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MASTER

PROCEEDINGS
of the
THIRD LAMPF USERS MEETING
October 29, 1969

LOS ALAMOS SCIENTIFIC LABORATORY
of the
University of California
LOS ALAMOS • NEW MEXICO

UNITED STATES
ATOMIC ENERGY COMMISSION
CONTRACT W-7405-ENG. 36

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Printed in the United States of America. Available from
Clearinghouse for Federal Scientific and Technical Information
National Bureau of Standards, U. S. Department of Commerce
Springfield, Virginia 22151

Price: Printed Copy \$3.00; Microfiche \$0.65

PROCEEDINGS
of the
THIRD LAMPF USERS MEETING
held at the
UNIVERSITY OF COLORADO
Boulder, Colorado
October 29, 1969

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Distributed: March 17, 1970

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FOREWORD

The Third LAMPF Users Group Meeting was held at Boulder, Colorado, on October 29, 1969. The meeting was attended by 119 scientists and physicians from 55 laboratories, universities, and institutions in the United States and Canada. It was the first general meeting held as a fully organized Users Group, following the initial meeting in June 1968 and the January 1969 meeting when the Charter was adopted.

During the year, the Users Group involvement in planning for experimental facilities has deepened considerably, especially in pion, muon, and nucleon physics, high-resolution proton spectroscopy, and nuclear chemistry. An important development is the strong interest now shown by the medical community, as evidenced by Dr. Kaplan's paper (p. 32) on "Potentialities of Negative π -Meson Beams in Radiotherapy of Cancer."

These proceedings constitute the permanent record of the meeting. Thanks are due to Dr. Henry S. Kaplan, our guest speaker, and Dr. David A. Lind and Dr. Ernest Rost of the University of Colorado, who gave much time and assistance in arranging for the meeting. The assistance of Katherine H. Harper in the preparation of these Proceedings is gratefully acknowledged.

Much credit for the success of the LAMPF Users Group should go to the following members of the Executive Committee who are concluding their work for 1969:

Harry Palevsky, Chairman
David A. Lind, Chairman-elect
Roy P. Haddock
Arthur M. Poskanzer
Harvey B. Willard

BUSINESS SESSION

Minutes of the
THIRD LAMPF USERS MEETING
Business Session
October 29, 1969

Chairman Harry Palevsky of Brookhaven National Laboratory opened the meeting and called for items of old business from the floor. None appearing, he went on to new business, reminding the members that proposed amendments to the Charter should be introduced at a general meeting.

W. Ploughe from Ohio State University, Chairman of the Nominating Committee, reported the results of the election of officers for 1970. The Nominating Committee, whose other members were B. Dropesky (Los Alamos Scientific Laboratory), H. B. Eldridge (University of Wyoming), J. Friedes (Brookhaven National Laboratory), and P. C. Gugelot (University of Virginia), had submitted the following slate of candidates:

Chairman: Roy P. Haddock, University of
California, Los Angeles

Chairman-elect: G. C. Phillips, Rice University

Executive Committee:

I. L. Morgan, University of Texas
Robert A. Naumann, Princeton University
Benjamin Zeidman, Argonne National
Laboratory

The result of the balloting was that the above slate was elected by a large margin, and they will serve for 1970. Ploughe pointed out that D. A. Lind, who had held the post of Chairman-elect for 1969, is currently with the AEC and has relinquished his Users Group office to avoid conflict-of-interest problems. He also noted that a number of write-in votes were recorded in the balloting for the Executive Committee: he commended these people to future Nominating Committees: H. B. Eldridge, D. A. Lind, W. B. Simon, Peter Gram, Robert Siegel, Donald Cochran, Lewis Agnew, Edward Knapp, J. Friedes, James Simmons, Kenneth Crowe, John E. Brolley, J. A. McIntyre, P. C. Gugelot, Stuart Meyer, and Vernon Hughes. It was also suggested that someone from the University of New Mexico School of Medicine serve on the Executive Committee.

USERS GROUP REPORT

Chairman H. Palevsky
Brookhaven National Laboratory

Speaking for the Users Group, we would like to thank Louis Rosen for his diligent effort and, in particular, for obtaining the funds that assure that the machine will be completed on schedule. I have done a little bit of traveling around the country in the last year, and I must say that the outlook in nuclear physics is gloomy every place except in Los Alamos. We hope that we will be able to do some good physics in Los Alamos in the coming years.

It was just about a year ago that we started the mechanics of forming this organization. In our first meeting, we had a response of about 200 people across the country. At present, there are more than 400 members, of which about one-fourth are at Los Alamos, and the rest are in the outside community. We have some 115 outside institutions represented. Based on experience of other Users Groups and the response that we have had here, I believe that this is about the size at which the organization will level off, and we would expect anywhere from one-third to one-half of the people to show up at the various Users Group meetings. I don't know if Bill Ploughe mentioned the fact, but two-thirds of the members voted in the election. That is a remarkable response, because in the APS elections, I think the number is about 10 percent.

In the response to various questionnaires that Users have sent back, it appears that the largest interest is in the physics that can be done with the π and μ beams; the second is the nucleon-nucleon area; and the third is the nucleon-nucleus area. In the last few months, as Louis Rosen has said, there has been a tremendous activity in the biomedical and biophysical aspects of the LAMPF machine.

Originally, with the formation of the Users Group, there were a number of tasks to be done: The Charter was drawn up and an Executive Committee was elected, and then the Charter provided that we appoint the Technical Advisory Panel (TAP). These groups then started to work and formed into smaller groups. The whole Users organization has pitched

in and there have been seven groups of Users and Los Alamos people who have worked together. These working groups have dealt with the following topics: the Stopped Muon Beam; the Facilities for Nucleon-Nucleon Research; the High-Resolution Spectrometer; the Nuclear Chemistry Experiments; the Biomedical and Biological Experiments; the Pion Spectrometer; and Polarized Beams. The purpose of these joint activities is to plan the best way to utilize the machine. For the nuclear physics community, it is clear that working at such a machine is going to be quite a different experience than working at a Van de Graaff or a cyclotron--just, for example, in the logistics of putting an experiment on the floor. For the people who have worked at the large proton accelerators, the difference is not going to be so great, but it is clear already, from the deliberations that have gone on in these various groups, that it is going to be different from, say, working in Brookhaven or at Berkeley. One of the results of this cooperative work is that the people who are participating in this activity are now beginning to get some inkling of what it is going to take to put an experiment on the floor. This is an important part of the Users activities.

The TAP has met once and will meet for a second time this evening. The sort of things they are going to talk about are a review of these seven projects that I have just listed. We tried to get representatives on the TAP from each of the disciplines. There is going to be a discussion of pool equipment and shared facilities; that is, things like on-line computers, general store of magnets, etc. This indicates the clear progress that we have made since a year ago when we were mainly concerned about large, general facilities. There will be a report to the TAP from the Executive Committee on the matter of housing for Users Groups at Los Alamos. There will also be some consideration of the mechanics for people who want to make concrete proposals for experiments at LAMPF when it comes on

the air and, finally, a discussion of the summer program for next year at Los Alamos.

The Executive Committee has met twice. In addition to the scientific matters, it has been concerned with the elections, the Newsletter, and housing. On the latter matter, the Executive Committee did a study on the probable housing requirements. Among the many factors considered were an extrapolation as to the budget for outside Users and a survey of the housing facilities at other national laboratories with large outside Users Groups. We came up with the recommendation to Louis Rosen that six months prior to the start-up there be available ten apartments and fifteen dorm rooms for outside Users Groups at Los Alamos. The basis of these recommendations is given in a memo from the Executive Committee to Louis Rosen, dated August 13, 1969, and it is available to any of the Users. Copies are available from the Liaison Officer, Lewis Agnew.

This is my last chance to address you gentlemen as Chairman of our group, and I thought it would be appropriate to make a few comments on the experience of the last year. I have been particularly impressed with the ease with which we have set up this Users Group. A number of people had been thinking about a Users Group in the year before we started, and we were sort of hesitant because we foresaw a tremendous amount of effort on the part of a few people. I am happy to say that this was not the case. Of some thirty or forty people that we called upon to do specific jobs for the Users organization, there was only one, or possibly two,

who declined. It just has not been a hard chore. Everybody has pitched in enthusiastically in every phase, such as the writing up of the Charter, the appointing of committees, etc. The activities with the Los Alamos people have gone smoothly, and there has been great progress. Equally gratifying and important has been the goodwill of Los Alamos people; that, together with the enthusiasm of the Users Group, has, in a little less than a year, given us a viable Users organization. Our group is pitching in and doing the required jobs and is also looking at the matters that may become important in the future.

A main concern of the people here in the group in the first year, in addition to the technical matters, has been the idea of setting a proper tone for the Users organization. Speaking for myself and my colleagues on the Executive Committee, one of the things we have tried to do is to provide good communications between the Users and Los Alamos as an underlying basis for having a good Users participation. I think that it would be a good thing to discuss here and now--whether you people are satisfied with the results. The purpose of the organization is twofold; one is to see that the needs of the experimenters are met in cooperation with Los Alamos people, the other is that this organization provide a means of letting the Users know what is going on. By that business of "what's going on," I mean it in as great detail as the Users wish it to be. With those words, I would like to say that I look forward next year to just being an ordinary member of our group and being kept informed by the officers who will succeed the present group.

Thank you.

OPEN DISCUSSION

The discussion period was opened with remarks from D. Lind, who recently joined the AEC Division of Research. He pointed out the importance of having Users proposals submitted at an early date. It is essential that requests for research support be on hand when AEC funds become available, as it takes some time to review and process the proposals. Proposals closely connected with LAMPF programs or that indicate collaboration with Los Alamos would probably have priority.

In response to a question from K. Crowe on whether AEC funds would be available for summer study, Lind replied that support of a broad summer study project would be somewhat difficult. Requests for individual grants to cover salary, students, and travel for a project at LAMPF are easier to handle.

J. Brolley asked about the availability of hardware money. Lind replied that what little hardware money there is can be found essentially in Louis Rosen's budget. The budget people tend to cut this item first.

H. Palevsky noted that the ratio of outside Users to the number of institutions is about 2:1, which means that many institutions are represented by only one or two individuals. He raised the question as to whether this represents just a group leader at a university who has other colleagues that aren't members, or whether one or two individuals really represent the total strength of the university.

M. Brussell commented that the nuclear physics community, many of whom are interested in LAMPF, is in a different position than the high-energy community. There are a number of local nuclear physics machines which people have access to or even responsibility for operation. Therefore, it may not be possible for them to commit all their time and students to a project at LAMPF. Perhaps it is a special problem for nuclear physicists to run at LAMPF because of competition of on-site nuclear facilities.

V. Perez-Mendez said he thought that it might be an advantage to be able to run problems either at

an on-site facility or at LAMPF. However, H. Knowles expressed agreement with Brussell and went on to ask whether the AEC could support the salary of research associates, which would be an important point to smaller universities when the staff has heavy teaching commitments. Lind replied that it is a regular practice to ask for funds for research associates or postdoctoral appointments. It is up to the institution to include in their request for funds, say, a year's salary for a research associate. Ploughe cautioned that if the AEC grants a smaller amount of money than requested, then the salary may have to be cut out.

In response to a statement that smaller universities may have to rely on collaboration with bigger places, Palevsky reported that there is a great deal of interest already from the younger people in the small universities in the geographical area who are planning cooperative efforts with Los Alamos. They represent a third kind of community that has to be served. How much can Los Alamos do for such a User in terms of shared facilities or shop time? What about the case where a young man needs something done in the shop that costs \$10,000, or he needs some auxiliary equipment in addition to existing equipment? Questions like these must be understood and discussed before policy can be decided.

Lind added a comment about proposals to the AEC: They should be very definitely tied to some planned experiment, even if it is only preliminary work. The reviewers would find it difficult to support a proposal that is not specific. Rosen added that Los Alamos will welcome collaboration on experiments of mutual interest. This may be the way for some Users to get started in this new field of research. Naturally, the LAMPF people are already committed to certain activities and researches, so it may not be possible to respond to every new idea, but Users are certainly welcome to participate during the summer, or at other times. This possibility should not preclude Users from making proposals to the AEC--in fact it should.

encourage Users to ask the AEC for funds needed to participate in LAMPF activities.

The role of the Associated Western Universities (AWU) in supporting summer activities was raised by Lind. Rosen explained that the funds, which are administered by G. Victor Beard, are not restricted to AWU people. Allocations can be made within the overall purpose, which is basically education. Support of a graduate student, for example, is appropriate.

P. Yergin pointed out that the time demands involved in running at a big facility like LAMPF can be rather large. If you have a local small facility, you may have to alternate your research or give up the local arrangements entirely. Palevsky pointed out that this could create problems: How does the administration of the university react if a faculty member wants to leave for three months of running?

C. Waddell noted that colleagues in other disciplines were critical when physics graduate students did research at other institutions, on a cyclotron only five miles or so away. Moving students even further away from the parent institution to Los Alamos where they are not "visible" may require a selling job. Los Alamos could help by providing talks to nonnuclear physicists at these various institutions and convincing them that nuclear physics is still alive. Rosen replied that LAMPF was always happy to supply speakers to talk to physics departments.

Knowles voiced agreement with Waddell in noting that the general view favored local research over outside research. Palevsky thought that some troubles of this sort could be avoided by a reasonable approach and by having the student give talks on his research. Someone else noted that the graduate student could carry out his program in two phases. First he takes courses and passes exams, then he can leave the institution for a year for research. Palevsky added that this problem has caused some concern at Brookhaven because in some cases the students have gotten lost. Now they are considering what is the obligation of Brookhaven to the graduate students. Yergin said he thought that efforts should be made to shorten the time away from the local institution. Palevsky said that it is done both ways at Brookhaven. Some groups come

with their graduate students as an autonomous unit and spend a minimum amount of time. Others just send the graduate student to join a group at Brookhaven for two years while he does his thesis. The latter case can be bad in that the student loses his connection with the university. Perez-Mendez pointed out that it is good experience to work independently.

F. Schmidt stated that the University of Washington Users Group was functioning. Students and faculty members go off when they have machine time somewhere. It seems to work out all right, as the faculty eventually gets acquainted with the students, the postdoctoral students, and the research associates.

G. Roy felt that effort must be made to convince the university administration, who often like to see graduate students teaching laboratory sessions and grading papers. Another participant pointed out that a graduate student who spent two years in a highly specialized group would miss out on essential preparation for oral examinations.

The session closed with a consensus that a whole spectrum of arrangements are possible and that the role of the graduate student is of great concern to the accelerator facility as well as to his home institution.

TECHNICAL PROGRAM

LAMPF STATUS REPORT

Louis Rosen

Los Alamos Scientific Laboratory

Budgeting Situation

Let me start my LAMPF status report by saying that it appears to me that the stock of LAMPF with the AEC is at an all-time high. I think that the AEC is doing everything they can, within their limitations, to complete this facility and to provide for the Users and for the LAMPF operating budget the funds necessary to mount a respectable experimental program as soon as the accelerator starts operation. As you may remember from our last meeting, it was about a year ago that the first large-scale funds were released to LAMPF by the Bureau of the Budget. At that time, some \$26.5 million were released. That, together with the previous funds, adds up to some \$33.5 million of total funds so far appropriated and allocated by the Bureau of the Budget. As of now, we have a little less than \$3 million of these funds left. Over the past year, we have been obligating funds at a rate of about \$2 million a month. I tell you this to assure you that we have not been idle--but that is not to say that everything is going along as fast as we would like to see it go. Some things are; but others, in particular the design of the large apparatus for the Experimental Area, and, in fact, the design of the secondary beam channels, have had to take a back seat while we have pressed ahead as hard as we could on design, engineering, and construction of the accelerator components.

Responsibilities of LAMPF Builders and Users

I want to transfer to you my very strong feelings that, just because the AEC has chosen to press ahead very vigorously on the LAMPF project, we are handed a very serious obligation. Although no one has said this to me, it would appear that the federal establishment is banking very heavily on LAMPF to help keep intact the foundations of nuclear science in this country during this period of fiscal stringency. This has the effect of intensifying the responsibility of those of us who are building the machine--to get it built on schedule. It

puts a heavy obligation on you, the Users, to develop experiments which can get on the machine and which will capture the imagination, not only of us old-timers, but also of some respectable fraction of the bright, new people who are coming along, because unless we do the latter our discipline is going to wither away. That would have, I think, the most drastic consequences, not only for science, but for the nation as a whole. By virtue of the fact that the AEC is emphasizing this project, we have to assume these very heavy obligations of making it a spectacular success.

Construction Progress: Buildings

Figure 1 summarizes the status of some of the major elements of the brick-and-mortar portions of the project. To minimize costs, all of the copper components of the waveguide accelerator, as well as much of the drift-tube accelerator, are being fabricated in the Equipment Test Laboratory. The modulators will also be assembled on site, for the same reason. The first two phases of the site utilities are in very good shape. The 100-MeV facility, which consists of the injector plus the drift-tube linac, is about 97 percent complete, and we have official occupancy of the building. The buildings which house the 800-MHz waveguide linac facility are progressing very well and are now about 40 percent complete. The 115-kV substation is nearing completion; the Laboratory-Office Building is about 50 percent complete, and we have just started the Operations Building.

Now we come to the most interesting portion of the facility--the Experimental Area. Since we last talked, we made a decision which was somewhat of a gamble. That decision was to proceed with the main experimental area in two phases. Lacking enough funds for all of Experimental Area A, we decided, after much soul-searching, to go ahead and build the shell of that experimental area to provide us with working space and to provide us with a start in the overall complex. It is not a small job: it is a \$3

million contract. D. Cochran, of our Experimental Facilities Group, and the LAMPF Engineering Task Force under P. D. Edwards, worked extremely hard under forced draft and got out the criteria and engineering drawings for the shell of Experimental Area A. It went out to bid, the bids were received, the low bidder was designated, and a letter of intent was written to him with the proviso that he would get the contract as soon as he executed the required Federal Equal Opportunity Employment forms. He delayed about three days, during which time President Nixon's September 1969 order freezing all construction was issued. Then ensued some hectic activities. We tried to get this Experimental Area contract under way, either by an exemption or by recognizing that a letter of intent had been written and the AEC had some responsibility here. None of this was recognized as being a determining factor. The AEC's Comptroller's Office ruled that this building must be built within the guidelines of the President's edict, or would not be built at all. But then, a marvelous, amazing thing happened. Fortunately, and I think wisely, the AEC decided that, from its overall construction projects, it would make funds available for our building within the guidelines of the President's directive. And so that building is going forward. This story has been told in some detail to emphasize how high in priority the AEC obviously holds this enterprise.

Figure 2 is to demonstrate to you that construction is really proceeding; the entire half-mile tunnel has now been completely formed, and we have occupancy of the first third of it, along with the part devoted to the drift-tube accelerator, and, of course, the Injector Building.

The Laboratory-Office Building is shown in Fig. 3. It will be ready by next summer. Please do not believe that we went to excess in beautifying this building. It does have a brick facing, which is a departure from AEC procedure. However, the difference in cost between this brick facing and the "prison-type" exterior is only about \$3,000.

Construction Progress: Accelerator Components

Figure 4 shows one of the first heats in the brazing facility where we are putting together the oxygen-free copper cavities and drift tubes--everything that has to be brazed under highly controlled conditions is being done at Los Alamos. Almost 10

percent of the waveguide accelerator's copper brazing has been completed as of this date.

