LAMS-2532 (Vol. I)

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LOS ALAMOS SCIENTIFIC LABORATORY

OF THE UNIVERSITY OF CALIFORNIA ○ LOS ALAMOS NEW MEXICO

MANHATTAN DISTRICT HISTORY

PROJECT Y

THE LOS ALAMOS PROJECT

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Printed in USA Price $ 4.00. Available from the

Office of Technical Services
U. S. Department of Commerce
Washington 25, D. C.

MANHATTAN DISTRICT HISTORY
PROJECT Y
THE LOS ALAMOS PROJECT

United States of America

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Under Secretary of War

Secretary of War

Washington, D.C., 6 August 1945

LAMS-2532 (Vol. I)
SPECIAL DISTRIBUTION

LOS ALAMOS SCIENTIFIC LABORATORY

OF THE UNIVERSITY OF CALIFORNIA LOS ALAMOS NEW MEXICO

REPORT WRITTEN: 1946 and 1947

REPORT DISTRIBUTED: December 1, 1961

MANHATTAN DISTRICT HISTORY
PROJECT Y
THE LOS ALAMOS PROJECT

VOL. I. INCEPTION UNTIL AUGUST 1945

by

David Hawkins

VOL. II. AUGUST 1945 THROUGH DECEMBER 1946

by

Edith C. Truslow
and
Ralph Carlisle Smith

Contract W-7405-ENG. 36 with the U. S. Atomic Energy Commission

This LAMS report has been prepared because of the demand for and interest in the historical information. The two volumes have not been edited except for classification purposes nor verified for accuracy. All LAMS reports express the views of the authors as of the time they were written and do not necessarily reflect the opinions of the Los Alamos Scientific Laboratory or the final opinion of the authors on the subject.

ABSTRACT

These two volumes constitute a record of the technical, administrative, and policy-making activities of the Los Alamos Project (Project Y) from its inception under the Manhattan District through the development of the atomic bomb (Vol. I), and during the period following the end of World War II until the Manhattan District relinquished control to the Atomic Energy Commission as of January 1947 (Vol. II).

Although security regulations have required some deletions in the original text of the two volumes, every effort has been made to retain the original language and expressions of the authors.

Editor's note: The name Los Alamos Laboratory was in use during the period covered by these volumes. In September 1945 the name of the contracting University of California could be made known. The present name, Los Alamos Scientific Laboratory of the University of California, was adopted in January 1947.

PREFACE

Project Y, the Los Alamos Project, has been one of a group of organizations known collectively as the Development of Substitute Materials project, (DSM), devoted to the wartime development of the atomic bomb. This branch of the DSM organization was created early in the year 1943. During the period of its existence it has been the center of activities connected with bomb development and production, as distinguished from the development and production of nuclear explosive materials.

The history of all DSM activities possesses a peculiar interest and importance, not only because of the remarkable achievements and potentialities of nuclear technology, but also because of the wartime character and motivation of its initial development. Because of its large social cost, a scrupulous accounting of the entire venture is required. Project Y has been, of itself, small compared to the other DSM projects. It has, however, occupied a crucial position. The wartime success of the entire undertaking has depended upon its success.

The nature of the present chronicle of Los Alamos is thus determined by the requirement that there exist a careful accounting of its technical, administrative, and policy-making activities. This document is a record, not an interpretation of events. Within the limitations thus implied, however, it has not been forgotten that the events recorded have taken place within a wider context, the evolution of organized scientific research and of world technology. The problems of organization and policy that lie here, sharpened by the advent of control over nuclear energies, will call for the most searching interpretation and analysis. It is hoped that in this record of fact nothing has been omitted or slighted that may be of interest to those who seek light upon questions still to be answered.

Another limitation is inherent in the nature of an official record. This is the necessary omission of many subjective factors. The success of so complex and uncertain a venture as Los Alamos depends upon its ability to extend knowledge of the explicit and publicly accountable sort at which science aims. But this ability depends, in turn, upon an accumulation of experience and skill in technical and human affairs inseparably connected with the qualities, and even the vagaries, of personality. What appears in retrospect as a natural unfolding of possibilities acquired this appearance only through the interaction and on occasion the clash of opinion, in an atmosphere dominated by the problematic and the uncertain. The omission is inevitable in an account which must itself be based upon objective evidence.

It is, however, proper to state here the writer's belief that these necessary omissions do not seriously distort the picture, as they would if important occurrences and tendencies were not objectively justified. That the pattern of development is so largely a rational one is a tribute to the unity of purpose of all concerned: administrators and scientists, civilian and military. A large share of the credit that this has been so must be given to the Director, Dr. J. Robert Oppenheimer, not only for his general leadership, but also more specifically because he understood the necessity for unity and sought in every way to foster it.

The reader will observe from the table of contents that the history of Los Alamos, Vol. I, has been divided into two periods, the first extending to August 1944, and the second from August 1944 to August 1945. This division does not correspond to any major break in the continuity of the Laboratory's work, although it does come at the time of an extensive admininstration reorganization. The real purpose of this division is to permit some chance to summarize and connect activities which, although constantly interrelated in practice, must be written about in separate chapters. And although no distinct separation into phases is possible, the date chosen marks as well as any the transition at Los Alamos from research to development, from schematization to engineering.

At this place I wish to acknowledge the assistance I have received from many members of the Los Alamos Laboratory. In particular I wish to thank the following: J. A. Ackerman, S. K. Allison, E. Anderson, K. T. Bainbridge, C. L. Critchfield, Priscilla Duffield, A. C. Graves, Elizabeth R. Graves, L. H. Hempelmann, A. U. Henshey, H. I. Miller, Emily Morrison, Philip Morrison, N. H. Ramsey, Frederick Reines, Ralph Carlisle Smith, and R. F. Taschek. These persons have materially helped me in gathering data, in drafting various sections of the report, or in extensive criticism of earlier drafts. I wish especially to thank Emily Morrison and Priscilla Duffield for ingenious researches in the records of an organization that was frequently too busy to be concerned with posterity. Mrs. Morrison has prepared the graphical material, has drafted several of the chapters, and has given invaluable general assistance. Finally it must be made clear that all errors of fact in this record are the sole responsibility of the author.

David Hawkins

August 6, 1946

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REASONS FOR NEW PROJECT

1.1 During the early period of the DSM project, the most urgent requirement was the large scale production of nuclear explosives. There could be no atomic bomb without usable amounts of fissionable materials. Both the separation of U^{235} and the production of Pu^{239} presented major scientific and industrial problems. Until these problems were on their way to solution, there was little need or time for detailed theoretical or experimental work on the mechanism of the nuclear explosion. This work had in fact not progressed very far beyond what was needed to show the probable feasibility and effectiveness of the fission bomb as a weapon for the present war. By the middle of 1942, however, it had become clear that the scientific and engineering problems connected with the development of such a weapon and its use in combat called for early and intensive effort. At this time over-all responsibility for the physics of bomb development had been given to the Metallurgical Laboratory of the University of Chicago. This organization was geared, however, to its own problems, and in particular to the development of the slow neutron chain reaction as a source of plutonium. Work on fast neutron chain reactions, looking toward bomb development, was going on, but largely under various subcontractors of the Metallurgical Laboratory.

1.2 The first step toward a more concerted program of bomb development was the appointment, in June 1942, of J. Robert Oppenheimer from the University of California as Director of the work. Although associated with the Metallurgical Laboratory, Oppenheimer carried on his work at the University of California with a small group of theoretical physicists. In his coordination of the experimental work on fast neutron physics, he was assisted by J. H. Manley of the Metallurgical Laboratory, and later by E. M. McMillan, who joined his group in Berkeley.

1.3 Late in June, a conference was called in Berkeley to discuss the theory of the bomb and plan work for the future. Present at this conference were Oppenheimer, J. H. Van Vleck, R. Serber, E. Teller, E. J. Konopinski, S. P. Frankel, H. A. Bethe, E. C. Nelson, F. Bloch. A considerable part of the discussion was devoted to a new type of explosive reaction that had been considered by Teller, a thermonuclear reaction in deuterium (1.46). There was some discussion of the theory of the shock waves produced in the chain reaction explosion, on the basis of work that had been done by Bethe and Van Vleck. Another topic was the damage to be expected in terms of energy release. This was discussed largely in a qualitative way, by scaling up from small explosions and by comparison with such disasters as the Halifax explosion. At this conference, there was a thorough review of theoretical and experimental work that had been done. By this time enough information was available so that there were no large gaps in the picture. Rough but qualitatively reliable data were available from work that had been done under Metallurgical Laboratory contracts; a good deal of relevant information had been obtained from British sources, from work done by Peierls, Fuchs, Davison, and Dirac. British theoretical results were also available. Although a fair part of the discussion at the conference was not along what subsequently turned out to be the main line of development, it served to clarify basic ideas and define basic problems. It also served to make clear that the development of the fission bomb would require a major scientific and technical effort.

1.4 Following the summer conference in Berkeley, there were a number of conferences in Chicago with experimentalists. At this time a number of subcontracts had already been let by the University of Chicago, for the purpose of pursuing the investigation of nuclear properties relevant to bomb designs. A loose organization was formed, including the subprojects at Rice Institute, The Department of Terrestrial Magnetism of the Carnegie Institution of Washington, the University of Wisconsin, the University of Minnesota, Purdue University, Stanford University, Cornell University, the University of Chicago, and the University of California.

1.5 By October of 1942, it had been decided that the magnitude of the difficulties involved made necessary the formation of a new project. Even the initial work of providing nuclear specifications for the bomb was seriously hampered by the lack of an organization united in one locality; it was clear that without such an organization the ordnance work would be impossible.

LOCATION

1.6 The site of Project Y was selected in November 1942. It was the Los Alamos Ranch School, located on an isolated mesa in the Pajarito Plateau, by highway about 40 miles north and west of Santa Fe, New Mexico. The reasons for the selection of such a site are of some interest and throw light on the character of the new project. First, there would be need of a large proving ground, with a climate suitable for outdoor work in winter. Second, the site would have to be remote from both seacoasts and the possibility - at that time not negligible - of attack. Locations might have been found which satisfied these requirements but were more accessible. The inaccessibility of Los Alamos, however, would not create serious problems for a small project such as this was intended to be. Its subsequent growth to many times its original size was not foreseen. In the light of the military security policy which prevailed at the time, inaccessibility was a deciding factor in favor of this location.

1.7 During the year 1942, steps were taken to transfer the entire DSM project from the auspices of the Office of Scientific Research and Development (OSRD) to that of the Manhattan District. The highest degree of secrecy had to be maintained throughout the entire program; the new subproject, moreover, was to be its most secret part. The need for an unusual degree of isolation was supported by two considerations. The first was inaccessibility from the outside. From this standpoint the location chosen was excellent. Access from the direction of populated areas is made difficult, except along certain roads and canyons, by a line of cliffs that mark the eastern edge of the Pajarito Plateau. The second consideration was the geographically enforced isolation of project personnel, which would minimize the possibility that secret information might diffuse outward through social and professional channels.

1.8 The choice of a site, determined by the considerations suggested above, was not the responsibility of the project director or his staff. Their views, nevertheless, had a bearing on the selection and served to strengthen in the minds of the military authorities the arguments for isolation. The task that confronted the project was not one of development and engineering in the ordinary sense. It was one of intensive and highly organized research in a region that had been only schematically explored. It required collaboration of physicists, chemists, metallurgists, and engineers in solving difficult problems, many of which could not even be anticipated until the work was well under way. The need for collaboration was made emphatic by the imposition of a definite time-scale: the bomb had to be ready for production by the time usable quantities of nuclear explosive became available. To carry out such a program successfully would require the highest kind of integration and therefore of decentralization and mutual confidence. To this end, free communication within the laboratory was indispensable.

1.9 In contrast with the requirements of scientific organization, as felt and stated by the scientific staff members, the normal military procedure for protecting secret information is one of subdivision. Each individual or working unit has access only to information immediately relevant to the work being pursued. This conflict of scientific and military requirements is, of course, not peculiar to nuclear research. Many members of the potential scientific staff were, or had been, engaged in other war research, and were from previous experience convinced of the evils of obstructing the normal flow of information within a laboratory. They were vigorously opposed to compartmentalization. Clearly, however, no alternative was acceptable which did not in some way satisfy the security requirements of the military authorities. Evidently these requirements could be met by allowing internal freedom and imposing instead more severe external restrictions than might otherwise appear necessary. The adoption of such a policy made necessary the choice of an isolated location for the project.

ORGANIZATION

1.10 The Los Alamos site, together with a large surrounding area, was established as a military reservation. The community, fenced and guarded, was made an army post. The laboratory, in turn, was built within an inner fenced and guarded area, called the "Technical Area." Both the military and technical administrations were responsible to Major General L. R. Groves, who had over-all executive responsibility for the work. The Commanding Officer reported directly to General Groves; he was responsible for the conduct of military personnel, the maintenance of adequate living conditions, prevention of trespass, and special guarding. Oppenheimer, as Scientific Director, was also responsible to General Groves, who had as his technical adviser J. B. Conant. In addition to his technical responsibilities, the Director was made responsible for the policy and administration of security. This provision represented a guarantee that there would be no military control of the exchange of information among scientific staff members, and at the same time fixed responsibility for the maintenance of security under these conditions. In carrying out his responsibilities for security, the Director was to be given the assistance and advice of a Military Intelligence Officer.

1.11 The financial and procurement operations of Project Y were handled by the University of California as prime contractor. During the early period of operations, when these had largely to do with the employment of personnel and establishing a procurement office, the University acted under a letter of intent from the OSRD, effective as of January 1, 1943. This letter was in turn superseded by a formal contract, W7405-ENG-36, effective April 20, 1943, with the Manhattan Engineer District of the War Department. The contract was retroactive to January 1, 1943. This contract, with subsequent supplemental agreements, has been the formal basis of the Project's operation throughout the rest of its history.

1.12 The financial operations of the University of California at Los Alamos were provided for by the appointment of a resident Business Officer, J. A. D. Muncy. The procurement of materials was arranged through a dual organization. In addition to the procurement division of the project, the University established in Los Angeles a special purchasing office. This arrangement was dictated primarily by reasons of security. It might be possible to determine both the nature and progress of the work from a knowledge of the nature and volume of its procurement operations. According to the procedure established, goods ordered through the Los Angeles office were received there and transshipped to Los Alamos. The procurement offices at the site were placed under the direction of D. P. Mitchell. Mitchell had for many years been in charge of laboratory procurement for the Physics Department of Columbia University, and most recently for a National Defense Research Council (NDRC) project at that University.

1.13 A statement of the responsibilities of the military and contractor organizations, and a directive outlining the scope and purpose of Project Y, were set forth in a letter to Oppenheimer dated February 25, 1943, from General Groves and Conant (Appendix 1). This letter contains also a statement of intention concerning the future organization of the project. According to this statement it was anticipated that the Project would remain an organization of the OSRD type during the first period of its operation, when it would be engaged mainly in nuclear research. During a later period of operation, when the project would be involved in the dangerous work of bomb development and assembly, it would be conducted on a military basis, with opportunity for its civilian staff members to be commissioned as officers. This anticipated reorganization proved unnecessary. Difficulties that had been expected with the initial form of organization did not in fact appear, and the plan was dropped.

INITIAL PERSONNEL, MATERIAL, CONSTRUCTION

1.14 The problem of personnel for the new organization was a difficult one. Work began at a time when the scientific resources of the country were already fully mobilized for other war work; many persons who would have been willing to join the project had other commitments which could not be broken. The nucleus of organization came from the groups that had been engaged in fast neutron work under Oppenheimer, and who transferred their work and equipment to Los Alamos. A number of other individuals and groups were released to come, in part through the assistance of Conant as Chairman of the NDRC. The greatest difficulty encountered was that of obtaining an adequate staff of technical and administrative employees, who also came mainly from occupational groups fully employed in war work. Here, moreover, the disadvantages of isolation and restriction weighed heavily, disadvantages largely overcome among the scientific staff by their interest in the work and recognition of its importance.

1.15 The principal groups and individuals who made up the initial scientific personnel are given below. Among those who had worked under Oppenheimer in the preceding period were: from the University of California, Robert Serber, E. M. McMillan, and others of Oppenheimer's group; E. Segre, J. W. Kennedy and their groups; from the University of Minnesota, J. H. Williams and group; from the University of Wisconsin, J. L. McKibben and group; from Stanford University, F. Bloch, H. H. Staub, and group; from Purdue University, M. G. Holloway and group. Among those who came from other parts of the DSM project, or from unrelated activities, were: from the Radiation Laboratory of Massachusetts Institute of Technology, R. F. Bacher and H. A. Bethe; from the Metallurgical Laboratory of the University of Chicago, Edward Teller, R. F. Christy, D. K. Froman, A. C. Graves, J. H. Manley and group; from Princeton University, R. R. Wilson and group, J. E. Mack, and R. P. Feynman; from the University of Rochester, V. F. Weisskopf; from the Bureau of Standards, S. Neddermeyer; from the Ballistic Research Laboratory at Aberdeen, D. R. Inglis; from the University of Illinois, D. W. Kerst; from Barnes Hospital, St. Louis, Dr. L. H. Hempelmann; from Memorial Hospital, New York, Dr. J. F. Nolan; from the National Research Council, C. S. Smith; from Westinghouse Research Laboratories, E. U. Condon; from Columbia University, E. A. Long; from the Geophysics Laboratory, Carnegie Institution of Washington, C. L. Critchfield. Many of these individuals were on leave from other universities, having accepted temporary war-time assignments in the above listed institutions.

1.16 In the procurement of laboratory equipment, machinery, and supplies, there were also difficulties and delays. Even a specialized laboratory requires a great variety of materials and equipment; as a going concern any laboratory depends in large measure upon the accumulation of its past, in stocks and in equipment that can be converted to new uses. Even though much material had been ordered in advance, procurement channels were at first slow, being indirect and newly organized.

1.17 Certain specialized equipment was brought to the project by the groups that were to use it. The largest single item was the cyclotron on loan from Harvard University. Before coming to Los Alamos, the Princeton group under R. R. Wilson had gone to Harvard to become familiar with the operation of this cyclotron and to disassemble it for shipment. McKibben's group brought with them from the University of Wisconsin two Van de Graaffs (electrostatic generators). Manley's group brought the Cockcroft-Walton accelerator (D-D source) from the University of Illinois. The Berkeley group brought chemical and cryogenic equipment, and all groups brought specialized electronic and miscellaneous apparatus. Because of this initial equipment, work was able to begin at Los Alamos much earlier than would otherwise have been possible.

1.18 The initial plan of the laboratory was drafted by Oppenheimer, Manley, and McMillan. It provided for an expected scientific staff of about one hundred, and a somewhat larger total number, including administrative, technical, and shop employees. The laboratory as planned contained the following buildings: Building T, an office building to provide space for administration, for the theoretical physics group, for a library, classified document vault, conference rooms, a photographic laboratory, and a drafting room; Building U, a general laboratory building; Building V, a shop building; Buildings W, X, Y and Z, specialized laboratory buildings for the Van de Graaffs, cyclotron, cryogenic laboratory, and Cockcroft-Walton accelerator, respectively. (See Appendix 4).

1.19 Oppenheimer and a few members of the staff arrived in Santa Fe on March 15, 1943. Prior to this time the project had been represented locally by J. H. Stevenson, a resident of Santa Fe. Construction work was incomplete. The laboratory buildings were still in the hands of the construction contractors, as was the housing that had been planned to accommodate Project Y and U. S. Engineer personnel. For this reason the first project office was opened in Santa Fe. Since it was undesirable for reasons of security to house the staff in Santa Fe hotels, guest ranches in the vicinity were taken over temporarily, and transportation arranged to the site. While the project office remained in Santa Fe, J. H. Williams lived at the site as acting site director.

1.20 There is no doubt that the Laboratory staff and families faced the prospect of life at Los Alamos with enthusiasm and idealism. The importance of their work and the excitement associated with it contributed to this feeling, as did the possibility of building, under conditions of isolation and restriction, a vigorous and congenial community.

1.21 The actualities of the first months were hard for many to view in this light. Living conditions in the ranches around Santa Fe were difficult. Several families, many with young children, were often crowded together with inadequate cooking and other facilities. Transportation between the ranches and Los Alamos was haphazard despite great efforts to regularize it. The road was poor; there were too few cars and none of them were in good condition. Technical workers were frequently stranded on the road with mechanical breakdown or too many flat tires. Eating facilities at the site were not yet in operation and box lunches had to be sent from Santa Fe. It was winter, and sandwiches were not viewed with enthusiasm. The car that carried the lunches was inclined to break down. The working day was thus irregular and short, and night work impossible.

1.22 Until mid-April, telephone conversations between the site and Santa Fe were possible only over a Forest Service line. It was sometimes possible to shout brief instructions; discussions of any length, even over minor matters, required an eighty mile round trip.

1.23 Frictions developed between the Laboratory members and U. S. Engineer staff mainly because of the slowness of the construction contractor. He was unable to get sufficient labor; he had trouble with the building trades unions; he did not procure or install rapidly enough the basic laboratory equipment. Pressure to accelerate this work had to be brought through, and therefore in part against, the military organization. In some cases technical supervisors were forbidden to enter buildings until they had been accepted formally by the contracting agency (The Albuquerque District of the U. S. Engineers). It was impossible to make minor changes, such as the placing of shelves or the direction of a door; the buildings had first to be completed and accepted as specified in the original drawings.

1.24 The initial problems were elementary and often enough, in retrospect, minute. The difficulties were heightened by an administrative arrangement which presupposed close cooperation without previous acquaintance between two groups of widely divergent background and perspective, namely, the project members and the military organization. Individually and in detail these early troubles are of little moment in the history of Los Alamos. Collectively, they had effects, some good and some bad, upon the spirit and tone of the emerging project organization.

Technical Introduction

1.25 The project offices were moved to Los Alamos in the middle of April; laboratory space and housing became available during April and May. Activities during the month of April can be summarized under three topics: (a) nontechnical adminstrative problems, (b) installation of laboratory equipment, and (c) discussion and planning of work. The present section will be devoted to the last topic. Topics (a) and (b) will be treated with the period following, to which they properly belong.

THE APRIL CONFERENCES

Introduction

1.26 During the last half of April a series of conferences were held at Los Alamos for the dual purpose of acquainting new staff members with the existing state of knowledge and of preparing a concrete program of research. These conferences were attended by the staff which had already moved to the project, by a few others mentioned above who could come permanently only at a later date, and by certain consultants who were specially invited. The last were I. I. Rabi of the Radiation Laboratory, Massachusetts Institute of Technology, and S. K. Allison and Enrico Fermi of the Metallurgical Laboratory, University of Chicago. All three of these men became heavily involved in the work of the Laboratory at a later time. During the conferences the project was visited by the members of a special reviewing committee which had been appointed by General Groves. This committee, whose report will be discussed, consisted of W. K. Lewis, Chairman, Massachusetts Institute of Technology; E. L. Rose, Director of Research for the Jones and Lamson Machine Co.; J. H. Van Vleck and E. B. Wilson of Harvard University; and R. C. Tolman, Vice Chairman, NDRC, secretary of the committee. Members of the committee also took part in the conferences.

1.27 Immediately prior to the conferences a set of lectures was given by Serber as a kind of indoctrination course. A summary of these lectures will provide an introduction and background for understanding the work of the conference. These lectures reflected the state of knowledge at the time. Within the scope indicated, and with much greater assurance and understanding of detail, they still constitute an adequate statement of the nuclear physics background.

Theoretical Background

1.28 Energy Release. The energy release from nuclear fission is about 170 million electron volts per nucleus. For U^{235} this amounts to about $7x10^{17}$ ergs per gram. The energy released from an explosion of TNT is about $4x10^{10}$ ergs per gram. Hence, roughly, a kilogram of U^{235} is equivalent in potential energy-release to 17,000 tons of TNT.

1.29 Chain Reaction. The large-scale release of energy from a mass of fissionable material is made possible by a neutron chain reaction. In fission the nucleus splits into two almost equal parts. These emit neutrons, on the average between two and three. Each neutron may, in turn, cause the fission of another heavy nucleus. This reaction can go on until it is stopped by the depletion of fissionable material, or by other causes. U^{238}, the principal isotope in ordinary uranium, fissions only under the impact of high-energy (about one million electron volt) neutrons. Neutrons from fission have more than this energy initially; a large percentage of them, however, are slowed by collisions to an energy below the fission threshold of U^{238}. The result is that each neutron is the parent of less than one neutron in the next generation and the reaction is not self-sustaining.

1.30 Ordinary uranium contains, however, about 0.7 per cent U^{235}. Neutrons of any energy will cause this isotope to fission; in fact, slow neutrons are more effective than fast ones. The result is that a chain reaction is just possible in the normal isotope mixture or "alloy," if a slowing-down material is added to bring the neutrons down to the velocities at which they most effectively cause the fission of U^{235}. It is this chain reaction that is used in the production of plutonium. Surplus neutrons are absorbed by the U^{238}, giving rise to the unstable isotope U^{239}, which decays by successive emission of two electrons to the end-product Pu^{239}.

