high-resolution system, Chris Morris was asked to prepare a detailed conversion plan and to inform the Technical Advisory Panel of this plan.

Richard Morgado summarized the changes made during the past year on the P^3 channel and on channel control facilities (hardware and software). He also discussed the new P^3 operating manual, in which both the old and the new channel features are described in detail. Copies of that manual will be sent to all active P^3 users by early next year. Richard's old job as P^3 beam-line physicist is unfilled at present; Martha Hoehn can be consulted concerning channel control software.

ATTENDEES

Los Alamos National Laboratory

Donald R. F. Cochran	Michael A. Paciotti
Peter A. M. Gram	Randall R. Sands
Peter Herczeg	Thomas Sanford
Robert J. Macek	Richard D. Werbeck
Richard Morgado	

Other Institutions

William J. Briscoe, University of California at Los Angeles
Daniel H. Fitzgerald, University of California at Los Angeles
Virgil L. Highland, Temple University
Harold E. Jackson, Jr., Argonne National Laboratory
B. Joseph Lieb, George Mason University
V. Gordon Lind, Utah State University
O. Harry Otteson, Utah State University
Hans S. Plendl, Florida State University

* * *

HIGH-RESOLUTION SPECTROMETER (HRS) WORKING GROUP

G. S. Blanpied, Chairman

The election of new members to the Technical Advisory Panel was discussed briefly. Following a consensus that the HRS Working Group is already sufficiently represented, the matter was dropped with no action taken.

J. McGill was elected Chairman of the working group for the coming year.

After discussion the following were nominated for consideration in filling the two vacancies in the Program Advisory Committee:

S. Austin, Michigan State University,
R. Lindgren, University of Massachusetts,
J. Heisenberg, University of New Hampshire,
G. Love, University of Georgia, and
F. Petrovich, Florida State University.

J. Amann gave a brief report of the first full year's operation of the focal-plane polarimeter. The salient points brought out were that (1) a total of $\simeq 10^6$ events are required to measure $D_{SS'}$ (more for the other depolarization parameters because of spin precession in the spectrometer), and (2) that small-angle data taking with the focal-plane polarimeter is currently limited by background singles in the polarimeter chambers. Two solutions to this last point were offered.

1. A third rear chamber could be installed and the "good event" definition changed to require two out of three chambers. The improvement thus expected would be a factor of 4 or 5.
2. The cathode wires in the chamber, to which the isobutane ions drift, could be used to determine the left-right ambiguity remaining after the identification of the proper anode wire. This would reduce the number of wire planes required by a factor of 2, without the need for more hardware.

No action was taken by the group.

H. Thiessen presented some preliminary suggestions regarding the role of the HRS facility in the planned upgrading of LAMPF. With a production target underground outside the dome area, a dispersion matched beam of $\simeq 10^8$ pions in the range 0.6-1.0 GeV/c could be delivered to the target. Interesting reactions suitable to the HRS were said to be (π,π'), (π,K), (K,K'), and (K^-,K^+). Thiessen asked that a group be formed to meet several times in the next 8 months to discuss the physics justification and facilities recommendations for such a program. George Igo volunteered and was appointed chairman; J. Moss, G. Hoffmann, and N. Hintz also volunteered. Membership was not limited, however, to those four.

George Igo asked about the progress of the Line C polarimeter and noted that beam polarization as measured by the device was consistently unreliable. Jim Amann suggested that a small group should meet later to discuss the problem.

Gerald Hoffmann asked about the progress of the Faraday cup. H. Thiessen responded that the Group MP-10 technicians were working on the Line C polarimeter, a modification to the Line B-C split, and the Line C insertable-strip ion chambers during the coming shutdown. He offered to change priorities if the working group reached a strong consensus to that effect, but no changes were made.