In that same building, the Equipment Test Laboratory, we have mounted a very strong effort aimed at achieving reliability and long life on our 805-MHz power supplies. Three test stands like the one shown in Fig. 5 are mounted in this building. They operate twenty-four hours a day--untended, unless there is trouble. In spite of considerable previous operating experience on these klystrons and their modulators to drive our Electron Prototype Accelerator, we found that after some 1500 hours of operation these modulators started giving trouble. The probable answer is that, although the design is flawless, the engineering is great, and the gradients are well within tolerance, such a system actually ages and the high voltages start producing low-level discharge almost continuously, and this brings dirt from the walls of the tank into the oil--dirt which just will not come out by the usual filtering means. Contamination builds up in the oil and after 1000 or 1500 hours it starts to break down. As a consequence, the whole modulator had to be redesigned. This is the kind of testing which must be done for all major components to build in the reliability and the lifetime necessary to operate the facility in an efficient and reliable way.

The first Cockcroft-Walton has arrived (Fig. 6). It has been installed and tested successfully at 1 MV by C. R. Emigh. It will be operated at 750 kV and will become the positive ion accelerator for the injector. You may remember that our plans call for three ion sources. The second one, for negative ions, has been ordered. The third one, for polarized ions, has not been ordered, nor will it until we see some indication from the Users that they have experiments on the back burner which will require polarized protons.

Remote-Handling Concept

One of the very crucial parts of the facility is the remote-handling apparatus. Mahlon Wilson has devised a novel approach to this problem. It comprises an iron-walled room--he calls this the Merrimac--weighing several hundred tons, which contains all the remote-handling equipment (Fig. 7). It is essentially a mobile hot cell that is moved to wherever maintenance must be done. The room is lowered over the volume which is to be maintained.

One of the expensive items is the gantry crane on which the room rides. You see there are many very large wheels; this gear alone would cost \$100,000. But a way to get around this expense has been found. B-52 landing gears are apparently just designed for carrying exactly the load we contemplate, or perhaps the reverse is the way it actually happened. In any event, B-52 landing gear is available, without charge, from surplus.

Accelerator Characteristics

The accelerator characteristics are shown in Table I. The latest calculations on the beam are also shown there: they have not changed drastically. It is now believed that something like 90 percent of the beam will be in $\pi/10$ mrad-cm.

Table I

LAMPF Accelerator - Proton Beam	
<u>Beam Energy:</u>	100 to 800 MeV, continuously variable
<u>Beam Quality:</u>	Energy spread, $\pm 0.4\%$ Transverse phase space, $\pi/3$ mrad-cm Macro duty factor, 6% rf microstructure, 0.25 nsec pulses separated by 5 nsec Macropulse length, 500 μ sec Repetition rate, 120 cps
<u>Beam Intensity:</u>	H^+ , 1 mA H^- , 100 μ A polarized, 60-600 nA

Experimental Areas: HRS

The Experimental Area is shown in Fig. 8. As mentioned earlier, the shell of Area A, where the pion and muon targets are, is now under construction. A lot of hard work is being done in design of Area C which will contain the High-Resolution Proton Spectrometer (HRS). There, frankly, we have run into some delay and some difficulty--both are not unassociated with cost. The HRS and the building are going to cost somewhat more than was estimated. But we are committed to this facility, provided that we can retain the budget figures within some reasonable bounds.

A contract has been given to LRL, Berkeley, for their fine engineering staff to engineer the magnets and beam transport for HRS. While waiting for the final design parameters for the HRS, they

are proceeding with the engineering of the magnets for the Stopped Muon Channel. That channel has been completely designed, and the Users Group associated with that channel has agreed on the size of the beams they want and their characteristics and intensities. This fiscal year we have about \$150,000 to commit to this purpose. In addition, we hope to have funds to start purchasing some of the magnets and some of the power supplies for the High-Resolution Spectrometer.

Experimental Areas: Neutron Area

Area B, the neutron area, was originally given low priority for construction. However, a great deal of interest in nucleon-nucleon problems has developed, and within a few months we will have a proposal that will rival the HRS proposal in depth and motivation. Therefore, I have been looking very hard at how we can undertake to construct the building and procure some of the equipment. I should tell you that the House Appropriations Committee has finished their deliberations on the AEC Appropriations Bill, and they have essentially accepted the President's medium-energy physics budget for construction for this fiscal year. Our present budget contains \$5 million for construction; we wanted \$14.5 million. This meant the deferral of \$10 million. That deferral would have cost us \$1 million--it costs about 10 percent a year as a result of inflation on deferred construction. If that deferral had gone through without any corrective action, we would certainly have had to postpone Area B until after the facility came into operation. However, when the AEC heard how badly that area is needed and how many people are desirous of using it, they did a marvelous thing: they told us that if this deferment of \$10 million remains, we can assume we have, not \$55 million, but \$56 million for construction of LAMPF. If they are able to keep their word, we will go ahead with construction of both Area B and Area C, as soon as fiscal year 1971 funds are available. Of course, the first priority will still go to the completion of Area A.

Experimental Areas: National Defense Area

Also shown on Fig. 8 is a future item of construction on the south side of the road that parallels the accelerator. This is a National Defense Facility. It will not come out of Division of

Research funds. If it is built, it will be funded by the Division of Military Applications. It will be quite apart and separated from all of the unclassified facilities and it will not compete in any significant way for beam time. Let me tell you how this comes about so you have no worries on this point. The aspects of this defense facility have to do with providing short bursts of neutrons of very high intensity. We have found that such bursts can be provided by simply chopping and deflecting perhaps one percent of the 500- μ sec pulse at the beginning or the end of that pulse. This would leave 95 percent or better of the beam for its basic purposes. This was one of our main criteria for favoring a separate facility for the neutron cross section and neutron-effects people. The original concept is still valid. When the Joint Committee of Atomic Energy held hearings on LAMPF, they wanted to know specifically whether some beam time would be available for defense work. Of course, it's a national facility; the answer must be "yes." The answer was qualified by setting at 10 percent the maximum accelerator time to be dedicated to such work. They agreed to this, and it is still valid. Now it appears that, with the facility so segregated from the other work, they can have as much time as they want without interfering with other uses of the accelerator. Let me emphasize that no funds are now available for the defense facility, and I do not know when and if funds will become available. We do, however, have an obligation to respond if and when funds become available.

Biomedical Area

A second facility, which I personally feel very strongly about and which we don't yet have funds for either, is the Radiobiology and Therapeutic Research Building. I am personally quite convinced that the π^- beams that we can produce will have a better chance to make a qualitative change in the treatment of cancer than any radiation-therapy technique now on the horizon. I think it would be the greatest shame if we were not supported in elucidating, evaluating, and doing preclinical work with these meson beams. Again, this program would be on a noninterference basis. The beam will penetrate all the meson-producing targets in Area A; it will then be reconstituted, refocussed, and taken to the beam stop. On the way to the beam stop, another target

will be inserted to produce the pions for this biomedical facility. I must be candid with you and tell you that there is some opposition to this facility. The opposition comes about because there are conflicting views as to how the funds available to the United States as a whole should be best spent in trying to devise better treatments using radiotherapy. Some people believe that heavy ions should be pursued because they are less costly and they have certain properties which are highly advantageous. Others believe that neutrons are the answer. Let me tell you frankly that, in my opinion, neither of these techniques can hold a candle to the potential of π^- beams. In the case of heavy ions, the reason is that their total cross sections for interactions with cell nuclei are so great that the depth-dose distribution becomes very unfavorable. For neutrons, the reason is that it is very hard to form neutron beams to the desirable shapes and distributions for a given volume, and the depth-dose distribution is also unfavorable. Neither of these problems are present with π^- . I do not believe that it will be possible for the federal government to long delay building a facility for trying this modality which so many people are now convinced is the most promising. Part of the reason for delay is that for quite some time there was not much interest from the people that would use this facility. Now, however, I am pleased to report that both the Biomedical Users Group of this organization, the local University of New Mexico Medical School people, the University of Colorado Medical School people, the Radiological Society of Colorado, and radiotherapists throughout the country are showing a great deal of interest in the potential of intense beams of negative pions. Medical school faculty members at the University of New Mexico have taken the lead in organizing a local group to sponsor the planning of a preclinical program and of studying methods of channeling patients in a reasonable, orderly manner into such a facility once that facility comes into being. To assure these people that their work will not be in vain, I have ventured to tell them that, even if this facility is delayed a year or two or whatever, we will make available to them π^- beams in our main area. In this way they can start doing fundamental radiobiology, animal work, and even preclinical work on patients as soon as we have usable

beams. I am sure that all of you will agree that the sacrifice, because of the inconvenience, is well worth it if we can show that a major accelerator can be immediately useful for practical application. I think this will be a great thing for the AEC. It would take the Division of Biology and Medicine up many, many steps on their ladder; they have had difficulties obtaining construction funds in the last few years, but this is a real opportunity for them to do something quite dramatic.

Figures 9 and 10 are expanded views of the overall Experimental Area and Area B, respectively. The latter is discussed in detail by J. Simmons in another paper in these Proceedings.

Pion and Muon Channels

As mentioned earlier, the design of the Stopped Muon Channel has been settled. It would be very good if we could also settle on the first pion channel soon. Several channels are under consideration. One of these, a low-energy, moderate-resolution channel is described in Table II and shown in Fig. 11. It is a four-bend channel and is the one which I would hope would be used by the biomedical people if we cannot have their channel in good time. Another is a high-resolution channel shown in Fig. 12. It is designed to have a spectrometer on the end of it and has been given quite a lot of exposure the last few days at a Users Group working meeting on pion spectroscopy.

Table II

Low-Energy Pion Channel*			
π^+ Flux:	$\Delta p/p = \pm 0.1\%$	$\pm 1.0\%$	$\pm 5.0\%$
$T_\pi = 50$ MeV:	$1.5 \times 10^7/\text{sec}$	0.6×10^9	1.5×10^9
100 MeV:	3×10^7	1.2×10^9	3.5×10^9
150 MeV:	2×10^7	0.8×10^9	2×10^9
<u>Solid Angle:</u>	5 msr	20 msr	10 msr
<u>Beam Spot:</u>			
cm:	1.5×4	3×4	6×8
mm:	30×80	60×160	80×120
<u>Length:</u>	14.4 m		
<u>Production Target:</u>	0.5 x 2 x 3.5 cm Cu target, 50-MeV thick		
<u>Production Angle:</u>	60°		
<u>Proton Beam:</u>	1 mA		

* R. Burman and M. Jakobson, MP-6/RB, MJ-1, LASL

Table III gives the characteristics of a High-Energy Pion Channel. Peter Gram will have some detailed remarks to make about this channel at the Pion Physics Session. I believe our budget will permit construction of at least two and possibly three pion channels by the time we have a beam in the main experimental area.

Table III

High-Energy Pion Channel*		
π^+ Flux:	$\Delta p/p = \pm 0.5\%$	$\pm 5.0\%$
$T_\pi = 550$ MeV:	4×10^6	7×10^7
400 MeV:	9×10^7	2×10^9
200 MeV:	2×10^8	3×10^9
<u>Solid Angle:</u>	1.6 msr	3 msr
<u>Beam Spot:</u>	1.6×1.5 cm	13×7 cm
	40×20 mrad	75×24 mrad
<u>Length:</u>	21 m	
<u>Production Target:</u>	0.5 x 2 x 3.5 cm Cu target, 50-MeV thick	
<u>Production Angle:</u>	10°	
<u>Proton Beam:</u>	1 mA	

* P. Gram, MP-6, LASL

The final version of the Stopped Muon Channel is shown in Fig. 13. Since it is a very expensive channel, we are going to leave out a good portion of it initially; I think it can be shown that the straight section only adds a factor of 2 or 3 to intensity. Eventually one wants it; but since so many demands have been made on our funds, I have proposed that the magnets in this straight portion be initially deleted. As soon as adequate funds become available, I will recommend that the entire channel become functional.

The expected performance of the muon channel is given in Table IV. The intensities, of course, are very impressive; as is the purity of the beam shown in Table V. To my way of thinking, as I look at the experiments that people want to do with that channel, the purity of the beam is almost as important as the high intensity.

Table IV
Expected Performance*

Momentum Range: 0 to 250 MeV/c

Muon Flux:

Central Momentum 90 MeV/c

From forward-decay muons, 0.7×10^7 (sec MeV/c)⁻¹ at 115 MeV/c
(near maximum of the pion-free muon spectrum)

From backward-decay muons, 0.6×10^7 (sec MeV/c)⁻¹ at 50 MeV/c
(lower maximum of the muon spectrum, pion-free)

Central momentum 180 MeV/c

From backward-decay muons, 7×10^7 (sec MeV/c)⁻¹ at 110 MeV/c
(lower maximum of the muon spectrum, pion-free)

Production Target:

I = 6.3×10^{15} protons/sec T = 750 MeV

$\frac{\partial^2 \sigma}{\partial \Omega \partial T} = 53 \text{ } \mu\text{b/MeV-sr}$

50 MeV loss in Cu

Muon Output Emittance: 1 cm-rad to 2 cm-rad

Muon Polarization P: Forward-decay muons $0.9 < P < 1.0$
Backward-decay muons $0.5 < P < 0.8$

Output Beams: Two separate beams with angular separation of 50°

* V. W. Hughes, S. Ohnuma, K. Tanabe, H. Vogel, "Stopped Muon Channel, Chapter V," July 2, 1969, MP-Division Internal Report.

Table V
Background particles in muon beam*

Type	Ratio: Background/ μ^+ (upper limit)
<u>Positive muon beam</u>	
π^+	0 (From beam optics): 0.05 (estimate with scattering)
p	0 (From beam optics): 0.01 (estimate with scattering)
e^+	10^{-4} (From beam optics); 0.004 (estimate with scattering)
n	10^{-5}
γ	10^{-6}
<u>Negative muon beam</u>	
π^-	0 (From beam optics): 0.05 (estimate with scattering)
e^-	10^{-4} (From beam optics); 0.01 (estimate with scattering)
n	10^{-6}
γ	10^{-7}

* V. W. Hughes, S. Ohnuma, K. Tanabe, H. Vogel: "Stopped Muon Channel, Chapter V," July 2, 1969, MP-Division Internal Report.

On-Line Isotope Separator

Another future item is shown in Fig. 14. This is an on-line mass separator. It will be built by the radiochemists, and probably will have to be funded by the Radiochemistry Group at Los Alamos, as we do not have Division of Research funds for this installation. It is very much wanted by the radiochemistry community because, with this on-line mass separator, one can look at many new short-lived nuclides and measure their properties. B. Dropesky and his colleagues have done an enormous amount of planning on how best to build this in the most economical way.

Construction Schedule

Our schedule, as we now see it in the light of the funds available to us, is shown in Fig. 15. Since we only have \$5 million in construction funds for this fiscal year, it will not be possible to start any additional experimental areas--nor even to finish Experimental Area A. This is not quite as bad as it seems because had we gotten an additional \$10 million, the President's freeze order would apply, and we could have spent no more than

one-quarter of it. So I am not quite as sad as I would be otherwise, with this \$10 million being deferred to next fiscal year. However, the deferment will mean that Areas C and B will come on rather later than I had hoped, perhaps not so late, though, that one could not start experiments within six months of when we would have been ready to start experiments in the best of circumstances. The figure shows the various major systems and the scheduled completion dates. The latest is the waveguide accelerator structure. Completion and check-out of that is May 1972.

We still hope that in July 1972 we are going to see the first beam from this accelerator. Let me say that with the \$5 million that we expect this fiscal year, we will have all the funds necessary to complete the accelerator; so construction can go ahead--as it has been now for a year--full tilt, without any hindrance from fund limitations. That is the first time I have ever been able to make that statement. It is a very pleasant statement to be able to make.

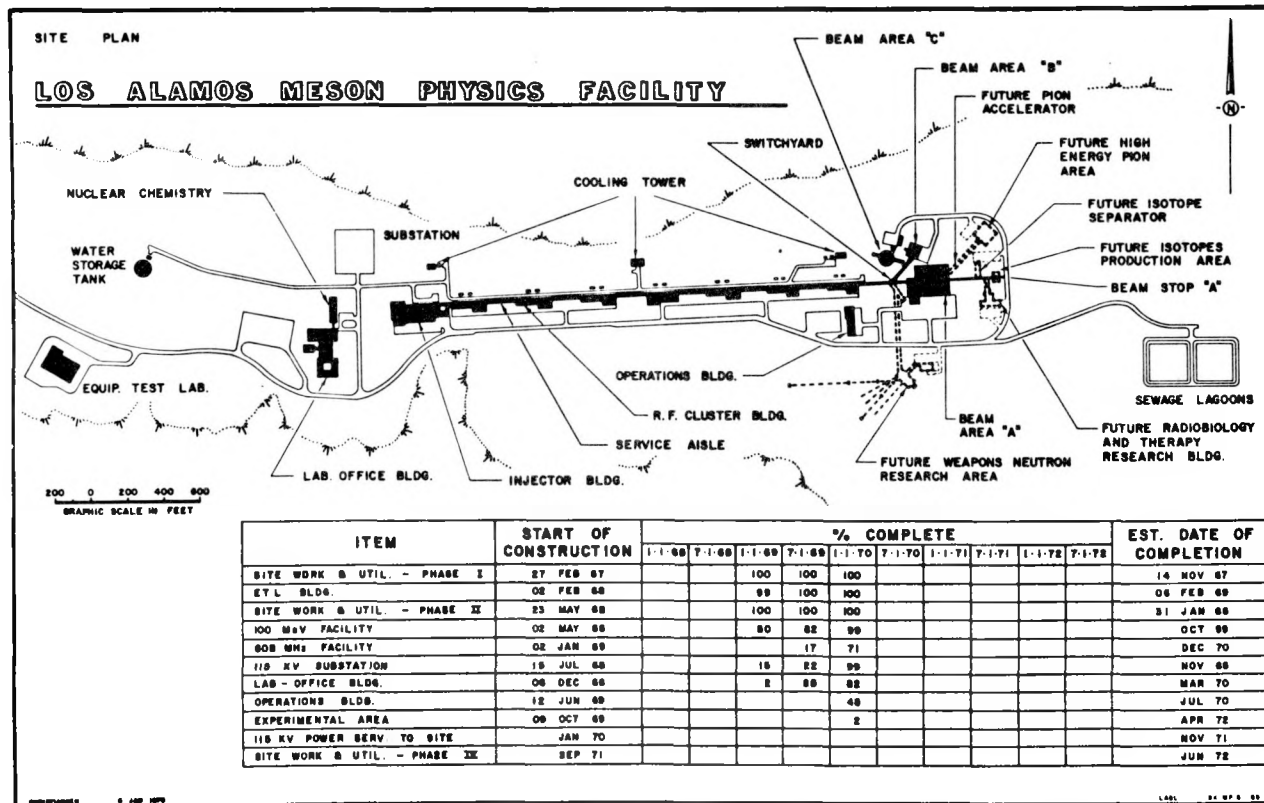


Fig. 1. Overall layout of LAMPF with construction status and estimated completion dates.