1.31 If the percentage of U^{235} in uranium alloy is increased, a chain reaction becomes possible with faster neutrons. A concentration is thus reached at which no special slowing-down or moderating is needed other than what is provided by the uranium itself. The fastest possible reaction is obtained from pure U^{235}.

1.32 Critical Size, Tamper, Efficiency. In the fast chain reaction, occurring in, say, metallic U^{235} or Pu^{239}, a further limiting factor becomes crucial. In practice only a fraction of the fission neutrons will cause new fissions. The rest will leak out through the boundaries of the material. If the fraction leaking out is too large, the reaction will fail to sustain itself. If we consider a spherical mass of fissionable material at normal density, the fraction leaking out will decrease with increasing radius of the sphere, until on the average the birth rate of neutrons just compensates for the rate at which they escape from the sphere. For a smaller sphere a chain reaction will die out; for a larger one, it will continue and grow exponentially. This limiting radius is called the critical radius, and the corresponding mass, the critical mass.

1.33 It is intuitively suggested that the critical radius should be of the same order of magnitude as the average distance which neutrons travel between successive fissions. For fast neutrons this distance of flight is much larger than for slow neutrons; it is in fact 10 centimeters. Because of the great cost and limited supply of the materials available, it was essential to reduce the critical size in any way possible. If the sphere of active material were surrounded by a shell of less expensive material, this would reflect at least some of the escaping neutrons back into the sphere, and thus decrease the critical mass. Early calculation had shown that any one of several available reflector or "tamper" materials would give a very substantial reduction of the critical mass.

1.34 What has been said so far concerns only the static aspect of the nuclear bomb. Given a more-than-critical mass of active material, what is the course of the reaction? Once the reaction is started, the rate of fissioning, and hence the release of energy, increases exponentially. From the energy release the material will be heated and begin to expand. From the decrease in density of the active material the path between fissions will increase more rapidly than the radius of the expanding mass, and hence more neutrons will escape. Thus at some point the system will become subcritical and the reaction will be quenched. The point at which this quenching occurs will determine the efficiency of the explosion, that is, the percentage of active nuclei fissioned.

1.35 The time available for an efficient nuclear reaction had been shown to be extremely short. Release of 1 per cent of the energy would give the nuclear particles a mean velocity of about a million meters per second. The reaction would be quenched by an expansion of the order of centimeters; this means that the energy release would have to occur in a time of the order of hundredths of a microsecond. Since the mean time between fast neutron fissions is about 0.01 microsecond, and since the largest part of the energy release occurs in the last few fission generations, a reaction of reasonable efficiency was evidently just possible.

1.36 Cross Sections. Calculation of the static and dynamic aspects of the fission bomb presented difficulties both because of the elaborateness of the theory involved and because of the dependence of these calculations on nuclear constants that were not, as yet, well measured. Within the system a neutron may be absorbed, scattered, or produce fission. The contributions of each process are measured by the corresponding cross sections, or effective target areas presented by the nucleus to an impinging neutron. The total cross section is divided into areas that win, lose, or draw (fission, absorb without fission, or scatter), these areas corresponding to the relative probabilities of the three processes. If the scattering is not isotropic, it is also necessary to specify the angular distribution of scattered neutrons. All of these cross sections, moreover, depend upon the nucleus involved and the energy of the incident neutron. Calculation of critical mass and efficiency depends upon all of these cross sections, as well as upon the number of neutrons per fission and density of material. It was clear that to obtain such measurements with the necessary accuracy would entail an elaborate program of experimental physics and a comparable effort of theoretical physics to make the best use of information obtained.

1.37 Effects of Tamper. The effect of tamper is not only to decrease the critical mass by reflecting neutrons back into the active material, but also to increase the inertia of the system and therefore the time during which it will remain in a supercritical state. These gains are somewhat lessened by the longer time between fissions of neutrons reflected back from the tamper. The lengthening of the time is caused not only by the longer path, but also by a loss of energy through inelastic scattering in the tamper. Calculations of the effect of tamper material depend thus on the absorption and scattering cross sections of tamper material. It is interesting to note that Serber's early calculations gave, for a tamper of U^{238}, a critical mass for U^{235} of 15 kilograms, and for Pu^{239} of 5 kilograms. Both figures are correct to within a reasonable error. This may be regarded as in part good fortune, since many of the assumption made were rough guesses. It nevertheless serves to illustrate the advanced state of basic theory at the time.

1.38 Efficiency, Detonation, and Predetonation. Some indication has been given above of the basis for efficiency calculations. The outcome of such calculations was to show that efficiencies would be low. There is, moreover, another essential factor in efficiency, connected with the problem of assembly and detonation, the early discussion of which is reviewed below.

1.39 It is inherent in the nature of explosive reactions that they can be set off by relatively minute forces, the requirement being, in general, a disturbance sufficiently great to initiate some type of chain reaction. Chemical explosives can be protected with greater or less certainty from such external forces as may initiate a reaction. A supercritical mass of nuclear explosive, however, cannot be protected from "accidental" detonation. Chain reactions will begin spontaneously with greater certainty than in the most unstable chemical compounds. Cosmic ray neutrons will enter the mass from outside. Others will be generated in it from the spontaneous fissions that constantly occur in uranium and plutonium. Still others come from nuclear reactions, most importantly from the (α, n) reaction in light element impurities. The problems presented by this "neutron background" are responsible for a considerable part of the project's history. From the first and weakest source alone (cosmic rays) any supercritical mass will be detonated within a fraction of a second, from other unavoidable sources within a very much shorter time.

1.40 The only method for detonating a nuclear bomb is, therefore, to bring it into a supercritical configuration just at the time when it is to be detonated. The required speed of assembly depends upon the neutron background. As the parts of the bomb move together, the system passes smoothly from its initial subcritical to its final supercritical state. Chain reactions may, however, set in at any time after the critical position has been reached. If the velocity of assembly is small compared to the rate of the nuclear chain reaction, and if predetonation occurs, the explosion will be over before assembly for maximum efficiency has occurred. Thus the explosion may occur, with a widely varying range of efficiencies, at any time between the critical and the final supercritical positions. To decrease the probability of predetonation and consequent low efficiencies requires either a higher speed of assembly or a lower neutron background.

1.41 Gun Assembly, Initiator. The considerations of the last section indicate the magnitude of the assembly problem: to initiate properly and reliably a reaction whose entire course occurs in a fraction of a microsecond, subject to the complementary needs for high velocity assembly and low neutron background. As was mentioned above, the principal source of neutron background is the (α, n) reaction in light-element impurities. To lower this background would require a strenuous program of chemical purification.

1.42 The most straightforward early proposal for meeting these difficulties was the method of gun assembly; the general proposal was that a projectile of active and tamper material, or of active material alone, be shot through or laterally past a target of active material and tamper. For U^{235} both the chemical purity requirements and the needed velocity of assembly were attainable by known methods. Many difficult engineering problems were evidently involved, but they did not appear as insuperable. For Pu^{239} the requirements for purity and speed were both somewhat beyond the established range. It seemed, however, that by rather heroic means they could be met.

1.43 High velocity assembly and the reduction of the neutron background would decrease the probability of predetonation; they would also decrease the probability of detonation at the desired time. Unless material could be assembled so as to remain in its optimum configuration for a considerable length of time, there was a danger that "postdetonation" too would give low efficiency, or that the system would pass through its supercritical state without detonation occurring at all. To overcome this difficulty it would be necessary to develop a strong neutron source that could be turned on at the right moment. Theoretically feasible schemes for such an initiator had been conceived, but their practicability was not assured.

1.44 Autocatalysis, Implosion. Two other methods of assembly had been proposed, and it was a part of the early program to investigate them. One of these was a self-assembling or autocatalytic method, operating by the compression or expulsion of neutron absorbers during the reaction. Calculation showed that this method as it stood would require large quantities of material and would give only very low efficiencies.

1.45 The second alternative method was that of implosion.

1.46 The Deuterium Bomb or "Super." There existed, at the time of the April Conference, one other important proposal to which considerable thought and discussion had been given in the previous months. This was a proposal to use the fission bomb as a means for initiating a nuclear reaction of a different type from that involved in the fissioning of heavy-element nuclei. Fissioning, the disruption of nuclei with liberation of energy, is a somewhat anomalous reaction restricted to the heaviest nuclei. Among the lighter elements the typical exoergic (energy-producing) reaction is the building up of heavier nuclei from lighter ones. For example, two deuterium (H^2) nuclei may combine to form a He^3 nucleus and a neutron, or a tritium nucleus (H^3) and a proton. The energy that is liberated goes into kinetic energy and radiation. If such a reaction occurs in a mass of deuterium, it will spread under conditions similar to those that control ordinary thermochemical reactions. Hence the reaction is called thermonuclear. The cross section for a reaction between two deuterium nuclei is strongly dependent upon the energy of the nuclei. At low energies the probability that the reaction will occur is very small. As the temperature of the material increases, the reaction becomes more probable. Finally a critical temperature is reached, where the nuclear reactions in the material just compensate for various kinds of energy loss, such as heat conduction and radiation. The thermonuclear reaction is in detail more complicated than has been indicated, because of the presence of a variety of secondary reactions.

1.47 Among available materials, deuterium has the lowest ignition temperature. This temperature was estimated to be about 35 kilovolts (about 400 million degrees), and is actually somewhat lower. Once ignited, deuterium is about 5 times as energy-productive per unit mass as U^{235}. Thus 1 kilogram of deuterium equals about 85,000 tons of TNT equivalent. Since it is not more difficult to ignite a large than a small mass of deuterium, and since it is more cheaply produced in usable form than either U^{235} or Pu^{239}, the proposed weapon, using a fission bomb as a detonator and deuterium as explosive, could properly be called an atomic super-bomb. The development of this super-bomb was perforce secondary to that of the fission bomb; on the other hand its potentialities were so great that research toward its development could not be completely neglected.

1.48 It should be mentioned at this point that in the early period of the project the most careful attention was given to the possibility that a thermonuclear reaction might be initiated in light elements of the Earth's atmosphere or crust. The easiest reaction to initiate, if any, was found to be a reaction between nitrogen nuclei in the atmosphere. It was assumed that only the most energetic of several possible reactions would occur, and that the reaction cross sections were at the maximum values theoretically possible. Calculation led to the result that no matter how high the temperature, energy loss would exceed energy production by a reasonable factor. At an assumed temperature of three million electron volts the reaction failed to be self-propagating by a factor of 60. This temperature exceeded the calculated initial temperature of the deuterium reaction by a factor of 100, and that of the fission bomb by a larger factor.

1.49 The impossibility of igniting the atmosphere was thus assured by science and common sense. The essential factors in these calculations, the Coulomb forces of the nucleus, are among the best understood phenomena of modern physics. The philosophic possibility of destroying the earth, associated with the theoretical convertibility of mass into energy, remains. The thermonuclear reaction, which is the only method now known by which such a catastrophe could occur, is evidently ruled out. The general stability of matter in the observable universe argues against it. Further knowledge of the nature of the great stellar explosions, novae and supernovae, will throw light on these questions. In the almost complete absence of real knowledge, it is generally believed that the tremendous energy of these explosions is of gravitational rather than nuclear origin.

1.50 More immediate and less spectacular global dangers to humanity arise from the use of thermonuclear bombs, or even fission bombs, in war: principally from the possible magnitude of destruction and from radioactive poisoning of the atmosphere (13.14).

1.51 Damage. So far we have reviewed only the early discussion of energy release. Since, however, the purpose of the project was to produce an effective weapon, it was necessary to compare the atomic bomb with ordinary bombs, not merely as to energy release, but more concretely as to destructive effects. Damage could be classified under several headings: The psychological effects of the use of such a weapon; the physiological effects of the neutrons, radioactive material and radiation produced; the mechanical destruction produced by the shock wave of the explosion. Estimation of the first was not of course within the means or jurisdiction of the project. Of the second, it was estimated that lethal effects might be expected within a radius of 1000 yards of the bomb. The radioactivity remaining might be expected to render the locality of the explosion uninhabitable for a considerable period, although this effect would depend on the percentage of activity left behind, which was as yet an unknown quantity. The principal damage would be caused by the mechanical effects of the explosion. These effects were difficult to estimate. Some rough data on the effects of large explosive disasters were available. More reliable information was available concerning the effects of small high explosive bombs, but it was not known for sure how these effects should be scaled upward for high energy atomic bombs. Serber's report gives an estimate of a destruction radius of about 2 miles for a 100,000 ton bomb. Members of the British mission who came to the project somewhat later were able to add to the understanding of this topic from their national experience and their research of recent years.

DEVELOPMENT OF PROGRAM

Introduction

1.52 From the previous outline of the state of knowledge at the beginning of Project Y, it is clear that the greatest problems were bound to arise on the side of development and engineering. There was still much work to be done in nuclear physics proper, but enough was known to eliminate great uncertainties from this side of the picture. It should not be concluded, however, that the stage of research was past its prime, to be dominated in turn by problems of application. The normal meanings attached to "research," "development," and "engineering" are altered in the context of wartime science generally; that is particularly true of the atomic bomb project. Two features have determined its general character. The first is the domination of research schedules by production schedules; the second is the nature of the weapon itself. Time schedules for the production of U^{235} and Pu^{239} were such that the laboratory had before it about two years until explosive amounts of these materials would be available. After that time every month's delay had to be counted as a loss to the war. The practical consequence was that many kinds of information had to be gotten at the earliest possible date, with greatest difficulty, even though at a later date the same information could be gotten more easily and reliably. The micrometallurgy of plutonium was investigated at Chicago, for example, because it was vital, among other things, to know the density of the new material as soon as possible; the first measurement was made with great labor, from a sample of only a few micrograms. The value of such information depended upon its capacity to influence decisions which could not be postponed. This meant a heavy dependence upon theory and upon measurements of the type needed to answer theoretical questions. To some extent reliance was placed upon theoretical anticipations because of the all-or-none character of the weapon. A purely experimental nuclear explosion would involve the dissipation of at least one critical mass of material that might have been used against the enemy. If tests were to be made at all, only one or two would be possible. For so small a number of tests to be meaningful, they would have to have a large a priori probability of success. Although this question of tests was not decided at the beginning of the project, certain general implications were clear: The bomb's component parts and phases of operation had to be designated and tested separately, with reliance upon theory to supply a picture of its integral operation.

1.53 It is not remarkable, in the light of what has been said, that the initial program, personnel, and equipment of the Laboratory gave it the appearance of a purely research organization. That it had this appearance was partly a matter of previous history; nuclear research was the most advanced part of the program, and its personnel and equipment were most easily available. In part, however, the research character of the organization was a matter of considered policy. Normally, the engineer is the "practical man" who translates ideas into practice. Here, not only the ideas but also the standards of practice were new. To keep the center of policy in the research group was not to minimize the importance of the engineering work, but to emphasize its difficulty. Secondary problems undeniably arose from this policy, which displaced the engineer from his normal position, and only through trial and error created for him a new place in the division of project labor.

Theoretical Program

1.54 Enough has been said to indicate the central position in the Laboratory of its theoretical program. As it emerged from the conferences this program had as its main goal to analyze the explosion and develop the associated techniques of calculation, and to give nuclear specifications for the bomb with increasing reliability and accuracy as new physical data became available. Calculations had to be made for three materials: U^{235}, Pu^{239}, and also a new compound, a hydride of uranium, which seemed to have certain advantages over metallic uranium as a bomb material. Calculations also had to be made for a variety of shapes of the active mass, and for different combinations of bomb and tamper material. For critical mass calculations the theory of neutron diffusion in bomb and tamper had to be refined, and account taken of the energy distribution of fission neutrons, as well as the dependence of nuclear cross sections upon those energies. For efficiency calculation, further study was needed of the hydrodynamics of the explosion, taking account of the effects of the large amounts of radiation liberated in the process. Further investigation was needed of the problems connected with time of assembly, detonation, and predetonation.

1.55 In addition to these problems relating to bomb design, the theoretical program included a variety of analyses and calculations connected with the experimental program, ranging from ordinary service calculations to the design of a slow chain-reacting unit with U^{235}-enriched uranium.

1.56 The program included, finally, the further investigation of bomb damage, of the possibility of autocatalytic methods of assembly, and the proposal to amplify the effect of fission bombs by using them to initiate thermonuclear reactions.

Program of Experimental Physics

1.57 The program of experimental physics formulated during and immediately after the conferences falls under two main headings: Detailed and integral experiments. Detailed or differential experiments are those which attempt to observe the effects of isolated nuclear phenomena. From a sufficient number of experimental data gained in this way, an integral picture of the operation of the bomb could be built up within a framework of theory. Integral experiments, on the other hand, were - at least in their early conception - attempts to duplicate in experimental arrangement some of the over-all properties of the bomb. Experiments of the two kinds were intended to supplement each other wherever possible, on the one hand to sharpen the interpretation of integral experiments, on the other to show up possible omissions of elements from the detailed picture. In practice, it has proved extremely difficult to devise integral experiments which in any way duplicate the conditions obtaining in the bomb. The integral experiments that have been performed have had rather the effect of checking theory in situations in some ways similar to the bomb.

1.58 A brief outline of the program as first developed will serve also to indicate the state of experimental knowledge carried over from the previous period.

1.59 Differential Experiments.

Neutron Number. The average number of neutrons per fission had never been measured directly, although the Chicago project had measured the number of neutrons from U^{235} per thermal neutron absorbed. The number of neutrons per fission could be calculated from this measurement and from the ratio of fissions to captures, which, however, was not known reliably in the region of thermal energies. The neutron number of Pu^{239} was completely unknown although it was expected to differ but little from that of U^{235}. The first experiments planned were, in fact, measurements of neutrons from Pu^{239}.

1.60 These latter measurements were of intrinsic importance, and were needed at the earliest date possible to confirm the wisdom of heavy commitments already made for the production of plutonium in quantity.

1.61 Fission Spectrum. The energy range of neutrons from the fission of U^{235} had been investigated by the British, and by the Rice Institute and Stanford subprojects. These measurements suffered from the large dilution of isotope 235 by 238 in normal uranium. Work had already been begun at Minnesota with enriched material, and this program was to be continued at Los Alamos.

1.62 Fission Cross Sections. Fission cross sections had been measured by the subproject under N. P. Heydenberg at the Department of Terrestrial Magnetism of Carnegie Institute, by McKibben's group at Wisconsin, and by Segrè's group in Berkeley. These measurements - for U^{235} - covered the neutron energy range above 125 kev, and the range below 2 ev. When the curve for fission cross sections over the high energy was extrapolated downward, a figure was obtained for thermal energy that was much larger than the cross section actually observed. Since the extrapolated region covered the important range of neutron energies in a bomb of uranium hydride, measurements were planned to investigate cross sections at these intermediate energies and resolve the apparent anomaly. Fission cross sections of Pu^{239} were already known at thermal energies and at a few high energies. Here also measurements were planned to cover the entire range of energies up to about 3 Mev.

1.63 Delayed Neutron Emission. Experiments at Cornell had shown that there was no appreciable delay beyond 10 microseconds in the emission of neutrons from fission; one of the initial experiments planned at Los Alamos was to push this time down to 0.1 microsecond; on theoretical grounds it was expected that the number delayed even for this time would be small.

1.64 Capture and Scattering Cross Sections. At the beginning of the project little was known about capture and scattering cross sections. Some measurements of capture and inelastic scattering cross sections had been made at Chicago for normal uranium. Experiments by the Minnesota group had given values for elastic and inelastic scattering in uranium for high energies. The Wisconsin group had measured large-angle elastic scattering in a number of potential tamper materials. Capture cross section measurements were made by Segrè at Berkeley. The principal work planned for the Los Alamos Laboratory was on the scattering and absorption cross sections of U^{235} and Pu^{239}, and the capture and scattering cross sections of various tamper materials.

1.65 One new type of scattering measurement, not previously undertaken, was planned for this Laboratory. This was the measurement of scattering into different solid angles. When so averaged as to give the effective scattering in a given direction, this average is the so-called transport cross section.

1.66 Integral Experiments. Certain integral experiments had been performed at Chicago, in connection with the development of the slow neutron chain-reacting pile. These were not of direct interest to the bomb project. Two types of integral experiments were, however, planned in the early experimental physics program.

1.67 Integral Tamper Experiments. Several experiments were planned to measure the scattering in potential tamper materials; these were designed to imitate the scattering properties of a tamper in the actual bomb.

1.68 The "Water Boiler." At the April Conferences there was some discussion of the possibility of constructing a slow chain-reacting unit, using uranium with enriched U^{235} content in water solution. The construction of such a unit would provide a useful neutron source for experimental purposes, and would also give practice in the operation of a super-critical unit. The decision to make such a unit was not reached until some time later.

1.69 Experimental Techniques. A large subsidiary program was called for, to investigate techniques for producing and counting neutrons of a given energy, for measuring fissions in various materials, and for measuring neutron-induced reactions other than fission. The systematic recording of nuclear properties entailed by the experimental program required both accuracy and standardization of a number of difficult techinques; the program of instrumentation represented therefore a major activity of the Laboratory.

Program of Chemistry and Metallurgy

1.70 During the course of the DSM Project a large amount of research had been carried out on the chemistry and metallurgy of uranium. The microchemistry and micrometallurgy of plutonium were investigated at Chicago as soon as small amounts of the material were available. The chemical investigations were necessary as a basis for designing methods of recovering plutonium from the pile material and "decontaminating" it, i.e., separating it from radioactive fission products.

1.71 At the beginning of the Los Alamos Project the exact division of labor between its chemistry laboratory and other laboratories had not been settled. There were objective difficulties and uncertainties of program. It was not known whether U^{235}, Pu^{239} or both would be used, or whether the bomb material would be metal or compound. U^{233}, producible from thorium by a process of "breeding" similar to that by which Pu^{239} is made from U^{238}, was also a possibility. Mechanical requirements for the bomb material could not yet be specified. Here also a characteristic difficulty appeared, in that the time for research with gram and kilogram amounts of material would have to be as short as possible, in order to avoid delay in bomb production.

1.72 One certainty was a schedule of purity requirements for U^{235} and Pu^{239}. Because of the large alpha radioactivity of the latter substance, light impurities had almost to be eliminated. Most light elements had to be present in not more than a few parts per million. For U^{235} these tolerances could be greatly relaxed. Although it was not yet determined whether the work of final purification would be carried on at Los Alamos or elsewhere, an analytical program was necessary to develop techniques for measuring small amounts of impurity in small samples of material.

1.73 A radiochemistry program was needed to prepare materials to be used in nuclear experiments and in the development of a neutron initiator for the bomb.

1.74 The metallurgy program included research and development on the metal reduction of uranium and plutonium, the casting and shaping of these metals and compounds such as uranium hydride, as well as various possible tamper materials. Investigation of the physical properties of uranium and plutonium was needed, and a search had to be made for alloys with physical properties superior to those of the unalloyed metals. As its main service function, the metallurgy group would be called upon to prepare materials for physical and ordnance experiments, particularly projectile, target, and tamper materials for the gun program.

1.75 As a somewhat autonomous part of the chemistry program, plans were made for the construction of a deuterium liquefaction plant at Los Alamos. This was to supply liquid deuterium for experimental purposes and for eventual use in the thermonuclear bomb, should its development prove feasible and necessary.

Ordnance Program

1.76 It had been recognized from the beginning that the most difficult of all problems facing the project was to find means for the assembly of several critical masses of material, fast enough to produce a successful high-order explosion. Subsidiary but still very difficult problems were those of incorporating active material, tamper, and assembly mechanism into a practical airborne bomb. These were the problems of the ordnance division of the project, a division which could hardly be said to exist at the beginning. As a matter of fact, no pre-existing group could have had much success in this work. A new field of engineering was being explored; experience has shown that those successful in this work come from a variety of technical backgrounds, all of which contribute to the field and none of which dominate it: physicists, chemists, and electrical and mechanical engineers.

1.77 A corollary feature of the ordnance program has been its simultaneous investigation of alternative methods. The uncertainties of nuclear specification, and the possibility that one or another line of investigation might fail, have made such a policy unavoidable. Of the three methods of producing a fission bomb (autocatalysis, the gun, the implosion) that have been discussed, the last two were singled out for early development. Autocatalysis was not eliminated; but it was not subject to development until some scheme was proposed which would give a reasonable efficiency. This did not occur during the course of the project, although autocatalytic methods continued to receive considerable theoretical attention. Of the remaining two methods, the gun appeared the more practical; it used a known method of accelerating large masses to high velocities. The problem of "catching" a projectile in a target and starting a chain reaction in the resulting supercritical mass was obviously a difficult one, but it seemed soluble.