ATTENDEES

Los Alamos National Laboratory

James F. Amann
Richard L. Boudrie
M. William Johnson
Joel M. Moss
J.-C. Peng
Susan Seestrom-Morris
E. Brooks Shera
H. A. Thiessen
Dieter Wohlfahrt

Other Institutions

Bjarne Aas, University of California at Los Angeles
Ewart W. Blackmore, University of British Columbia, TRIUMF
Gary S. Blanpied, University of South Carolina
Robert E. Chrien, Brookhaven National Laboratory
Gerald W. Hoffmann, University of Texas
Bo Höistad, University of Texas
David B. Holtkamp, University of Minnesota
George J. Igo, University of California at Los Angeles
J. McGill, Rutgers University
Barry G. Ritchie, University of South Carolina

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GRADUATE STUDENT/POSTDOC WORKING GROUP

Steven J. Greene, Chairman

This report combines the meetings of November 3 and December 2. John Faucett was elected Chairman and Steve Greene was elected Vice-Chairman of the working group for 1982.

A number of possibilities for speakers for the LAMPF Lecture Series were discussed, as the Series' organizer is interested in having the graduate students and postdocs select these.

Topics and speakers for short courses in 1982 were considered. Topics suggested were pion-nucleon interactions, practical and theoretical aspects of multiwire proportional chambers, and implementation of scattering calculations on computers. Chandra Pillai was appointed to organize the first course.

Interest was expressed in having a time for very informal talks by graduate students.

ATTENDEES

Los Alamos National Laboratory

Carolus Boekema
Sobhendranath Datta
Takamitsu Oka

Other Institutions

John Faucett, University of Oregon
Michael Greene, Yale University
Chandra Pillai, Oregon State University
Stephen A. Wood, Massachusetts Institute of Technology

* * *

WORKING GROUP DISCUSSION OF A KAON FACTORY

November 3, 1981

Low-Energy Pion (LEP)	Thomas Sanford Los Alamos	Physics with a Stopping K^- Beam
π^0 Spectrometer	Jen-Chieh Peng Los Alamos	(K^-,π^0) and Other Experiments
Energetic Pion Channel and Spectrometer (EPICS)	Nelson Stein Los Alamos	Hypernuclear Physics with a High-Resolution 700-MeV/c Beam
Polarized Facilities	Michael McNaughton Los Alamos	High-Energy Polarized Facilities Beams in Area B
Stopped Muon Channel (SMC)	Gary Sanders Los Alamos	Muon Beams at a Kaon Factory
Neutrino Facilities	Richard Allen University of California, Irvine	Yield of Neutrinos at a Kaon Factory
High-Energy Pion (P^3)	Cyrus Hoffman Los Alamos	A K^0 Beam for CP Violation Experiments and a 5-GeV/c Separated $K^{\pm}\bar{p}$ Beam
High-Resolution Spectrometer (HRS)	H. A. Thiessen Los Alamos	Possibilities for a High-Resolution (3×10^{-5}) Separated π, K, and $\bar{p}$ Beam at HRS
Nucleon Physics Laboratory (NPL)	Robert Eisenstein Carnegie-Mellon University	Uses of Antiproton Beams at LAMPF — Including Polarized Antiprotons
Nuclear Chemistry	Bruce Dropesky Los Alamos	Nuclear Chemistry at a Kaon Factory

CONFERENCE STAFF

Linda Tyra, Julia Anderson, Alice Horpedahl, Margaret Eustler

Harry Otteson

V. Gordon Lind

B. Joseph Lieb

George A. Rinker

H. A. Thiessen

Murray Moinester and J. David Bowman

Kok-Heong McNaughton
Jean Oöstens
John Faucett

Peter Barnes and Harold Jackson

Background: Harold Jackson and Roy Holt
Foreground: Clarence Richardson and Kenneth Crowe

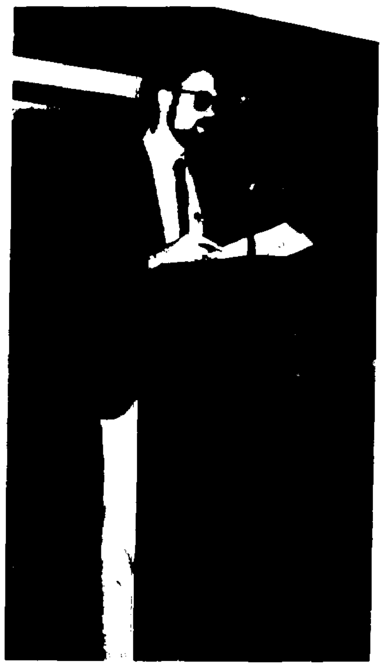
Don Kerr, Director of Los Alamos National Laboratory