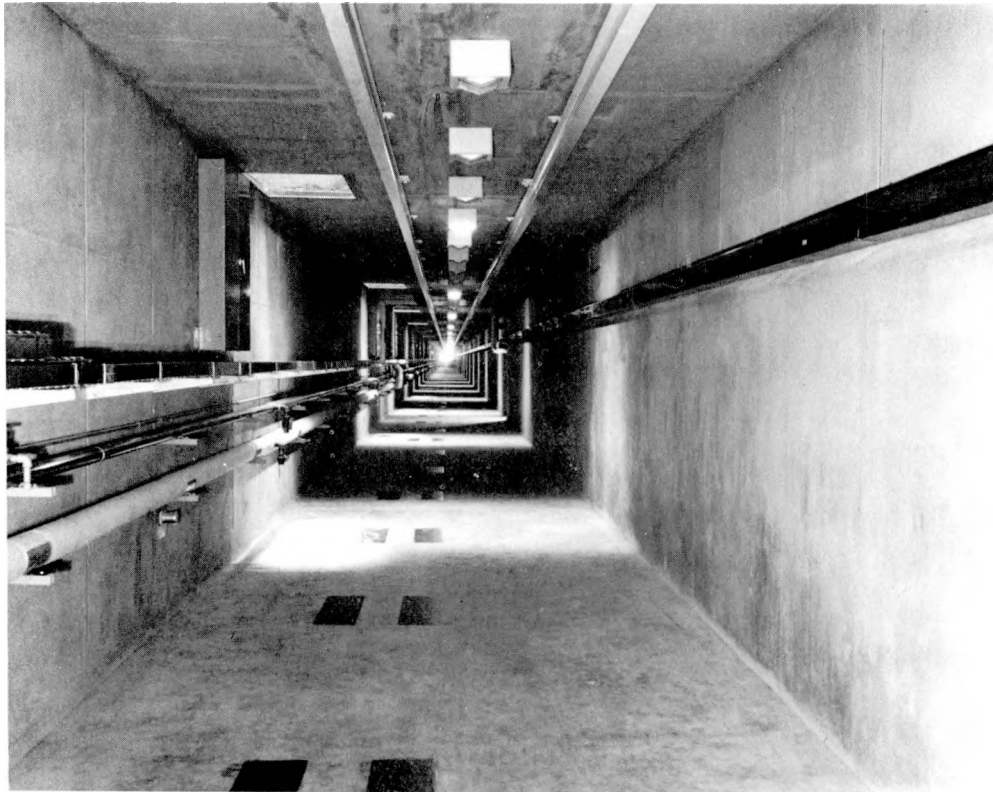


Fig. 2. Interior view of beam channel as of late fall of 1969.

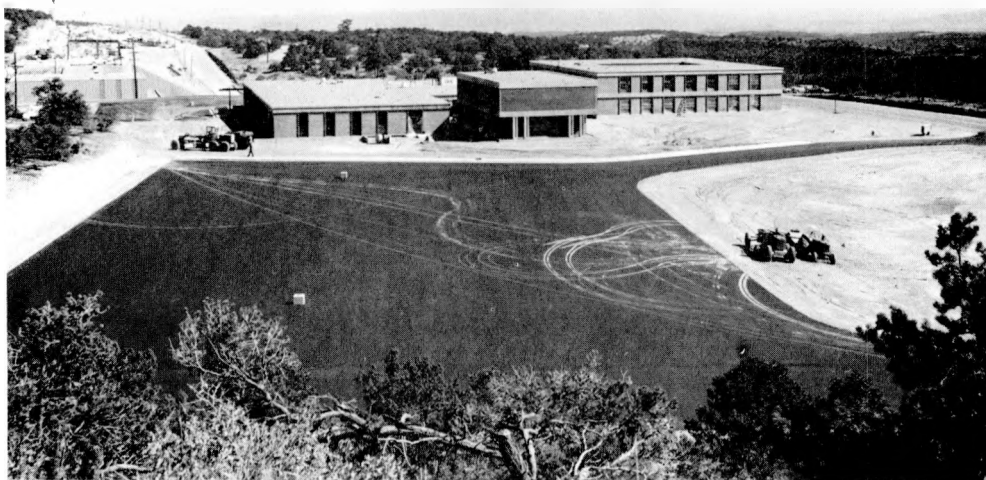


Fig. 3. Laboratory-Office Building at the LAMPF site.

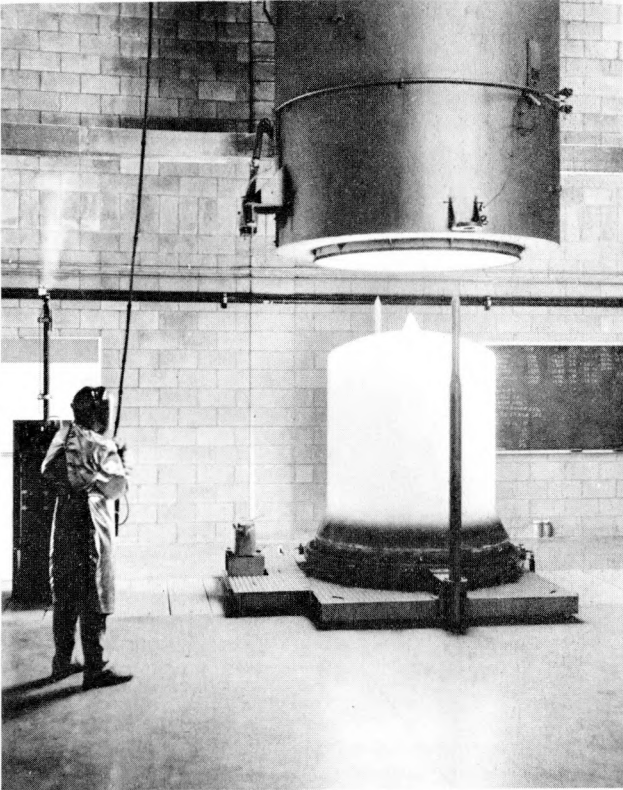


Fig. 4. Brazing facility showing furnace with outer lid raised.

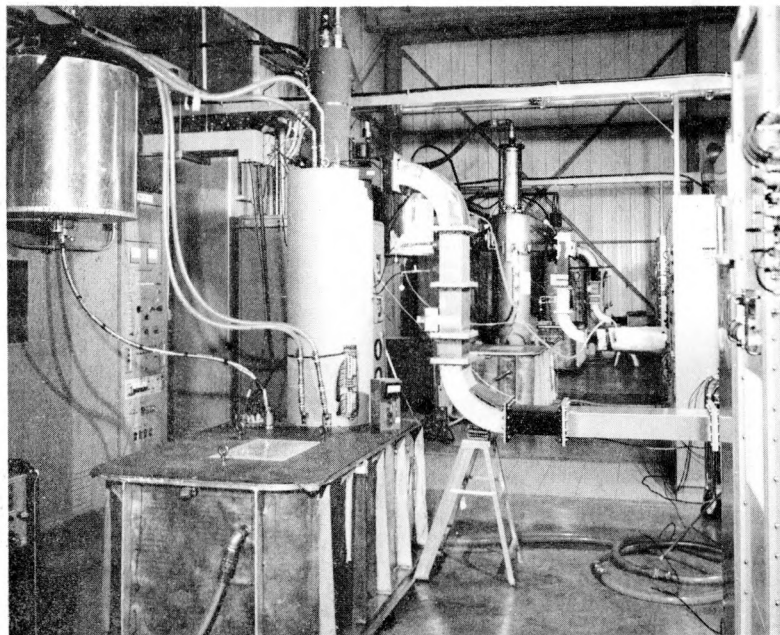


Fig. 5. Klystron power supply on test stand in Equipment Test Laboratory.

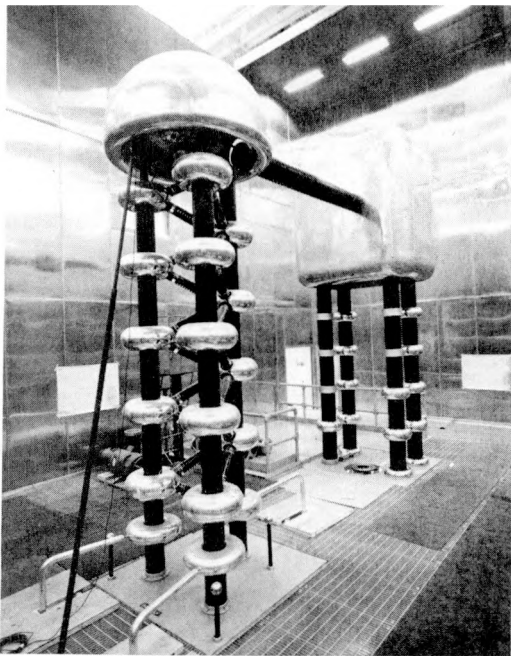


Fig. 6. First Cockcroft-Walton installed in Injector Building.

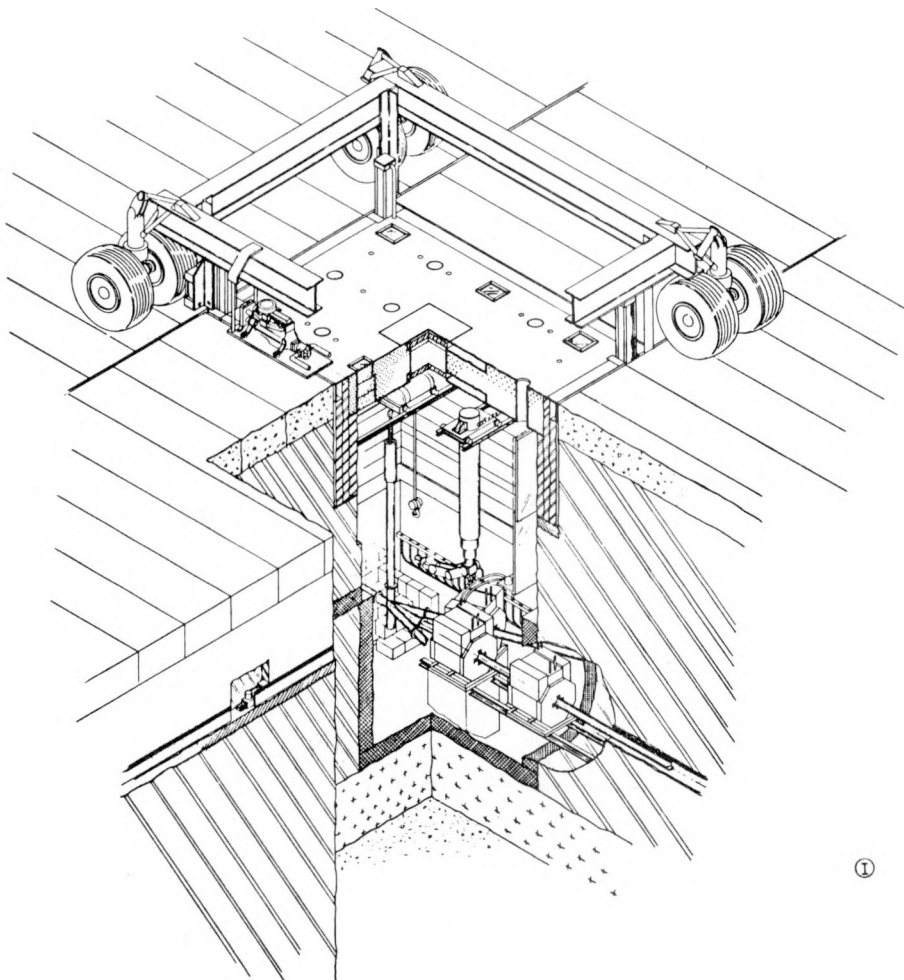


Fig. 7. Schematic of Merrimac in place over target area.

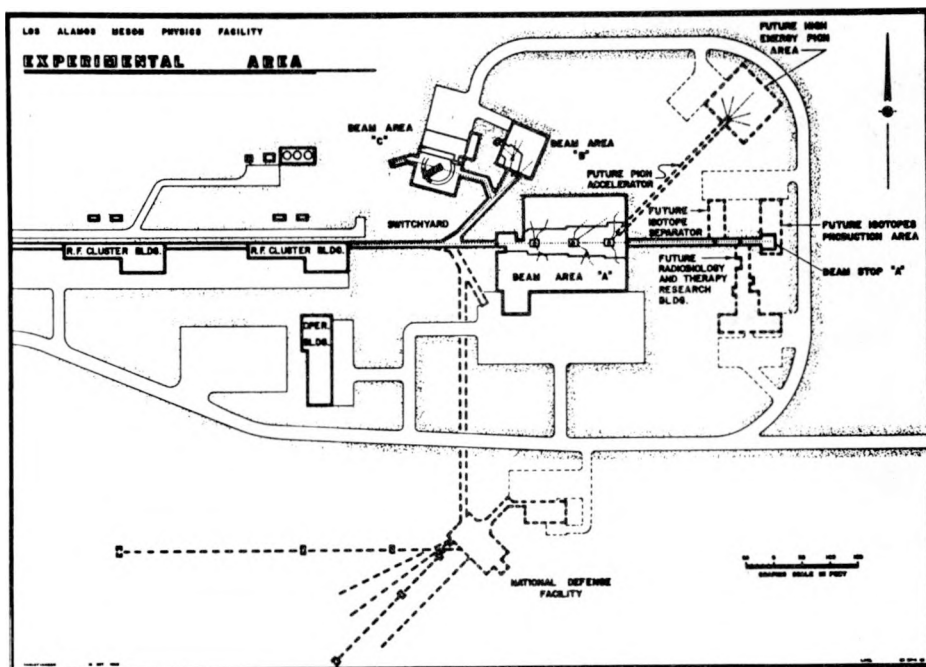


Fig. 8. Overall plan of LAMPF Experimental Area.

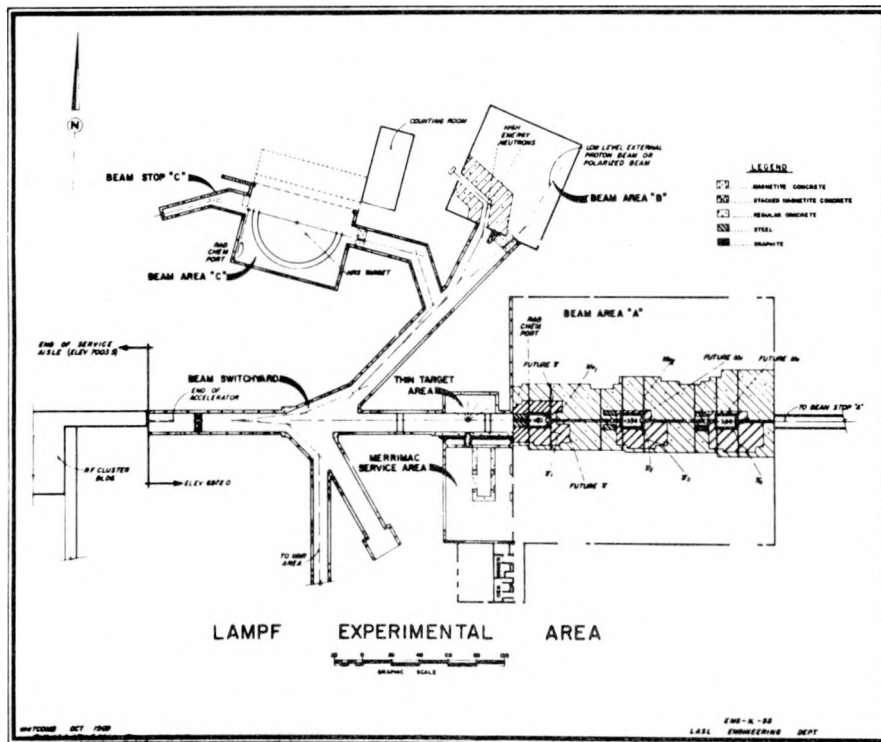


Fig. 9. Expanded view of LAMPF Experimental Area

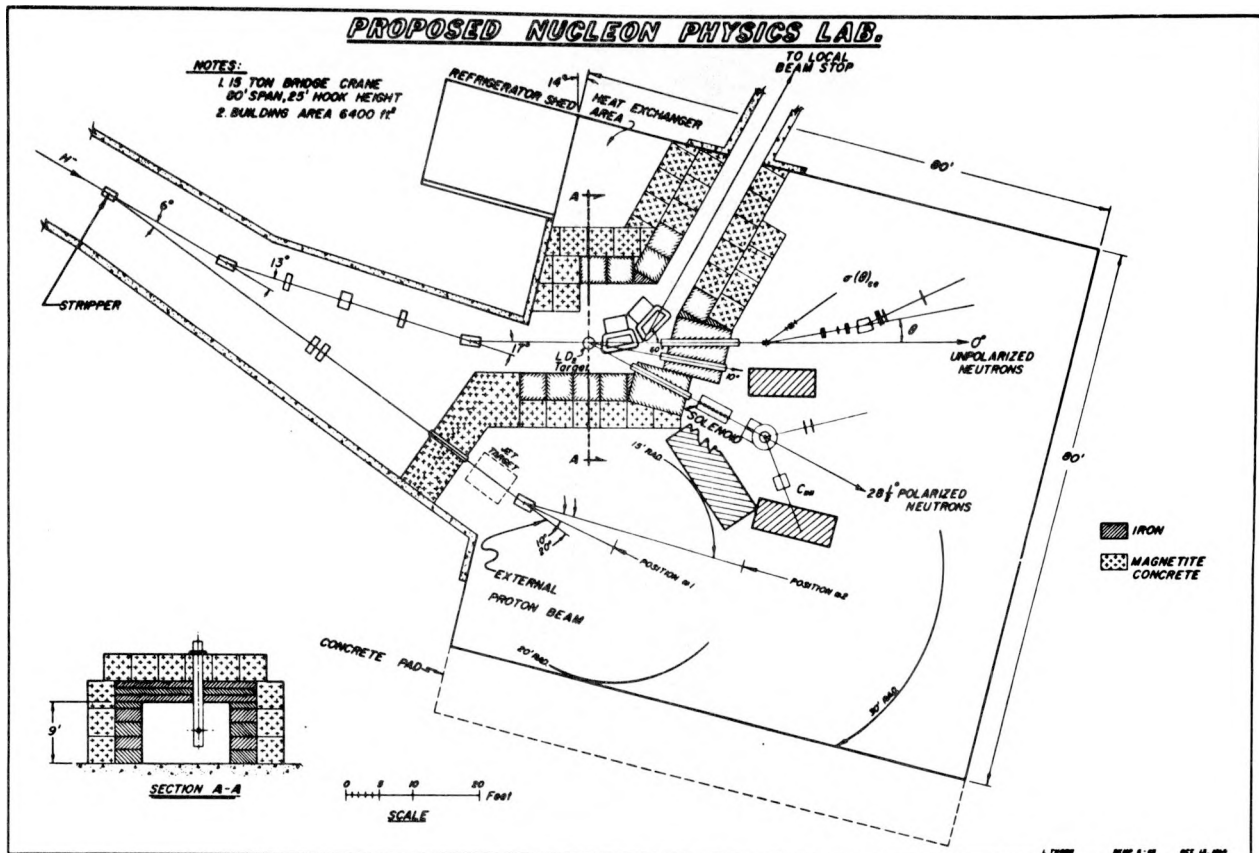


Fig. 10. Experimental Area B, the Nucleon Physics Laboratory.