1.78 The method of implosion, on the other hand, was much farther removed from existing practice. The requirement of simultaneous detonation over the surface of a high explosive sphere presented unknown and possibly insoluble difficulties; the behavior of solid matter under the thermodynamical conditions created by an implosion went far beyond current laboratory experience. As even its name implies, the implosion seemed "against nature." Its investigation was at first undertaken as something to fall back on in case the gun should, contrary to expectation, fail. Credit for the early support and investigation of this method should be given to S. H. Neddermeyer, who at the beginning was almost alone in his belief in the superiority of the method. At a meeting on ordnance problems late in April, Neddermeyer presented the first serious theoretical analysis of the implosion. His arguments showed that the compression of a solid sphere by detonation of a surrounding high-explosive layer was feasible, and that it would be superior to the gun method both in its higher velocity and shorter path of assembly. Investigation of the method was begun almost immediately. It subsequently received two increases of priority, until at the end of the project it had become the dominant program throughout the Laboratory.

1.79 During the April conferences, the discussion of ordnance served mainly to outline the problems. Considerable attention had been given to the problem of gun design by R. C. Tolman. One member of the reviewing committee at Los Alamos in April was E. L. Rose, an expert in problems of gun design. Rose showed that by the sacrifice of durability, a quite inessential property, the otherwise prohibitive size and weight of a large gun could be reduced to a point where, together with the target, it could be included in a practical bomb. Other elements of the ordnance program discussed were: internal ballistics of the gun, external and terminal ballistics (guiding and seating of the projectile, initiating of the chain reaction), safety, arming and fuzing devices, release, and trajectory of the bomb from a plane.

1.80 It is inappropriate to discuss in detail the experimental program of ordnance at this point. Experimental work did not get under way for several months. On the agenda for immediate action were the prior problems of obtaining test guns and high explosives, of building a proving ground, and of employing or training personnel to carry on the research.

Report of the Reviewing Committee

1.81 The reviewing committee referred to in paragraph 1.26 was appointed by General Groves to report on the organization of the Los Alamos Project and on the status and program of its technical work. The chief question before this committee was the status of the ordnance program. The initial conception of the project's general program was that research in nuclear physics should be virtually completed before undertaking a large-scale ordnance development. In March 1943, however, Oppenheimer had written a memorandum on ordnance, in which he urged that experimental work be undertaken as early as possible, and that it receive recognition as one of the most urgent of the project's outstanding problems. Tolman recognized the importance of the issue thus raised, and recommended the appointment of Rose to the reviewing committee as an expert on ordnance matters.

1.82 The report of the reviewing committee, dated May 10, 1943, was concerned with the administrative organization of the project, and with the status and program of the technical work. Since certain of the recommendations of the committee had an important bearing on the further development of the project, the main features of its report are outlined below.

A. NUCLEAR PHYSICS RESEARCH

1.83 After an extensive review of the program of nuclear physics, the committee stated its approval of all of this, the most advanced part of the work. It took note of the newly discovered possibility for use of uranium hydride. Pointing out that the existence of the hydride had been learned of at Los Alamos somewhat by accident, the committee recommended a more systematic technical liaison between this and other branches of the larger project. It also recommended that the study of U^{233} as a possible explosive material be continued.

B. LESS DEVELOPED PARTS OF THE PROGRAM

1.84 The committee reported on the program for investigation of the thermonuclear reaction, the chemistry and metallurgy program, and on the program of engineering and ordnance.

1.85 As for the thermonuclear bomb, the committee recommended that its investigation be pursued, but along mainly theoretical lines, and with priority subordinate to that of the fission bomb. This confirmed the Laboratory policy already established.

1.86 Concerning both the chemistry and engineering programs, the committee recommended a substantial revision of earlier policy. One of the principal organizational questions at the time was the jurisdiction of the chemistry purification program. As stated above, the purification of active material, particularly Pu^{239}, presented a major technological problem. The chemistry of plutonium was first investigated by Kennedy, Seaborg, Segrè, and Wahl, its discoverers. The investigation was pursued and would first be practiced by the Metallurgical Laboratory chemists, in connection with their problem of separation and decontamination of plutonium produced in the piles at Oak Ridge and Hanford. It was arguable that the further step of purification, upon which such stringent requirements were placed, should be carried out by the same group. The committee recommended, however, that the purification program be carried on at Los Alamos instead. Its reasons for this recommendation were not only that the Los Alamos Project would be responsible for the correct functioning of the ultimate weapon, but also that a considerable amount of repurification work would in any case be a consequence of the experimental use of material at this project.

1.87 The second major recommendation of the committee was in agreement with the earlier statement of Oppenheimer - that the work of ordnance development and engineering should be undertaken as soon as possible. The committee stated its opinion that the time had arrived for close connection between nuclear and engineering research. While there remained from the side of nuclear specifications a wide range of possible designs for the final weapon, the committee believed that further determination of design would have to depend as well upon engineering specifications. The committee also pointed out that engineering research was needed in connection with the development of safety, arming, firing, and detonating devices, portage of the bomb by plane, and determination of the bomb trajectory.

1.88 Both the above recommendations entailed a major expansion of project personnel and facilities. For the purification program, the estimated increase of chemists and technicians was thirty, and a corresponding increase of laboratory facilities. For ordnance and engineering work, the committee estimated that this would require a two-fold increase of project personnel, with an extensive increase of offices, drafting rooms, shops, and test areas for ballistic and explosives work.

C. ADMINISTRATIVE RECOMMENDATIONS AND GENERAL CONCLUSIONS

1.89 The committee's recommendations on matters of organization and administration fall under the headings of personnel, procurement, security, and morale. Under the first the committee gave strong commendation to Oppenheimer as director. The creation of three administrative positions was recommended, as soon as competent persons could be found to fill them. The first was a director of ordnance and engineering, to take charge of the recommended program. The second was an associate director, a man in charge of some major phase of the scientific work and able to assist the director and take charge in his absence. The third was an administrative officer, to take charge of nontechnical administrative matters; in particular, to maintain cordial and effective relations with the military administration. On the general personnel situation the committee reported favorably, both as to the competence and the work assignments of scientific personnel.

1.90 The committee was dissatisfied with the organization and functioning of the procurement system. The procurement officer, Mitchell, they found to be well qualified for the position by technical training and experience. Their principal criticism was directed toward the operation of the University of California Purchasing Office in Los Angeles, which in their opinion had been responsible for serious and avoidable delays. The committee recommended establishment of a second purchasing office in New York under separate contract.

1.91 The security policy established by the Director under the authority granted him met with the committee's approval.

1.92 The final administrative recommendation of the committee, one which in its nature could not be entirely specific, concerned morale and the maintenance of the "special kind of atmosphere that is conducive to effective scientific work." The committee recognized that this was made difficult by the isolation and military character of the post, and it was therefore in the achievement of better relations between the military and technical organizations that the committee saw hope for the maintenance of morale.

SUMMARY

1.93 The period of the April Conferences and of the reviewing committee's examination of program and organization provides a natural introduction to the problems of the new project. Enough has been said to indicate that the greatest problems were connected with the need to develop a new type of engineering research, translating the schematic conception of an atomic bomb into an effective military weapon. Both objectively and subjectively, these problems were rendered more difficult by the newness and isolation of the Laboratory, and by the duality of military and technical organizations.

Chapter II

THE BRITISH MISSION

2.1 In December 1943 the first representatives of the British atomic bomb project came to Los Alamos. Their arrival marked the climax of a long series of negotiations between the British, Canadian, and American governments seeking to integrate the scientific work being done in all three countries on atomic bomb research (1.3). These first representatives were O. R. Frisch and E. W. Titterton.

2.2 Although Britain's T. A. Project (The Directorate of Tube Alloys) had had a very high priority in 1942, so many of her physicists and so much of her industrial capacity were engaged in other urgent war work that it was impossible to undertake as large a program as the United States had launched. The British organization decided to limit itself to particular phases of the problem, and established research teams in various university and industrial laboratories.

2.3 In the summer of 1942, sufficient progress toward collaboration had been made so that the British reports on the theory of fission and the fission bomb were accessible to Oppenheimer's group in Berkeley, as well as reports of experimental measurements of nuclear constants. At that time the British analysis of the bomb mechanism was somewhat more advanced than in the United States, so that access to these reports was of substantial value. In November 1942 a memorandum was written by Oppenheimer to R. E. Peierls describing the theoretical work that had been done at Berkeley and discussing certain points of difference between British and American theoretical work. The incompleteness of collaboration at this time is indicated by the fact that in the memorandum referred to there could be no mention of the deuterium bomb.

2.4 In the fall of 1943 President Roosevelt and Prime Minister Churchill had discussed the possibilities for closer collaboration between the two countries in hastening the production of atomic bombs. As a result of their discussions a Combined Policy Committee was set up in Washington. One of this committee's decisions was to move a large number of British scientists to work in American laboratories. Evidence of the genuineness of cooperation that resulted from this sacrifice on Britain's part is the fact that British scientists were given assignments in all parts of the American project, especially at Los Alamos, the most highly classified section of all.

2.5 At this time Niels Bohr, the eminent Danish physicist, escaped from Denmark to England, where he was appointed adviser on scientific matters to the British Government. His scientific advice was made available to the United States as well. Bohr and his son Aage came to Los Alamos in December 1943, a short time after Frisch and Titterton. To ensure his personal safety and as a security precaution, Bohr was known as Nicholas Baker and his son as James Baker. Great care was taken to prevent any reference to their real names, even in classified documents. The Bohrs did not become resident members of the Los Alamos Laboratory, but made several extended visits as consultants.

2.6 When Bohr came to the Laboratory he found there a large number of his former students, and his coming had a very healthy influence on research. He came at the right moment. The exigencies of production, the innumerable small problems which confronted the physicists, had led them away from some of the fundamental problems of the bomb. The study of the fission process itself, for example, had been neglected, and this obstructed reliable predictions of important phenomena, such as the energy-dependence of the branching ratio between fission and neutron capture (6.44). Here Bohr's interest gave rise to new theoretical and experimental activities which cleared up many questions that were left unanswered before. Some of the most important experiments on the velocity selector were made at his instigation (6.38). His influence was felt strongly in research on the nuclear properties of tamper materials.

2.7 Bohr's criticism and his concern for new and better methods enlivened the discussion of alternative means of bomb assembly. Although these discussions showed in the end that the "orthodox" implosion was still the best method, their value was to prove that its choice was, despite its many difficulties, the correct one. Bohr participated very actively in the design of the initiator.

2.8 Last but not least, his influence on the morale of the Laboratory must be mentioned. It went further than having the great founder of atomic research in the Laboratory, and farther than the stimulus of his fresh suggestions. He saw the administrative troubles of the Laboratory in a better and longer view than many of those enmeshed in them. His influence was to bring about stronger and more consistent cooperation with the army in the pursuit of the common goal. And what can be least overlooked, he gave everybody who was in contact with him some of his understanding of the ultimate significance of the control of atomic energy.

2.9 Another of the Laboratory's most useful British consultants was Sir Geoffrey I. Taylor. Los Alamos was staffed primarily with nuclear physicists, who lacked experience with hydrodynamical investigations. In investigating the hydrodynamics of the implosion and the nuclear explosion, therefore, their work suffered from being too formal and mathematical. Apart from the contributions of the American consultant John von Neumann (7.54), most of the simple intuitive considerations which give true physical understanding came from discussions with Taylor. His most important general contribution was the understanding of the "Taylor instability," which is the generalization that when a light material is pushing a heavy material, the interface between them is unstable (5.26). This principle was important in the theory of jets, in the interpretation of high-explosive experiments, in the design of the initiator, in the design of the implosion bomb, and in the predictions about the nuclear explosion (5.43). To him also was due the stimulus for serious theoretical investigation of the "ball of fire" phenomena (11.20).

2.10 Technical contributions of the resident staff of the British Mission are mentioned in appropriate parts of the text on the same basis as the work of their American colleagues.

2.11 Sir James Chadwick of the Cavendish Laboratory, scientific adviser to the British members of the Combined Policy Committee in Washington, came to Los Alamos early in 1944 to head the British Mission. It was not certain at first whether the British group would work under Chadwick on the problems of his choosing, or whether they would be assigned to existing groups in the Laboratory. The latter arrangement was adopted, and eventually British scientists worked in nearly all of the Laboratory divisions. Seven were experimental nuclear physicists, two were electronics experts, five were theoretical physicists, and five were experts in the properties and effects of explosives.

2.12 Lord Cherwell, Churchill's personal adviser on scientific matters, visited Los Alamos in October 1944.

2.13 Chadwick stayed in Los Alamos only a few months. His successor as head of the Mission was Peierls.

2.14 Apart from the consultants already mentioned, the British Mission staff consisted of the following: E. Bretscher, B. Davison, A. P. French,

O. R. Frisch, K. Fuchs, J. Hughes, D. J. Littler, Carson Mark*, W. G. Marley, D. G. Marshall, P. B. Moon, W. F. Moon (secretary), R. E. Peierls, W. J. Penney, G. Placzek, M. J. Poole, J. Rotblat, H. Sheard, T. H. R. Skyrme, E. W. Titterton, J. L. Tuck.

*Although several members of the Mission came to Los Alamos via the Canadian and United Kingdom Laboratory in Montreal, all were attached to the British staff except Mark, who remained in the employ of the Canadian Government.

Chapter III

THE PERIOD APRIL 1943 TO AUGUST 1944, GENERAL REVIEW

General Administrative Matters

LABORATORY ORGANIZATION

3.1 The first period of the Los Alamos Laboratory's existence presented the problems common to organizational beginning: the definition of program, the division of responsibilities, and liaison. Of these the first has been discussed in the first chapter. The division of responsibilities follows that of the program: Experimental Physics, Theoretical Physics, Chemistry and Metallurgy, Ordnance. Each of these was organized as an administrative division, consisting of a number of operating units or groups. Group Leaders were made responsible to their respective Division Leaders, and Division Leaders to the Director. In a position of responsibility parallel to that of the Director was established a Governing Board. This consisted of the Director, Division Leaders, general administrative officers, and individuals in important technical liaison positions.

3.2 The building of the Laboratory was more than the planning and implementing of its technical work. Especially at first the Governing Board meetings were the only regular occasions for viewing in a general political way the many questions that appeared. As a center for planning and policy-making, the Board considered a wide variety of topics.

3.3 On the technical side the Board provided a means for relating the work of the different divisions, and for relating the program of the Laboratory to other Manhattan District activities. It heard reports of the latest nuclear calculations and measurements, and on the basis of these set basic specifications for Ordnance and Chemistry. As experimental and design data became available from Ordnance, the Board set fabrication requirements for the metallurgists to meet.

3.4 The progress of procurement and production was frequently reviewed, particularly of active materials and separated isotopes needed in the program. The Board supervised the liaison with other project laboratories on these and related matters.

3.5 For the first eight months perhaps two-thirds of the Governing Board's time was devoted to lay matters. Frequent topics were housing, construction and construction priorities, transportation, security restrictions, personnel procurement, morale, salary scales, and promotion policy. In most of these discussions, the Governing Board again provided a link, here between technical program and general administration.

3.6 The adversities of the first months are illustrated by a few very minor items chosen at random. In the first meeting of March 30, 1943, it was mentioned with some triumph that a calculating machine had finally been obtained, on loan from the Berkeley Laboratory. The scarcity of transportation is illustrated by the fact that a request for assignment of one pickup truck was brought, for decision, as high as the Governing Board. In May, the housing shortage was so serious that the Board took upon itself the assignment of the six remaining apartments.

3.7 The membership of the Governing Board was: Bacher, Bethe, Kennedy, Hughes (3.20), Mitchell, Parsons (7.3), and Oppenheimer. Later additions were McMillan, Kistiakowsky (7.55), and Bainbridge (7.4).

3.8 A short time after the beginning of the Laboratory, a Coordinating Council was established, whose membership was at the Group Leader level or above. In contrast to the Governing Board, the Coordinating Council was not a policy-making body, although at times policy problems were delegated to it; for example, the Coordinating Council was asked to establish criteria for deciding which members of the Laboratory should be classed as staff members with unrestricted access to classified information. Its meetings were generally informative rather than deliberative, consisting of reports of an administrative and technical character. Since its members were the heads of operating groups and were collectively in contact with all members of the Laboratory, it served also as a vehicle of general opinion concerning technical, administrative, and - on occasion - community affairs.

3.9 Divisions and groups, in turn, held their own regular meetings and seminars. These, together with informal discussions and regularly published reports, were the main vehicles of technical information in the Laboratory.

3.10 There was, finally, a weekly Colloquium which all staff members were privileged to attend. Staff members, as distinguished from other

Laboratory employees, were defined as those with scientific degrees or equivalent training in the field of their work, and therefore presumed capable of giving or receiving benefit in general discussions of the technical program. The Colloquium was less a means of providing information than an institution which contributed to the viability of the Laboratory, to maintaining the sense of common effort and responsibility.

3.11 Among all these channels of communication the Colloquium raised the most serious question of policy. From a narrowly technical point of view it was the least easy to justify; on the side of military security it appeared to present the greatest hazard. Regular attendance would give any staff member a generally complete and accurate picture of the problems and progress of the Laboratory. Just this, however, was its purpose. Any essential withholding of scientific information from the Colloquium would have defeated this purpose, and would have represented a compromise of basic policy. In practice, the relatively scientific and academic tone of Colloquium discussions made it possible to avoid mention of many matters of relatively small scientific, and relatively great tactical, value; where this was not possible the tactical value of information was sometimes lessened by omission of quantitative details. Despite these qualifications it remains true that the policy adopted concerning communication represented a considerable departure from the customs normally surrounding the protection of military secrets.

LIAISON

3.12 The last organizational problem, establishment of liaison, presented somewhat unusual difficulties, reflecting the complexity of Manhattan District organization. As the reviewing committee had pointed out (1.83), it was important that some machinery be established for the interchange of pertinent information between this and other branches of the project. The isolation of Los Alamos even from other branches of the project was a basic policy of the Manhattan District. Apparently it was difficult to separate the virtues of this isolation from its vices: the needed liaisons were achieved to any extent only after the most earnest representations.

3.13 The procedure established in June 1943 for liaison with the Metallurgical Laboratory at Chicago is fairly typical. Permission was given for the exchange of information by correspondence between specified representatives of the two projects or by visits of the Los Alamos representatives to Chicago. Information was restricted to chemical, metallurgical, and certain nuclear properties of fissionable and other materials. It was permissible for the representatives to discuss schedules of need for and availability of experimental amounts of U^{235} and Pu^{239}. No information could be exchanged on the design or operation of production piles, the design of weapons, or to permit comparison of schedules of need for and availability of production amounts of active materials. Three members of the Los Alamos Laboratory were to be kept informed of the time estimates for production of large amounts of these materials. In addition to the above, it was agreed that special permission would be granted by the office of General Groves for visits to Chicago by other members of this Laboratory to discuss specific matters.

3.14 As the program developed, a number of topics were of great interest to the workers at Los Alamos. Information was needed on the results of chemical and metallurgical research at the Metallurgical Laboratory and at the University of California and Iowa State College. This work was concerned with the chemistry and metallurgy of uranium and plutonium and methods for the analysis of impurities in these substances. Information was needed on results of nuclear research at the Argonne Laboratories at Chicago. It was important to know when materials would become available from the production plants at Oak Ridge (Site X), the form in which the material would be received, and the processing which it would have undergone. It was also essential to know the analytical procedures to be used by the production plants in determining the impurities and active content of this material.

3.15 The need for careful information on time schedules of production was the most urgent and difficult part of this problem. The estimates received during the first summer of the Laboratory were vague, incomplete, and contradictory, so that it was difficult to make sensible schedules of bomb research and development. The Governing Board in fact said that with the existing state of information scheduling was impossible, and that unnecessary delays would certainly result from this kind of blind operation. It was strongly urged by the Board that Los Alamos maintain a full-time representative at Oak Ridge. An agreement was finally reached in November 1943, by which Oppenheimer was permitted to visit the production plants at Oak Ridge. When material began to arrive at Los Alamos in the spring of 1944, the situation improved somewhat of itself.

3.16 The need for getting information required by the Ordnance and Engineering Division presented special difficulties. Most of this information had to be sought in agencies outside the Manhattan District. Knowledge of the purpose and even the existence of Los Alamos had to be concealed from them. Many devices were used: blind addresses, a Denver telephone number, NDRC identification cards. The office of Dr. Tolman, Vice-Chairman of NDRC, was instrumental in obtaining reports for this Laboratory on such subjects as gun-design, armor plating, explosives, detonators, bomb damage, etc. The liaison with Army and Navy Ordnance, and with the Army Air Force, will be discussed later (7.67ff and Chapter XIX).

3.17 Among the more troublesome and less obvious liaison needs were those required with the University of California and within Los Alamos itself. Although the work was to be carried out under a more or less standard type of War Department contract, the University of California was, in matters of policy, virtually unrepresented at the site. Security regulations and practices were such, moreover, that its officers were excluded from discussions of technical and administrative policy, and were allowed to concern themselves almost exclusively with a rather narrow range of legal and contractual affairs. At the Los Alamos site there were two administrative offices, that of the military and that of the Laboratory. Even though the division of labor was defined in a general way, most of the difficulties of dual organization had to be lived through before effective cooperation was established. Because of security policy, the officers charged with administering the community and post were for the most part in ignorance of the Laboratory's work. Thus, although the Manhattan District was the basic organization in the DSM project, its local military representatives were excluded from the sphere of Laboratory policy. Added to these difficulties, and complicated by them, were the troubles of life in an isolated and unpractised community.

3.18 Under such circumstances a very great administrative burden fell upon the shoulders of the Director. Whereas his primary responsibility was the success of the scientific program, it was equally his concern that this success not be jeopardized by extraneous difficulties. The administrative recommendations of the Reviewing Committee had been aimed principally at improving this situation. Apart from the specific difficulties of the procurement office, the committee's main concern had been the need to improve relations between the Laboratory and the Post Administration, and to relieve the Director of as many nontechnical administrative responsibilities as possible. At the beginning, the job of operating the project was taken over by a temporary organization of scientific staff members and technicians. The important thing was to avoid delay in research work. In the way stood a host of small problems: transportation, warehousing, procurement, planning of laboratory construction, and housing. The enthusiasm with which these jobs were undertaken was notable, as was the *esprit* that developed in the process. There was in it, nevertheless, an element of antagonism between the Laboratory and the military organization. However justified or unjustified this antagonism may in particular cases have been on either side, it set a general problem for the future. For those who have lived through the course of the project, what stands out is not this initial element of conflict, which only reflected the diversity of

American life, but the fact that through common purpose and by the measure of actual accomplishment, this conflict was reduced to secondary importance.

3.19 The members of the staff were considerably heartened by a letter which Oppenheimer read at a colloquium early in July. The letter dated June 29, 1943, was from President Roosevelt and said, in part: "I wish you would express to the scientists assembled with you my deep appreciation of their willingness to undertake the tasks which lie before them in spite of the dangers and the personal sacrifices. I am sure we can count on their continued wholehearted and unselfish labors. Whatever the enemy may be planning, American science will be equal to the challenge. With this thought in mind, I send this note of confidence and appreciation."

3.20 Apart from the business and procurement offices, the administrative organization of the Laboratory had only two officers other than the Director. These were E. U. Condon of Westinghouse Research Laboratories, and W. R. Dennes of the University of California. Of these, neither had fully determined to remain with the Project, and both did in fact leave, Condon in May, and Dennes in July of 1943. The reviewing committee had recommended the appointment of an associate director, and of an administrative officer to coordinate nontechnical administrative functions and to act as liaison with the Post Administration. It was possible to fill neither position at the time. Certain urgent requirements were met, however, by the appointment of new administrative officers. David Hawkins of the University of California came in May 1943 to take the position of liaison with the Post Administration. D. L. Hughes, Chairman of the Department of Physics, Washington University, St. Louis, Missouri, was made Personnel Director in June. B. E. Brazier, formerly of the T. H. Buell Company, Denver, came to the site in May to take charge of construction and maintenance. In January 1944, David Dow of the legal firm of Cadwalader, Wickersham and Taft, New York, was appointed Assistant to the Director, in charge of nontechnical administrative matters.

3.21 By July 1944, the Administration of the Laboratory was organized into the following groups:

A-1	Office of Director	D. Dow
A-2	Personnel Office	C. D. Shane (Assistant Director)
A-3	Business Office	J. A. D. Muncy
A-4	Procurement Office	D. P. Mitchell (Assistant Director)
A-5	Library, Document Room, Editor	C. Serber, D. Inglis
A-6	Health Group	Dr. L. H. Hempelmann
	Maintenance	J. H. Williams
	Patent Office	Major R. C. Smith

Personnel Administration

3.22 The administration of the Laboratory was faced at the beginning with a conflict of form and content. Because of the newness of large-scale organized research, there does not exist for it a class of professional scientific administrators. In the main a choice had to be made between a large administrative organization staffed with persons unacquainted with the peculiarities of scientific research, and a system by which the major share of administrative responsibility fell to the scientists themselves. Here again as with the engineering program it was partly a matter of expediency and partly of policy that the center of gravity remained in the scientific staff. The policy adopted meant, especially at the beginning, a gain of unity in the Laboratory. It entailed, undeniably, a loss of administrative efficiency.