John Kane and Michael W. Gladisch

George Igo, James Shepard, and Steve Wallace

Jean-Pierre Blaser, Darragh Nagle, and Ewart Blackmore

Felix Boehm and Milla Baldo-Ceolin



Robert Eisenstein

Jen-Chieh Peng
and
Keh-Chung Wang

Gerald Garvey
Louis Rosen
Harvey Willard

Robert Redwine

Louis Rosen

and

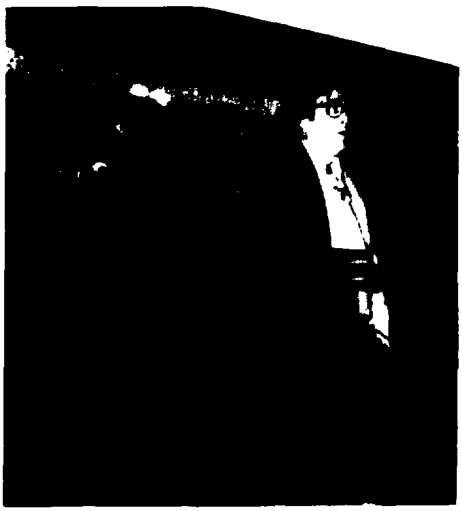
Alex Zehnder

James Amann

Susan Seestrom-Morris

Barry Ritchie

Don C. Hagerman



G. Stephenson

Sirish Nanda and L. Wayne Swenson

PARTICIPANTS OF THE FIFTEENTH LAMPF USERS GROUP MEETING

Bjarne Aas
UCLA/Los Alamos National Laboratory
MS 831

Lewis E. Agnew
Los Alamos National Laboratory
MP-7, MS 840

Richard C. Allen
Physics Department
University of California
Irvine, CA 92717

John C. Allred
Los Alamos National Laboratory
MP-DO, MS 850

James F. Amann
Los Alamos National Laboratory
MP-10, MS 841

Herbert L. Anderson
Los Alamos National Laboratory
P-DO, MS 434

Larry E. Antonuk
Physics Department
University of South Carolina
Columbia, SC 29208

Daniel Ashery
Argonne National Laboratory
Argonne, IL 60439

Andreas Badertscher
University of Berne/
Yale University
LAMPF, MS 831

Helmut Baer
Los Alamos National Laboratory
MP-4, MS 846

Milla Baldo-Ceolin
University of Padova/CERN
Via Marzolo 8
Padova, Italy

Peter D. Barnes
Physics Department
Carnegie-Mellon University
Pittsburgh, PA 15213

Joe Bergstein
Los Alamos National Laboratory
MP-2, MS 812

Jean J. H. Berlijn
Los Alamos National Laboratory
E-2, MS 824

Aron M. Bernstein
Physics Department, 26-433
Massachusetts Institute of Technology
Cambridge, MA 02139

Tarlochan S. Bhatia
Department of Physics
Texas A&M University
College Station, TX 77843

Judith Binstock
Los Alamos National Laboratory
X-4, MS 250

Ewart W. Blackmore
University of British Columbia
TRIUMF
Vancouver, B.C.
Canada V6T 2A3

Gary S. Blanpied
Department of Physics
University of South Carolina
Columbus, SC 29208

Jean-Pierre Blaser
SIN
CH-5234, Villigen
Switzerland

Felix H. Boehm
Physics Department, 161-33
California Institute of Technology
Pasadena, CA 91125

Carolus Boekema
Los Alamos National Laboratory
MP-3, MS 844

Mark Bolsterli
Los Alamos National Laboratory
T-9, MS 452

Billy E. Bonner
Los Alamos National Laboratory/
Rice University
Bonner Laboratory
Houston, TX 77001