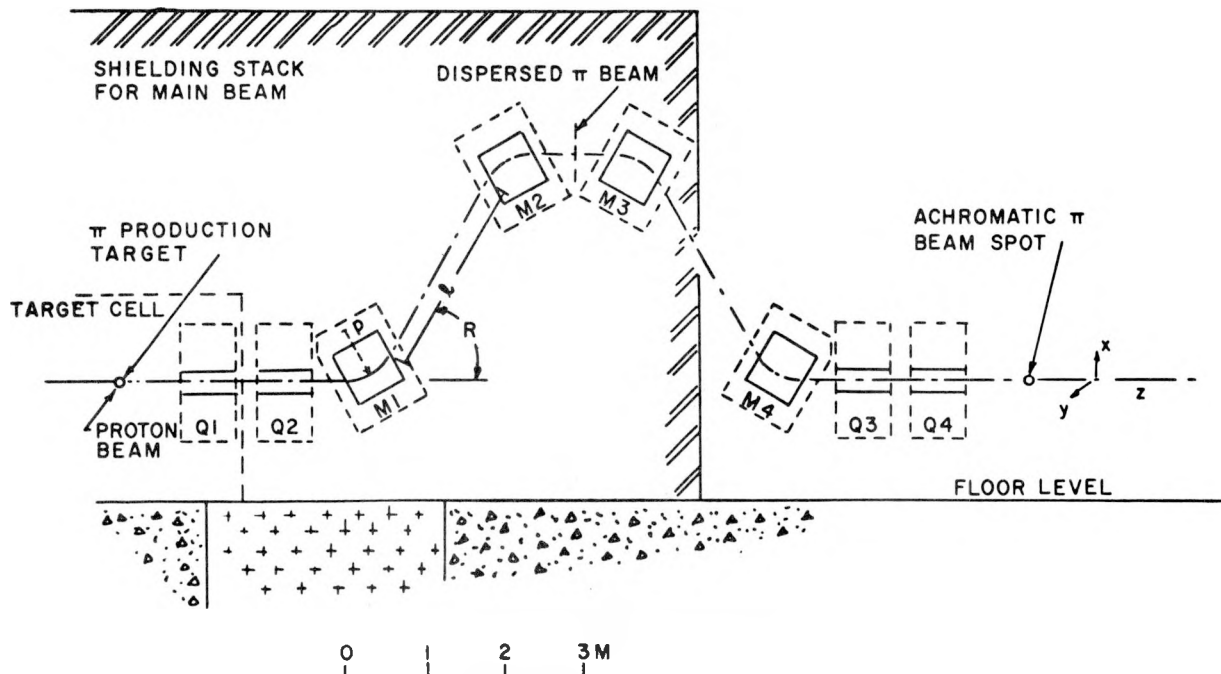


Fig. 11. Low-energy moderate-resolution channel.

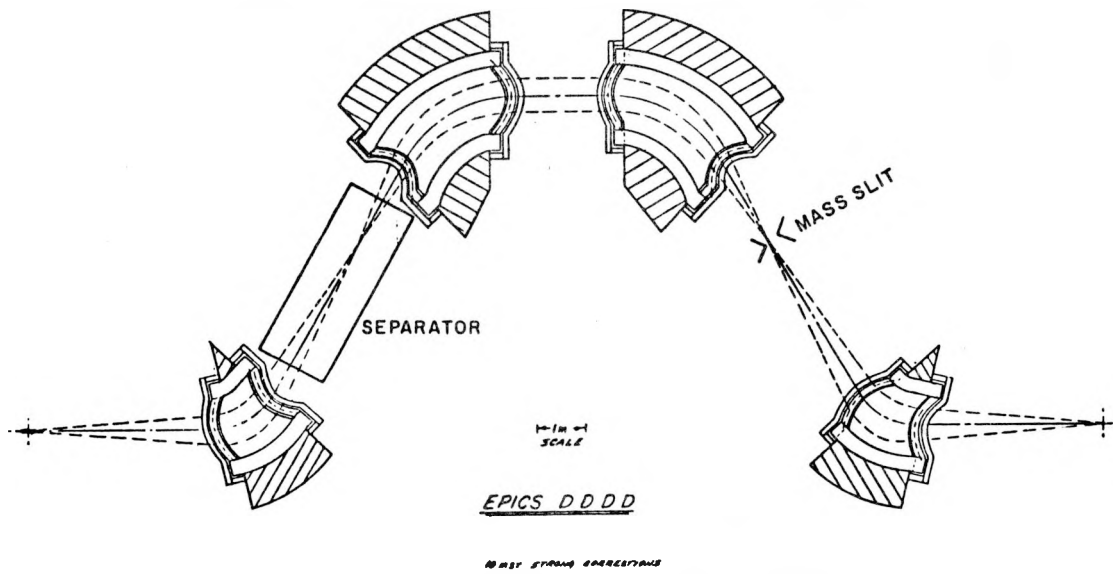


Fig. 12. High-Resolution Pion Channel (EPICS).

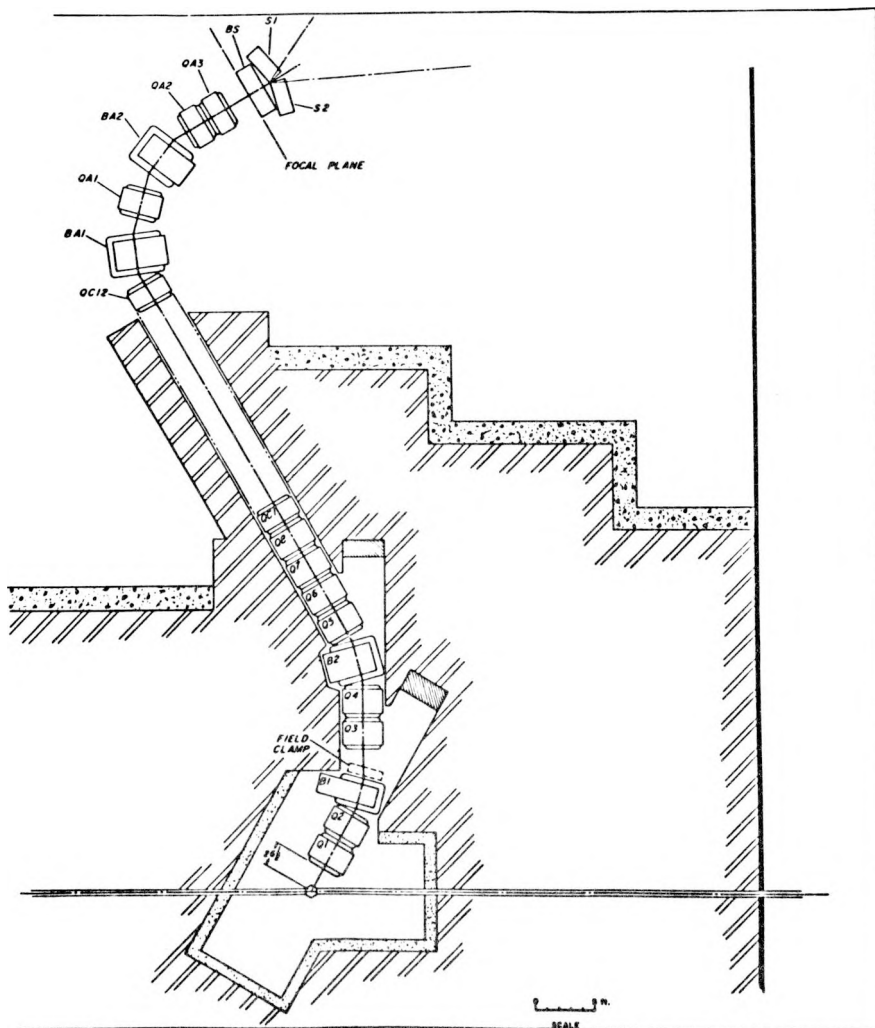


Fig. 13. Stopped Muon Channel

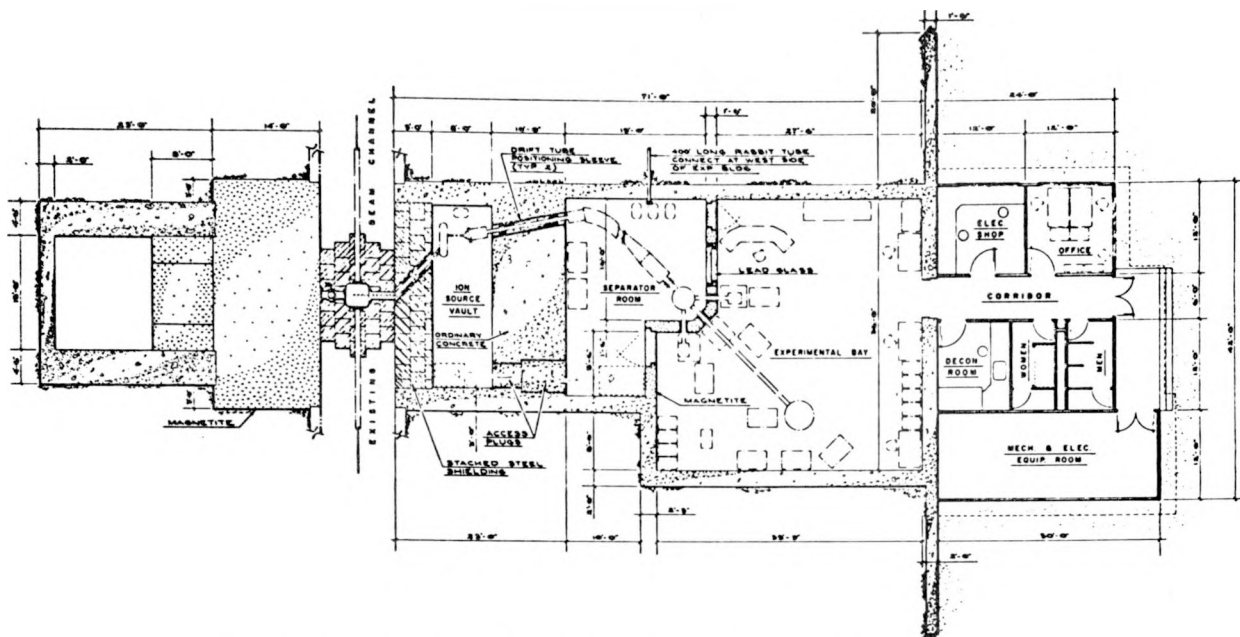


Fig. 14. On-line mass separator facility floor plan

LAMPF ACCELERATOR SUMMARY

ITEM	SYSTEM	SCHEDULE	
		START DATE FOR FABRICATION / PROCUREMENT	COMPLETION DATE FOR CHECKOUT
1	INJECTOR SYSTEM	A 16 JAN 67	DEC 69
2	DRIFT TUBE ACCELERATOR STRUCTURES	A 01 NOV 67	JUN 71
3	DRIFT TUBE R.F. POWER SYSTEMS	A 18 OCT 68	APR 70
4	WAVE GUIDE ACCELERATOR STRUCTURES	A 30 AUG 68	MAY 72
5	WAVE GUIDE R.F. POWER SYSTEMS	A 05 NOV 68	AUG 71
6	INSTRUMENTATION & CONTROL SYSTEM	A 02 DEC 68	MAR 72
7	BEAM SWITCHYARD	DEC 69	APR 72
8	BEAM AREA EQUIPMENT	DEC 69	APR 72

Fig. 15. LAMPF accelerator summary showing schedules for fabrication or procurement and completion.

THE NUCLEON-NUCLEON PROBLEM AT LAMPF

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I. INTRODUCTION

In 1964 two chapters were included in the LAMPF proposal¹ concerning the need for work on the nucleon-nucleon problem. This field has remained active since that time in regard to both phenomenological data analysis and experimental work. The Yale group² has provided leadership during the past decade in the development of energy-dependent phase parameters for energies up to 350 MeV. The Livermore group³ has likewise been active, having recently completed Paper X of a series of phase-shift analyses which concludes work of the past 5 years. Corresponding activity has occurred at Dubna,⁴ East Lansing,⁵ and to a lesser extent at Harwell⁶ and Kyoto.⁷ Two of the Dubna analyses were labeled as "unambiguous"--which indicates either difficulty in translation or that the people over there are very optimistic. On the purely experimental side, about 30 percent of the publications on nucleon-nucleon data between 10 and 800 MeV have appeared since 1964. Also, during this time, the one-boson exchange potentials have been refined to the point where they qualitatively describe the elastic interaction in an economical and realistic manner.

The question is: Where do we stand? What are we planning? In the first part of this paper, I will review the situation in a summary manner, looking first at the body of existing data, and then I will review our knowledge of the low-order phase shifts. The last part of the talk will concern our planning for a nucleon-nucleon experimental facility at LAMPF.

II. COMPARISON OF THE NUCLEON-NUCLEON DATA SETS

I would like to begin with a brief numerical comparison of the p-p and n-p data sets in the energy range of 300-800 MeV. Below this energy, it is safe to say that the p-p situation is in reasonably good shape. The n-p data is not in very good shape at any energy, except possibly at 200 MeV.

In Table I, we present a summary of data points collected for the p-p and n-p interaction over the

range 300-800 MeV. The data are summed over energy. The top row lists the observables: σ_T is the total cross section; σ_R is the reaction cross section for π production; $\sigma(\theta)$ is the differential cross section; P is the polarization function; next comes the 5 spin-change parameters, known as triple-scattering parameters, D, R, A, R', A': and finally, two of the spin-correlation parameters, C_{NN} and C_{KP} . The second row gives total data points for the p-p case, while the third row gives the corresponding figures for the n-p case.

Table I
Nucleon-Nucleon Data Summary, 300-800 MeV*

	σ_T	σ_R	$\sigma(\theta)$	P	D	R	A	R'	A'	C_{NN}	C_{KP}
p-p	8	204	298	25	23	17	2	9	110	9	
n-p	7	2	120	89	11	8	6				
Total p-p - 705											
Total n-p - 242											

* Compiled from UCRL-70075 and UCRL-50426 (1968) and updated to October 1969.

Comments on Table I:

- σ_T : p-p data are omitted as they are mostly higher energy work. For n-p only 7 data points are counted, many of which are old and of low relative accuracy (~3-5 percent).
- σ_R : One or 2 n-p data points exist, compared to 8 p-p data.
- $\sigma(\theta)$: Twice as many p-p as n-p data are available, in spite of the fact that the n-p observable covers twice the angular range.
- P(θ): There are three times as many p-p data as n-p data. Again, the n-p case covers twice the angular range.
- Spin-change parameters D, R, A, R', A': There are three times as many p-p data as n-p data, in each case concentrated at

300, 400, and 600 MeV. Both sets of data are poor at the higher energies.

- f. Spin-correlation parameters, C_{NN} and C_{KP} : Most of the p-p, C_{NN} data result from much activity in the last 5 years with polarized targets. Nevertheless, clarification is required between 400 and 800 MeV for the trend of the C_{NN} values at 90° . There are as yet no n-p data at these energies.

The totals are 705 accumulated data points for the p-p system and 242 lower weight data for the n-p system. This comparison tells us that more n-p data are required if we expect to have comparable information on the $T = 0$ and $T = 1$ parts of the interaction. More data on the p-p interaction is also required if we expect to obtain a satisfying description of the nucleon-nucleon system from the onset of meson production up to 800 MeV.

III. PHASE SHIFT ANALYSES

To provide a summary impression of the N-N situation, data are presented in Figs. 1-5 that illustrate progress in our empirical knowledge of the low-order phase shifts. The analysis groups of interest here are the following:

- a. The Yale group,² which has done energy-dependent analysis up to 350 MeV.
- b. The LRL group,³ which has done single-energy and energy-dependent analysis up to 750 MeV.
- c. The Dubna group,⁴ which has done single-energy analysis up to 600 MeV.
- d. The East Lansing group has done considerable work, of which a combined n-p and p-p analysis⁵ at 200 MeV is noteworthy.
- e. The Kyoto group⁷ has done work near 970 MeV and higher.

In Fig. 1, the p-p, $^1S_0$ phase shift as a function of energy is shown. The solid line represents the LRL energy-dependent analysis (EDA); the Yale fit overlaps the LRL curve. The open circles are several LRL single-energy analyses (SEA). The square at 400 MeV is an analysis published this year by the Dubna group, which claims to be an unambiguous solution. At 600 MeV, several Dubna solutions of last year are shown by upside-down triangles. At 970 MeV, the Kyoto analysis, based on fewer data, confirms the trend. Thus, the $T = 1$, S-wave phase shift appears to be reasonably well

known up to 400 MeV. At 600 MeV, a definite spread of values is evident. It has been known for some time that near 200 MeV the interaction in this state becomes repulsive, as evidenced by the crossover to negative values.

In Fig. 2, the p-p, $^3P_1$ phase shift versus energy is shown. This is an example of the triplet P-wave phase shifts, which exhibit very reasonable behavior up to 400 MeV. However, considerable spread is evident at 600 MeV. It is noted that at 600 MeV, the Dubna analysis turned up with 8 identifiable solutions. Four were eliminated by probability considerations. They gave 3 or 4 as the final number of phase-shift sets. Three are shown in this series of slides.

Figure 3 shows the $T = 0$, $^3S_1$ phase shift versus energy. Two EDA curves from LRL, one solid up to 750 MeV and one dashed up to 400 MeV, are plotted. At 200 MeV, LRL, Yale, and East Lansing all agree. At 400 MeV, the open LRL circle at -25° represents a definite downward revision of earlier analyses. This occurred as a result of inclusion of recent Chicago p-n triple-scattering data. This result implies that the $^3S_1$ parameter goes negative near 300 MeV with fairly steep slope. At 600 MeV, there is a spread of SEA values from Dubna and Livermore between 0° and -25° , or so. A rather confusing picture is presented by the 400-MeV values taken together with the values at 600 MeV.

The $T = 0$, $^1P_1$ phase shift versus energy is shown in Fig. 4. Looking first at 600 MeV, it is seen that the recent Dubna results have considerable spread among themselves, and with respect to LRL. At 400 MeV, there is considerable spread between the LRL point at -25° and the unambiguous Dubna value. The earlier Yale EDA curve has a definite negative slope, and would tend to agree with the Dubna value. At 200 MeV, the agreement is fair.

Let us now consider the variation of the ϵ_1 phase parameter as a function of energy (Fig. 5). Shown here is the $T = 0$, S-D coupling parameter, representative of the tensor force. The situation is unclear at all energies but one: At 200 MeV, the Michigan group claims a firm value, which also agrees with Yale (dashed). At 400 MeV, the revised LRL value (circle) shows substantial deviation with the Dubna point (square). At 600 MeV, the single-energy analysis gives a generous spread of positive

values, contradicting the negative trend of the LRL EDA curve.

In summary, the following comments can be made with regard to the present status of the N-N phase shifts:

- a. The p-p phases are reasonably well known up to 400 MeV. At 600 MeV, the knowledge is qualitative at best, being complicated by 30 percent meson production in $T = 1$ states.
- b. It seems evident that the $T = 0$ phases are not tied down at any energy above 200 MeV (and not too well below 200 MeV). In analyses made so far, the $T = 0$ phases are dependent on the $T = 1$ phases and the n-p data set.
- c. At 600 MeV, about 12 percent of the $T = 0$ states result in meson production. The analyses to date have assumed purely elastic phase shifts, to avoid any additional complication. Sooner or later the $T = 0$ phase shifts will have to reflect this inelasticity.