3.23 The Personnel Office, in particular, illustrates these remarks. The Director, Hughes, was a physicist with administrative experience as Chairman of the Department of Physics, Washington University. The organization of the Laboratory was such that the Personnel Office was almost entirely dependent upon the representations of Divisions and Group Leaders.

3.24 Apart from its connection with the Divisions and Groups of the Laboratory in matters of employment and salary, the Personnel Office had charge of a Santa Fe office of the Laboratory (for receiving and employment), and of the Housing Office at the site. Under its jurisdiction fell personnel security, draft deferment, placement of military personnel assigned to the Laboratory, and certain miscellaneous matters. Although the scope of the present history does not include the affairs of the Los Alamos Community, the Laboratory became administratively involved in a number of these - particularly when, through their effects on the morale of the Laboratory staff, they had a bearing upon the success of the work. Although these matters were not all under the direction of the Personnel Office, they belong by their content to the present section.

HOUSING AND OTHER COMMUNITY AFFAIRS

3.25 One of the most urgent community problems at the beginning was the construction and organization of a school for the children of Los Alamos residents. There had been at the old Los Alamos Ranch School a small public elementary school for the children of its employees. In view of the Laboratory's small original size it was believed that the old building would be adequate for the project's elementary school, and that a high school could be established, making use of another of the original Los Alamos buildings. This plan soon proved unfeasible, and a school committee was appointed by the Director and the Commanding Officer, Col. J. M. Harmon, succeeded shortly thereafter by Lt. Col. Whitney Ashbridge. The committee made plans for a school building, and supervised the planning of curriculum and employment of teachers. The committee employed W. W. Cook of the University of Minnesota as consultant. A building to house the elementary and high schools was designed by Cook and Brazier. Construction was begun late in the summer of 1943, and by virtue of a high construction priority was completed in time for the opening of a fall school term. The committee was continued as a school board.

3.26 The elementary and high schools were operated as free public schools, salaries and procurement expenses being borne by the Government through the contractor. A nursery school, for which a building had been provided in the original plan of construction, was operated on a partially self-supporting basis. This school made possible the part-time, or more rarely full-time, employment of women with young children. In this case the financial deficit was also carried by the Government.

3.27 Another matter in which the Laboratory administration was interested was that of community representation in the civil affairs of Los Alamos. In June 1943, a "Community Council" was established and its members elected by popular vote. This superseded an earlier appointed committee. It was intended to be purely advisory in its function. In its first form it was a body elected only by the members of the Laboratory and their wives, and did not represent the entire community. It was advisory, not to the Commanding Officer, but to the Laboratory administration. In August 1943 a more representative council was approved by the new Commanding Officer, Lt. Col. Whitney Ashbridge, and the Laboratory Director. This council met with representatives of the Laboratory and the Commanding Officer. The Council was regarded by some as a thorn in the side of the community administration. At times it was. The council sought, however, to guide its deliberations and recommendations by the single standard of the success of the project. Sometimes its recommendations could not be carried out because of limitations of manpower and material. Sometimes the limitations derived from the customs of army administration. On the other hand many recommendations were accepted. Under the guidance of the council a system of small community play areas were built for the children of the Post. Traffic laws were written with the advice of the council, which also acted as a traffic court under a voluntary fine system. Other topics frequently considered were: the operation of post exchange, messes, commissary, milk supply, maid service, public transportation, and hospital.

3.28 A major community problem, which dogged and in many ways hampered the Laboratory from the beginning, was housing. Los Alamos was originally conceived as a small community of research scientists, more or less stationary in character, whereas in fact it developed into a large and complex industrial laboratory. Much of this development could not be foreseen, coming as it did from self-development of the research program. The housing problem was such as to put a constant drag upon the efforts of the Laboratory to get and keep an adequate staff.

3.29 Construction at Los Alamos was not easy. Growth of population strained power and water supplies. Construction was expensive of critical manpower and materials; the presence of a large group of construction workers put a further strain on community facilities. These difficulties, moreover, plus a constantly shortening period of amortization, necessitated corresponding cheapening of construction. To the shortage of housing, therefore, was added a troublesome inequality.

3.30 The drag upon Laboratory expansion caused by the difficulties of maintaining an adequate rate of housing construction is illustrated by the fact that it was twice necessary, and a third time almost necessary, to make use of outside housing facilities. It has been mentioned that at the very beginning of the project members of the Laboratory had to be housed temporarily in nearby "guest ranches." By the beginning of summer, 1943, the original housing accommodations were filled, and new housing was not yet provided. For the period of June 19 to October 17, therefore, the project acquired from the Park Service, and operated Frijoles Lodge at the Frijoles Canyon headquarters of the Bandelier National Monument, fourteen miles from Los Alamos. After its acquisition for the purpose by the Albuquerque District Engineers, Frijoles was operated under the jurisdiction of the Personnel Office. For this purpose the Laboratory obtained the services of S. A. Butler, Assistant Manager of La Fonda, Santa Fe, who later became Assistant Personnel Director. Frijoles Lodge was used again from July 17 to August 5, 1944, when the project faced another critical housing shortage.

3.31 Another facility in which the Laboratory had an administrative interest was the community hospital. This hospital was operated for the benefit of military and civilian personnel at Los Alamos, under the jurisdiction of the Chief of the Medical Section of the Manhattan District. The existence and excellent record of this hospital was an important contribution to project morale. Another important function of the hospital was its cooperation with the health and safety program of the Laboratory, whose work is discussed in detail in a later section (3.87ff).

SECURITY ADMINISTRATION

3.32 It would be difficult to exaggerate the security precautions that were taken at the beginning of the project, particularly in connection with personnel. During the early period, moreover, the administration of security policy was a matter of importance not only in safeguarding information, but also because of the effects of restriction on morale, and the possibility that serious breaches of security might lead to the imposition of even more stringent external control.

3.33 Formal clearance of personnel for work on the project was arranged through the Intelligence Officer stationed at the site. This procedure was slow and cumbersome, especially in the first months. In September 1943, a plan was approved to supplement clearance where necessary by an interlocking system of vouching for the loyalty and good faith of the members of the Laboratory.

3.34 The administration of security matters pertaining to Laboratory personnel and their families was delegated to Hawkins as Contractor's security agent, with the assistance of a security committee composed of himself, Manley, and Kennedy, meeting with the Intelligence Officer.

3.35 Recurring topics of discussion in the security committee were the pass and badge system, the monitoring of the Laboratory for classified material left unattended, the means of preventing classified discussions in the presence of outsiders, the publication and revision of security regulations.

3.36 The most irksome restrictions placed on the Laboratory staff were those affecting personal freedom. Travel outside a limited local area and any contact with acquaintances outside the project were forbidden except on Laboratory business or in cases of personal emergency. In the main these restrictions were accepted as concessions to the general policy of isolation. A small group thought they were not strict enough, and no one was satisfied with the working definitions of "personal emergency." The removal of these restrictions in the fall of 1944 was a cause of general relief after a year and a half of extreme restriction. Another feature of the security policy of Los Alamos was censorship of mail. This was unusual in itself, and amusing in the circumstance from which it began, namely, the suspicion of unannounced censorship. Not long after the Laboratory began, this suspicion spread as a rumor. A certain amount of evidence that letters had been opened was presented, varying considerably in quality. Once started, such a rumor would no doubt have spread in any case. The Director, who was in no position to guarantee that such censorship was not occurring, made strong representations to the office of General Groves. An investigation was instituted by General Groves, which brought a negative result. Under the circumstances, however, it was urged by many members of the Laboratory that official censorship be instituted and this was done in December 1943. Once begun, censorship did serve as a deterrent to the inadvertent spreading of information about the project, of a sort which might contribute to consistent rumors and continuing public interest in its activities. Censorship was carried on by a standard military censorship office located in Santa Fe, under the direction and with the advice of the Intelligence Officer, Captain P. de Silva.

SALARY POLICY AND ADMINISTRATION

3.37 The most pressing problem of personnel administration in the early months of the Laboratory was that of salaries. Salaries for scientific employees were determined by either of two standards. One standard was the OSRD scale, based upon scientific degrees held and number of years since their conferment. The other was the "no loss no gain" principle, with provision that individuals from academic positions, whose salaries are normally based on a ten-month year, be paid at twelve-tenths their previous rate. One source of difficulty was that men from industry had received a higher rate of pay than those from academic positions. Another was that technicians, men without academic degrees but often with considerable technical skill, had to be employed at the prevailing rates in this labor market. Although technicians ranked below the younger professional scientists, they often received higher salaries. A final difficulty was that a general commitment had been made to a policy of length of service and merit increases, but that no administrative mechanism existed to implement it.

3.38 The first major responsibility of Hughes, upon his arrival in June 1943, was to prepare a set of recommendations on salary policy, based upon a survey of this and other comparable laboratories. This statement of policy proposed within the regulations of the National War Labor Board a salary scale for the various classes of Laboratory employees, and a plan for wage and salary increases. According to this plan the younger scientists would be employed at a rate determined by the OSRD scale previously followed, and their rate of salary increase determined accordingly. No provision was made for increase of salaries above $400 per month, which were virtually frozen.

3.39 The proposed salary scale and schedule of increases was presented for approval to the Manhattan District and the University of California in June. Approval was, however, postponed. A further effort to obtain approval was made in October and again in December. At this time it was learned that certain formal changes had to be made because of changes in national policy. After appropriate modifications had been made, approval was further postponed until January, when a conference was held on the subject at Los Alamos. Approval was finally granted February 2, 1944, after a year of operation. During this period no system of promotion was possible, although the proposed policy was followed in determining the salaries of individuals newly hired.

3.40 The chief difficulty in matters of salary increase and promotion concerned the younger scientific group who had been hired under the OSRD scale. This scale, being based on length of time since conferment of academic degrees, made provision for an annual salary increase, which however would not be approved by the Contracting Officer, Lt. Col. S. L. Stewart, in the absence of an approved Laboratory salary policy. Inequities, as measured by the degree of responsibility and usefulness of various individuals, were numerous both within this OSRD group and between it and those who had been employed on a "no loss no gain" basis.

3.41 Final agreement about salary policy was not reached until the end of the war, but improvement resulted from a reorganization in July 1944 (3.56ff) at which time a new working agreement was reached.

DRAFT DEFERMENT

3.42 The Laboratory policy of draft deferment reflected its general personnel and security policies. Because of the absolute scarcity of trained scientists and technicians in the United States during the war years, every effort had to be made to prevent induction of men in these categories whose services were essential and satisfactory. It was desirable from the standpoint of security that the turnover of such personnel be kept at an absolute minimum. On the other hand, the requirements of secrecy made it impossible to give Selective Service any real information concerning the nature and importance of the Laboratory's program, or of the work of an individual. The average age of scientific and technical employees, moreover, was under thirty, which placed the great majority in the draft-vulnerable category. (See Graph 1 in the appendix section.)

3.43 Because of the very importance of the project, paradoxically, deferment of Laboratory employees was a matter of some complexity.

Dennes came to the Laboratory empowered to act in deferment matters as representative of the University War Council. The position was later assumed by Hawkins. By the time of his departure Dennes had rescued relations with Selective Service from the confusion unavoidable in the first days of Laboratory organization.

3.44 The most essential Selective Service liaisons were with the New Mexico State Director of Selective Service and the Selective Service Agencies of the Manhattan District. From the former the Laboratory enjoyed the utmost cooperation in all matters pertaining to Selective Service rules and policies, and their interpretation. From the Selective Service Office of the Manhattan District and from the Washington Liaison Office the Laboratory received the greatest consideration in difficult individual cases.

3.45 Most developments in draft deferment procedure were only technical and did not reflect a change of policy. As the war progressed and the needs of the Army and Navy increased, deferment requirements became more stringent. The Laboratory therefore depended increasingly upon official certification of its needs by the Manhattan District. In February 1944, the War Department adopted a policy forbidding the deferment of men under 22 years of age in the employ of the Department or its contractors. There was in the Laboratory a small but highly trained and essential group under 22. Under the circumstances they could not be deferred. When these men were inducted, therefore, there was no choice but to have them reassigned to the Laboratory as members of the Special Engineer Detachment.

PERSONNEL PROCUREMENT

3.46 Some mention has been made in Chapter I of the difficulties in staffing the original Los Alamos Laboratory. Its subsequent growth, moreover, was such that the working population doubled, on the average, about every nine months. Although a declining proportion of new employees were of scientific staff classification, the absolute number increased month by month until almost the end of the war. (See Graph 2.) At the same time the difficulty of finding competent scientists increased. The difficulty was greatest in the upper technician and junior scientist brackets. Senior scientists were needed in small numbers, and were usually well known to members of the Laboratory. They were, in many cases, anxious to join the Laboratory, and releases from less critical work, or in some cases from other Manhattan Projects, could be obtained through the efforts of the Washington Liaison Office. Junior men were needed in great numbers; recruiting trips to universities, however, were impossible because of security regulations. In November 1943, the assistance of Dean Samuel T. Arnold of Brown University was obtained in these matters. An arrangement was also made with M. H. Trytten of the National Roster of Scientific and Technical Personnel by which he could spend a part of his time visiting universities and employing young scientific personnel for the Laboratory. Trytten was of assistance to the Laboratory for a period of several months. Arnold remained as liaison in Washington in personnel matters throughout the course of the project.

MILITARY PERSONNEL

3.47 A small number of officers of the Army and Navy with scientific training were obtained at various times for work in the Laboratory. The largest group of military personnel in the Laboratory came, however, from the Women's Army Corps, and as enlisted men in the Special Engineer Detachment (SED). The latter detachment was originally established as a small detachment (about 300 for all Manhattan projects) in which men essential to the work of the Manhattan District could be placed in cases where deferments were no longer possible. At a time when junior scientific personnel were extremely difficult to find (November 1943), the Laboratory was informed that a group of new graduates of the Army Specialized Training Program would be available at the beginning of the year and could be assigned to the Laboratory in the SED.

3.48 Although it remained the basic policy of the Laboratory that its work should be carried out on a civilian basis, it had become clear that young civilians, of the type most urgently needed, were increasingly difficult to find. They were in fact being rapidly inducted into the Army, where in many cases their assignment would be less appropriate to their training than if they were transferred to the SED. The inconsistency and potential personnel difficulties involved in obtaining these men were fully appreciated. In view, however, of the Selective Service policy that resulted in the induction of many men from essential fields already seriously undermanned, there was no choice but to welcome into the Laboratory all technically trained enlisted personnel for whom civilian counterparts could not be found. From a tabulation made in May 1945, it was found that 29 per cent of all SED personnel held college degrees, including several Doctor and Master degrees. Most of the degrees were in the fields of Engineering, Chemistry, Physics, and Mathematics. (See Graphs 2, 3 and 4.)

3.49 In the case of the WAC detachment also, several competent scientists were obtained, as well as a larger number of technical and office workers. (See Graph 4.)

3.50 The personnel policy regarding enlisted men and women was in essence identical with that for civilians, with obvious adjustments. After arrival at the site and assignment to the Laboratory, all further matters of placement, job classification, transfer, and promotion within the Laboratory were under the jurisdiction of the Laboratory Personnel Office.

3.51 The establishment and rapid growth of the SED at Los Alamos brought a number of administrative problems connected with the morale, accommodations, and working conditions of the group. The most serious problem arose from the shortage of multiple unit housing, which made it impossible for the Post Administration to provide quarters for married enlisted men. Further, Major P. de Silva objected to the hiring (except as nurses) of the wives of enlisted men, although they could have been quartered in the dormitories for women workers on the project. Also security regulations made it impossible for them to bring their wives to Santa Fe or other nearby communities. Security restrictions against travel and association with persons away from the project worked therefore a very much greater hardship on enlisted personnel than on civilians, whose wives and children lived at Los Alamos.

3.52 Another problem was created by the fact that military promotions, which were the responsibility of the SED Commanding Officer, were also the only material means available for recognition of responsibility and excellence in technical work. The SED Table of Organization permitted promotion of one-third of the men to each of the grades T/3, T/4, T/5, with the provision that about one-tenth of those in T/3 could be promoted to the ranks of Technical and Master Sergeant. Since the great majority of the men arrived with a rank no greater than T/5, there was, at least in the first period, ample opportunity for promotion. The ground for and rate of promotion had, however, to be agreed upon between the SED Commanding Officer and the Laboratory, and for several months no such policy was firmly established or consistently followed.

3.53 A third difficulty arose from the conflict of military and technical duties. Although the official hours of work in the Laboratory were eight hours a day for six days a week, it was the practice of many groups in the Laboratory, particularly research groups, to work more irregular and usually much longer hours. This practice created conflict with barracks duties and formations.

3.54 The presence of other detachments (engineer and military police) required some consistency of treatment of the military personnel in accordance with the usual military organization. However, a number of steps were taken which improved the position of the SED, although they did not entirely solve its problems. In June 1944, general supervision of the military administration of SED matters was given to Major P. de Silva. As Intelligence Officer his work brought him into close connection with the Laboratory Administration. In August, the regulation forbidding travel and outside social contact was relaxed for military personnel in the Laboratory so that they might visit their wives and families on furlough. In August, also, Major T. O. Palmer was appointed Commanding Officer of the SED. A large part of the credit in maintaining SED morale under difficult conditions must be given to Major Palmer. A system of promotion recommendations by Group and Divisions was soon worked out which was satisfactory to him and to the Laboratory administration. The problem of conflicting duties was not and perhaps could not be solved adequately. The amount of overtime work done by many groups and individuals required essentially civilian conditions of life.

CONCLUSION

3.55 The Laboratory personnel department found itself confronted by an unusually broad range of difficulties. To the problems of a peacetime urban laboratory were added those of a special military and civilian community, the whole being complicated by a corresponding duality of jurisdiction.

3.56 The greatest single difficulty was undoubtedly that of salary policy. The facts as stated are by no means self-explanatory. As the matter appeared to those charged with personnel responsibilities at Los Alamos, the underlying reasons for these difficulties were somewhat as follows: The Laboratory did not enter the scene as a going concern, such as would have been the case with a large contracting corporation or a university operating with its own staff in its own plant. The University of California was, on the contrary, remote from the concrete problems of Laboratory administration. Both the general salary policy and its detailed administration, moreover, were under the supervision and subject to the direct veto of the Contracting Officer, Lt. Col. Stewart. He, however, on whom the responsibility devolved, found it impossible, because of his situation, to discharge it to the satisfaction of the Laboratory. He was stationed in Los Angeles where his services were urgently needed in connection with procurement matters (3.78): he had only general and over-all acquaintance with the problems of the Laboratory. Either of two conditions would have remedied the situation: (1) that the

Laboratory have a strong, well-organized personnel office, capable of representing its need with sufficient consistency, detailed justification, and vigor to compensate for the Contracting Officer's remote position; or (2) that the Contracting Officer be stationed at Los Alamos, where he would be in a position to understand the detailed needs of the Laboratory (cf 3.17, 3.22). As matters finally developed, it was the partial satisfaction of both conditions that tended to solve the Laboratory's salary problems.

3.57 In fact, by June 1944 it was apparent that a considerable administrative reorganization was necessary. Hughes' previous experience and Los Alamos function had been primarily in the building of a competent scientific Laboratory staff. The rapid expansion of the Laboratory and its ramification in many directions not covered by the term "research" created personnel problems of a new and different order. After a year spent in building up the scientific staff of Los Alamos and seeking to formulate and work out its personnel policies under increasingly difficult conditions, Hughes returned to his previous position at Washington University. His position was taken by C. D. Shane of the Radiation Laboratory, Berkeley. As his general assistant Shane brought Roy E. Clausen of the University of California. Armand Kelly, formerly at the Metallurgical Laboratory of the University of Chicago was brought as an expert in matters of salary and salary control. Hawkins, who had until this time been only loosely connected with the Personnel Office, was made responsible under Shane for draft deferment and for military personnel matters.

3.58 The most serious personnel problem at this time was still that of salaries. After reviewing the situation in the Laboratory in June, Shane had accepted the position as Personnel Director with the understanding that in matters of salary control he and his office would have a reasonable degree of autonomy, not subject to veto by the Contracting Officer except in terms of Federal salary policy and regulations. As was stated above, an agreement with the Contracting Officer to this effect was reached in July 1944.

Other Administrative Functions

BUSINESS OFFICE

3.59 In February 1943, shortly before the administration of the Laboratory moved to Santa Fe, the University of California appointed J. A. D. Muncy as Business Manager for the Laboratory. His responsibilities included all the normal activities of a business office, but security restrictions put quirks into its operations and added a number of unusual functions. For security reasons it had already been decided to locate the Purchasing Office in Los Angeles (1.12). It was considered desirable to have the Accounting Office physically connected with the Purchasing Office. For this reason a general business office in Los Angeles for the most part took over operations from the Business Office at the point where money was disbursed. Complete records of all transactions were kept in that office, and government and university audits were made there. In practice, however, a small account maintained in the Santa Fe bank for emergency purchases, travel advances, and for cashing personal checks for Contractor's employees reached considerable proportions. It was, in fact, the second largest account in the bank, and since it was in Muncy's name, he frequently received circulars from charitable organizations suggesting large contributions.

3.60 The "normal" functions of the Business Office were payroll control, issuance of travel advances and preparation of travel expense bills, procurement of materials on the emergency purchases fund, maintenance of records for workmen's compensation and for the California State Employees' Retirement System, to which employees of the University were obliged to contribute after six months of employment.

3.61 Scientific workers were not permitted to maintain accounts in the local bank to avoid giving the bank a list of Laboratory personnel. This rule was maintained for all monthly salaried employees. The Business Office at the site therefore made up the monthly payroll and forwarded it to the Los Angeles office where checks were written and mailed to the banks designated by the employees. However, in 1943 the Contractor employed a large group of laborers and construction workers who were paid on an hourly basis, and beginning in January 1945 the salaries of machinists and other shop workers were computed on an hourly basis. These payrolls were made up and checks written by the Business Office at Los Alamos. Approximate monthly payroll figures of $50,000, $160,000 and $175,000 for the months of June 1943, 1944, and 1945, respectively, indicate the tremendous growth of the staff of the project. The payroll for hourly workers in June 1943 was roughly $23,000; in June 1945 it was approximately $130,000. The figure for 1944 is negligible, covering substitute school teachers and some part-time clerical workers. (See Graph 2.)

3.62 In keeping payroll records at the site there was considerable difficulty with accurate records of attendance. The university procedure of having a supervisor certify monthly that all employees in his charge were present with the exceptions noted was not considered adequate by the Manhattan District. On the other hand, certifications by the Group Leader as to attendance by days and half-days was considered completely impractical by the direction of the Laboratory for a number of reasons: personnel was too scattered, particularly in those groups doing field work; scientific workers frequently worked at night though not on regular shifts. Scientific workers often worked a good deal more than 48 hours a week, and since the contract did not allow for overtime payments it was felt that deductions for absences could not reasonably be made. The only procedure used until September 1945 for the scientific and administrative staff was a negative report made monthly by each employee, without any Group Leader certification. Although this system was never considered satisfactory, it is probably true that a more rigorous control would have imposed an almost prohibitive administrative burden, and would have had an unfortunate effect on the morale of scientific workers who were actually giving more than 48 hours a week to project work.

3.63 Reimbursement for travel on project business was handled in the same manner as the payroll. Although advances were issued from the local account, travel expense bills were forwarded to the Los Angeles office and checks mailed from there to the bank of the payee.

3.64 The emergency purchases fund was used for materials for which it was not practical to route the request through the Los Angeles Purchasing Office, either because of the urgency of the request or because of the character of the materials. The bulk of the material purchased on this fund in 1943 fell into the former category, since it was mostly construction supplies needed immediately for work being done by the Contractor. The amount of disbursements from this fund in June 1943 was approximately \$23,000, and in June 1944 it had dropped to \$4,000. In the latter year the materials purchased were principally batteries, dry ice, and cylinders of gas, items not suitable for shipment from Los Angeles. In June 1945, during the preparations for the Trinity shot, some \$38,000 were spent for miscellaneous items, ranging from radio tubes to canvas water bags, plus an increased volume of the normal batteries, gases, etc. Among the unusual purchases made with this fund were 88 cows which apparently had suffered radioactive burns during the Trinity test.

3.65 One of the first of the somewhat extraordinary duties of the Business Office was handling the financial end of the temporary housing mentioned in Chapter I. The cost of opening and operating the ranches used made the expenses to employees considerably greater than they would have been at the site. It was felt that the project should assume this extra cost, since housing was not ready at the site as had been promised. The Contractor therefore operated the ranches and billed each individual or head of family for the amount of his living expenses at the site (rent plus \$25 per month per person for food). In all, five ranches were operated from the end of March to the end of May 1943 at a cost of some $7,000. Claims for damage to the temporary housing occupied by Contractor's personnel were also settled by the Business Office, with the assistance of the Contracting Officer.