Richard L. Boudrie
Los Alamos National Laboratory
MP-10, MS 841

Thomas J. Bowles
Los Alamos National Laboratory
P-3, MS 456

J. David Bowman
Los Alamos National Laboratory
MP-4, MS 846

James N. Bradbury
Los Alamos National Laboratory
MP-3, MS 844

William J. Briscoe
Department of Physics
University of California
Los Angeles, CA 90024

Andrew Browman
Los Alamos National Laboratory
MP-DO, MS 832

Robert D. Brown
Los Alamos National Laboratory
MP-7, MS 840

Ronald E. Brown
Los Alamos National Laboratory
P-3, MS 456

Robert M. Brugger
Los Alamos National Laboratory
P-8, MS 805

Merle E. Bunker
Los Alamos National Laboratory
CNC-5, MS 776

George R. Burleson
Physics Department, Box 3D
New Mexico State University
Las Cruces, NM 88003

Robert L. Burman
Los Alamos National Laboratory
MP-4, MS 846

Gilbert W. Butler
Los Alamos National Laboratory
CNC-11, MS 514

Harold S. Butler
Los Alamos National Laboratory
MP-DO, MS 832

Edwin P. Chamberlin
Los Alamos National Laboratory
MP-11, MS 823

Herbert H. Chen
Department of Physics
University of California
Irvine, CA 92717

Robert E. Chrien
Department of Physics
Brookhaven National Laboratory
Upton, NY 11973

David A. Clark
University of New Mexico
LAMPF, MS 831

Donald R. F. Cochran
Los Alamos National Laboratory
MP-DO, MS 830

Nance L. Colbert
Physics Department
University of California
Irvine, CA 92717

Joseph J. Comuzzi
Physics Department
Massachusetts Institute of Technology
Cambridge, MA 42139

Martin D. Cooper
Los Alamos National Laboratory
MP-4, MS 846

Wayne D. Cornelius
Los Alamos National Laboratory
MP-10, MS 841

George A. Cowan
Los Alamos National Laboratory
ADCEL, MS 102

Kenneth M. Crowe
Lawrence Berkeley Laboratory
Building 50, BM 205
Berkeley, CA 94720

Steven H. Dam
Los Alamos National Laboratory
X-4, MS 250

Robert P. Damjanovich
Los Alamos National Laboratory
MP-8, MS 826

Sobhendranath Datta
Los Alamos National Laboratory
LS-1, MS 880

John F. Davis
Los Alamos National Laboratory
C-8, MS 294

Thomas Dombeck
Los Alamos National Laboratory
P-3, MS 456

Joey B. Donahue
Los Alamos National Laboratory
MP-7, MS 840

Hermann J. Donnert
Department of Nuclear Engineering
Kansas State University
Manhattan, KS 66506

Bruce J. Dropesky
Los Alamos National Laboratory
CNC-11, MS 824

Gerald Dugan
Columbia University
Nevis Laboratory
P. O. Box 137
Irvington, NY 10533

Patrick O. Egan
Physics Department
Yale University
New Haven, CT 06511

Robert A. Eisenstein
Department of Physics
Carnegie-Mellon University
Pittsburgh, PA 15213

Richard J. Ellis
Los Alamos National Laboratory
P-DO. MS 805

David J. Ernst
Physics Department
Texas A&M University
College Station, TX 77843

John A. Faucett
University of Oregon
LAMPF, MS 831

Daniel H. Fitzgerald
University of California
LAMPF, MS 831

Dolracy P. Fontenla
University of Campinas
Brazil

H. Terry Fortune
Department of Physics, E1
University of Pennsylvania
Philadelphia, PA 19104

James Frank
Los Alamos National Laboratory
MP-4, MS 848

Christopher J. Gardner
Yale University
LAMPF, MS 831

Gerald T. Garvey
Argonne National Laboratory
Argonne, IL 60439

Donald F. Geesaman
Argonne National Laboratory
Argonne, IL 60439

William R. Gibbs
Los Alamos National Laboratory
T-5, MS 454

Benjamin F. Gibson
Los Alamos National Laboratory
T-5, MS 454

Gregg C. Giesler
Los Alamos National Laboratory
CNC-11, MS 824

Michael W. Gladisch
University of Heidelberg/
Yale University
LAMPF, MS 831

George Glass
Department of Physics
Texas A&M University
College Station, TX 77843