Understanding the nucleon-nucleon problem in the presence of meson production--from threshold to 800 MeV--is exactly the challenge of the situation. Achieving this objective will require a much improved and enlarged data set, and advances in the method of analysis.

IV. CHARACTERISTICS OF LAMPF FOR THE N-N PROBLEM

Before proceeding, it is worth while to enumerate those characteristics of LAMPF which will be important to the nucleon-nucleon problem.

- a. Beam Sharing: The basic philosophy of beam sharing of plus and minus beams makes it advantageous to have different and separate experimental programs going on simultaneously.
- b. Energy Variability: Very important with respect to energy dependence of the N-N interaction.
- c. Low Emittance Beam: Important for small-angle measurements.
- d. Polarized Proton Beam: Very important to p-p and p-n spin-type measurements.
- e. Neutron Production: The intensity and quality of external proton beams will allow large improvements in neutron produc-

tion.

- f. Micropulse Structure: Improved quality of timing information for neutron detection, assuming suitable modifications in micro-structure can be made.

One of the great experimental difficulties in previous n-p work has been lack of intense, high-quality neutron beams at medium energies. The Dubna workers have invested large effort with neutron beams of 600-MeV energy; however, they are seldom able to obtain usable flux greater than $2 \times 10^5 \text{ cm}^{-2} \text{ -sec}^{-1}$, with energy spreads of 90 MeV. They never used a polarized neutron beam. I would like to consider briefly at this point the production of neutron beams by the $p + d \rightarrow n + 2p$ process. This process appears highly favorable at LAMPF in regard to generation of monoenergetic neutrons at 0° and polarized neutrons at larger angles.

The advantages of the $D(p,n)2p$ reaction are listed below:

- a. At 0° the width of the neutron peak is narrowed by the final-state singlet p-p interaction. Calculations by Phillips⁸ indicate a ΔE of about 1.5 MeV FWHM.
- b. The 0° cross section is reasonably high: 16 mb/sr at 700 MeV.⁹
- c. At 28° lab, polarized neutrons are produced by the quasi-elastic scattering. A polarization of 35 percent has been measured at an incident proton energy of 700 MeV at Berkeley.¹⁰
- d. Flux normalization⁹ is possible by the sequential charge symmetric process $D(p,n)$, $D(n,p)$. By this means, the absolute cross section may be obtained and flux calculated.

It is interesting to note that Gluckstern and Bethe¹¹ first suggested the use of liquid deuterium for production of neutron beams in 1951. They were concerned with theoretical understanding of the nucleon-deuteron interaction. Certain of their results showed clearly the narrowing of the outgoing neutron spectrum, well in advance of experimental data. Figure 6 represents experimental results on the shape of the 0° neutron spectrum as given by the 1967 Harwell measurements.¹² The narrow dot-dash curve in the center is a theoretical peak shape of 1.5 MeV FWHM. The solid curve passing through the

data points is the theoretical one broadened by the resolution function. The measurement is compatible with a peak width of a few MeV.

The 0° neutron flux from this reaction is estimated to be $\approx 1.7 \times 10^7 \text{ cm}^{-2} \text{ -sec}^{-1}$ for 750-MeV neutrons at 6 m from the target. The following parameters were used: incident beam intensity 84 μA at 750 MeV; liquid deuterium target 15-cm long; heat input to the LD_2 cell 500 W; neutron energy spread ≈ 6 MeV FWHM. The beam heating of a practical target would be removed by a suitable LD_2 circulation system and heat exchanger.

V. THE PROPOSED EXPERIMENTAL AREA

During the past year, two meetings have been held for the purpose of planning future nucleon-nucleon work at LAMPP. The last of these was held during the week of August 4, 1969, with gratifying response. As a result, we have been able to consolidate our ideas about the size and function of a suitable experimental area, which we call the Nucleon Physics Laboratory (NPL). Later at this meeting, we will consider progress on a proposal on this subject. The production and use of neutron beams for n-p studies plays an important role in these plans. Also important is the availability of polarized and unpolarized proton beams for p-p and p-n measurements. Both the neutron and proton parts of the problem must advance together.

The experimental program is expected to include all aspects of the interaction between two nucleons, namely:

- Elastic scattering with emphasis on spin measurements.
- Meson production.
- Bremsstrahlung.
- Invariance tests: time reversal, charge symmetry, and long-range charge independence.

In addition, there will be a class of experiments involving interaction of proton beams with nuclei of which the three-body problem has special importance.

The general layout of Experimental Areas A, B, and C is shown in Fig. 7. Area A is on the straight-ahead line where the primary business of generating meson beams goes on. The High Resolution Spectrometer (HRS) is located in Area C on the 45° beam line, followed by our proposed location of Area B--

the NPL. Both the HRS and NPL are served by the H^- beam which is accelerated simultaneously with the primary positive beam. At present the H^- beam intensity is planned for 100 μA . After a part of the beam is stripped to H^+ and deflected for the HRS, the residual beam is further divided so that it enters the NPL as two beams: one of higher intensity for neutron production, the other of lower intensity for use as an external proton beam. This is shown in greater detail in Fig. 8, which gives a more detailed layout of the proposed experimental area. (Note that the building outline has been rotated $\sim 90^\circ$ clockwise with respect to the last figure.)

The main characteristics of the NPL are :

- The area is 80 x 80 ft with a 15-ton bridge crane.
- The radiation shielding is planned for 500 W dissipated by the proton beam in a liquid deuterium target.
- The H^- beam enters from the left. Part of it is stripped by a foil, after which the beam is split in a magnetic field. The main part proceeds through an achromatic bend to the LD_2 neutron-producing target and through two bending magnets to a local stop.

The 0° neutron beam appears in the upper right-hand part of Fig. 8. Shown there is a possible experimental setup for small-angle n-p charge exchange scattering.

Also shown is the 28° beam line which provides polarized neutrons (at somewhat lower energy) simultaneous with the 0° beam. The intensity will be lower by a factor of approximately 3. Shown on this beam line is a possible n-p, C_{NN} experiment with a polarized target. A solenoid is shown for the purpose of precessing the neutron spin direction $\pm 90^\circ$ in polarization experiments.

In the lower part of the picture, the external proton beam is shown entering the experimental area and being deflected by a steering magnet. It is expected that the external proton line will be frequently used to deliver the polarized proton beam--a function which is expected to be very important. When unpolarized beam is entering the area, experiments such as p-p small-angle scattering, p-p meson production, p-d interactions, or p-alpha back-angle

scattering can be carried out. Polarized proton beams of lower intensity could also be obtained by scattering from hydrogen in the cryogenic target, if the polarized ion source should not become operational on schedule.

It appears practical to have 2 or 3 experiments on the floor and operating simultaneously. Very probably one of them would be the controlling experiment, operating in a data-taking stage. The others would be operating at lower priority, either setting up or taking data.

Before concluding, I want to acknowledge that the efforts of many people have gone into our planning. In particular, the collaboration with Lewis Agnew and John Hopkins at IASL has been very important. Lee Northcliffe of Texas A and M University gave considerable help last summer.

VI. CONCLUSION

In the preceding presentation, I have tried to give an impression of the current status of the nucleon-nucleon interaction and to summarize the current planning for a suitable experimental facility. LAMPF has characteristics ideally matched to the requirements of the situation. Clarification of this problem at LAMPF energies will count as a major accomplishment. These experiments will be interesting and highly diversified and well suited to graduate-student participation. It is to be expected that the theoretical analysis of results will proceed in a parallel way, such that a powerful, unified effort can be focussed on the problem.

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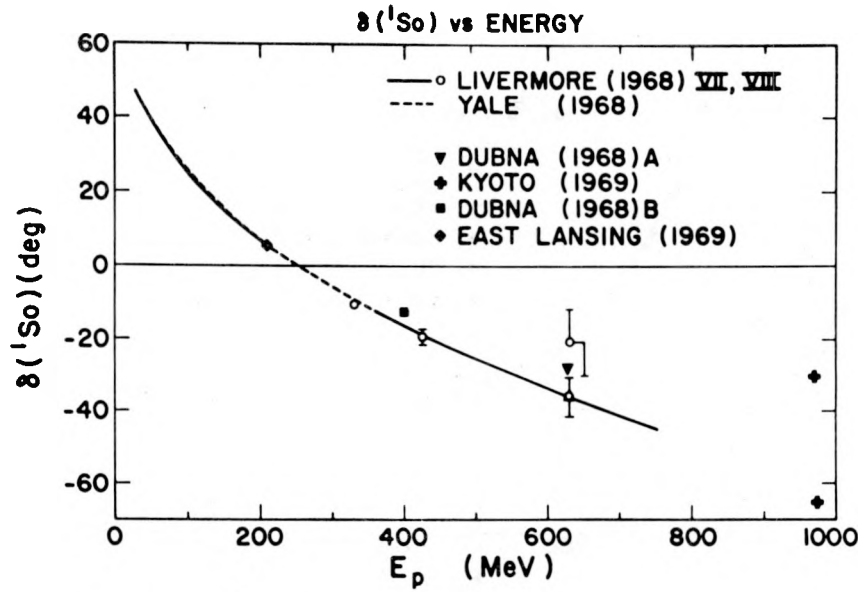


Fig. 1. The p-p $\delta(^1S_0)$ phase shift in degrees as a function of energy.

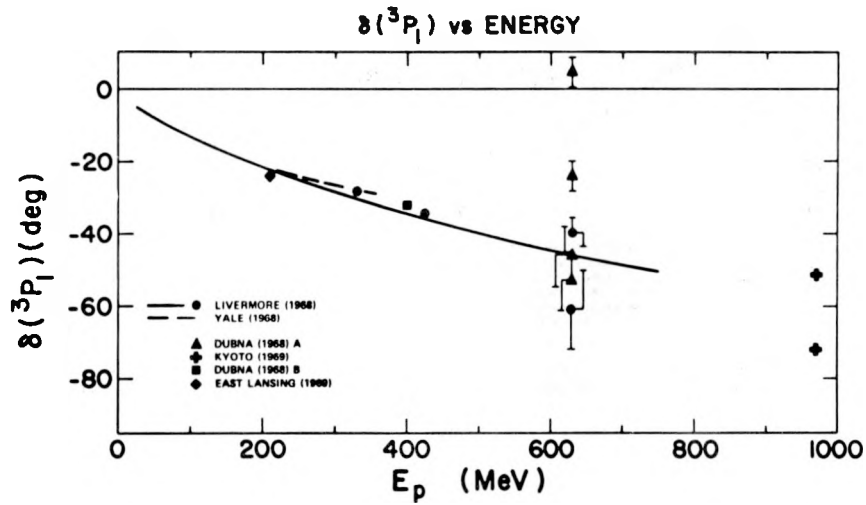


Fig. 2. The p-p $\delta(^3P_1)$ phase shift in degrees as a function of energy.

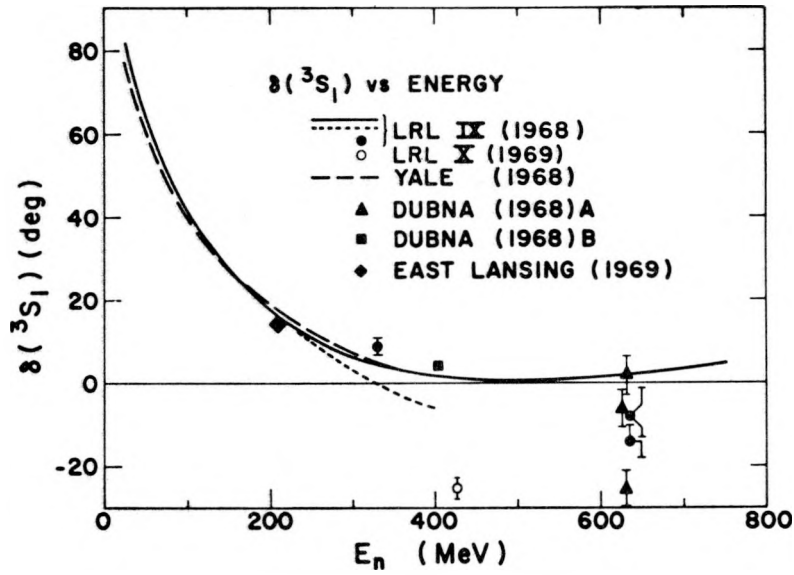


Fig. 3. The $T = 0$, n-p, S-wave phase shift $\delta(^3S_1)$ in degrees as a function of energy.

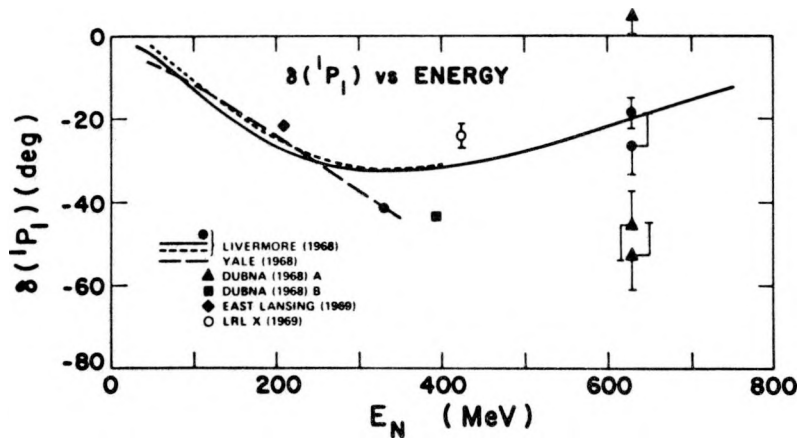


Fig. 4. The $T = 0$, n-p, P-wave phase shift $\delta(^1P_1)$ in degrees as a function of energy.

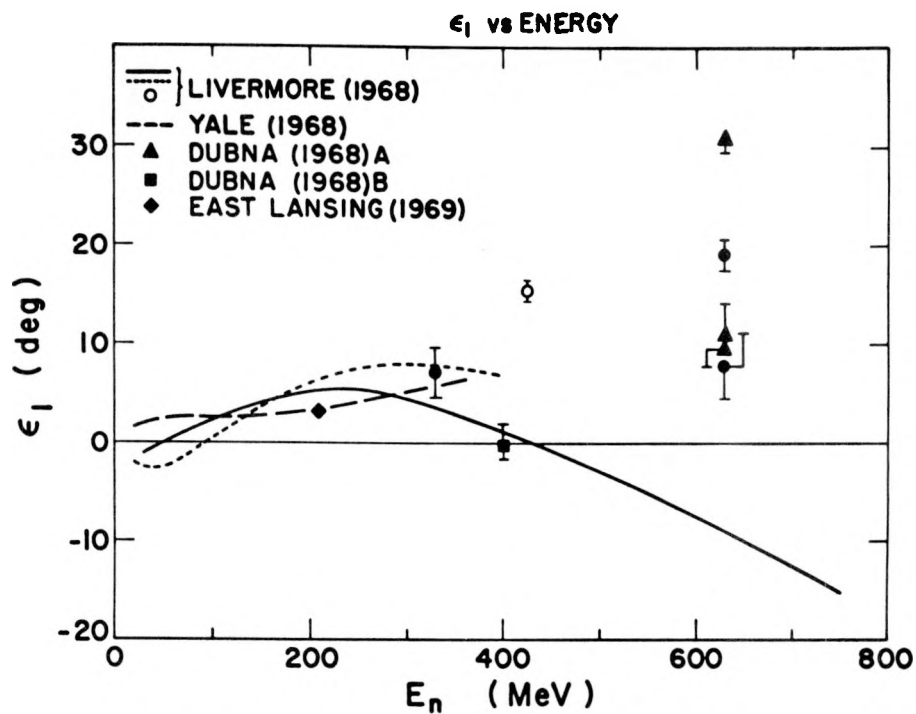


Fig. 5. The ϵ_1 phase parameter as a function of energy.

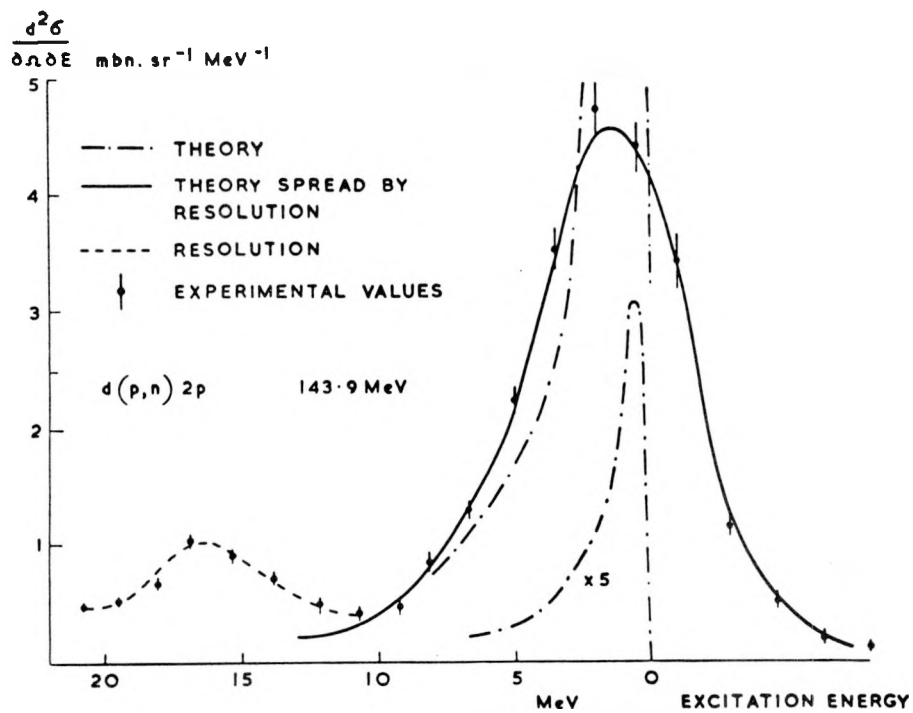


Fig. 6. Harwell data for 143-MeV protons on deuterium, showing the shape of the 0° neutron peak.

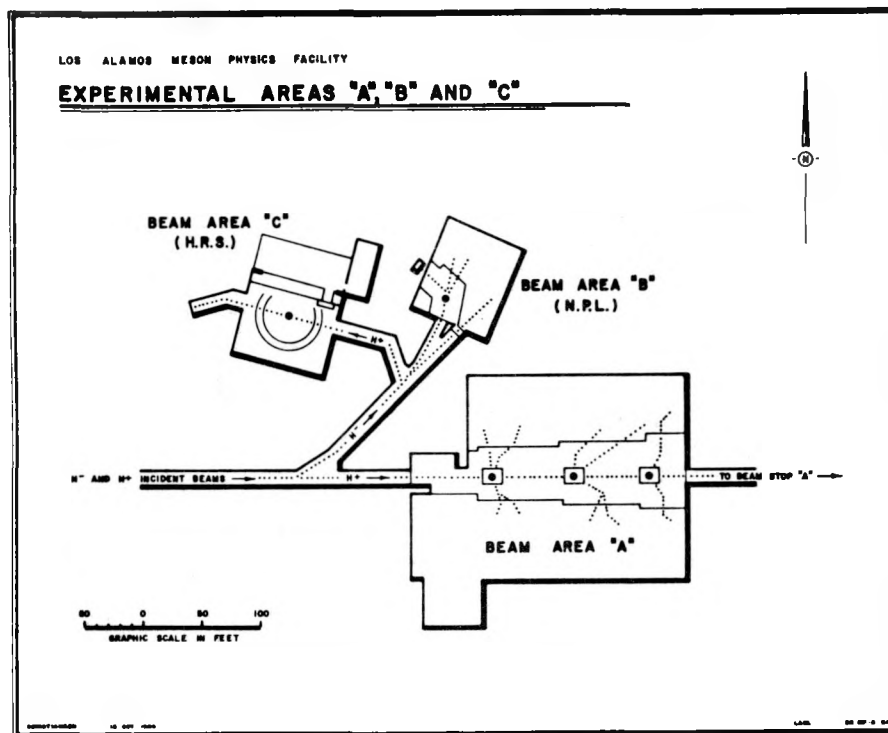


Fig. 7. The general layout of the LAMPF experimental area.