3.66 Other unusual functions of the Business Office stemmed for the most part from the attempt to prevent a list of personnel accumulating outside the project. Thus personnel were requested not to cash personal checks in Santa Fe, and check cashing facilities were provided at Los Alamos. By 1945 the daily average of checks cashed was between $3,000 and $4,000. All personal long distance calls and personal telegrams were charged to a Business Office account, and the daily telephone bill increased from $57 in June 1943 to $745 in June 1944. When New Mexico income tax returns were due, the Business Office assigned a number to each employee, and reported to the income tax bureau the amount of income paid to that number in New Mexico during the year. The employee then used his number instead of his name on his return.

3.67 It can be seen from this brief account that the volume of work handled by the Business Office grew considerably beyond what had been anticipated. In spite of the limitation imposed by the availability of housing, the staff increased to some 15 people by mid-1945. It is clear, however, that the decision to keep the main accounting office in Los Angeles was a wise one, both because of the advantage of proximity to the Purchasing Office, and because its staff, which grew to some 70 people, would have required a housing project all its own at Los Alamos.

3.68 Because security regulations made it impossible for personnel to take out new life insurance and because of the extra-hazardous character of the work done at the project, the problem of providing insurance for employees proved extremely complex and was never adequately solved, although a long series of efforts were made by the Director's Office in cooperation with the Business Office. When the project was organized, employees of the technical area were covered by an OSRD health and accident policy covering injury, illness or death, placed with the Fidelity and Casualty Company of New York. In September 1943 this policy was replaced by Manhattan District Master Policy 1 with the Sun Indemnity Company, which offered additional benefits including extra-hazardous insurance. In July 1944 Master Policy 1 was replaced by Master Policy 2, with premiums to be paid by the individual or the contractor rather than by the government. Master Policy 3 provided for accidents not arising out of employment. At about this time there was considerable discussion of the fact that the extra-hazardous insurance policy in effect for people working on radioactive substances was inadequate, since no provision was made for the fact that injuries might not appear for 10 or

15 years after they were received. Eventually this problem was solved in part by a special arrangement made with the University of California. A fund of $1,000,000 was deposited with the University by the Government to be used for payment by the University with the consent of the Government of up to $10,000 for injuries resulting from a number of specified extra-hazards listed in a secret letter to the University. Statutory Workmen's Compensation of the State of New Mexico was provided by the Contractor for all persons assigned permanently to work in New Mexico. The total of claims paid under Workmen's Compensation through December 1945 was only $18,000, of which $12,000 covered death benefits for two laborers killed in a motor vehicle accident in 1943. Accident policies, essentially the same as Master Policies 2 and 3 which expired, were made available in September 1944 for purchase by individual employees. For some time there was no coverage for travel on noncommercial, nonexperimental aircraft used by project employees, but eventually this was covered by a personal accident policy with Aero Insurance Underwriters for civilian employees.

PROCUREMENT

3.69 The community's isolation created many problems but the most acute and serious of these were faced by the Procurement Office. Supplying a large research laboratory from the ground up is in itself a difficult task; doing this secretly, in wartime, 1200 miles from the nearest large market and 100 miles from the nearest rail and air terminal would appear to be an impossible one. Yet the Procurement Office succeeded in overcoming all the obstacles of time, space, and security, and in satisfying the exacting and apparently insatiable demands of the laboratory. The fact that the Laboratory was able to meet its tight time schedule is a tribute to the competence and efficiency of its Procurement Office, guided from the beginning by D. P. Mitchell of Columbia University.

3.70 In February 1943, Mitchell, Oppenheimer, and several other scientists met with representatives of the Army and of the University of California to discuss purchasing policies. At the insistence of the University, it was agreed that all matters of purchasing and payments would be administered directly by members of the University staff, and within their entire discretion as to appointees but subject to the general supervision of the Contracting Officer. In effect, this meant that while Mitchell was in charge of ordering materials for the Laboratory, the actual purchasing would be done by University appointees. This organizational complication brought with it an additional security complication - the University's purchasing office would have to be located in Los Angeles, and its employees would not be permitted to come to Los Alamos to deal directly with persons placing orders, or to know anything about the work of the Laboratory.

3.71 At about this time, the basic policies which were to govern Mitchell as Procurement Officer were outlined. He was to be guided primarily by the necessity for speed and was not to be held responsible for the kind or quality of items to be purchased. He was to be authorized to place orders by requisitions signed by qualified members of the scientific staff, such requisitions to show quantity, description of item, date required, urgency, and suggested source. On the basis of the ordering individual's statement of urgency, Mitchell would judge the degree of priority required, and the means of communication and transportation to be used in order to meet the delivery date. Primarily the policy of the Procurement Office was to supply the needs of the technical staff as promptly as possible, and with as little red tape as possible. On the whole, this policy was maintained successfully.

3.72 A great many things had to be ordered before the Laboratory could begin to function, and until the Los Angeles Purchasing Office was established, such purchases were made through the Purchasing Office of the Radiation Laboratory at Berkeley.

3.73 The Los Angeles office was organized by D. L. Wilt and was in operation March 16, 1943. After September 1943, A. E. Dyhre was in charge of this office. In early discussions about procurement it was proposed that branch purchasing offices be established in New York and Chicago, to be subordinate to the Los Angeles office. These were set up in April 1943. Except in cases of unusual emergency the Laboratory's Procurement Office dealt directly only with the Los Angeles Office, either by mail or teletype. Requisitions for items not readily available in the Los Angeles area were forwarded to the New York and Chicago branches from Los Angeles. The three offices together employed a total of about 300 at their peak, including 33 buyers and 22 expediters. An average monthly dollar volume was about $400,000, covering an average of about 6,000 items purchased. However, in the peak month (May 1945) these figures were over a million dollars for more than 10,000 items. (See Graph 7.)

3.74 A certain amount of local purchasing was permitted. At first "local" was defined as a radius of 500 miles including Denver, but as security restrictions tightened, "local" was limited to a radius of 100 miles, including Albuquerque. Originally local purchases were intended to satisfy only emergency needs for items not obtainable in time through normal channels, and authorization for such purchases had to be secured from the

Business Office. Later, however, local purchases became a regular function of the Procurement Office, and included not only emergency items but also many bulk items such as fuel and building supplies, which could be purchased advantageously from local suppliers. In no case was it possible to place items on back order or ask local vendors to place orders for the Laboratory. For security reasons, purchases were made in Muncy's name. The Post Supply Section, under the able direction of Major Edward A. White, was frequently called upon to supply various items for the Technical Area. A system was set up whereby the Technical Area could requisition on Major White's office and this channel of procurement was of no small help to the technical work.

3.75 As has been mentioned before (1.17), the first groups of scientists brought with them a cyclotron, a Van de Graaff generator, a Cockcroft-Walton accelerator, and a certain amount of electronic equipment. Aside from these things, there was nothing at Site Y to constitute a laboratory. Most of the scientists had come from universities where they had fairly well-equipped laboratories and stockrooms which had been building up supplies in specialized fields for years. Within a few months, the Procurement and Purchasing Offices had completely to equip physics, chemistry, and electronics laboratories as well as machine shops, and also to prepare stockrooms of supplies for these laboratories and shops. The range of materials required for this task was incredibly great - everything from women's work clothes to 10-ton trucks. It has been stated, without exaggeration, that in variety of items the requirements of this laboratory exceeded those of Bell Laboratories, one of the largest research organizations in the world. The Procurement Office bought such things as rats, meteorological balloons, sewing machines, restaurant equipment, jeweller's tools, and washing machines in addition to what might be considered standard items of shop and laboratory equipment. At the time the Procurement Office for site Y was organized, early in 1943, the nation's industry had been thoroughly converted to war production. Stockpiles were running low in many items considered standard for research laboratories but not important for any other wartime use. Some things were almost completely unavailable; others could be secured only with high priorities. Project Y was assigned AA-1 priority by the War Production Board (WPB), but often it was necessary to request the District's help in securing higher priority or a WPB directive for particular items. The Procurement and Purchasing Offices succeeded in having equipment on hand almost as soon as there were buildings to house the various laboratories. Stockrooms were ready in short order - one for chemical supplies - K stock, one for general laboratory supplies - S stock, one for special electronic supplies, and one for each of the shops.

3.76 Once the various laboratories were established, the task of the Procurement Office became that of meeting the continuing demands of the scientific and technical staff for equipment and keeping stockrooms adequately supplied. Responsibility for the electronic and shop stockrooms was turned over to the various operating groups. The duties of procurement could never become routine, because of the Laboratory's continuous expansion and because of the constant and necessary changes in the technical program. Since the Laboratory was operating on a rigid time scale, time was always the most critical factor, and shipping instructions made by the Procurement Office on requisitions sent to the Purchasing Office were often extremely important. Occasional failure of the Purchasing Office to carry out shipping instructions precisely was one of the minor sources of friction between the two offices. On several occasions the Procurement Office was obliged to re-order by air express an item that was being shipped by freight contrary to instructions. Waiting for freight delivery would have meant holding up vital experiments that would cost much more in time and money than the cost of duplicating an order.

3.77 Second only to time in importance was the question of security, and this too caused innumerable difficulties to the Procurement and Purchasing Offices. For security reasons Site Y was located far from any large city, and therefore separate purchasing offices had to be established in marketing centers. For security reasons, the employees of these purchasing offices could have no direct contact with the using groups at the Laboratory, could know nothing about the work of the Laboratory, and therefore could not understand its significance or appreciate the urgency and responsibility of their own work. Employees of the Chicago and New York offices dealt directly only with the Los Angeles office, except in emergencies. For security reasons, using groups were almost never able to deal directly with manufacturers and dealers; when questions about design or fabrication arose, these questions had to be transmitted through the New York or Chicago Purchasing Offices to the Los Angeles Purchasing Office, from there to the Los Alamos Procurement Office, and finally to the using groups; the answer had to be sent back along this same path to the supplier. For security reasons no direct shipments could be made to Site Y; all suppliers were instructed to ship goods to Chicago and Los Angeles warehouses, from where they had to be transshipped to Y with their original labels removed in order to prevent unauthorized persons from learning what kinds of things were being received. Originally the Los Angeles and Chicago warehouses did nothing but transship orders, and the Site Y warehouse checked shipments and approved invoices. Because of government regulations insisting upon prompt payment of bills to avoid loss of discount, procedure was changed so that invoices were checked against shipments at Los Angeles and Chicago and approved there. This procedure led to minor difficulties: goods would be received that were neither usable nor returnable, items would be missing from the shipment but checked on the invoice, packages of photographic film would be opened for inspection.

3.78 Periodically, technical groups in the Laboratory submitted criticisms of the Los Angeles Purchasing Office to the Director, and periodically the Director would transmit these criticisms to the appropriate Army and University officials. The theme of most of these criticisms was that the Los Angeles office was staffed by inefficient and inexperienced buyers. For some time these offices were seriously understaffed, and some of these criticisms may have been justified; on the whole, however, circumstances made unavoidable much of the apparent inefficiency. The University and the Purchasing Office maintained with considerable justice that much of their difficulty was directly traceable to the strict security regulations under which they operated. Statistics compiled from time to time by the Contracting Officer, Lt. Col. Stewart, on the efficiency of the Purchasing Office show fairly commendable results. To some extent the criticisms of the Purchasing Office by using groups in the Laboratory were caused by their isolation from and unfamiliarity with the actual state of the market. They had come from universities whose equipment had largely been purchased under peacetime conditions, when time was not at a premium and manufacturers' catalogues actually represented stock on hand. During the war many manufacturers stopped publishing current catalogues, and those catalogues which were available in no way represented existing conditions. Men had been in the habit of designing apparatus, starting to build it, and then ordering parts they did not have on hand. This habit nearly proved disastrous on several occasions. For example, one group designed a special kind of camera to be used in connection with the Trinity test, proceeded to work on construction, and ordered necessary parts. After the work was well under way, the group was notified that the particular lenses they had ordered were not on the market, would have to be ground to order, and might not be ready in time to be useful. Also, the particular kind of plate backs which they had incorporated into the camera design were no longer available on the market and were not being manufactured. Purchasing Offices scoured the country, and succeeded in finding about one-third of the required number of plate backs. To secure the rest, it was necessary through the Washington Liaison Office to get a WPB directive ordering the former manufacturer of these items to stop his current production and make the necessary amount for the Laboratory. The cameras were ready in time for Trinity, but only after a tremendous expenditure of effort by all concerned. Such incidents were not frequent, but serve to illustrate some of the difficulties encountered by the Purchasing

Office. From the very beginning, the Procurement Office had made an effort to teach the using groups the importance of finding out about the availability of certain materials before completing designs and starting work, but it was difficult to change old habits and difficult for men to realize that a small loss of time spent in studying the market might mean a large saving of time in completing a satisfactory piece of equipment.

3.79 Just as the Laboratory groups were hampered by their unfamiliarity with the state of the market, so the Purchasing Offices were hampered by their unfamiliarity with the kind of work being done at the Laboratory. Knowing nothing about the work, they could know nothing about the uses for which particular items were needed, and therefore could not understand which specifications were critical and which were simply listed for convenience. A buyer in New York receiving an urgent request for some item ordinarily made of metal might be notified by the manufacturer that other users were accepting wartime substitutes made of plastic. The buyer would see no obvious difficulty with this substitute - no reason to ask the Los Angeles office to check with the Y office to check with the user - and would place the order. He could not be expected to know that for the scientist's purpose, size, color and shape were convenient but not indispensable, whereas the chemical composition of the material was the one all-important criterion. Such incidents occurred again and again, and the only possible solution was to have members of the technical staff make their specifications as complete and explicit as possible without revealing the nature of their work. The local Procurement Office made a serious effort to have the using groups prepare accurate and complete specifications, and the Procurement Office itself checked such specifications closely before transmitting them to Los Angeles.

3.80 The organization established at Y to handle some of the complex problems outlined above was in itself rather simple. In accordance with its policy of eliminating red tape and supplying the Laboratory as quickly and efficiently as possible, Mitchell organized his department into two main sections - Procurement, under the supervision of E. E. Olsen, and Service and Supplies under the supervision of H. S. Allen. The Procurement Section consisted at first of two groups, Buying and Records. Later a third - Property Inventory - was added. The Buying Group was responsible for checking specifications on purchase requests, suggesting a possible manufacturer or vendor to the Los Angeles office, justifying high urgencies, and answering questions initiated by the Los Angeles Purchasing Office. Essentially the local buyers existed to give the Los Angeles buyers the information they required to purchase the things needed at the Laboratory. The Records Group was responsible for maintaining files of correspondence and purchase requests, and also Kardex files of expendable and nonexpendable goods on hand. When one understands that for every purchase request an average of sixty pieces of paper was involved, including printed forms and teletypes, the importance of the Records Group becomes evident. The Kardex files were later transferred to the Property Inventory Section (9.24). The Service and Supplies section consisted of four groups - the stockrooms, receiving, shipping, and records. Originally one man was in charge of all the stockrooms - the general laboratory supply, the chemical supply, electronic, machine shop, and a few small specialized supply rooms. The Receiving Group was responsible for opening packages, identifying items with purchase orders, and directing distribution either directly to Laboratory groups or to the appropriate stockroom. The Records Group maintained files of purchase orders for follow up purposes, files of stocks on hand, and various receiving record files.

3.81 Certain special procurement channels by-passed the University Purchasing Office in Los Angeles. These concerned parts for the completed bomb-mechanism, materials including uranium and plutonium coming from other branches of the project, and materials obtained directly from the Army or Navy such as electronic components and completed devices of an electronic nature, guns, propellants, and high explosives.

LIBRARY AND DOCUMENT ROOM

3.82 One of the minor but extremely important groups in the Laboratory was the Library. No research laboratory can exist without a library well stocked with standard technical reference works, files of technical journals, and reports of work in progress, especially when that laboratory is isolated from all other universities and libraries. The Los Alamos library served its purpose well, and was one of the few administrative groups in the Laboratory about which there were substantially no complaints from the scientific staff. The library was organized and directed by Charlotte Serber.

3.83 Like the Procurement Office, the Library faced the problem of providing in a few months a comprehensive collection of books and journals on physics, chemistry, engineering, and metallurgy that had taken other libraries years to accumulate. A large part of this initial problem was solved by loans, chiefly from the University of California library. A tentative list of book requirements submitted by various staff members planning the laboratory consisted of approximately 1200 books and 50 journals (complete files from 1920 for the most part). Many of these were impossible to secure on the market, but fortunately the University library was able to supply nearly all of the rare out-of-print titles. New publications were bought, but only through a circuitous route, because of security restrictions. Orders from the Los Alamos Library were sent to a forwarding address in Los Angeles, and from there to the University library in Berkeley; from Berkeley, orders were placed with book publishers and dealers to be sent to the Los Angeles receiving warehouse, and from Los Angeles the books were forwarded to Los Alamos. By July, 1945, the Library included approximately 3,000 books, 160 journals per month, and 1500 microfilm reproductions of specific articles and portions of books.

3.84 The largest part of the Library's work, however, was that of reproducing and distributing reports of work in progress. For this purpose, the Library staff included two small subgroups, known as the workshop and the document room. The workshop typed, reproduced, and assembled technical reports and manuals submitted by the various scientific groups of the Laboratory. The workshop group collaborated very closely with the editorial section and with the photography and photostating shop. Completed reports were turned over to the document room for distribution in accordance with security regulations, since nearly all of the work of the project was classified. The Laboratory's guiding policy for distributing information among its own workers was simply that in no case should information be withheld from anyone who could work more effectively if information were in his hands, or who would be in a better position to maintain a high level of security in his possible dealings with outside workers if he were more fully informed. To carry out this policy, the document room of the Library was supplied with a list of personnel entitled to have access to all or certain catagories of classified documents, and this list was kept up to date by advice from group and division leaders. In general, comparatively few documents were distributed to individuals; the majority were kept in the document room to be read there or borrowed temporarily by qualified persons. In addition to maintaining a complete and current file of Los Alamos reports, the document room kept a file of documents received from other Manhattan District projects. Some notion of the amount of work handled by the document room can be gained from the fact that by January 1945 there were 6090 reports on file, exclusive of extra copies of the same report, and that approximately 10 per cent of the total circulated each week.

3.85 Among minor duties of the Library was that of instructing the secretarial staff in the preparation of reports for reproduction, and the handling of classified documents. In January 1945, the library document room assumed from the Patent Office (3.123) the duty of issuing patent notebooks, keeping a record of notebooks issued, and collecting them from individuals upon separation from the project.

EDITOR

3.86 From the beginning the Los Alamos Laboratory produced large quantities of reports. Whole fields of research were amplified by the results of work done here so that regularly published papers in these fields were made obsolete, and reports written here became standard reference works for this and other Manhattan District projects. It was therefore necessary to have experimental results reported speedily and accurately in a form that would be readily accessible to other employees requiring the information for their own work. It soon became apparent that responsibility for the editing and reproduction of all reports should be centralized to insure accuracy as well as speed. Early in 1943, D. R. Inglis of Johns Hopkins University was appointed Project Editor. All reports of completed work (known as documents) or of work in progress (known as manuscripts) which were to be reproduced in any form went through the office of Inglis. The reports were checked thoroughly from both a technical and editorial point of view. An appropriate form of reproduction was then selected, and when finished they were routed to the workshop or photostating shop. Through Inglis' efforts the Laboratory was assured a series of technically accurate, and editorially consistent, reports of work completed and in progress.

HEALTH AND SAFETY

3.87 A Health Group reporting to the Director was part of the Laboratory administration from the beginning. Throughout the present history this group was under the supervision of Dr. L. H. Hempelmann.

3.88 Health problems of the Laboratory may be classified as (1) standard industrial health and safety problems, (2) the definition of health standards in relation to special hazards, (3) the establishment of safe operating procedures, and (4) routine monitoring and record keeping. At the beginning all of these were part of the Health Group's responsibility with Dr. Hempelmann acting as chairman of the Laboratory's Safety Committee. By April 1944 this committee felt that it had become too unwieldy to handle effectively the increased safety problems resulting from the rapid growth of the project, and suggested that the Director accept its resignation and organize a new committee better qualified to handle the problems. Mitchell, Procurement Office leader, became head of the new committee whose function was defined to be supervision of all safety installations, inspections, and activities connected with the Technical Area and the outlying sites. This was to include fire, general safety, and maintainance as well as technical safety. Dr. Hempelmann remained a member of the committee representing the Health Group. Later the execution of safety policies was taken over by the Safety Group under a full-time safety engineer (9.37). The establishment of safe operating procedures and routine monitoring and record keeping remained under the Health Group's general jurisdiction, but such duties were delegated, wherever possible, to the operating groups or appropriate subcommittees of the Safety Committee.

3.89 The central responsibility of the Health Group was the establishment and dissemination of health standards, specifically, of safe tolerance levels of exposure to radiation and to radioactive and chemical poisons. In this and in its general supervisory work, the group was concerned primarily to protect the health of Laboratory employees. Secondarily, it sought also to protect the legal interests of employees and of the Contractor. To this end it kept records of the hazards to which individuals were exposed, the extent of exposure, accidents, and tests for overexposure. In addition it obtained and recorded pre-employment medical examinations for all technical personnel. It made complete examinations, including necessary tests, of all employees on termination. Ordinary industrial accident records, however, such as shop injuries, were kept by the Post Hospital.

3.90 In the original plan of Laboratory activities it was assumed that biological and physical research related to health problems would be entirely the responsibility of other laboratories within the Manhattan District. Reliance on the work of others did not, however, always provide necessary information at the time it was needed. Research sections were set up as needed within the Health Group or by its request in other groups. Thus the development of apparatus needed for monitoring was undertaken at Los Alamos in the spring of 1944, and a large share of the instruments were built in the Electronics Group. Again, in August 1944 it became necessary to investigate biological methods of testing for overexposure to radioactive poisons, and this work was undertaken by a section of the Health Group (9.30).

3.91 During its first year the work of the Health Group was relatively uncomplicated. A semi-research problem which appeared almost immediately was to discover the extent of variation in normal blood counts. It was discovered that variations which were at first thought symptomatic of overexposure to radiation were, in fact, common in normal blood.

3.92 Operation in this period was confined largely to the hazards of external radiation from accelerating equipment and radioactive sources. The danger of heavy-metal poisoning from uranium had to be guarded against, as did other chemical hazards, but these problems were not serious.

3.93 The really serious problems of the Health Group appeared in the early spring of 1944, with the arrival at Los Alamos of the first quantities of plutonium. The nature of these problems is suggested by the following brief account of the toxicology of plutonium.

3.94 The metabolism of plutonium is similar to that of radium in that it is deposited in the bone where its alpha radiation may cause bone sarcoma. But while radium is deposited with calcium in the living bone, plutonium is deposited in the surface membranes of the bone, and is presumably not overlaid by subsequent calcium deposition. Among other body organs the heaviest deposition occurs in the kidneys, where in sufficient quantities its radiation causes destruction of tissues responsible for kidney function. This effect, however, will not become serious except for dosages considerably greater than those needed (over a sufficient period) to cause bone injury. Another unfavorable circumstance in the comparison of plutonium with radium is the much slower rate of elimination from the body in the case of plutonium. In compensation for these bad qualities, plutonium has a much lower alpha activity than radium, and is less easily absorbed from the digestive tract. In general, the problems of handling plutonium are comparable with those of handling radium, with the allowances for the vastly larger quantity of the former material that is processed, and for the fact that empirical information on the toxic effects of small amounts over a 10- or 20-year period is not available.

3.95 Although not all this information was available at the time, the general similarity with the radium hazard had just been discovered; as a result Hempelmann and representatives from Chicago and Oak Ridge visited a luminous paint company in Boston to learn how the radium hazard was handled in that industry. On his return three committees were established in the Chemistry and Metallurgy Division to develop methods for control of the plutonium hazard. An instrumentation committee was appointed to design counters suitable for measuring the radioactive contamination of laboratories and personnel. A second committee was responsible for the design of apparatus and equipment for handling plutonium. Apparatus was designed by this committee in consultation with the chemists concerned, and was built or procured by the Chemistry and Metallurgy Service Group. A third committee drew up rules and recommendations for the safe handling of radioactive materials. The procedures recommended were put into effect in March 1944, with the understanding that willful noncooperation would result in immediate dismissal from the Laboratory. A section of the Service Group was established under W. H. Popham to enforce these procedures. It had the positive functions of providing personnel with proper protective equipment, laundering this equipment, monitoring the laboratories and decontaminating them when necessary, and of keeping complete records. The group worked very closely with the Health Group.

3.96 In addition to organizing the safety measures described above, the Health Group carried on an extensive educational campaign among the groups working with plutonium. Lectures were given on the toxicology of plutonium, and numerous conferences were held with operating groups to work out the application of general recommendations. The Health Safety Handbook was given new members of the Division.