Terry Goldman
Los Alamos National Laboratory
T-5, MS 454

Peter A. M. Gram
Los Alamos National Laboratory
MP-4, MS 846

Michael G. Greene
Yale University
LAMPF, MS 831

Steven J. Greene
New Mexico State University
LAMPF, MS 841

Donald C. Hagerman
Los Alamos National Laboratory
MP-DO, MS 828

A. Dayle Hancock
Department of Physics
University of Houston
Houston, TX 77004

James F. Harrison
Los Alamos National Laboratory
MP-1, MS 828

Robert H. Heffner
Los Alamos National Laboratory
MP-3, MS 844

Leon Heller
Los Alamos National Laboratory
T-5, MS 454

Peter Herczeg
Los Alamos National Laboratory
T-5, MS 454

Virgil L. Highland
Physics Department
Temple University
Philadelphia, PA 19122

Robert A. Hilko
University of New Mexico
Cancer Research and Treatment Center
LAMPF, MS 809

Robert E. Hill
Los Alamos National Laboratory
MP-2, MS 812

Norton M. Hintz
Physics Department
University of Minnesota
Minneapolis, MN 55455

Martha V. Hoehn
Los Alamos National Laboratory
MP-13, MS 838

Cyrus M. Hoffman
Los Alamos National Laboratory
MP-4, MS 846

Earl W. Hoffman
Los Alamos National Laboratory
MP-1, MS 828

Gerald W. Hoffmann
Department of Physics, RLM 5.208
University of Texas
Austin, TX 78712

Gary E. Hogan
Los Alamos National Laboratory
MP-13, MS 838

Bo Höistad
Department of Physics
University of Texas
Austin, TX 78712

Charles L. Hollas
University of Texas
LAMPF, MS 831

Roy J. Holt
Argonne National Laboratory
Argonne, IL 60439

David B. Holtkamp
University of Minnesota
LAMPF, MS 841

John C. Hopkins
Los Alamos National Laboratory
NSP-T+V, MS 670

Ed V. Hungerford III
Department of Physics
University of Houston
Houston, TX 77004

Lloyd G. Hyman
Argonne National Laboratory
Argonne, IL 60525

Michael V. Hynes
Los Alamos National Laboratory
MP-4, MS 846

George J. Igo
Physics Department
University of California
Los Angeles, CA 90024

Kenichi Imai
Physics Division
Argonne National Laboratory
Argonne, IL 60439

Harold E. Jackson, Jr.
Physics Division, D203
Argonne National Laboratory
Argonne, IL 60439

Nelson Jarmie
Los Alamos National Laboratory
P-3, MS 456

M. William Johnson
Los Alamos National Laboratory
P-3, MS 456

Wayne A. Johnson
University of California, Irvine
LAMPF, MS 831

John R. Kane
Physics Department
College of William & Mary
Williamsburg, VA 23185

Donald M. Kerr, Director
Los Alamos National Laboratory
MS 100

Thomas R. King
Los Alamos National Laboratory
WX-11, MS 948

Jere D. Knight
Los Alamos National Laboratory
CNC-11, MS 514

H. Brook Knowles
University of New Mexico/
BDN Corporation
11113 Brentwood Hill Blvd., NE
Albuquerque, NM 87112

Thomas Kozlowski
Los Alamos National Laboratory
MP-1, MS 828

Raymond Kunselman
Physics Department
University of Wyoming
Laramie, WY 82071

Michael J. Leitch
Los Alamos National Laboratory
MP-4, MS 846

B. Joseph Lieb
George Mason University
Fairfax, VA 22030

Raimond Liepins
Los Alamos National Laboratory
CMB-10, MS 528

V. Gordon Lind
Physics Department
Utah State University
Logan, UT 84322

Lon-Chang Liu
Los Alamos National Laboratory
CNC-11, MS 824

Robert J. Macek
Los Alamos National Laboratory
MP-13, MS 838

Donald R. Machen
Scientific Systems International
3491 B Trinity Drive
Los Alamos, NM 87544