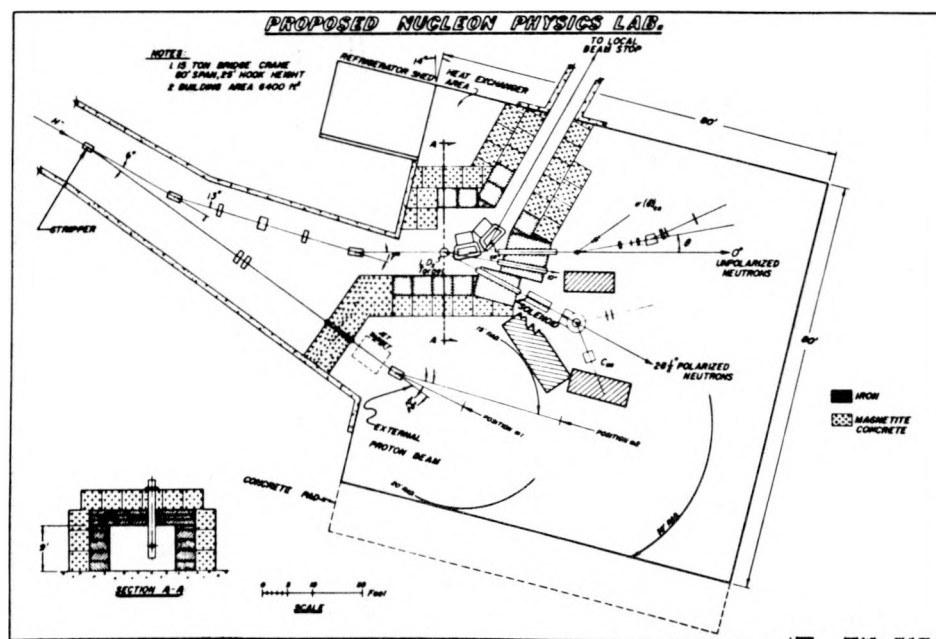


Fig. 8. The proposed Nucleon Physics Laboratory in Experimental Area B.

POTENTIALITIES OF NEGATIVE π -MESON BEAMS

IN RADIOTHERAPY OF CANCER

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Historically, it is fair to say that it was not the radiotherapists or the radiobiologists who furnished an impetus for investigations in this area, but the physicists. It was the early publications and statements by Fowler,¹ Richman,² and others that stimulated interest in the potentialities of π^- mesons as a new type of particle suitable for clinical radiotherapy. A number of measurements have now become available that begin to add a little substance to what, at one time, was largely theoretical. This has gradually attracted the interest of people like myself, who are now prepared to take a very serious look at these possibilities.

In this paper, I would like to review the present status of radiotherapy and then to consider some of the reasonable clinical expectations for beams of π^- mesons, based on what is now known about their radiobiologic and physical distribution characteristics. I think it would be a mistake to start from the point of view that all patients currently treated with radiotherapy die and that all of them after the advent of π^- mesons will be saved. We ought to start from some realistic vantage point. Currently, between 40 and 50 percent of all cancer patients receive radiotherapy at some point during their treatment. However, many of them receive their radiotherapy after they have become known failures of surgical management, at a time when their chance for cure has almost vanished. Perhaps, then, half of this 40 to 50 percent arrive on the doorstep of the radiotherapist as primary, fresh, previously untreated cases, and most of these have some finite chance for cure.

In the last twenty years or so, a number of new kinds of machines have come into general use in the field of radiotherapy, so that today the modalities available to us go beyond the one-time standards of the field, such as the old 250-kV x rays

and the interstitial sources of radium and similar isotopes. These new sources include gamma rays from cobalt: x rays in the range of 6 to 10 MeV--mostly from linear accelerators, but from other types of devices as well, including betatrons: and electrons, primarily from betatrons and to some extent from linear accelerators operating in the energy range roughly from 15 to 35 MeV. These new megavoltage sources have made possible a significant improvement in the physical distribution of the beam energy within the tumor volume. The fact that this physical change alone has made a striking and important difference in the cure rates of a variety of cancers is a point worth stressing in the context of this discussion. Today, the radiotherapist can offer a permanent cure rate of 55 to 60 percent for patients with locally inoperable cancer of the prostate,³ a tumor which was simply not treatable with curative intent in the days of 200-kV x rays. Cure rates of the order of 30 percent are now reported for cancer of the urinary bladder: the figure was essentially zero with 200-kV x rays. A number of very substantial gains have been made in the case of the malignant lymphomas, notably Hodgkin's disease.⁴

Nonetheless, it must be admitted that failure is still too often the end result of our efforts, even with the aid of megavoltage beams. It is important to recognize that there are different types or causes of failure and that they do not all have the same significance. Thus, we can distinguish between local failure--the failure to cure a primary tumor at the site where it first grew, and regional failure--the failure to control tumor in the regional lymph nodes to which tumors have a tendency to spread or metastasize. A third type of failure is distant, blood-borne spread of the cancer, over which we have little control and against which an essentially local treatment modality like radio-

therapy has little to offer. Finally, the type of failure which is perhaps the most distressing of all is that in which the cancer is successfully eradicated, but the patient dies of complications secondary to radiation-induced normal tissue injury.

Recently, Suit⁵ has tried to make a careful and systematic analysis of the probability of local or regional failure for all of the many kinds of cancer. His best estimate was that, at the present time, approximately 58,000 of the 175,000 cancer deaths per year in patients who had been treated with radiotherapy are attributable to local or regional failure. This is, to a first approximation, some measure of the job that remains to be done by improving the efficacy of any radiotherapeutic modality at our disposal--roughly 58,000 deaths in the United States alone every year.

What can we say about the immunological background on which these responses or failures are being seen? It is now generally recognized that, although there may be some immunological reaction of patients to their own tumors, such reactions appear to be extremely weak, so weak that we have no useful way to exploit them as an aid to therapy. At the present time, then, we must assume that it is necessary to destroy every last cancer cell in order to cure the patient. In doing so, we must at the same time maintain the integrity of the normal tissues in the vicinity of the cancer, at least to an extent compatible with a reasonably comfortable and useful life.

Radiobiological studies in recent years have shed light on some of the kinetic aspects of the complex interaction between effects on the tumor and effects on the normal tissues during the course of radiotherapy. A point that must be stressed very strongly and that people have a tendency to lose sight of is that the mere fact that a beam can produce a greater effect, relatively speaking, in a hypothetical tumor is to no avail if it produces the same relative incremental effect on the normal tissues in the immediate vicinity of that tumor. We must look for differential effects. If we cannot find differential effects, there is no reason to expect that the end result will be modified in any way.

Studies by Puck⁶ and others have established

the general character of the x ray dose-survival curves for a variety of mammalian and other cells. Typical curves are seen in Fig. 1. Note that the distal part of each curve appears to be exponential, relative to dose. However, the curves do not extrapolate to the origin at 100 percent survival, but instead to a point significantly above 100 percent, indicating that there is an initial "shoulder" region in which there is not an exponential relationship between dose and survival. It is now generally agreed that this shoulder is due to the existence of a recovery system, some type of repair process, which can cope with small degrees of damage but which becomes saturated at relatively low doses. If small doses are given and then a period of time is allowed to elapse, it has been shown that cells return quantitatively to their former state.⁷ Thus, successive fractionated doses are not linearly additive; instead, a considerable amount of the effect of each is repaired.

An important feature of the radiation sensitivity of virtually all cell systems studied to date is their dependence on the oxygen concentration present at the time of irradiation. When irradiations are carried out under nitrogen in an oxygen-free environment, there is a striking, nearly threefold decrease in the slope of the exponential portion of the curve, relative to that of the curve obtained when the same cells are irradiated in air or oxygen (Fig. 1). X- and gamma-ray beams and electrons would all be expected to yield this kind of result.

[Question from the audience: What is the difference between oxygen and nitrogen?]

The oxygen effect has nothing to do with the metabolism or respiration of the cell, but rather is determined by radiochemical reactions which may be influenced to go in a forward direction in the presence of oxygen but have a better chance of going in a reverse direction in the absence of oxygen, so that the cells can restore themselves to the pre-existing state. Injury is increased by roughly a factor of 3 with these types of radiations in the presence of oxygen. This nearly threefold increase in slope is referred to as the oxygen enhancement ratio, or OER. All mammalian cells would be expected to exhibit about the same OER.

Figure 2, taken from the work of Barendsen,⁸ shows similar mammalian cell-killing curves, but for other types of radiations, namely, 2.5-MeV alpha particles, 4-MeV alpha particles, and 15-MeV deuterons. For the low-energy alpha particles, irradiation in an atmosphere of either oxygen or nitrogen yields identical curves. There is thus no oxygen dependence of the low-energy alphas. For the slightly higher energy alphas, Barendsen observed an OER of 1.3. For the 15-MeV deuterons, the ratio rose to 2.6, very close to that obtained for x rays or for electrons. This, then, tells us that the rate of deposition of energy per unit length of path of different radiations is related to their oxygen dependence. Barendsen has plotted the quantitative relationship between OER and linear energy transfer, or LET, which is familiar to physicists as dE/dx . The 2-MeV alphas had a dE/dx of 166 keV per micron, whereas on the other end of this spectrum, the deuterons were at 5.6 keV per micron. In general, high LET is associated with low oxygen dependence, and low LET is associated with high oxygen dependence. The oxygen enhancement ratio begins to fall rapidly as LET approaches 100 keV per micron and finally reaches a value of 1, indicating the complete absence of an oxygen effect (Fig. 3).

Another parameter of response that varies with LET is the relative biological effect, or RBE. It is simply the ratio of the doses of two different types of radiations when compared for the same biological effect. The RBE is fairly stable in the low LET stage, but as LET increases, the RBE begins to increase to a factor of severalfold in the LET range of heavy ions, and then falls again.

What relevance do some of these considerations have for us clinically? It is well-known that tumors tend to outgrow their blood supply in focal areas, in which there will then be avascularity and a very low oxygen concentration, often associated with death of focal zones within the tumor. Imagine the situation of a tumor cell lying immediately adjacent to a region which is dead because of lack of oxygen. It will also lack oxygen, but may have just barely enough to stay alive. Such a cell may be expected to be two to three times more radioresistant than well-oxygenated tumor cells. If the rest of the tumor responds to treatment, the tumor will shrink, bringing the "half-dead" hypoxic cell closer

to its blood supply again, whereupon it will be re-oxygenated and will grow again. On the basis of such considerations, it has been postulated by the late L. H. Gray⁹ that anoxia, or hypoxia, may be an extremely important cause of failure of the local eradication of tumors. Although this is still not proven at the clinical level, it is a very plausible argument. If it is true, it would follow that any radiation that exhibits a significantly lower oxygen dependence than that of x rays might be less vulnerable to this type of failure.

In actual clinical practice, however, radiotherapy treatments are not given as single, massive doses, because they produce rather severe reactions to the normal tissues and better results were achieved when multiple fractionated doses were employed. We have tried to understand for many years why fractionation works. It has recently been discovered¹⁰ that tumors exhibit reoxygenation in the interval between successive fractionated doses of radiation. Thus, the use of fractionated doses of x rays may be one way to circumvent the oxygen effect. If this interpretation is correct, it would tend to diminish the expectation that using a radiation with a lesser dependence on oxygen would significantly improve our results of treatment.

In practice, although we speak about using tumoricidal doses, we have to admit that we seldom deliver a dose that can be expected to destroy all tumors of any given type. What we really deliver is the highest dose that will produce little or no normal tissue injury. Thus, we are really dose-limited by normal tissue injury. Figure 4 shows the situation diagrammatically; dose is plotted against frequency of tumor eradication and of normal tissue injury. More and more tumors are eradicated, in a Gaussian distribution, as the dose increases; but at some level of dose, which usually overlaps the dose range for tumor destruction, another Gaussian curve for normal tissue injury appears. What we try to do in clinical practice is to find a dose at the very beginning of the second curve, where few patients would suffer significant tissue injury. There are certain types of tumors (lymphomas and seminomas) which are more radiosensitive; their Gaussian curves for tumor destruction are shifted to a lower dose level, with the result that they can be cured at doses that produce almost no detectable

tissue injury. We also know that normal tissue injury is dependent on the volume of tissue irradiated; the smaller the volume, the greater the dose it is likely to withstand. Thus, if we could confine the high-dose region as tightly as possible to the tumor volume and minimize the volume of adjacent normal tissue that is heavily irradiated, we could increase the dose to the tumor with less fear of normal tissue injury. This should produce a substantial increment of additional cures.

What can we say about the radiobiological and physical properties of π^- mesons in this context? I am indebted to Dr. Raju for making available to me a preprint of a paper¹¹ he recently presented in Tokyo. I will also draw on some of the data of Fowler,¹ Curtis and Raju,¹² Thiessen,¹³ Rosen,¹⁴ and others. The available data are still sparse, unfortunately, because the intensity of the beams that are currently available, primarily at Berkeley, are still very limiting for this kind of work. Nonetheless, Raju and his associates have done OER and RBE studies at 4°C and at room temperature for the roots of the plant Vicia faba.¹⁵ They found an oxygen enhancement ratio of 1.35 for π^- meson irradiations at room temperature. However, this was done at a low dose rate, and the OER is sensitive to dose rate. At these very low dose rates, cobalt gammas would have an OER of only 2, instead of the usual 3. Thus, the π^- meson OER value of 1.35 should be compared with a cobalt value of 2 for this case. When the same irradiations were carried out at 4°C, at which there is a lesser dependence on dose rate, Raju, *et al.*, observed an OER of about 1.5. Curtis has calculated an expected OER for π^- mesons in the range of 1.8 or 1.9, relative to a normalized value of approximately 3 for x rays or gamma rays. Thus, it would appear that π^- mesons are indeed advantageous in terms of having a decreased oxygen dependence. However, it should be pointed out that the OER for π^- mesons is not appreciably better than (and perhaps not quite as good as) that which has been measured for neutrons,⁸ which are certainly a more readily available kind of radiation. Thus, if one had to make an argument for selecting another radiation solely on the basis of OER, it would not be possible to make a compelling argument for using π^- mesons in radiotherapy.

Raju has also collected data from published and

unpublished sources on the RBE of π^- mesons in various systems; the reported values of RBE vary rather widely from around 1.8 to as high as 5. RBE will have to be measured very much more carefully in systems that closely resemble clinical radiotherapeutic situations before any actual radiotherapy can be undertaken, because the radiotherapist must know the RBE with great accuracy to estimate the biologically equivalent dose delivered to the tumor. This in turn brings us to the dosimetric aspects of π^- mesons; that is, their anticipated physical distribution within a patient.

Figure 5 is from the paper of Fowler.¹ Fowler showed that alphas and protons of appropriate energy could be arranged to produce a higher dose at a depth of 10 to 15 centimeters below the surface of the body, relative to the entrance dose or to the dose in the first several centimeters of tissue. The OER to be expected of such radiations would, of course, be similar to that of x rays. The upper two curves of the right half of Fig. 5 are for ⁶⁰Co gammas and 8-MeV x rays, the types of radiations that we now work with. They have OER's of about 3 and deliver their peak dose very near the surface, after which relative dose decreases exponentially with increasing depth of penetration. In striking contrast are the two lower curves for π^- mesons, calculated by Fowler and showing a very striking concentration of relative dose at the chosen depth of 10 to 15 centimeters occupied by a hypothetical tumor, together with a relatively favorable OER.

The depth at which this concentration of dose occurs is energy-dependent and is due to nuclear capture of the pions as they slow down and to the production of "stars" which contribute very heavily to the dose in the stopping region, but not at all, of course, to the dose in the first several centimeters (Fig. 6). Thiessen¹³ has recently done some very elegant preliminary work on the optics of π^- meson beams. His work shows what might be done with the beam at Los Alamos, for example, with a series of lenses and slits to define a parallel beam with properties which could be modified pretty much at will with respect to energy and momentum spread.

Figures 7 and 8 from Thiessen's paper show the hypothetical distribution of dose for π^- mesons stopping in water, which is of tissue-equivalent

density. The calculations assume that 30 MeV of energy is deposited per star. If nothing were done to alter the characteristics of the beam, it would produce a somewhat higher dose at the proximal end (closest to the entrance point) than at the distal end, yielding an unsatisfactory inhomogeneity of dose across the volume occupied by the hypothetical tumor. There are various ways of overcoming this. One simple way is to direct the beam into the patient from two opposing surfaces, thus canceling out the inhomogeneity in the star region. This is a technique that we often use today with x rays. However, with the π^- meson beam, as Thiessen has pointed out, it is possible to modify the relative energy distribution in the beam by using a trapezoidal cross-section collimator. Figure 8 shows the effect of this; there is now a lesser percentage of low-energy and a larger percentage of higher-energy particles in the beam. This results in a flattening out of the spatial distribution of the stars so that the effective dose in the volume occupied by the tumor can be made extremely homogeneous. Yet, very close to the high dose, high RBE star region the dose falls off so sharply that tissues just one to two centimeters away receive trivial doses. This is a very exciting kind of potentiality of negative π -meson beams, and one which I think any clinical radiotherapist would find enormously intriguing.

Raju and Richman¹¹ have summarized the radiobiological and dosimetric attributes of π^- mesons in the "surrounding normal tissues" versus the tumor volume (Table I). The relative dose, because of the physical distribution of the stars and their high RBE, can be made very much higher in the tumor. There is a high oxygen dependence in the low LET region and a moderately low oxygen dependence in the high LET region which encompasses the tumor volume. The factor of recovery is of somewhat debatable significance. One point that I should stress about their summary table is that it lends itself to a serious misinterpretation. It is very easy, looking at a column labeled "surrounding normal tissues," to think that all of the normal tissues are "surrounding" and that "tumor volume" contains nothing but tumor. This, of course, is simply not true. The tumor grows through normal tissues; it permeates normal tissues. There are normal tissues within the tumor and at the margins of the tumor. It is

therefore impossible to irradiate the tumor volume without irradiating some normal tissue. I think the problem resolves itself to finding methods for irradiating as little normal tissue as possible.

Figures 9, 10, and 11 are from some isodose curves prepared by Dr. Morton M. Kligerman for the Pion Review Committee convened by the Atomic Energy Commission to look into this subject. Figure 9 shows a four-field distribution for cobalt gamma rays entering through all four sides of the pelvis of a patient with a hypothetical cancer of the cervix which is schematically occupying the volume shown by the dashed line. Although the dose is moderately well-concentrated in the depth of this patient by this four-field technique, nonetheless, very appreciable doses are received in each of the entry and exit fields.