3.97 Despite these precautions the members of the Health Group and of the Chemistry and Metallurgy Division were not satisfied with the progress of biological studies on plutonium made by the other projects responsible for this work. This dissatisfaction was crystallized by an accident which occurred in August 1944, when by a minor chemical explosion a number of milligrams of plutonium were thrown in the face of one of the chemists. A research program was undertaken, aimed primarily at developing tests for detecting overdosage of plutonium (9.30).

3.98 Another continuing difficulty was the lack of adequate monitoring equipment. Alpha ray counters lacked either sensitivity or portability, and were not received in adequate numbers. The lack of sensitive portable meters made it necessary to wipe surfaces suspected of contamination with oiled filter paper and to measure the activity collected with stationary counters. Contamination of hands and nostrils was measured in the same fashion. Because instruments received from Chicago did not meet the local monitoring requirements, development of such equipment was begun in the Electronics Group of the Physics Division in May 1944 (9.31).

3.99 One further activity of the Health Group in this period was the control of the danger of poisoning in the work of high explosive casting. Standard protective measures were put into effect, and no serious trouble was encountered in the period covered by the present history. The medical group performed monthly examinations of all exposed personnel and gave periodic lectures as to the dangers of toxic effects from high explosive work. The education of the workers was aided by the fact that all of the plant supervisors were seasoned in this type of work. The number of cases of TNT dermatitis was in keeping with the number exposed. This is an allergic reaction which cannot be entirely prevented in any plant operation.

SHOPS

3.100 The principal shop facilities of the Laboratory were machine shops, drafting rooms, a glass shop, and photographic shops. Although for the most part these were service groups of a standard type, it was true at least of the machine shops that they encountered a number of administrative and technical problems of an unusual kind. Although the machine shops did not become part of the Administrative Division until after the general reorganization in September 1944, they are discussed in this chapter because their problems were related and can be more logically treated here than under the separate Divisions in which they were organized at first.

3.101 In the original program of the Laboratory, plans were made for a drafting room and machine shop (known as V shop), for the design and fabrication of laboratory tools and instruments, primarily to serve the Experimental Physics and Chemistry-Metallurgy Divisions. The glass shop was an adjunct of the Chemistry groups. Two photographic shops were added during 1943, one mainly for routine recording and duplication, the other as an adjunct of the ordnance research program, responsible for technical photography and a considerable program of optical research (15.48).

3.102 After the beginning of the ordnance program, additional plans were made for an ordnance drafting room and large ordnance machine shop (later called C shop). A number of small student shops or special shops were built at various times. The largest of these was the Graphite Shop of the Miscellaneous Metallurgy Group (8.52).

3.103 Responsibility for organizing the first shops was assumed on an interim basis by Mack (1.15). The Laboratory was fortunate in obtaining Gus H. Schultz, from the University of Wisconsin Shops, as foreman of the Laboratory shop (V shop). Schultz was not only thoroughly familiar with the requirements of a laboratory shop, but also had a substantial background of industrial experience.

3.104 The original area of V Shop was 8000 square feet, planned for 30 toolmakers and machinists, representing an expected shop load of about 1500 man-hours per week. This goal was reached in October or November of 1943, by which time, however, the goal had been set considerably higher. (See Graph 10.)

3.105 In July 1943 Mack resigned as shop supervisor and set up the Optics Shop and research group in the Ordnance Division. His place was taken by E. A. Long, head of cryogenic research in the Chemistry and Metallurgy Division.

3.106 In March and April of 1944 some rescheduling of shop work became necessary, because of the rapidly increasing load in V Shop. At that time about half of the load came from the Chemistry and Metallurgy Division, whose requirements were rapidly increasing. This problem was met by shifting some of the Metallurgy work to C Shop, and in May adding about 500 square feet to V Shop. Introducing a night shift would have been an alternative, but it was difficult at the time to find machinists willing to work in the night shift.

3.107 Three examples may be given of outstanding fabrication problems solved in V Shop. One was the fabrication of beryllium oxide bricks for the Water Boiler (13.29): the dies were developed in V Shop as well as the technique of facing the bricks. Another was the development of apparatus and technique for welding the thin stainless steel envelopes of the Water Boiler. Another was the machining and grinding of tungsten carbide. In all cases the primary responsibility was borne by the operating group, but the actual development work was done by shop personnel.

3.108 Construction of C Shop was begun in July 1943 and completed in October. Its area was 8800 square feet, planned for about 40 machinists and toolmakers, representing a load of about 2000 man-hours per week. Its foreman was Rex Peters, under the supervision of C. Cline.

3.109 The career of the experimental shop was relatively smooth and harmonious, while that of the ordnance shop was full of crises. Some of the reasons for this contrast came from the nature of the work of each. The experimental shop was organized after a familiar pattern, staffed and supervised by men with adequate training and experience. C Shop, by contrast, was designed for a type of work that was not completely anticipated. Both the equipment and personnel proved inadequate to the demands that arose. By the time the difficulties were fully appreciated, the rate of growth of the Laboratory had become so large that it was impossible fully to overcome the existing lag.

3.110 In May 1944 Cline was transferred to the Engineering Group, and his place was taken by W. M. Brower. Whereas Cline had relatively little experience in shop supervision, Brower was a man with considerable experience in handling difficult shop situations in Berkeley and Oak Ridge, who it was thought would be able to represent shop needs and problems in the councils of the Ordnance Division. Brower obtained support for the rapid procurement of needed equipment and made some reorganization of shop procedure. Despite these efforts the problems of C Shop deepened, and Brower left the Laboratory in the middle of August 1944.

3.111 The nature of the C Shop difficulties may be illustrated in three ways. The first point is that very little of its work was routine production. Most items were produced singly or in small lots. Every item had to be given detailed specifications in the engineering drafting room. This created an enormous load of work and involved close cooperation between detailers and the scientists preparing rough drawings. This is a common problem in laboratory shops, where as a result machinists become very skilled at working from rough drawings supplemented by informal consultation with users. In C Shop this was impossible because of its size and because few of the machinists had the necessary training. The result was more or less constant complaint about delays in the drafting room and inadequate checking.

3.112 A second symptom of inadequacy was that even a rigid priority system was insufficient to prevent delays of urgent work. There were constant small irritations connected with this priority system, in deciding for example between two such unrelated programs as the gun and the implosion.

3.113 Lack of experience with peculiar fabrication problems added to the difficulties. As one example, the machining of hemispheres may be mentioned. The implosion program called for a large number of hemispheres of various materials and sizes. A 60-inch lathe was acquired for turning large hemispheres, which proved useless for this work. Peters finally solved the problem of producing these hemispheres with a specially rigged boring machine. Eventually, the lathe was needed for other jobs; the point is that none of Peters' immediate superiors knew how this work, which is nonstandard, should be done.

3.114 As the above illustrations would suggest, the problems of C Shop had their roots in the more general difficulty of developing an adequate Ordnance Engineering Group. Although the shop had a competent foreman, he was not in a position to overcome the general lack of foresight in obtaining men and equipment. This lack, moreover, was not solely the responsibility of Peters' superiors in the Engineering Group; these were in a poor position to understand the emerging needs of the ordnance research and development groups, who in turn were not yet geared to their role as weapon designers (1.53).

3.115 It is not true, however, that the shop and engineering difficulties were inseparable. They were connected primarily because of organizational arrangements. The original plan, by which the C Shop was placed under the Engineering Group of the Ordnance Division, was plausible in terms of the contemplated narrow range of the ordnance program. As that program broadened out to include not only the gun program but also the rapidly expanding implosion program, such arrangements became less plausible. The C Shop became in fact a service organization doing work for a number of semi-independent organizations. Throughout the Laboratory the emphasis of work began to shift toward development work. The line of division between the two big shops became less well defined. In the end, therefore, it became clear that the proper remedy for shop troubles was to place both C and V Shops under unified management. This would not only make for greater flexibility in the division of labor between shops, but would also give to C Shop the strong leadership needed to overcome its constant difficulties and to prepare it for the even more difficult days ahead. Such a step, moreover, would simplify the remaining problems of the Engineering Group, being a step away from the conception of the latter as a key administrative organization, and toward concentration on the increasingly difficult problems of design-development, of engineering in the narrow sense.

3.116 At the time of the August 1944 reorganization, accordingly, the C Shop was moved from the Ordnance Division to the V Shop administration of Long and Schultz.

CONSTRUCTION AND MAINTENANCE

3.117 Some of the construction problems have already been described in Chapter I, and in particular the construction situation at the time Laboratory personnel began to arrive. The procedure used for the construction of the original buildings was standard for Army installations. Specifications for the original buildings had been given to the Manhattan District Engineer's Office in New York by Oppenheimer, McMillan, and Manley. Plans were drawn by the Stone and Webster Corporation of Boston since it was originally expected that they would do the construction. The drawings were transmitted to the Albuquerque District Office of the U. S. Engineers, and a contract was let by this office to the M. M. Sundt Company. On completion of the buildings the Sundt Company transferred them to the Albuquerque District, which in turn transferred them to the Santa Fe Area Office of the Manhattan District Engineers, in theory the "using service." The actual using service, the technical staff, had no official position in this process, and since during the critical period of actual construction they were still scattered about the country, liaison was totally inadequate. The Albuquerque District remained in formal charge of construction until early 1944, at which time the Manhattan District assumed complete responsibility.

3.118 By May 1943, the original buildings had been occupied and were in process of being expanded. The Sundt Company had undertaken two relatively large structures: a new warehouse and an addition to the cyclotron laboratory, but was not going to be able to complete the necessary work in time. Ordinarily the Army was responsible for providing additional construction workers, but in this early period was not able to do so, and the Contractor (University of California) had employed a number of carpenters, plumbers, electricians, and laborers. Under contract regulations, these workers could not be employed for any permanent construction, but only for maintenance work and the construction of shacks and "lean-to's." These men at first worked under the direction of members of the scientific staff, and later under Brazier who was employed by the Technical Area as supervisor of construction and maintenance. Brazier's responsibilities were not altogether well-defined at any time, but it can be said that he was responsible for the preliminary design of the major expansion program which began in June and which included a new office building, offices and laboratories for the ordnance program, and a heavy machine shop. Brazier's staff grew from about a dozen men in May to 264 in January 1944, when he left the site.

3.119 General Groves had wanted for some time to have all construction handled by the Army Engineers, and his final decision in this matter was hastened by a series of complaints made by the War Manpower Commission, the United States Employment Service, and the American Federation of Labor, that there were certain irregularities in the project's procedure of employing construction workers. In January 1944, Brazier's entire staff was turned over to the Army payroll with the exception of three foremen who remained on the University of California payroll. The scientific staff saw considerable advantage both from the point of view of security and that of efficiency in having a separate construction and maintenance group for the Technical Area. Although it was not found possible to keep the entire group on the University of California payroll, the three key men, Charlie Stallings, Melvin Foley, and Dan Pfaff - in charge, respectively, of carpenters, plumbers, and electricians - were kept, and their assistants assigned permanently to the Technical Area. The group, under the direction of John Williams, was responsible for the maintenance, repair, and installation of all scientific equipment or machine tools under the jurisdiction of technical personnel, and also for building and remodelling apparatus and equipment of a scientific nature.

3.120 The construction and maintenance group under the jurisdiction of the Army Engineers was responsible for all alterations and additions and repairs to buildings, including services and installations, and for the installation of new, and repair of existing, utilities. The Army also found it necessary to establish separate organizations for the maintenance and construction of the Technical Area and outlying sites and for the post, housing, and administrative areas. Separate priority lists were maintained for both groups in accordance with urgency ratings assigned by those requesting service.

3.121 Nearly all major new construction was handled by contractors under the supervision of the Post Operations Division. The original contractors, M. M. Sundt Company, remained in charge until the end of 1943. They were succeeded by the J. E. Morgan Company which built a section of the housing area during the first three months of 1944. They in turn were succeeded by R. E. McKee who remained in charge of construction with an average force of between 700 and 1000 men. The architect, W. C. Kruger, whose contract was originally issued by the Albuquerque District, was retained by the Manhattan District throughout the life of the project. (See Graph 8 for rate of growth of technical construction.)

3.122 Requests for all but the most minor construction had to be made by group leaders or their superiors, and urgency ratings assigned in the same way as those for orders on the Procurement Division. Such requests were submitted to the office of David Dow who acted as liaison between the using groups and the Post Operations Division. Frequent conferences were held to determine priorities and set up tentative completion schedules. One of the most frequent causes of difficulty between the using groups and the construction services was the inability of the former to foresee their needs very far in advance, since construction depended in many cases upon the results of experiments in progress.

PATENT OFFICE

3.123 In accordance with procedure outlined by the Office of Scientific Research and Development for the protection of Government interests in scientific research, the Contractor was required to "report the progress of all studies and investigations undertaken, disclose to the Government all inventions made in carrying out the work of the contract, and furnish a complete final report of findings and conclusions." Here again security was an important factor in determining administrative organization. Since few Contractor's representatives were permitted to visit the Laboratory or to know much about the technical details of the work being done here (3.17), they could not make the necessary reports for patent purposes. Consequently, the University turned over much of its responsibility for protecting Government interests to Major Ralph Carlisle Smith, the Patent Officer, who arrived in July 1943 to establish the Patent Office.

3.124 The work of the Patent Office was conditioned in many ways by considerations of security. The most serious effect was the limitation of personnel of this office to the absolute minimum. Since it was the duty of the Patent Division to report the progress of all the scientific work done on the project, and since this was the only office where all of this information would necessarily be compiled in language understandable to an individual having a general scientific background, the Director and the Security Officer felt that only a few absolutely trustworthy individuals could be permitted to work here even in a clerical capacity. For some time Major Smith had no assistants at all, but eventually he obtained among the enlisted men and women already employed on the project a minimum staff which had to be trained on the job. Only after a year and a half was he able to secure two legally trained, scientific assistants.

3.125 In addition to limiting the staff of the Patent Office, security considerations increased its burden to include responsibility for all patent matters affecting subcontractors or involving project employees who had come here from other projects. Thus, the Patent Office assumed responsibility for the early subprojects such as those at Purdue University and Stanford University (1.4), as well as for the later subcontractors, such as F. Flader and the California Institute of Technology (9.15). Furthermore, employees who had transferred here from other government projects were not permitted to communicate directly with their previous colleagues in the patent field, and therefore any unfinished patent matters had to be transferred to this project for completion.

3.126 The Patent Office established the methods and procedure of recording work done and secured the cooperation of the technical staff in keeping the necessary records. Numbered notebooks were issued, originally by the Patent Office and later by the Library document room, and in these staff members currently recorded the details of experiments and the exact dates of the various stages of development of inventions and discoveries made. Completed notebooks and those turned in by people leaving the project were kept on file by the Library document room. Through the Business Manager's Office, patent agreements were secured from every employee, subcontractor, and consultant of the University of California. The Patent Office obtained special patent agreements from military personnel and civilian employees of the War and Navy Departments, and special patent contracts from individuals on loan directly to the Manhattan District from other employers. Employees of foreign governments were not required to sign agreements, but did prepare records of inventions and executed U. S. applications to the benefit of the U. S. Government. Monthly reports of the activities of foreign personnel were prepared by the Patent Division. These and similar reports of visits by consultants and foreign personnel were sent to General Groves' office. All terminating personnel were required to appear before the Patent Officer and assert that they had made no inventions without recording them with the Patent Office, and they had turned in all original records to the document room, or other appropriate depository.

3.127 The most important duty of the Patent Office was, of course, that of preparing patent applications to protect the Government and to prevent outside interests from later dominating the pertinent fields of research and development. Circumstances at this project made it necessary for Major Smith and his staff to be not only experienced patent attorneys, but also expert in a variety of technical subjects. Experiments covered much more than nuclear physics; they included chemistry, metallurgy, ordnance, and explosives and electronics, to mention only the largest fields. Patent cases submitted can be classified into five principal groups: the production, chemistry, and metallurgy of fissionable materials; isotope separation; power reactors; electronic equipment; and the bomb itself with its various developments and improvements. Altogether there were about 500 patent cases reported to Washington OSRD Headquarters covering work done at this Laboratory, and about 300 handled in connection with work done on other projects. Of these, a substantial number have been filed in the U. S. Patent Office.

3.128 Because of the pressure of time and the very limited staff of the Patent Office, it was not possible to write cases in the usual manner. Ordinarily an inventor or research scientist prepares invention reports of things he considers new and useful and submits these to a patent attorney for approval and the preparation of a formal application. Here the members of the Patent Office read the daily records and other reports of research workers, inspected the laboratories and test sites, held periodic discussions of work accomplished with various individuals, and attended seminars and conferences of the various groups and divisions. In all of these sources, the Patent Office found ideas and practices that were new and useful, prepared the applications so as to give maximum scope to the inventions in their relation to the entire project and associated fields, and submitted these applications to the inventors for final approval. Since members of the technical staff were pressed for time and, in any event, were reluctant to take time from research for preparing reports, this rather unorthodox procedure proved to be extremely helpful. An additional complicating factor in the work of preparing cases was the fact that by reason of the nature of the work, a great many developments had to be covered before there was any physical embodiment proving that the inventions were workable - before any "actual reduction to practice," in legal jargon. The test shot at Trinity was the first reduction to practice for many inventions, the success of which was long before anticipated by the completion and filing of a series of patent applications. Completed cases were transmitted by Army courier to the OSRD Washington Patent Headquarters, headed by Captain R. A. Lavender, USN, and filed with the U. S. Patent Office.

Chapter IV

TECHNICAL REVIEW TO AUGUST 1944

Introduction

4.1 The role of theory in the formation of basic decisions in the DSM project is well illustrated by the fact that even after the establishment of Los Alamos there was still no absolute experimental confirmation of the feasibility of the bomb in terms of its basic nuclear processes. In April 1943 it was still possible, although extremely unlikely, that an efficient nuclear explosion might be ruled out on either of two counts, and on a third count so far as the use of plutonium was concerned. First, the neutron number had not been measured for fissions induced by fast neutrons, but only for "slow" fission. Second, the time between fissions in a fast chain might be longer than had been assumed. Finally, the fissioning of plutonium had been studied by observation of fission fragments, but this gave no proof that the neutron number was the same as for U^{235}.

4.2 The first physical experiment completed at Los Alamos - in July 1943 - was the observation of neutrons from the fissioning of Pu^{239}. In this experiment the neutron number was measured from an almost invisible speck of plutonium and found to be somewhat greater even than for U^{235}. As was mentioned earlier, this result justified the decision already taken to construct the plutonium production pile at Hanford.

4.3 The other early confirming experiment - the measurement of delayed neutrons - also gave favorable results, as expected. It showed that delays in neutron emission were negligible. The third possibility, that the neutron number might be radically smaller for fast neutron fissions than for slow, was not investigated until the following year. Assurances on this score were, however, considerable.

Nuclear Specifications of the Bomb

4.4 With the completion of the Laboratory and of the preliminary experiments described above, the work of the nuclear physics program entered its main course. As stated in the introduction (1.54) this program aimed, through experiment and calculation, at providing specifications for the bomb so far as these depended on its nuclear properties.

4.5 This work of nuclear specification was not done once and for all, but proceeded by a series of successive refinements. At each stage the information gained served to determine the work of the Laboratory in a more concrete way, and as the basis for further refinements in research.

4.6 At the beginning it was of the greatest importance to estimate with some reliability the amount of active material per bomb that would be needed. Without this there was no way to determine the size of uranium separation and plutonium production plants under development.

4.7 In the next stage more accurate information was needed to provide a basis for concurrent work in the ordnance program. It was, for example, one of the requirements of the gun design program to estimate with some assurance the size and shape of the projectile and the muzzle velocity it would have to be given. For another thing, the effective mass of the active material would set limits to the over-all size of the bomb, which would in turn determine the type of plane to be used in its delivery.

4.8 In the last stage after certain basic engineering specifications had been frozen, it was necessary to determine, for instance, the exact mass and shape of active and tamper material, and in general to guide the final reduction to practice.

4.9 Although convenient as an aid in understanding the work of the Laboratory, such a separation into stages would give a false chronology. Even the preliminary stage did not end with the feasibility experiments described above. As the understanding of requirements became more detailed and reliable, the Los Alamos Laboratory continued to exert influence on the U^{235} and Pu^{239} production plans of the Manhattan District. A number of quite basic weapon specifications, to go to the next stage, remained undetermined for a considerable length of time. One was the choice of a tamper; another was the uranium hydride possibility; and a third was the mechanism of assembly - gun or implosion.

4.10 Despite these overlappings so characteristic of war development and of the whole Manhattan Project, one can trace the gradual shift that occurred from nuclear physics, through the difficult problems of the bomb assembly mechanism, to final development. This first half of our history is the period in which the organizing role was played by nuclear studies, and was gradually shifted to the study of assembly mechanism.

4.11 One of the first problems was the more precise determination of critical masses. As explained earlier (1.33) in a qualitative way, the critical mass depends on the rate of diffusion of neutrons out of the active mass as compared with the rate at which they are generated in it. One of the essential tools was, therefore, the statistical theory of neutron diffusion. Ordinary diffusion theory is valid in the range where the mean free path of diffusing particles is small compared to the dimensions which are of interest. This is not true of the bomb. The number of neutrons in a given small region depends not only on that in adjacent regions, but on the entire distribution throughout the mass. An integral diffusion theory had therefore to be employed, and means found to apply it in practical calculation. This problem was one focus of development. Another was the refinement of certain rough assumptions that had been necessary in making earlier calculations. One such assumption had been that neutrons were scattered isotropically. The correction was to take account of angular dependence. Another assumption was that core and tamper gave the same mean free path, which in general they do not. Still other assumptions subject to correction were that the neutrons had the same velocity, that the various cross sections were independent of velocity, and that there was no energy loss through inelastic collisions.

4.12 Most of these refinements implied a need for more precise experimental knowledge. For one thing they took account of new dependencies; for another the errors from theory and experiment had to be kept of comparable magnitude. Thus the early nuclear experiments, other than those already described, were centered around the measurement of cross sections, their energy dependence, and the number and energy spectrum of fission neutrons. Of these experiments the most time-consuming were the fission cross sections of Pu^{239} and U^{235} as a function of energy from the thermal to the high energy end of the fission spectrum. From a combination of relative and absolute fission cross section experiments performed over the period to August 1944, it was possible to plot fission cross section curves as a function of energy for both U^{235} and Pu^{239} from thermal energies to several million electron volts. These results were not only used in more accurate critical mass and efficiency calculations, but also were partially responsible for the abandonment of the uranium hydride program; partly because they showed that the energy-dependence which would make the hydride an efficient weapon did not occur, and partly because, through the evidence they provided for the existence of considerable radiative capture at thermal energies, the critical mass and efficiency estimates of metal uranium bombs became more optimistic. Investigation, suggested by the behavior of fission cross sections at low energies, led to the discovery that radiative capture in U^{235} was indeed significant, and even greater for Pu^{239}. Since measurements of the neutron number had been made at thermal energies for total absorption (capture plus fission) and not fission alone, and since capture would become less important at the high energies of neutrons operative in the bomb, it followed that the effective neutron number in both materials was higher than had been assumed. As a result of these considerations, the hydride program was carried on after the spring of 1944 only at low priority.

4.13 Although the hydride program was unsuccessful, the process of learning enough to understand its limitations contributed in a number of ways to the whole program. For example, the use of the assumption that the fission cross section was inversely proportional to neutron velocity made clear the importance of inelastic scattering in the tamper. In the first approximation it had been assumed that only neutrons scattered back elastically would contribute in any important way to the reaction. But if decreasing neutron energy was compensated for by increasing fission cross sections, this assumption could not safely be made. A lengthy series of back-scattering and transmission experiments with a considerable list of potential tamper materials was made, in which the scattering cross sections were measured for neutrons of various energies and for various scattering angles, and in which the energy degradation of scattered neutrons was also measured.

The Gun Method

4.14 During the first six months of the Laboratory, the gun method of assembly was the focus of administrative and technical activities in the ordnance program. The procurement of personnel and the design and construction of facilities centered around the gun; the implosion program was considered as a standby, and its facilities were an adjunct to those of gun development. During the period to August 1944 the main focus of activity was the plutonium gun, which was farther from standard practice than the U^{235} gun. The gun had several unusual features. The assembly velocities required to insure against the predetonation of a plutonium bomb were near the upper limit of standard gun design, 3000 feet per second. The gun had, in addition, to be light, and was expendable. The tube had to be as short as possible, for inclusion inside a practicable airborne weapon. This meant operation with the highest possible peak pressure. Finally, the gun had to operate with the highest attainable reliability.

4.15 The first guns were designed and being produced from the Naval Gun Factory by September 1943, and were received at Los Alamos in March 1944. Proof firing to test the behavior of propellants and to investigate problems of projectile and target design was begun in September with a 3 inch Naval A. A. gun.

4.16 Proof firing was also undertaken at a still smaller scale, 20 millimeters. The object of this program was to investigate "blind" target assembly, and to investigate an α-n gun initiator.