Hansjurg Mahler
Physics Department
University of California
Irvine, CA 92717

Howard Matis
Los Alamos National Laboratory
MP-4, MS 846

Bill W. Mayes, II
Physics Department
University of Houston
Houston, TX 77004

W. Kenneth McFarlane
Physics Department
Temple University
Philadelphia, PA 19122

Joseph L. McKibben
Los Alamos National Laboratory
P-9, MS 480

Michael W. McNaughton
Los Alamos National Laboratory
MP-13, MS 838

Michael M. Meier
Los Alamos National Laboratory
P-9, MS 805

Richard E. Mischke
Los Alamos National Laboratory
MP-4, MS 846

Murray Moinester
Physics Department
Tel Aviv University
Tel Aviv, Israel

Ernest J. Moniz
Massachusetts Institute of Technology
Center for Theoretical Physics
Cambridge, MA 02139

C. Fred Moore
Physics Department, RLM-5.208
University of Texas
Austin, TX 78712

Michael S. Moore
Los Alamos National Laboratory
P-3, MS 442

Christopher L. Morris
Los Alamos National Laboratory
MP-10, MS 841

Joel M. Moss
Los Alamos National Laboratory
P-3, MS 456

Darragh E. Nagle
Los Alamos National Laboratory
MP-DO, MS 846

Michael M. Nieto
Los Alamos National Laboratory
T-8, MS 453

J. Rayford Nix
Los Alamos National Laboratory
T-9, MS 452

Jan K. Novak
Los Alamos National Laboratory
MP-7, MS 840

Felix E. Obenshain
Physics Division
Oak Ridge National Laboratory
Oak Ridge, TN 37830

Harold A. O'Brien
Los Alamos National Laboratory
CNC-3, MS 514

Takamitsu Oka
Los Alamos National Laboratory
T-5, MS 454

Jean M. Oöstens
Physics Department
Oklahoma University
Norman, OK 73019

Michael A. Oothoudt
Los Alamos National Laboratory
MP-1, MS 828

Charles J. Orth
Los Alamos National Laboratory
CNC-11, MS 514

W. Z. Osborne
Los Alamos National Laboratory
WX-11, MS 985

O. Harry Otteson
Physics Department
Utah State University
Logan, UT 84322

Michael A. Paciotti
Los Alamos National Laboratory
MP-3, MS 809

Jen-Chieh Peng
Los Alamos National Laboratory
P-3, MS 456

Gerald C. Phillips
Bonner Laboratory
Rice University
Houston, TX 77001

Lawrence S. Pinsky
Physics Department
University of Houston
Houston, TX 77004

Hans S. Plendl
Physics Department
Florida State University
Tallahassee, FL 32306

Richard J. Powers
Physics Department
California Institute of Technology
Pasadena, CA 91125

Thomas M. Putnam
Los Alamos National Laboratory
MP-DO, MS 842

Ali A. Rahbar
Physics Department
University of California
Los Angeles, CA 90024

Robert P. Redwine
Physics Department, 27-447
Massachusetts Institute of Technology
Cambridge, MA 01239

Clarence R. Richardson
Department of Energy
MS J-309, GTN
Washington, DC 20585

Peter J. Riley
Department of Physics
University of Texas
Austin, TX 79712

George A. Rinker
Los Alamos National Laboratory
T-4, MS 212

Barry G. Ritchie
Department of Physics
University of South Carolina
Columbia, SC 29208

Peggy D. Robertson
Los Alamos National Laboratory
P-3, MS 442

R. G. H. Robertson
Los Alamos National Laboratory
P-3, MS 456

Louis Rosen, Director
LAMPF, MS 850

Gary J. Russell
Los Alamos National Laboratory
P-9, MS 805

Richard D. Ryder
Los Alamos National Laboratory
P-9, MS 805

Vernon D. Sandberg
Los Alamos National Laboratory
MP-4, MS 846

Gary H. Sanders
Los Alamos National Laboratory
MP-13, MS 838

Randall R. Sands
Los Alamos National Laboratory
MP-13, MS 838

Mario E. Schillaci
Los Alamos National Laboratory
MP-3, MS 844

Ralph E. Segel
Physics Department
Northwestern University
Evanston, IL 60201

Susan Seestrom-Morris
Los Alamos National Laboratory
P-3, MS 456

James R. Shepard
Cyclotron Laboratory
University of Colorado
Boulder, CO 80309

E. Brooks Shera
Los Alamos National Laboratory
P-7, MS 456

Robert T. Siegel
Physics Department
College of William & Mary
Williamsburg, VA 23185