Figure 10 shows a similar four-field pattern for 6-MeV x rays from a linear accelerator. This is better, but is basically similar to the previous one. Finally, Fig. 11 is intended to show hypothetically what a four-field pion distribution would look like in the same patient with the same hypothetical tumor. The difference is really very dramatic. There is a very insignificant entry dose, about 10 percent of the peak homogeneous isodose zone around the tumor. Even tissue quite close to the tumor receives a dose only about one-third to one-fifth as much as the tumor itself. This is surely a real advantage.

However, I would emphasize that if we continue with π^- mesons to treat spheres, cubes, and other idealized geometrical volumes rather than the actual geometry of the tumor, no matter how irregular and complex, we will fail to contribute as much as π^- mesons are hypothetically capable of contributing. Methods must be developed to permit the clinician to determine exactly where the margins of the tumor are and to be able to convert this information into coordinates that can be understood by a computer. Secondly, we will need to develop far more elegant computer-controlled beam optics to take maximal advantage of the unique pattern of distribution which pion beams can offer.

To summarize, it seems to me that there are several reasons for believing that π^- mesons might have significant advantages for clinical radiotherapy. I feel strongly that intensive research

efforts aimed at this goal are definitely warranted by the evidence already in hand. What are the needs that confront us at this time? First and foremost, we need high-intensity sources of π^- mesons; the developments at Los Alamos, at Vancouver, and at Zürich will begin to satisfy that need. However, even though these machines, which are primarily intended for physics research, will make available facilities with which preliminary biomedical studies can be undertaken, it is clear that \$55 million machines are too costly for general clinical radiotherapy. In the second stage of development, less costly machines which are intended more or less exclusively for medical work will have to be designed. At the present time, some of my physicist colleagues at Stanford and our group are conducting discussions about the possible usefulness of superconducting electron or proton accelerators for this purpose.

When such beams become available, we will need to do really very careful dosimetric studies. The place where the dosimetry must be most precise is in the transition zone between the essentially pure pion-beam region and the star region. This is the dangerous area in which we will need to know the dose most precisely. We will also need to know how OER and RBE change as a function of dose and of depth inside the patient, with particular attention again to the junction between the low LET region and the star region. This is the point of greatest danger and of greatest interest.

On the clinical side, we must solve the difficult job of finding better ways to delineate the exact dimensions and contours of tumors. I assure you that this is not a trivial problem. If we are to try to envelop a tumor as closely as possible with a negative π^- -meson beam, then surely the first requirement is to know exactly where the tumor begins and ends. We have some reason to hope that an important advance may come from currently emerging techniques for labeling antibodies to tumor-specific antigens with radioactive isotopes. Thus, if the clinicians can indeed learn to delineate the irregular contours of the volume occupied by the tumor

with far greater accuracy and if the physicists can give us a beam which can be made to "wrap around" such an irregular tumor volume with great precision and elegance, then I believe that the chance that π^- mesons will contribute significantly to cure rates in radiotherapy is a very real one. Of course, this will ultimately require a carefully designed and properly monitored randomized clinical trial, intended to prove whether all of the foregoing is reality or merely wishful fantasy.

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Table I
Relative properties of π^- mesons in normal tissue vs. tumor*

Surrounding Normal Tissue Region	Tumor Region
A. Low dose	High dose
B. Low LET	High LET
C. Low RBE (same as conventional radiation)	High RBE
D. High OER (same as conventional radiation)	Low OER
E. More recovery during fractionated treatment	Less recovery during fractionated treatment

* M. R. Raju and C. Richman, UCRL-18806 (1969).

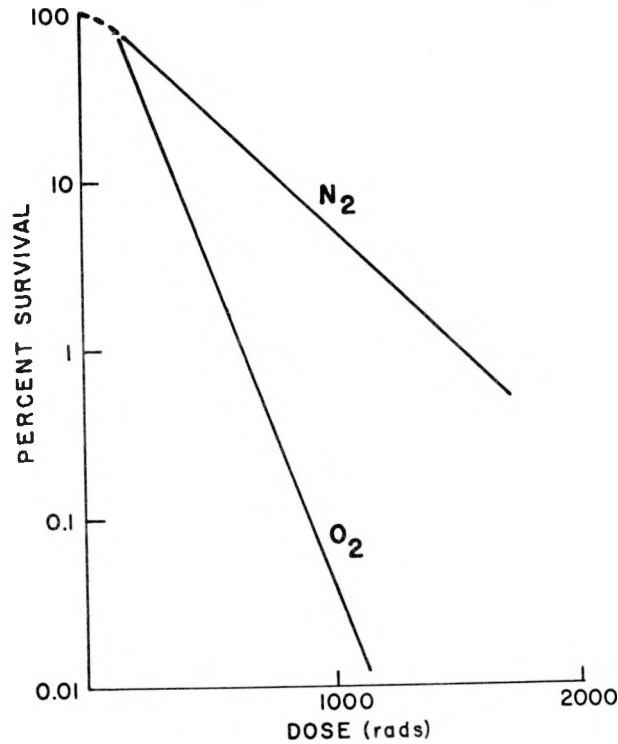


Fig. 1. Typical x-ray dose-survival curves. Cells irradiated in N_2 show marked resistance over those irradiated in O_2 .

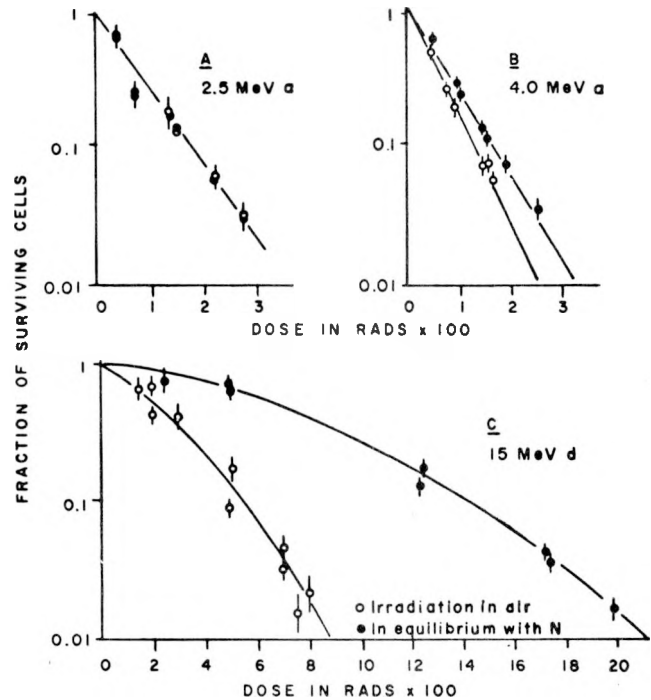


Fig. 2. Barendsen's work⁸ showing relative effectiveness of alpha particles and deuterons on cultured human cells.

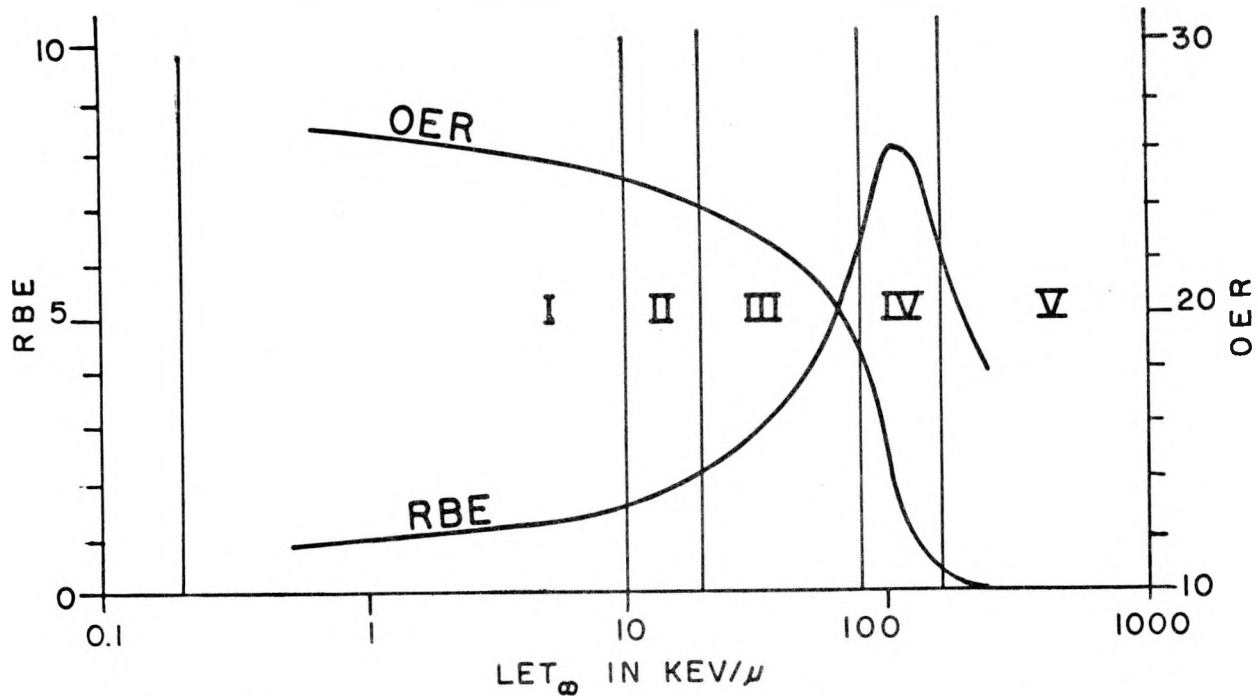


Fig. 3. Barendsen's relationships⁸ of RBE and OER with LET_{∞} in different regions of LET.

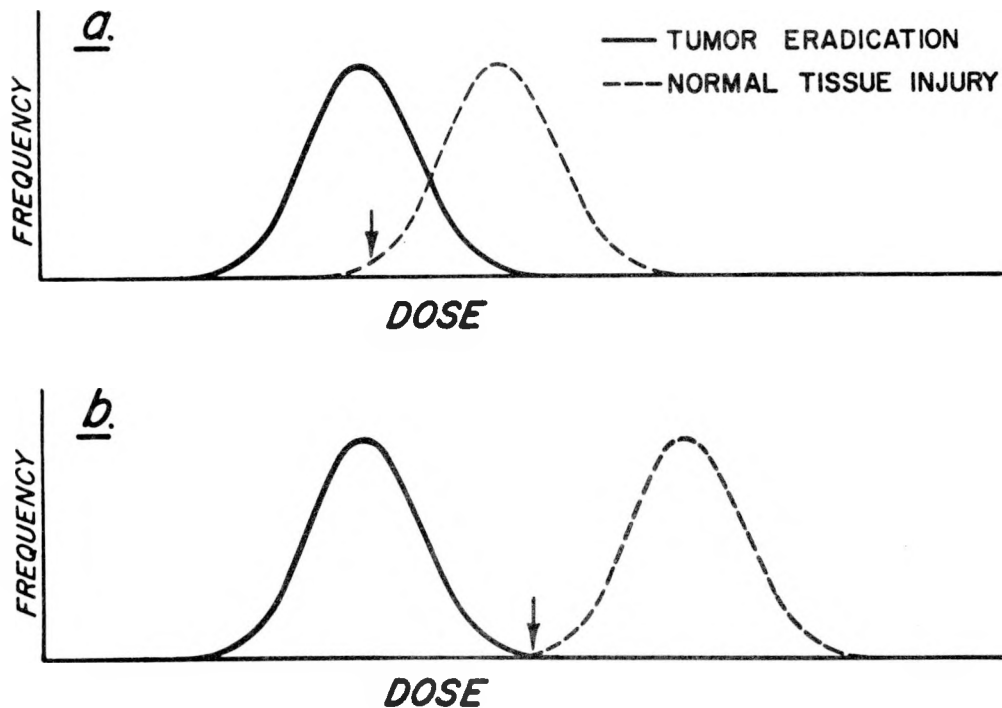
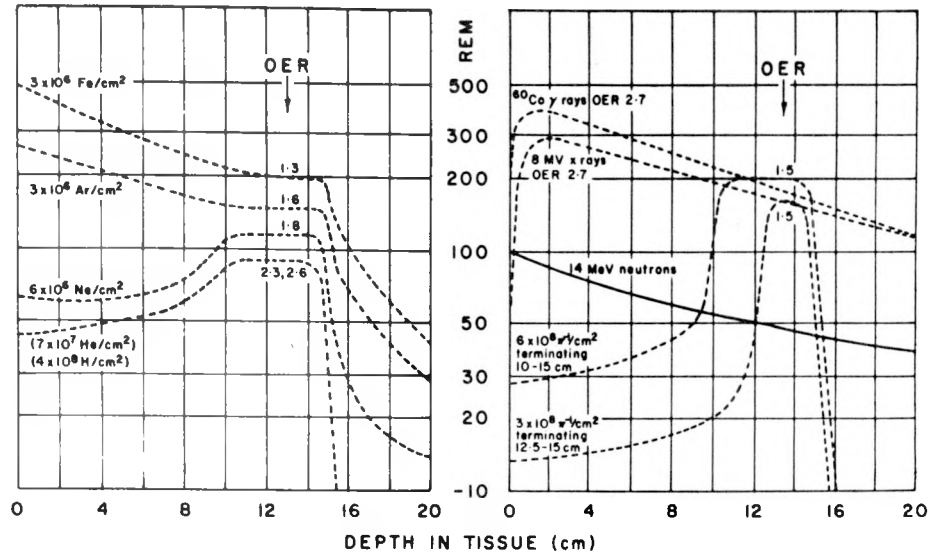


Fig. 4. Frequency of tumor and normal-tissue injury as a function of total dose. a. Usually the two curves overlap; the therapist attempts to irradiate at the beginning of the overlap region (arrow) to optimize tumoricidal dose with minimal normal-tissue damage. b. A modality that would spread the two curves by decreasing the probability of normal-tissue injury would permit higher doses (arrow), thus effecting cure of a substantially higher proportion of tumors.

DEPTH DOSE DISTRIBUTIONS



---- (Fowler, Proc. Phys. Soc. **85**, 1051 (1965))

— (Horst and Conrad, Fortschritte auf dem Gebiete der Röntgenstrahlen u. der Nuklearmedizin, Diagnostik, Physik, Biologie, Therapie **105**, 299 (1966))

Fig. 5. Fowler's data¹ showing comparison of depth-dose distributions of various types of irradiations.

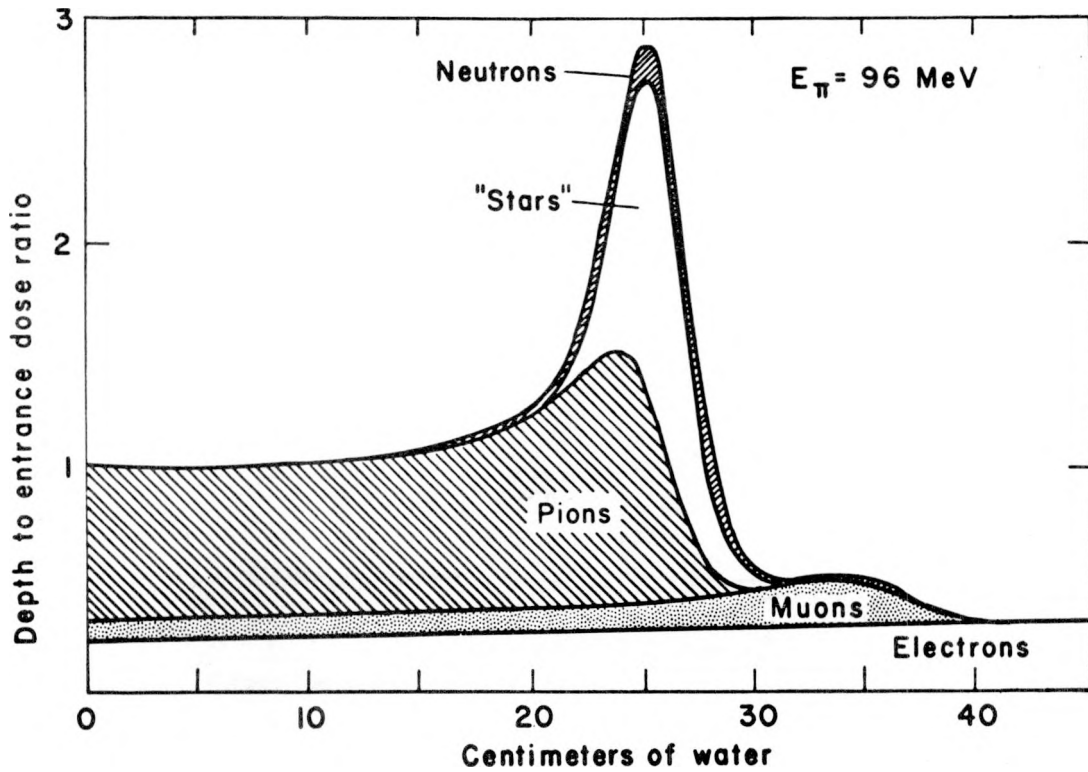


Fig. 6. Depth-dose curve for incident negative pions in water. (S. B. Curtis and M. R. Raju, UCRL-17606, June 5, 1967.)

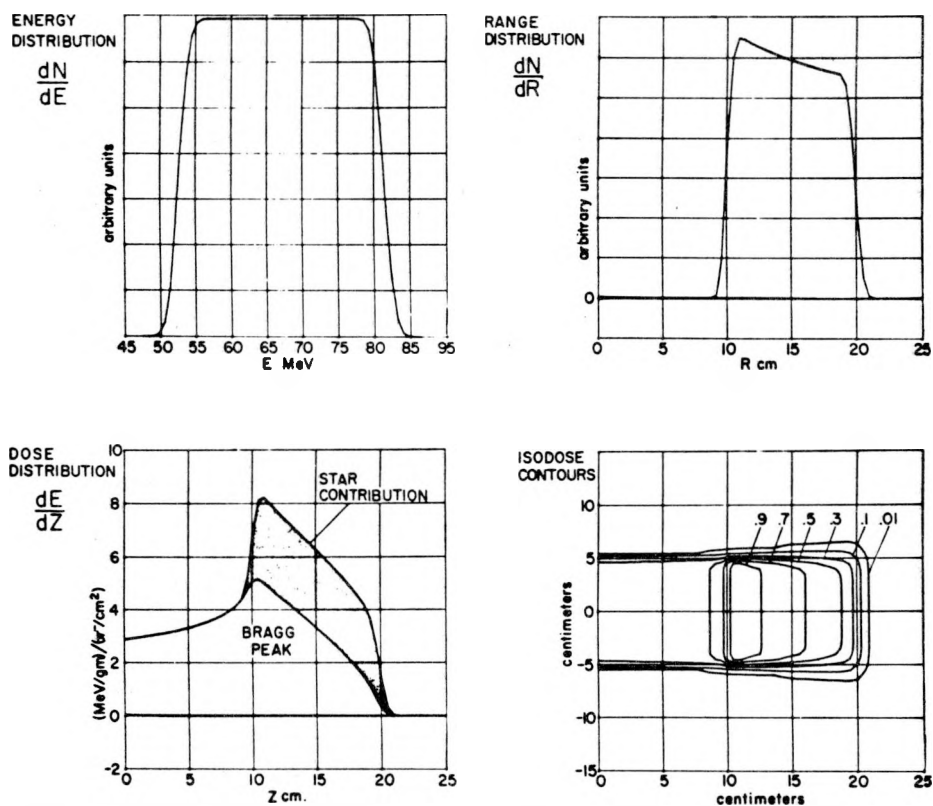


Fig. 7. Negative pions stopping in water; 30-MeV energy deposit/star; 26 percent $\Delta P/P$. (Calculated by A. Thiessen, LASL.)