4.17 In August 1944, when the plutonium gun assembly program was abandoned, the high velocity gun had been thoroughly proved, and the techniques of proof well developed. The subsequent development of the U^{235} low velocity gun could therefore proceed without meeting new basic difficulties, while the main effort of the Laboratory was directed to the mounting difficulties of the implosion program.

The Implosion

4.18 The proposal for the implosion assembly was to make use of the plastic flow tamper and active material under high-explosive impact. A subcritical sphere of these materials would be compressed into a supercritical sphere. The first acknowledged advantage of the implosion over the gun was its much shorter time of assembly. This was of especial importance for the assembly of plutonium because of its expected high neutron background, which for slow assembly would make predetonation a serious danger. From this early conception there were several steps of evolution to the implosion mechanism finally employed in the Trinity explosion and the Nagasaki bomb. These steps arose out of the results of hydrodynamical calculations, of the discovery of a still higher neutron background in plutonium than had been anticipated, and from the difficulty of achieving symmetry in the imploding shock wave. It was recognized that larger charges would give more rapid assembly and might give some advantageous compression if the implosion were symmetrical enough. But in the existing state of the art observation of results was impossible. Both for this reason and because the decisive virtues of the fast implosion were not realized, it was on the deferred list.

4.19 One difficulty with the fast implosion as early conceived was the uncertainty in the time of initiation of a successful chain reaction. There was some discussion of a modulated initiator which would be "turned on" at the time of complete assembly, but this represented a serious added complication in bomb design.

4.20 The first decisive change in the conception of the implosion was a rough quantitative analysis of the assembly velocities attainable with very large charges of high explosive (HE), which suggested that because of the focussing effect of the converging material, one could introduce a strong steady source of neutrons into the bomb (e.g., by deliberately leaving the material in an impure state), and still beat the chain reaction and attain complete assembly. It was only a step from this to the realization that a large part of the kinetic energy of the imploding material would be transferred at the center of the converging mass into potential energy of compression. This remarkable phenomenon, of the compression of "incompressible" solid matter under the extreme pressures produced by the implosion, was too far from the course of ordinary terrestrial experience to be grasped immediately or easily.

4.21 These two steps were responsible for a marked change in the priority of the implosion program. They made it clear that the implosion had qualitative advantages over gun assembly, and that the many difficulties involved in its development (not all of which were yet appreciated) would be worth overcoming. At a Governing Board Meeting of October 28, 1943, the program was reviewed and the decision made to strengthen and push it. Within the limitations of hindsight, it seems fair to say that the de facto priority of the program only increased slowly, with the addition of new personnel and the strengthening of their organization. Ordnance and engineering work was geared to the gun program, and could not be redirected overnight. By the end of 1943 the implosion had caught up with the gun in priority; by April 1944, its facilities had been greatly expanded, and enough experimental evidence was in to show the great magnitude of the difficulties that were still ahead.

4.22 It is convenient to treat the theoretical and experimental aspects of the implosion separately during this period; for they started at opposite ends, and their point of convergence lay much farther ahead than was at the time anticipated.

4.23 Although the new understanding of the implosion was a great spur to the program, the gains to be made were by no means recognized at first as, in retrospect, they should have been.

4.24 It has been explained that assembly time and neutron background are complementary; to increase the latter requires that the former be decreased proportionately. At the very beginning, however, they had hardly been given equal weight. Raising the chemical purity standards set a difficult problem; but the chemists were able to accomplish difficult things. If higher purity was possible, it was only a gain so far as engineering the weapon was concerned. To increase velocities by the gun method, on the other hand, required a gun weight increased as the square of the velocity.

4.25 The quantitative investigation of the hydrodynamics of the implosion proved a very difficult job. An approximate method adaptable to hand-calculation was tried, but gave uninterpretable results. In the spring of 1944, the problem was set up for IBM machine calculation. These machines, which had recently been procured to do calculation on odd-shaped critical masses, were well adapted to solve the partial differential equations of the implosion hydrodynamics.

4.26 As was not unnatural at the beginning of this new line of investigation, there was some thought given to the implosion of uranium hydride. The density of this material was about half that of uranium, and the space occupied by the hydrogen would be recoverable under sufficient pressure. Samples of hydride prepared at Los Alamos were investigated at the high pressure laboratory of W. P. Bridgman at Harvard. Pressure density data up to 10 kilobars, still very low pressure from the point of view of the implosion, gave indication that the hydride was not in fact very easily compressible.

4.27 While theoretical investigation was familiarizing the Laboratory with the enormous potentialities of the implosion, its empirical study was getting under way. During the period to April 1944 some data were obtained from terminal observation, from the HE flash photography of imploding cylinders, and from flash X-ray photography of small imploding spheres.

4.28 Whereas the theoretical studies of the implosion assumed a symmetrical converging detonation wave, the only feasible method of detonating the HE was to initiate one or several diverging waves. It was assumed or, better, hoped that with several detonation points symmetrically spaced around a sphere, the difference would not be essential. From terminal observations some indications of asymmetry of collapse were obtained, but it was difficult to ascertain their cause. The first successful HE flash photographs of imploding cylinders showed that there were indeed very serious asymmetries in the form of jets which traveled ahead of the main mass. A number of interpretations of these jets were proposed, including the possibility that they were optical illusions.

Metallurgy

4.29 Another virtue of the hydride program not mentioned in paragraph 4.13 was the interest taken in the preparation and fabrication of this material. Studies were begun, among the first undertaken by the metallurgists, in the art of preparing high density compacts of this material. The result was that although after a year or so it was known that the hydride would not yield an efficient weapon, this material could be easily fabricated, and was used in making experimental reactors.

4.30 The main goal of metallurgical research in this period was the development of techniques for handling the final preparation of active and tamper materials in the large amounts necessary for the bomb. Apart from early work with the hydride, effort was first concentrated on the metallurgy of uranium. This subject was already fairly well developed in other branches of the project. The Los Alamos requirements were, however, somewhat different and more exacting. There was much greater emphasis on maintaining a high chemical purity and on yield. A bomb-reduction technique was developed in the first period and perfected in the second, which admirably satisfied these requirements.

4.31 One of the reasons for the early work on uranium metallurgy was its hoped-for resemblance to that of plutonium, as yet nonexistent in workable amounts. When the first such amounts of plutonium appeared - in March 1944 - techniques for its reduction were already under development; by the end of the first period satisfactory bomb-reduction methods had been perfected.

4.32 The investigation of plutonium metallurgy was one of the principal undertakings of the metallurgical groups. A properly scientific study of the properties of the new element was of necessity limited by the time available and the pressure for usable methods. The standards of usability, moreover, were much harder to meet than in the case of uranium. According to the original purity requirements, all operations would have to be carried out in such a way as to avoid contamination with light elements, even of a few parts per million. This made necessary a large subsidiary program for the development of heavy-element refractories. The substantial relaxation of purity requirements that came with the abandonment of the plutonium gun program at the end of the first period was sufficient to guarantee success. Indeed by this time the original high purity goals had nearly been reached, and some simplification of techniques became possible. In July 1944 experimental proof was obtained of the alpha (room temperature) and beta phases.

4.33 Aside from the metallurgy of active materials - uranium hydride, uranium, and plutonium - several techniques were developed for the fabrication of materials with important nuclear properties, notably boron and beryllia. These were techniques of powder metallurgy, and the object in both cases was to attain the highest possible densities. The main pressure for the production of boron came again from the hydride gun program, for which it would be difficult to dispose a sufficient number of critical masses of hydride into gun and target.

4.34 In this connection the Laboratory undertook to procure large amounts of boron enriched in B^{10}, which constitutes about 20 per cent of normal boron. A method for the separation of B^{10} had been developed by Urey, and was further developed by him at the request of the Los Alamos Laboratory. A pilot plant was constructed in the fall of 1943, to develop the method and to provide experimental amounts of the separated isotope. Early estimates (February 1944) set the needed production rate of the isotope at a figure comparable to the production of separated uranium. Plant construction was undertaken by Standard Oil of Indiana. Difficulties in construction and a decreasing probability that boron would be used in large amounts caused a decrease in the scheduled capacity of the plant by 25 per cent.

4.35 Even after there was reasonable assurance that a bomb made of hydride would not be used, and especially not a hydride gun, it was decided to maintain production of the B^{10} isotope because of its potential usefulness in an autocatalytic bomb, if one could be developed. This isotope was, indeed, very useful in small quantities in counters and as a neutron absorber.

4.36 Beryllia compacts of high density were developed by the metallurgists for use in the Water Boiler (4.48) tamper, actual production being carried out by The Fansteel Metallurgical Corporation under subcontract.

Chemistry

4.37 The principal work of the chemists in this period lay in the field of uranium and plutonium purification, analysis, and recovery.

4.38 The first purification work was begun at a time when there was very little plutonium in existence, and when only microchemical investigation had been undertaken. The first plutonium arrived at Los Alamos in October 1943. Until then work was necessarily limited to the study of various stand-ins, including uranium. After that time until the arrival of the first Clinton plutonium, stand-in work plus microchemical work, plus the results of similar work at other branches of the project provided the only information available. Despite these handicaps, by August 1944 there was strong assurance that purity specifications could be met on a production basis. The first, the "wet" stage of purification, had been worked out in essentially final form, as had been the recovery processes for re-cycling plutonium in chemical and metallurgical residues. The final "dry" stage of purification was worked out in outline, but exact procedures were not yet settled. One of the most serious difficulties, from a technical point of view, was the prevention of contamination from dust, etc., that would undo the work of purification. It was this factor that made necessary the construction of an air-conditioned laboratory building. In the latter part of this period a new technical difficulty was discovered, the serious danger from plutonium poisoning (3.94), which made necessary the development of enclosed apparatus wherever possible.

4.39 The extraordinary purity requirements for plutonium necessitated the development of supersensitive analytical techniques. In some cases impurities amounting to only a few parts per million had to be measured. To add to the difficulties, samples assayed had to be small, especially in the period when analytical techniques were first being developed. The principal methods developed were spectrochemical and gasometric. Spectrochemical methods were developed by or in close liaison with the chemists at the Metallurgical Laboratory. Gasometric methods for oxygen and carbon analysis were developed at Los Alamos.

4.40 The end of the first period saw the virtual completion of the difficult program of plutonium purification and analysis. The corresponding processes for U^{235} had also been carried out, but were of relatively minor difficulty. At this time the relaxation of purity requirements made it unnecessary to pursue these researches farther. The period that ensued was one of transition to production methods, made difficult primarily by the increasingly serious dangers of plutonium poisoning.

4.41 The radiochemists worked in cooperation with the experimental physics groups and ordnance groups. One of their principal contributions to experimental physics was the preparation of thin foils of a wide variety of materials and specifications. They developed several new techniques for preparing foils, carrying this activity to a much higher level than had been possible in other physics laboratories. Another contribution was the development of very sensitive neutron counters. Early in 1944 a radon plant was constructed as part of a program looking for neutrons associated with alpha radioactivity (6.21) and as a source of material for a possible radon-beryllium initiator. Another possible choice for the initiator was polonium. Research polonium was prepared by irradiation of bismuth in the Clinton pile, and purified at a special plant. Aside from research on polonium, the other main activity of the radiochemists in the summer of 1944 was the design and construction of a "mechanical chemist," a remote control plant for extracting and handling the highly radioactive radio-lanthanum to be used at the Bayo Canyon RaLa site.

The Discovery of Pu^{240}

4.42 There is perhaps no better illustration of the interconnection of research and development at Los Alamos than the series of developments that led to the discovery of the 240 isotope of plutonium in the Clinton product. As was mentioned above (4.1) there was room for doubt as to the value of plutonium as bomb material, up to the time when, in the summer of 1943, its neutron number was first measured. Even with the favorable result of this measurement there were still serious difficulties: from 1 gram of plutonium there are 2×10^9 alpha particles emitted per second. To keep the neutron background from (α, n) reactions down to the level where fast gun assembly was feasible required high purity; in the case of three light elements, less than one part per million.

4.43 Spontaneous fission measurements had been undertaken first at Berkeley, for the direct purpose of ascertaining the neutron background from this source of U^{235}. At Los Alamos these measurements were refined and extended to Pu^{239} and other materials. In the summer of 1943, meanwhile, there came through from France a report that Joliot had found a neutron emission associated with the alpha radiation of polonium, but not coming from the action of this radiation on light element impurities. Although this report was not believed correct, it was recorded in the Minutes of the Governing Board, and the general intention stated of looking into all the questions connected with spontaneous neutron emission.

4.44 As a result of the Joliot report, work was begun to develop highly sensitive neutron counters, and a radon plant was obtained. The reason for the latter was that radon was the alpha emitter which could be most highly purified. If it was found that there was heavy neutron emission from alpha emitters as such, this might make a modulated initiator impossible. It might also mean a prohibitively high neutron background in plutonium itself.

4.45 As the spontaneous fission measurements increased in reliability, it was found that the spontaneous fission of plutonium was slow enough to make the neutron background from this source not serious. In the meantime, however, another piece of research entered into the story. Fission cross section measurements at low energies, whose programmatic justification was to obtain data to be used in calculating the uranium hydride critical mass, showed the presence of resonances in the U^{235} fission absorption spectrum. This led, for theoretical reasons, to the expectation of sizable radiative neutron capture. In the case of Pu^{239}, this meant the production of a new isotope, Pu^{240}. Since this isotope would be produced by the absorption of two neutrons in U^{238}, its concentration in the pile plutonium would go up with heavier irradiation.

4.46 In the summer of 1944, therefore, when the first Clinton plutonium made by chain reactor arrived - much more heavily irradiated than the previous samples made by cyclotron bombardment - the existence of Pu^{240} was verified, as was the fear that it might be a strong spontaneous fissioner. Neutron background in the plutonium which would be produced at full power was pushed up into the region where, to prevent predetonation, assembly velocities would have to be much greater than those possible with the plutonium gun.

4.47 The only alternative to abandoning the gun method for plutonium was to find means of separating out the offending isotope. This would mean another major investment in separation plant, and could hardly be accomplished within the time alloted before military use. The implosion was the only real hope, and from current evidence a not very good one. Nevertheless the Laboratory had at this time strong reserves of techniques, of trained manpower, and of morale. It was decided to attack the problems of the implosion with every means available, "to throw the book at it." Administratively, the program was taken out of the Ordnance Division, and divided between two new divisions. One of these was to be concerned primarily with the investigation of implosion dynamics, the other primarily with the development of adequate HE components. And this story marks the beginning of the second part of the present history.

The Water Boiler

4.48 The implication of gloom at the fate of plutonium gun method and the difficulties of the implosion do not misrepresent the atmosphere of the Laboratory in the spring and summer of 1944. Yet the program was many sided; during this same period the Laboratory enjoyed its first major success. This was the operation of the Water Boiler to produce divergent chain reactions. This was first accomplished on May 9, 1944, and from this time until August a number of experiments were carried out to determine nuclear quantities of interest. The Water Boiler was itself an integral experiment, and provided a general check of theory. In fact, the critical mass of the Water Boiler had been predicted by the theorists with almost perfect accuracy. Although they pointed out that the exactness of this prediction was certainly fortuitous in view of some blind assumptions which they had been forced to make, their prestige in the Laboratory was given a well deserved boost. In its difficulties and its successes, the Laboratory was moving into a stage of heightened activity, and preparing itself to face the final problems of weapon development.

Chapter V

THEORETICAL DIVISION

Organization

5.1 The broad purpose for which the Theoretical Division was formed, as had been said (1.54-1.56), was to develop nuclear and hydrodynamical criteria relating to the design of the atomic bomb, and to predict the detailed performance of the weapon designed. At the beginning the bulk of the division's effort, accordingly, was devoted to the investigation of two closely related key problems: the calculation of the critical mass and the nuclear efficiency.

5.2 The first organization of the division centered around these problems. With the rise of the implosion to prominence the organization of the division, under H. A. Bethe as Division Leader, was formalized into groups as follows (beginning March 1944):

T-1	Hydrodynamics of Implosion, Super	E. Teller
T-2	Diffusion Theory, IBM Calculations, Experiments	R. Serber
T-3	Experiments, Efficiency Calculations, Radiation Hydrodynamics	V. F. Weisskopf
T-4	Diffusion Problems	R. P. Feynman
T-5	Computations	D. A. Flanders

5.3 During June 1944, R. Peierls took charge of the Implosion Group in place of E. Teller who formed an independent group outside the Theoretical Division (13.3). This group acquired full responsibility for implosion IBM calculations. During July 1944 Group O-5 (E-8, 7.1) joined the Theoretical Division on a part time basis, its work in the Ordnance Division being largely completed (14.1).

Diffusion Problems

5.4 One of the tasks of the theoretical program at the beginning of the Laboratory was the development of means for predicting accurately the critical mass of active materials. The essential and most difficult factor in these calculations was the theory of neutron diffusion. The other factors were principally matters of evaluating data from scattering and fission experiments to obtain the appropriate cross sections and the number of neutrons emitted per fission. The critical mass is defined as that amount of material from which neutrons will disappear by leakage and nuclear capture at just the rate at which they are born from fissions occurring in the mass. But to calculate this requires a knowledge of the way in which neutrons will distribute themselves on the average in the mass. This is the problem of neutron diffusion theory.

5.5 It was possible, at the outset, to write down the integral equation whose solution would give the exact neutron distribution, taking account of the variation in velocity of the neutrons, the dependence of scattering and fission cross sections on velocity, and the anisotropic nature of scattering. This equation, which is written simply on the basis of conservation considerations, was formulated by Boltzmann and bears his name. But as it stands this equation has no known exact solution.

5.6 Two kinds of approximate solutions were possible, however, and some calculations had been made by means of them at the beginning of the Laboratory. One was that of ordinary differential diffusion theory, in which the diffusion of neutrons was treated by analogy with heat diffusion. Calculations here were relatively simple, but the results were known to be quite inaccurate. In fact the neutron diffusion problem does not meet the requirements of differential diffusion theory: among other requirements, that of a small change of neutron density per mean free path. This condition is satisfied approximately in a large pile; but in the bomb the critical size is itself of the order of the mean free path. The other attack that had been developed was based on an exact solution of the integral equation of diffusion for one special case. This solution was found at Berkeley by S. P. Frankel and E. C. Nelson and completed in the first months at Los Alamos. The conditions under which this solution was valid are enumerated below, since much of the subsequent work of the Theoretical Division consisted in an effort to find solutions valid under less restrictive conditions. The conditions are:

(a) Neutrons have a single velocity.
(b) The core and tamper nuclei are treated as stationary, and all neutron collisions with them as elastic.
(c) Neutrons are scattered isotropically.
(d) The mean free path of neutrons is the same in core and tamper.

The method of solution found, called the "extrapolated end-point" method, was first worked out for untamped spheres, and later extended to the case where the mean free path in core and tamper were equal. The method was developed independently, but was later found to be an extension of a procedure due to Milne for the solution of certain astrophysical problems.

5.7 Using the extrapolated end-point method, it was possible to calculate the critical mass of a solid U^{235} sphere with an effectively infinite tamper. Three problems were thus defined: (1) to allow for the finite thickness of the tamper, (2) to make calculations for shapes other than spherical, and (3) to find means for calculating the critical mass when the mean free paths in core and tamper were not identical. The extrapolated end-point method could not be applied in these cases except as an approximation of uncertain accuracy. In essence it is a method which applies differential diffusion theory to a fictitious scattering material whose boundary or end-point extends a calculable distance beyond the actual boundary of the material. It is strictly valid only under the conditions enumerated.

5.8 The first problem enumerated above was met by ad hoc methods, such as replacing the finite tamper by an infinite tamper plus a fictitious neutron absorber. The second problem could not be solved simply. What was done in practice was to resort to the inexact methods of diffusion theory (with the extrapolated end-point), from which calculations for odd shapes could be made. The ratio of the critical masses of an odd-shaped body to a spherical body, obtained by this inexact method, could then be applied as a correction to the accurately known critical mass for a sphere.

5.9 The third problem, the case of unequal mean free paths in core and tamper, was responsible for a much longer series of developments. The first of these was an effort to employ variational principles to solve the original integral diffusion equation. The variational approach was applied successfully to spherical and cylindrical shapes, to slabs, etc. This gave a useful check on the accuracy of the extrapolated end-point method. The agreement was very close. When, however, the use of the variational techniques was extended to the case of unequal mean free path in core and tamper, they proved to be extremely laborious. At about this time, moreover (June 1944), a technique previously developed at the Montreal Project was introduced. This technique, based upon an expansion of the neutron density in spherical harmonics, was considerably easier to apply than the earlier variational method, and in test cases gave very accurate results.

5.10 This particular part of the story is completed sometime later (11.4 ff). But by the end of the period reviewed, it was possible to say that the neutron diffusion problem had been solved under restrictions (a), (b), and (c) above, but with (d) eliminated. Solutions for particular cases were still sometimes rather expensive to obtain.

5.11 In the meantime assumption (c), that neutrons were scattered isotropically, was being looked into. It was found in a number of test cases that very accurate results could be obtained by assuming isotropy, and substituting for the scattering cross section the so-called transport cross section, a kind of weighted average which gives the effective scattering in the initial direction of motion of the scattered neutron. Assumption (b) was entirely reasonable, except in the case where inelastic scattering in tamper materials had to be considered seriously. With this exception, therefore, the main limitation remaining was assumption (a), that all neutrons have a single velocity. The greater part of the work described so far was done by Group T-2, but every group had a hand in it at some point.

5.12 The attack on the many-velocity problem had proceeded simultaneously with the work described above, in the sense of investigating methods by which the many-velocity problem could be reduced to a series of one-velocity problems. This work was done primarily by Group T-4. The problem posed itself naturally in connection with the investigation of the uranium hydride bomb, for in this case the energy degradation of neutrons from elastic collisions with hydrogen was one of the essential characteristics of the chain reaction. Quite early, methods were found for treating the hydride problem, with a continuum of velocities, under quite unrealistic assumptions, such as an infinite medium of core material in which there was a sinusoidal distribution of neutrons. The case involving two media, i.e., core and tamper of different materials, could not be treated at first. By July 1944, however, a method had been developed which was applicable to a spherical core and tamper. This method allowed the treatment of a continuum of velocities, and was subject only to the restriction that there be no inelastic scattering in the tamper medium. Unfortunately this inelastic scattering was not a negligible effect with the tampers that were being considered. Within a fairly short time this difficulty had been overcome, although only to the extent of allowing for three or four neutron velocity groups instead of the continuum.

5.13 In the case of hydrogenous material it could not be assumed that neutrons were scattered isotropically [assumption (c) above]. It was found however, semi-empirically, that this fact was adequately accounted for by the use of the transport cross section, as in the case of the all-metal diffusing medium.

5.14 Other means for accounting for the continuum of velocities were adopted in special problems, such as that of calculating the distribution of thermal neutrons in the Water Boiler.

Water Boiler

5.15 One of the first practical requirements in critical mass calculation was to estimate the critical mass of the Water Boiler. These calculations were made by a variety of methods. In this case as in that of the hydride calculations, the slowing down was an essential factor; in fact, the boiler would be of small critical dimensions only because it slowed neutrons down to thermal velocities, taking advantage of the larger thermal fission cross section of U^{235}. The standard method, the "age theory" that had been developed by Fermi for calculating the thermal neutron distribution in piles, was inaccurate when applied to a small enriched reactor, because it required a very gradual slowing down of the neutrons. This condition was satisfied for a carbon moderator, with mass 12 times that of the neutrons; it was not satisfied with a hydrogenous moderator such as water, because the neutrons and hydrogen nuclei are of the same mass, and energy loss can occur rapidly. A group method was developed at Los Alamos which used differential diffusion theory but assumed that neutrons were of three velocities (fission energies, intermediate, and thermal). A number of other methods mentioned above were also tried out on this problem, primarily with the purpose of examining the variation among them and as a test of their power when applied to a new problem. The finally predicted value of the critical mass for the Water Boiler was almost exactly correct; a pleasing, though rather fortuitous result in view of subsequent revisions of the cross-sections involved.

5.16 A number of other problems affecting the operation of the water boiler were examined theoretically; cooling and shielding, effects of temperature changes on the degree of criticality, effect of sudden changes in the position of the control rods, etc. After the boiler was put in operation, the theorists were of service in connection with experiments, such as the interpretation of fluctuations in its operation. This work was essentially completed by the beginning of 1944. Most groups participated in one or another type of Water Boiler calculation, but the main work was that of R. F. Christy in Group T-1.