James E. Simmons
Los Alamos National Laboratory
P-3, MS 456

Walter F. Sommer
Los Alamos National Laboratory
MP-13, MS 838

Rolf M. Steffen
Physics Department
Purdue University
West Lafayette, IN 47906

Nelson Stein
Los Alamos National Laboratory
P-3, MS 456

Phillip H. Steinberg
Department of Physics and Astronomy
University of Maryland
College Park, MD 20742

Gerard J. Stephenson
Los Alamos National Laboratory
P-DO, MS 454

Daniel D. Strottman
Los Alamos National Laboratory
T-9, MS 452

Jules W. Sunier
Los Alamos National Laboratory
P-3, MS 456

L. Wayne Swenson
Physics Department
Oregon State University
Corvallis, OR 97331

Richard L. Talaga
Los Alamos National Laboratory
MP-4, MS 846

W. L. Talbert, Jr.
Los Alamos National Laboratory
X-5, MS 420

Nobuyuki Tanaka
Los Alamos National Laboratory
MP-10, MS 841

Pinelli Tazio
University of Pavia
Instituto Fisica Nucleare
Via Bassi 6 27100
Pavia, Italy

Henry A. Thiessen
Los Alamos National Laboratory
MP-10, MS 841

Oland D. Thompson
Los Alamos National Laboratory
H-6, MS 691

Daniel Tieger
Boston University
Boston, MA 02215

James W. Toevs
Los Alamos National Laboratory
P-14, MS 410

Stephen E. Turpin
Rice University/
Los Alamos National Laboratory
P-3, MS 456

Eric A. Umland
Bonner Nuclear Laboratory
Rice University
Houston, TX 77001

Olin B. van Dyck
Los Alamos National Laboratory
MP-13, MS 838

Phillip Vergamini
Los Alamos National Laboratory
P-8, MS 805

Dave J. Vieira
Los Alamos National Laboratory
CNC-11, MS 824

Donald I. Walker
Associated Western Universities
136 E. South Temple, 2200
Salt Lake City, UT 84111

Stephen J. Wallace
Physics Department
University of Maryland
College Park, MD 20742

Keh-Chung Wang
Physics Department
University of California
Irvine, CA 92717

Richard D. Werbeck
Los Alamos National Laboratory
MP-7, MS 840

William R. Wharton
Department of Physics
Carnegie-Mellon University
Pittsburgh, PA 15213

Harvey B. Willard
National Science Foundation
Physics Division
1800 G Street, NW
Washington, DC 20550

Steve Wilson
Stanford University
LAMPF, MS 846

Dieter Wohlfahrt
Los Alamos National Laboratory
P-3, MS 456

Bernard E. Wood
University of Oregon/
Los Alamos National Laboratory
P-15, MS 662

Stephen A. Wood
Massachusetts Institute of Technology
LAMPF, MS 831

Richard Woods
Los Alamos National Laboratory
P-9, MS 805

Mary Anne Yates
Los Alamos National Laboratory
P-4, MS 554

Robbie L. York
Los Alamos National Laboratory
MP-11, MS 823

Vincent Yuan
University of Illinois
LAMPF, MP-4, MS 846

Alex F. Zehnder
ETH
Hönggerberg
CH-8049 Switzerland

Benjamin Zeidman
Argonne National Laboratory
203 Physics Department
9700 S. Cass Avenue
Argonne, IL 60439

Hans Ziock
Los Alamos National Laboratory
MP-4, MS 846

Klaus O. H. Ziock
Physics Department
University of Virginia
Charlottesville, VA 22901

John D. Zumbro
Physics Department
Notre Dame University
Notre Dame, IN 46556

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