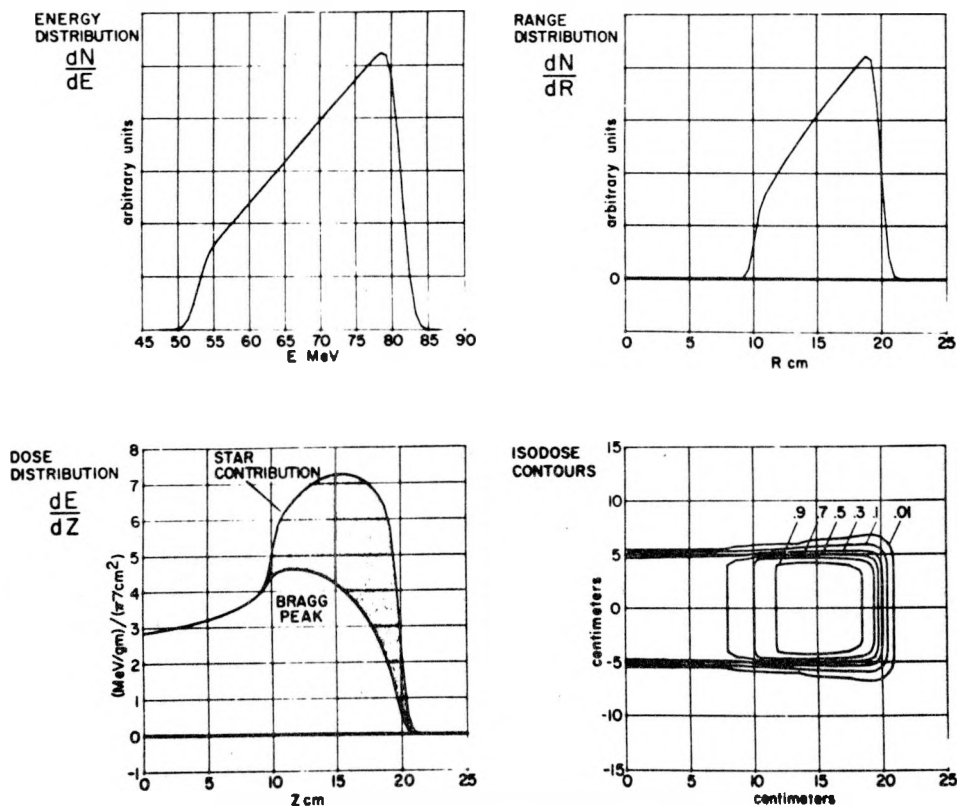
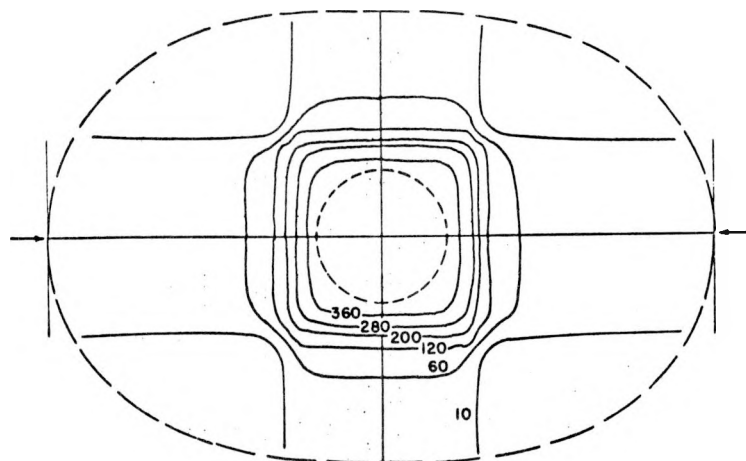
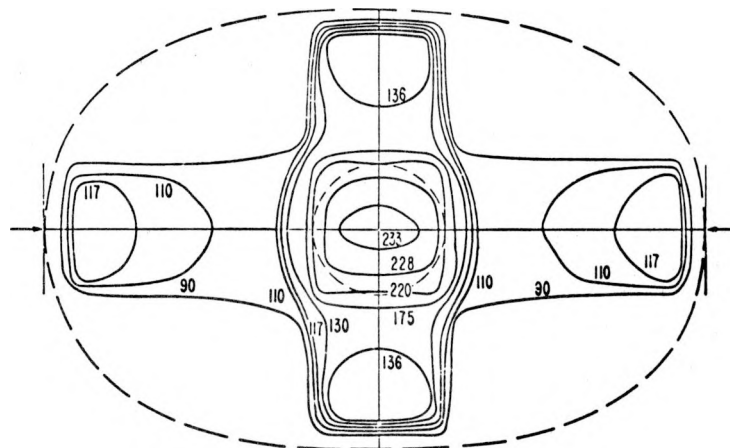
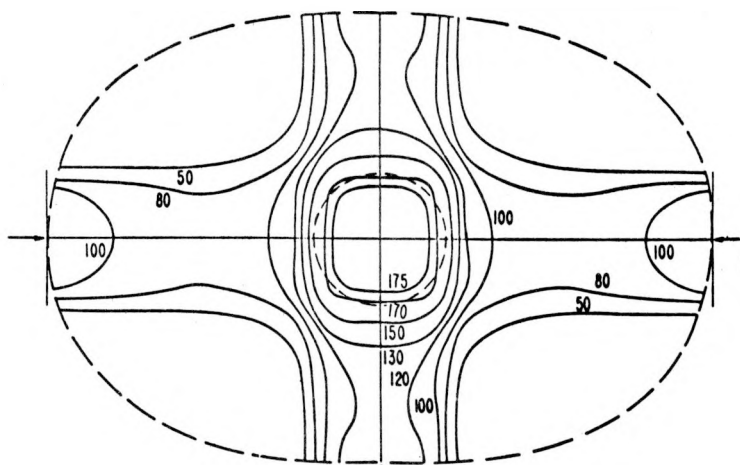


Fig. 8. Negative pions stopping in water; 30-MeV energy deposit/star; 26 percent $\Delta P/P$. Energy distribution changed for optimum effect. (Calculated by A. Thiessen, LASL.)



Figs. 9, 10, and 11.

Isodose curves showing the four-field technique for ^{60}Co , 6-MeV x rays, and π mesons, respectively, in a 20 x 30 cm oval phantom. (Prepared by Morton M. Kligerman, Yale University School of Medicine, and included in Report of the Pion Review Committee to the Division of Biology and Medicine, U. S. Atomic Energy Commission, May 1, 1969.)

REPORTS FROM WORKING GROUPS

Informal Study Session

on

PION BIOMEDICAL PROBLEMS

Chairman D. Groce presided over a short discussion of some problems relevant to the biomedical research being planned for LAMFF. The following is a brief summary of these discussions:

Linear Energy Transfer (LET) Calculations--S. Curtis (LRL)

Survival Curves. Curtis described development of a semiphenomenological theory for predicting survival curves. He then compared predictions of the theory with survival curves of T-1 kidney cells irradiated with π^- beams. The form of the survival curve was based on the experimental work of Todd and Barendsen.

Oxygen Enhancement Ratio (OER). Based on OER vs LET results of Barendsen, OER's for neutrons and pions were derived and compared with experiment. The LET distribution used for π^- stars was based on the results of Fowler.

For 3- and 15-MeV neutrons, the predictions gave OER's of 1.47 and 1.44, respectively, compared with a measured OER of about 1.5. In the star region of the pion dose distribution, calculations gave an OER of ~ 1.8 compared with measured values typically from 1.2 to 1.8.

Curtis pointed out that we still do not have accurate knowledge of relative biological effects (RBE) as a function of LET, particle type, or cell type.

RBE and OER; Tokyo Radiology Meeting--M. Raju (LRL)

Raju described measurements of dose and LET distributions for 67- and 91-MeV π^- beams, and also RBE measurements with respect to both proliferative capacity and to polyploidy induction using Ascites tumor cells in mice.

Studies of the mutation of yeast cells to arginine independence as a result of pion irradiation were described. The same effect as a result of heavy ion irradiation is being studied. Raju also pointed out that with respect to the T-1 kidney cell system we know little about the OER's for heavy ion irradiation.

Studies of the sensitivity to pion irradiation of lymphocytes at various stages of development were described, as were measurements, which gave OER's in the range of 1.35 to 1.5 for the pion star region, using Vicia faba root meristems.

Raju commented upon the enthusiastic interest in pion therapy expressed by the Japanese radiologists at the Tokyo meeting.

Mesic γ Rays--V. Perez-Mendez (LRL)

Perez-Mendez pointed out that mesic γ rays arising from the pion star events scatter less than the lower energy mesic x rays and should therefore enable one to locate the pion stopping region more accurately. He described experiments in which mesic γ rays (>15 MeV), collimated with 1/8-in.-spaced lead sheets, were detected with a spark chamber to determine the location of the stopping region with a resolution of about 1/4 in. (resolution limited by the lead sheet spacing).

It was also suggested that one look at the back-to-back 65-MeV gamma rays arising from the $\pi^- + p \rightarrow \pi^0 + n \rightarrow 2\gamma + n$ process.

Mesic X Rays--I. L. Morgan (University of Texas)

Morgan described dosimetry work being done at the University of Texas. The work includes passive dosimetry using thermoluminescent devices and active measurements using small diodes.

With respect to the problem of locating the pion stopping region, two methods were proposed.

1. Observation of the 160-keV x rays from π^- on ^{16}O .
2. Observation of the back-to-back e^+e^- annihilation γ rays arising from the e^+ decay of ^{13}N formed in the reaction,
 $\pi^- + ^{16}\text{O} \rightarrow ^{13}\text{N} + 3n$.

LAMFF Radioisotopes--H. O'Brien (IASL)

O'Brien described the plans at IASL to utilize the excess proton beam that reaches the main LAMFF beam dump to produce radionuclides. The results of calculations of the rate of production of some of

the isotopes that can be prepared by proton spallation reactions were presented, and some of the possible uses of these isotopes were discussed.

Radioisotope users were requested to contact O'Brien about those isotopes which are presently unavailable they are interested in obtaining, and what the potential uses of the isotopes might be.

Plans for establishing cooperative research studies were also discussed. Anyone interested in obtaining additional information about this program should write O'Brien.

Progress in LAMPF Biomedical Facility--D. Groce (IASL)

Groce summarized the status of the planning for the biomedical facility. He mentioned very briefly the proposal that has been submitted to the Division of Biology and Medicine of the AEC requesting funds for construction and also the planning grant request submitted to the National Cancer Institute by the University of New Mexico Medical School with the cooperation of people from the University of Colorado Medical Center, IRL, and IASL.

D. Groce

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C. Crannell	G. Kenney	H. Sannan
S. Curtis	L. Morgan	J. Shoop
J. Dowdey	T. Mueller	M. Stewart
A. Foss	J. Nunnally	B. Tolbert
J. Gibson	H. O'Brien	R. Wallace

Informal Study Session
on the
STOPPED MUON CHANNEL

Chairman H. Vogel presided and gave a brief status report.* The number of magnet types in the channel has been reduced from seven to six by using septum magnets in the beam splitter. Two of these six types of magnets are being designed at the present time: engineering specifications and drawings are scheduled for completion by the end of January 1970.

In the development effort for radiation-hardened magnets, magnetic steel samples and some samples of insulation will be inserted in the Omega West reactor by the middle of November 1969. We

expect to learn from the steel samples whether or not we may have to worry about possible radiation effects on the magnetic properties of steel.

We are investigating various temporary arrangements of magnets in the periodic alternating gradient portion in case all the magnets have not been purchased before beam day.

*A detailed description of the Stopped Muon Channel can be found in V. W. Hughes, S. Ohnuma, K. Tanabe, and H. Vogel, "Stopped Muon Channel Design Status," January 12, 1969, with addenda dated July 2, 1969 and July 28, 1969. These documents are available from H. Vogel, LASL.

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R. Cohen

K. Crowe
M. Eckhause
R. Perkins

H. Vogel

Informal Study Session

on

NUCLEON PHYSICS

Chairman J. Simmons opened the meeting with a general description of the proposed Nucleon Physics Laboratory (NPL), emphasizing how the building concept (form) is related to the general experimental program (function).^{*} In response to a request for comments, both R. Cole and H. Willard noted that the space allocated to proton experiments seemed inadequate. It was deemed necessary to allow at least 5 m of clear space around the target location on the external proton beam. This would entail an addition of about 10 ft to the east end of the proposed building.

The question was raised as to whether there was a need for a better energy resolution than the ± 0.4 percent nominal beam spread. No clear-cut requirement was noted, but Cole indicated that resolutions of ± 1 MeV might be desirable in (p,2p) experiments.

L. Agnew gave a brief description of the proposed NPL location and mentioned compatible beam-sharing modes of operation utilizing the LAMPF H^- beam. The possibility of future expansion of the NPL to the north and east was pointed out. A preliminary cost estimate for the NPL was presented, which included (in round figures) \$600,000 for buildings and construction, \$200,000 for the primary beam transport system, \$250,000 for the beam elements in the experimental area, and \$200,000 for shielding. This totals \$1,250,000, which should be augmented by a contingency allowance of about 25 percent. J. Hopkins then described plans for submitting a detailed proposal to the LAMPF administration. The proposal will be prepared by the NPL Steering Committee, which consists of C. Leavitt (University of New Mexico), I. L. Morgan (University of Texas), L. C. Northcliffe (Texas A and M University), H. B. Willard (Case Western Reserve University), and IASL members L. Agnew, J. Hopkins, and

J. Simmons (Chairman). It will outline the broad experimental objectives and describe the facilities needed. The proposal will include a number of contributions from potential users, giving descriptions of specific experimental programs which might be carried out at the NPL. Hopkins solicited additional contributions from those present.

The balance of the meeting was devoted to brief reports of various experimental programs proposed for the NPL.

Cole mentioned proton experiments on small nuclei, such as elastic scattering on ^3He , ^4He , ^{12}C , etc., light nuclei break-up experiments, (p,2p) experiments, and correlation or cluster experiments of the (p,d), (p,t), and (p, ^4He) type.

Northcliffe pointed out the importance of n-p differential, spin-dependent, and total cross section measurements. This was followed by a statement from Leavitt on the n-p inelastic problem. He emphasized the point that the high quality neutron beams expected to be available at the NPL will enable measurement of charged and neutral pion production in n-p collisions near threshold.

Morgan gave a list of interests of the University of Texas group, including (p,2p) and (p,xn) experiments, small angle scattering studies, and break-up processes leading to three-particle final states.

Willard then pointed out the advantages of the future LAMPF polarized beam, which, together with new developments in polarized hydrocarbon targets, enable complete measurements of the p-p system.

Finally, S. T. Emerson commented that precise measurement of n-p and p-p scattering parameters is needed as an adjunct to the LAMPF program of high resolution proton spectroscopy on complex nuclei.

L. Agnew

(continued next page)

^{*} A rather detailed description of the NPL can be found on pp. 23-31 of these Proceedings.

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B. Cohen	C. Hwang	H. Orr	J. Wells
R. Cole	L. Johnston	W. Ploughe	H. Willard
J. Cramer	H. Knowles	T. Putnam	

Informal Study Session
on
PION AND PARTICLE PHYSICS

Chairman P. A. M. Gram presided over a general discussion that resulted in a preliminary definition of the class of experiments in which the fundamental pion interactions, both strong and weak, are studied, including, for example, pion-nucleon scattering, rare decay-mode measurements, and pion production by pions.

A working group was formed to lobby for and plan the apparatus and facilities that this class of experiments will require. The group, which calls

itself P³ for Pion and Particle Physics, has elected P. C. Gugelot chairman and will hold its first working meeting at Los Alamos on January 20. For this first meeting, the Los Alamos members of the working group will prepare the design of a beam which is versatile enough to supply the pions for such experiments, and the user members are preparing experiment designs to see how close to matching their requirements we have come. The group welcomes the help of anyone ready to work on these problems.

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B. Dropesky	R. Kunselman	N. Tanaka
K. Gotow	D. Lobb	H. Thiessen
P. Gram	R. Macek	B. Wildenthal
C. Gruhn	E. Martin	P. Yergin
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	W. Olsen	

Report on the Working Session of October 27, 1969*

on the

ENERGETIC PION CHANNEL AND SPECTROMETER, EPICS

A working session of the EPICS Committee was held on October 27, 1969 at Los Alamos with Chairman S. E. Sobottka presiding. K. Crowe described various experiments proposed for this channel and the requirements they place on the design of the system. H. A. Thiessen discussed the results of a search for a channel which would satisfy the design criteria proposed at the last meeting. In the ensuing discussion, questions were raised about the need for a separator, methods of beam monitoring, target size, as well as foreground and background counting rates.

In the discussion of different channel designs, Thiessen emphasized that there is a factor of 40 to be gained in intensity by using a system that is dispersive rather than achromatic at the target. For a 1 mA proton beam on a 3 mm x 3 cm x 4 cm carbon target and a dispersive channel 27 m long with a production angle of 30° , an acceptance solid angle of 10 msr, and a one percent momentum bite, the flux on target is $\sim 5.8 \times 10^8 \pi^+/\text{sec}$ or $0.6 \times 10^8 \pi^-/\text{sec}$ at 300 MeV. When used in conjunction with a spectrometer (10-m flight path, 10-msr solid angle), a level with 50 $\mu\text{b}/\text{sr}$ cross section would give a counting rate of 400 counts/h for the π^+ beam. Thus the factor of 40 cannot be easily given up. A more detailed description of the EPICS channel is available from Thiessen (IASL) on request.

It was tentatively assumed that we are dealing with a vertical-horizontal-vertical (VHV) or vertical-horizontal-horizontal (VHH) system. Attention was chiefly focused on the design of the pion channel since the experience gained in the design of the HRS suggests that the spectrometer specification will not be as difficult to meet as the channel requirements. Thiessen has studied the optics of a number of channel configurations in first order

and several in second order. A promising three-bend design with both rotated and curved entrance boundaries was discussed in detail. A separator is placed between the first and second magnets and a mass slit between the second and third magnets. For the separator, a static electric field of 600 keV across a 15-cm gap, 5-m long, is required. This system was optimized in second order to reduce aberrations at the separator and at the target. The final system shows promise, but at present requires rather large targets. This problem will be investigated.

The desirability of and requirements for a separator were discussed. The ratio of protons to pions at 300 MeV, 400 MeV, and 500 MeV will be 3, 10, and 300. The rejection ratio of an electrostatic separator as well as the desirability of rf, time-of-flight, and dE/dx systems will be investigated.

The committee would welcome suggestions as to the requirements of any experiments planned for this channel. It is very desirable for all people who would like to participate in the design and use of this channel to communicate with Thiessen.

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R. Burman	D. Nagle
K. Crowe	S. Sobottka
J. Friedes	H. Thiessen
R. Kenefick	

* Editor's Note: This meeting took place at Los Alamos just before the Boulder meeting. A summary of the meeting is included here for completeness.

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