The Gun

5.17 Critical mass calculations for the gun assembly were complicated primarily by the odd shape of the assembly. The critical mass problem for the gun was not only that of estimating the number of critical masses in the completed assembly, but also of estimating the amount of active material that could safely be disposed in the two parts before assembly. It was also necessary to know how the system went from its initial subcritical to its final supercritical position, in order to be able to calculate the probability of predetonation. The early rough specifications for the gun had been based on critical mass estimates from differential diffusion theory. By February 1944, there was pressure from the Ordnance Division to obtain more reliable specifications, and at this time sufficiently accurate calculations had been made so that, for the U^{235} gun, Group T-2 specified the actual bore. The specification of the gun for the Pu^{239} assembly was reached a short time later. The same group was able to give essentially complete specifications by the summer of 1944 for both gun assemblies, fortunately after cross-section measurements by the Detector Group had resulted in slightly lower average values for U^{235} than those used in earlier calculations.

The Implosion

5.18 The history of theoretical implosion studies lies mostly outside the Theoretical Division until the Fall of 1943. The idea of something like an implosion, as an alternative to gun assembly, had entered several heads before the beginning of Los Alamos. Its first history at Los Alamos belongs mainly to the Ordnance Division, where the initial calculations of attainable assembly velocities were made.

5.19 The Theoretical Division entered the picture when the fast implosion was proposed by von Neumann, and its potentialities as a weapon qualitatively superior to the gun were appreciated. The general story of this development is told in Chapters 7, 15 and 16. Here the emphasis will be upon the theoretical problems that were involved. Implosion studies were the responsibility of Group T-1, with the assistance of other groups, particularly T-2.

5.20 The first problem attacked was that of the time of assembly when (as proposed by von Neumann) large amounts of explosive were used. In this case, the energy required for the work of plastic deformation was small compared to the total energy of the explosive, so that to a first approximation the kinetic energy of the mass moving inward could be assumed to be conserved.

5.21 The numerical solution of the partial differential equation describing the implosion was too difficult for hand calculation with the computing staff available at Los Alamos, when a realistic equation of state was employed. As a result the first effort made was to find simpler approximate equations of state. The first method was based on a multiphase model, in which the state of the imploding material was assumed to change discontinuously. A considerable amount of effort was put into the multiphase model, but the results proved very difficult to interpret.

5.22 Some time was gained in solving this calculational problem by virtue of the fact that IBM machines had already been ordered by the division, with the original intention of using them for the difficult calculations of critical masses of odd-shaped bodies. These machines arrived in the first part of April 1944, and in the meantime preparations had been under way for numerical integration of the hydrodynamical equation by means of them. Preliminary calculations had to be made to determine the initial conditions at which to start the IBM calculations. It was necessary to derive the equation of state of uranium at high pressures, a calculation based on the Thomas-Fermi model of the atom. Results at low pressures were obtained from experimental data of P. W. Bridgman, and the intermediate region determined by interpolation.

5.23 The first results of IBM calculation of the implosion were extremely satisfactory. As a result the unrealistic multiphase implosion model was dropped.

5.24 Just at the time of these first IBM results, a new problem arose which brought the work of the division into closer connection with experimental implosion studies going on at the time. Calculations of implosion dynamics had started with the initial condition of an inward-moving spherical shock wave. But the creation of such a wave had so far proved impossible to achieve. The rather erratic results obtained from multipoint detonations, and in particular the observation of jets, directed theoretical attention to the problem of interference of detonation waves. It was found that a diverging spherical wave will accelerate materials less rapidly than a plane wave, and still less rapidly than a converging wave. In an implosion with many detonation points, the explosive waves are divergent to start with, but it had been assumed that their interaction would make them convergent. When this question was examined theoretically, it was immediately discovered that this smoothing out was by no means assured, and that the fact to be concerned about was the development of high pressure at the point where detonation waves collided. The most obvious method of avoiding these difficulties was to employ explosives so arranged that they would produce converging waves to start with. The use of such lens configurations had just been suggested at this time by J. L. Tuck, and the above observation on shock interactions was an argument in favor of its adoption. It was, however, a completely untried and undeveloped method, which no one wished to employ unless it became absolutely necessary to do so.

5.25 Another important hydrodynamical principle was brought to bear on the problems of implosion by the first visit to the Laboratory of G. I. Taylor in May 1944. He presented arguments to show that an interface between light and heavy material is stable if the heavy material is accelerated against the light material and unstable in the opposite case. This created the possibility of serious instability in the implosion, where light high explosive would be pushing against heavier tamper material, or where a light tamper might be pushing against the heavy core. A similar difficulty, leading to mixing, was also foreseen in the nuclear explosion, as the core became less dense on expanding against the compressor tamper.

5.26 From these two developments there started a trend of thought that radically altered the whole implosion program. From the IBM results the behavior of the symmetric implosion was soon rather completely understood. But at the same time it became more and more doubtful whether a symmetric implosion could be achieved. Thus it was that in the remainder of the year the design of the explosive charge moved in the more radical direction represented by the lens program, while the design of the inner components moved in a more conservative direction.

5.27 As a result of calculations on the development of asymmetry, it was possible to give the Explosives Division a preliminary statement of the asymmetry that could be tolerated. A variation in velocity by 5 per cent was considered the maximum allowable.

5.28 During the remainder of the period under review, more IBM and associated calculations were made, the stability studies referred to above were continued, and calculations were undertaken to determine the shape of lenses to convert the detonation wave to a plane or spherically convergent form. The possible need for various corrections to the simple theory - borrowed from geometrical optics - were also considered.

Efficiency

5.29 The calculation of efficiency was perhaps the most complex problem that the Theoretical Division had to face. The theory of efficiency had to follow the neutron chain reaction and neutron distribution in the bomb, in a medium of fissionable and tamper material that was itself being rapidly transformed by the reaction in both its nuclear and dynamic properties. Every factor involved in the critical mass calculations was involved here, but in a dynamical context which made dubious some of the simplifying assumptions underlying those calculations.

5.30 The first efficiency calculations had been made prior to Los Alamos, at Berkeley, for the case of small excesses over the critical mass. These calculations were preceded by investigation of the hydrodynamical behavior of the core and tamper during the chain reaction, a study which led to the theory of the shock wave which travels into the tamper, and of the rarefaction wave which travels into the core, from the core-tamper interface. The effects of these phenomena on the efficiency were calculated. The diffusion of neutrons was treated by differential theory, which allowed simple estimates of the dependence of efficiency on various tamper properties, such as mean free path, absorption, and density.

5.31 The next step in efficiency calculation - by Group T-4 - was applicable to bombs having a mass far greater than critical. These calculations were based on results obtained by Group T-2, which gave the decrease of the multiplication rate for small expansions of the exploding bomb.

5.32 Once estimates of efficiency in these two cases had been obtained, a semi-empirical formula was developed which fitted the Los Alamos calculations for large excess masses, and reduced in the limit of small excesses to the earlier efficiency formula developed in Berkeley. This formula developed by Bethe and Feynman provided an easy means for making efficiency estimates when the critical mass (or more precisely, the radius to which the core of a given bomb must expand before neutron multiplication is stopped) and the initial multiplication rate were known.

5.33 The possibility of using the Bethe-Feynman formula for intermediate excess masses was justified by the following argument. For small excess masses the effect on the mean density of the ingoing rarefaction and outgoing shock waves approximately canceled. For large excess masses the same thing was true, since in this case the waves would be reflected back and forth many times before the multiplication was stopped, and one could regard the multiplication as a function of the average pressure. A plausibility argument was then invoked to the effect that since this independence of the hydrodynamical details held at both extremes, it also held in the intermediate cases.

5.34 Certain restrictions and unproved assumptions involved in all of the calculations referred to above are listed below:

(a) The effects of radiation can be neglected.
(b) The neutron multiplication can be calculated by an adiabatic approximation.
(c) The tamper and core have the same neutron scattering per unit mass.
(d) The density of material in core and tamper is the same.
(e) The absorption in the tamper is equivalent to that in an infinite nonabsorbing tamper.
(f) The effects of depletion in the material are unimportant.

5.35 Of these six assumptions, (f) was the easiest to allow for. The effects of depletion were negligible for small efficiencies, and could be calculated for larger ones. Rough methods were found for estimating the effect of relaxing (c), (d), and (e). Assumption (b), the error involved in the adiabatic approximation, was investigated in some detail. In this approximation the total number of neutrons in the expanding bomb is assumed to increase at a rate proportional to itself, the rate being calculated for any instant from the excess over critical at that instant, assuming the nuclei of core and tamper to be at rest. This is the same assumption as assumption (b) discussed earlier in connection with diffusion problems. In that case the nuclei are relatively at rest and the assumption is a good one. But during the explosion the bomb material acquires a very high mean mass motion, and the assumption is questionable. A correction factor was found by considering the nonadiabatic theory of small expansions of a slightly supercritical bomb.

5.36 With the exception of assumption (a) as to the effect of radiation, it was possible, by the end of 1943, to give a reasonably good account of the efficiencies to be expected from proposed weapon designs.

5.37 After this time the emphasis in efficiency studies shifted to more specific problems. One was to develop the best possible criteria for the choice of tamper material. A second was to investigate the efficiencies obtainable from implosion bombs. A third was to try to obtain a better understanding of the effects of radiation on the course of the explosion and on the attainable efficiencies.

5.38 The factors affecting the choice of a tamper were investigated in some detail by Group T-3. Apart from radiation (discussed below) the virtues of a tamper could be summarized under two main heads: (1) its neutron reflecting properties, and (2) its effect on the hydrodynamics of the explosion. Point (1) would be understood perfectly by knowing the number of neutrons the tamper scattered back into the core, the time delays involved in this back scattering, and the energy of the neutrons returned. Calculation of these effects depended upon a knowledge of elastic scattering cross sections as a function of the angle of scatter, and of inelastic and absorption cross sections. Point (2) involved calculation of the extent to which various tampers tended by their inertia to hold the active material together during the explosion, and of the behavior of the shock wave in the tamper.

5.39 In connection with its investigation of tamper problems, Group T-3 performed extensive calculations in collaboration with the D-D Group of the Experimental Physics Division, to interpret the scattering data obtained by the latter for various tamper substances. These calculations were limited by the fact that they had to bridge the gap between a detailed theory for which the differential constants were unknown, and a semi-integral type of experiment in which only certain average effects were measured.

5.40 The effects of radiation on the nuclear explosion were, as has been said, the most problematic of the factors that had to be taken into account. A knowledge of the role of radiation was important not only in predicting the efficiency for a given design of weapon, but also in the choice of a tamper. This is so because different tampers have different degrees of transparency to radiation, a property which will affect the course of the explosion and its efficiency. The effect of radiation on the course of the explosion may be described roughly as follows. During the initial expansion of the bomb, the active material is being heated exponentially by the release of fission energy. The tamper is also heated, but far less rapidly. In the time available, the only effective mechanism for the transfer of heat from core to tamper is the outgoing shock-wave.

5.41 Simultaneous with its work on tamper problems and radiation, Group T-3 began, early in 1944, to re-work the earlier calculations of efficiency. The assumption that the multiplication rate depended only on the average pressure over the core and tamper was set aside, and its dependence on the shock and rarefaction waves examined in detail. For this purpose it was first assumed that these were plane waves. Sometime later this assumption was replaced by an "informed guess" as to the effects of convergence and divergence. Only much later were these effects actually calculated. In these calculations it was possible to set aside assumptions (c) and (d), and consider an arbitrary combination of core and tamper materials. Another refinement introduced in these calculations was the replacement of differential diffusion theory by more exact methods.

5.42 In May 1944, while the work described above was under way, the stability considerations brought to the Laboratory by Taylor (5.25) created a new worry about efficiency. When the hot core material pushed against the cold tamper, according to Taylor's principle, the interface would be unstable, and mixing of core and tamper would occur. This might lessen the effectiveness of the tamper. Investigation showed that this effect would probably not be large, since the loss of active material that leaked into the tamper would be partly compensated by the tamper fragments that remained behind. It was observed, moreover, that by the time instabilities could become serious, radiation would have moved the interface between light and dense material some distance out into the tamper, and the mixing that would occur would be mainly of tamper with tamper.

5.43 Another aspect of the efficiency studies of the implosion bomb is that of predetonation. It is true that the initial pressure and density distributions in the implosion are nonuniform, whereas in the gun assembly they are uniform. This difference, however, was shown to be unimportant. The great difference between the two methods lay in the larger neutron background of Pu^{239} and in the dependence of the predetonation probability on the course of the implosion. For a long time, moreover, it was hoped that an efficient weapon would be possible which used only a steady neutron source. In such models the efficiency had to be regarded as a random variable with a rather large dispersion, depending upon the particular moment when a neutron managed to start a divergent chain reaction. This involved the development of the statistical theory of chain reactions in which not only the average number of neutrons per fission played a role, but also the random variation of this number from fission to fission.

The Super

5.44 The deuterium bomb or Super project was relatively divorced from the main work of the Laboratory. As a development secondary to that of the fission bomb, its importance was nevertheless such that it was carried on throughout the course of the Laboratory. From its first conception, before Los Alamos, this work was under the direction of Teller. In the last period of the Laboratory Teller was joined by Fermi. By coincidence the first idea of such a bomb, at least in relation to the Los Alamos program, had been evolved in a lunchtime discussion between Fermi and Teller early in 1942.

5.45 A fundamental understanding of the fast thermonuclear reaction had been reached by the beginning of Los Alamos. In the first rough calculations Teller had ignored the effect of radiation, which is to drain off energy at a rate that increases rapidly with temperature. These early rough calculations indicated that the reaction would take place if ignited by the explosion of a fission bomb as "detonator." They also indicated, in fact, that the reaction would go too well, and that the light elements in the Earth's crust would be ignited.

5.46 The energy transfer phenomenon was well enough understood in the Summer of 1942 to make it apparent that a Super could, in principle, be made. At the Berkeley summer conference in 1942, Teller presented his analysis of the mechanism and argued that such a bomb was feasible. A good part of the discussion at this conference was devoted to the examination of Teller's proposals.

5.47 One further suggestion of great eventual importance was made by Konopinski. This was to lower the ignition temperature of deuterium by the admixture of artificially produced tritium (H^3). The apparently very much greater reactivity of tritium led him to this proposal. It was not immediately followed up because of the obvious difficulty of manufacturing tritium and the hopefulness of igniting pure deuterium. Eventually, as it will develop, new difficulties of ignition were to be uncovered so that the introduction of artificial tritium began to appear necessary.

5.48 One further topic was discussed at the Berkeley conference, the effect of secondary nuclear reactions. Products of the deuterium-deuterium (D-D) reaction were, with about equal probabilities, a He^3 nucleus plus a neutron, or a tritium nucleus and a proton. It was pointed out by Bethe that the reaction of deuterium with tritium, even though secondary, was of considerable importance. The T-D reaction releases nearly five times as much energy as the D-D reaction; the reaction cross section was, moreover, likely to be considerably larger.

5.49 The consequences of the Berkeley discussions of the Super were that its investigation was continued, that measurements of the D-D and T-D cross sections were undertaken, and that, when the Los Alamos Laboratory was being planned, a research program on the Super was included.

5.50 After the conference and before Los Alamos the measurement of the D-D cross section was undertaken by Manley's group at Chicago, and that of the T-D cross section was undertaken by Holloway's group at Purdue.

5.51 At Los Alamos no systematic theoretical work on the Super was undertaken until the Fall of 1943. A Cryogenic Laboratory was started by the group under E. A. Long, with the object of building a deuterium liquefaction plant. A considerable amount of work on the properties of liquid deuterium was carried out by Prof. H. L. Johnston under subcontract at Ohio State University (8.95 to 8.98).

5.52 In September, Teller proposed that there be more intensive investigation of the Super. Experimental cross sections had been revised upward, so that the bomb would be feasible at lower temperatures. In addition there was some slight evidence that the known German interest in deuterium might be directed toward production of a similar bomb. Work was resumed at this time, but not with high intensity. Teller and his group were largely occupied with other and more urgent problems.

5.53 The program of the Super was re-evaluated in February 1944 at a Governing Board meeting. Theoretical difficulties made it appear that it might be difficult to ignite deuterium because of energy dissipation. In case investigations should show that the difficulty of igniting deuterium was too great, there was one remaining alternative, which was to return to the proposal of Konopinski to lower the ignition temperature by admixture of tritium. A small percentage of tritium would bring the ignition temperature down from the neighborhood of twenty kilovolts to around five.

5.54 The practicability of using tritium-deuterium mixtures was limited by the very great difficulty of obtaining tritium. It could be produced from the reaction of neutrons with Li^6, yielding tritium and He^4. The very small sample of tritium that had been used in cross section measurements at Purdue had been produced by cyclotron bombardment. Larger scale production would be possible in such a pile as the Hanford pile, but could utilize only the small percentage of excess neutrons not needed to keep the pile in production.

5.55 Both because of the theoretical problems still to be solved and because of the possibility that the Super would have to be made with tritium, it appeared that the development would require much longer than originally anticipated. Even though this was the case, it was decided that work on the feasibility of so portentous a weapon should be continued in every way possible that did not interfere with the main program. Tolman, who was present at this meeting as General Groves's adviser, affirmed that although the Super might not be needed as a weapon for the war, the Laboratory had a long range obligation to carry on this investigation.

5.56 Although no final decision was made at the meeting referred to, it in fact defined subsequent policy. In Teller's group further theoretical work was carried on, which confirmed the difficulty of igniting pure deuterium.

In May 1944 Dr. Oppenheimer discussed the matter of tritium production with General Groves and C. H. Greenewalt of the du Pont Company. It was there decided that experimental tritium production would be undertaken, using surplus neutrons in the Clinton pile.

Damage

5.57 The detailed investigation of damage and other effects of nuclear explosion was not pursued very far in the period under review. Some results, going beyond the rough estimates reported in paragraph 1.57 were, however, obtained in the summer and fall of 1943. There was further investigation of the shock wave in air produced by the explosion, of the optimum height for the explosion, of the effects of diffraction by obstacles such as buildings, and of refraction caused by temperature variation. There was some calculation of the energy that might be lost through the evaporation of fog particles in the air. Estimates were made of the size of the "ball of fire" after the explosion, and the time of its ascent into the stratosphere. The theory of shallow and deep underwater explosions was investigated, and led to the suggestion of model experiments.

5.58 One important question was cleared up at this time, which was the nature of the dependence of damage upon the characteristics of a shock wave in air. For small explosions damage is roughly proportional to the impulse, which is pressure-integrated over the duration of the pulse (i.e., the average pressure of the pulse times its duration). Investigation made clear the fact (not unknown elsewhere) that existing blockbusters are near the limit of size at which further increase of the duration of the pulse has any advantageous effect on the damage. For large explosions such as those contemplated, damage depended only on the peak pressure. This was important because the peak pressure depended on the cube root of the energy, whereas the impulse depended on its two-thirds power. Large bombs are relatively less effective (from the point of view of purely physical damage) than small ones for this reason. Calculations made at the time showed that for bombs of the order of 10,000 tons of TNT, the peak pressure would fall below the level of "C" damage at a radius of 3.5 kilometers.

5.59 Another important point was clarified at this time, connected with the optimum height of detonation. It had been known that the reflection of shock waves by solid obstacles increases the pressure of the shock wave. It was shown at this time, however, that this effect was much greater for oblique incidence than had been believed from elementary considerations; in fact oblique incidence up to an angle of 60 or 70° from the vertical gives a greater pressure increase than normal incidence. Hence it was concluded that a considerable improvement in the damage radius could be obtained by detonation at an altitude not small compared to the expected radius of damage - in fact of 1 or 2 kilometers.

Experiments

5.60 Some of the more important cooperative work between the Theoretical Division and the other divisions of the Laboratory has already been mentioned; for example, the interpretations of scattering data, and calculations of the water boiler and hydride critical masses, and the calculations made of the hydrodynamical characteristics of the implosion. There was, however, a more extensive cooperation than these isolated instances would suggest. Work done ranged from cases such as these in which the theorists played a large and semi-independent role, to ordinary service calculations, particularly the analysis of experimental data. For this latter work and for consultation in the design of experiments, every experimental group had theorists assigned to it. Calculations of a fairly extensive sort were necessary in all experiments in which "integral" considerations were involved, i.e., in which the results depended upon nuclear constants in a complex statistical way. For it then became necessary to relate the measured quantities with these constants by theory, and first to use this theory to decide whether a given experimental design would yield sufficient accuracy to justify its execution, and second to interpret the data obtained. The theorists played this part in most of the experimental determinations of nuclear quantities described in Chapters VI and XII.

5.61 One rather conspicuous example of theoretical influence on the design of experiments was the "Feynman experiment," an experiment which was never performed but whose principle was embodied in several experiments. This was simply the proposal to assemble near-critical or even supercritical amounts of material safely by putting a strong neutron absorber (the B^{10} boron isotope) uniformly into the core and tamper. For an absorber with an absorption cross section inversely proportional to the velocity of the neutrons absorbed, it could be shown that the effect was to decrease the multiplication rate in the system by an amount which was directly proportional to the concentration of absorber. Thus an amount of material which would be supercritical could be made subcritical by the addition of boron; from a measurement of the rate at which the neutron died out in this system, the rate could be simply calculated at which they would increase if the boron were absent.

5.62 The theoretical groups assisted the Detector Group of the Experimental Physics Division and others in the theoretical analysis of the efficiency and other characteristics of detectors and counters.

5.63 Aside from its main work in connection with the gun and implosion assemblies, discussed above, the Theoretical Division made numerous other analyses and calculations relative to the experimental work of the Ordnance Division. In preparation for the RaLa experiments for example, Group T-3 analyzed the attenuation of gamma rays in a homogeneous metal sphere surrounding the source, and calculated the way in which this attenuation would be increased with compression during the course of an implosion of the metal sphere. As another example, the theory of the magnetic method of implosion study was investigated in the Theoretical Division in collaboration with the experimentalists.

5.64 Mention should be made here of safety calculations made by Group T-1 and later by Group F-1 for the Y-12 and K-25 plants. The Group Leader, E. Teller, was appointed as consultant for the Manhattan District as a whole on the dangers of possible supercritical amounts of material being collected together in the plants producing separated U^{235}.

5.65 During the period described the computations group, T-5, carried out innumerable calculations for other groups in the division, and for related investigations in the mathematical theory of computation. Like other service groups, its scanty mention is no indication of the importance of its work, without which the work of the division would have been, in fact, impossible.

Chapter VI

EXPERIMENTAL PHYSICS DIVISION

Organization

6.1 The Experimental Physics Division was among the first organized at Los Alamos. The initial groups were the following:

P-1	Cyclotron Group	R. R. Wilson
P-2	Electrostatic Generator Group	J. H. Williams
P-3	D-D Source Group	J. H. Manley
P-4	Electronics Group	D. K. Froman
P-5	Radioactivity Group	E. Segrè

In addition to these groups, two new groups were created in July and August 1943, under H. Staub and B. Rossi, respectively. It was the function of the first of these to develop improved counters, and of the second to develop improved electronic techniques. Because of the close relationship between these two aspects of instrumentation development, the groups were combined in September as the Detector Group, P-6, under Rossi. Group P-7, the Water Boiler Group, was created in August under D. W. Kerst. There were no further changes in the gross organization of the division until the general reorganization of August 1944. R. F. Bacher was Division Leader from the time of his arrival in July 1943.

Equipment

6.2 When the first members of the experimental physics groups arrived in March 1943, the buildings to house the accelerating equipment were not completed. The first few weeks were spent in unloading equipment from

Princeton, Harvard, Wisconsin, and Illinois. Then came the period of installing the cyclotron, van de Graaff, and Cockcroft-Walton.

6.3 The bottom piece of the Harvard cyclotron was laid at Los Alamos on April 14, and the first week in June saw the initial indications of a beam. The early work with the cyclotron was done with an internal beam on a beryllium target probe and gave an intense neutron source. Early in 1944, an external beam was developed.

6.4 The two electrostatic (van de Graaff) generators were moved onto their foundations in Building W in April. The "long tank," which at Wisconsin had given 1 microampere at 4 million volts, gave the first beam May 15. The "short tank," which had operated at 2 million volts, with higher current, gave a beam June 10. Both machines were used to produce neutrons from the Li(p, n) reaction, covering - by properly exploiting the peculiarities of both machines - the energy range from 20 kev to 2 Mev. After providing some useful information the short tank generator was rebuilt to give higher energy, and thereafter ran satisfactorily at 2.5 million volts. It was again ready for use in December 1943.

6.5 Building Z, which was to house the Cockcroft-Walton accelerating equipment, was completed later than Buildings X and W. Installation of the equipment was, therefore, not begun until the end of April. In this case the first beam was obtained June 7. This accelerator was used to produce neutrons from the D(d, n) reaction, whence it was usually called the "D-D source," and P-3 the D-D Group. This source gave neutrons up to 3 Mev.

6.6 That all the accelerating equipment was installed and put in operating condition in such a short time speaks of long hours and hard labor by the members of these groups. While the accelerating equipment was being set up, moreover, plans and instrumentation for experiments were going ahead. At the cyclotron, a 5' x 5' x 10' graphite block was set up to give a flux of thermal neutrons; it was later rebuilt and increased in size. The cyclotron, by the use of modulation, was able to cover the energy range from thermal energies up to the kilovolt region, where it overlapped the low energy region of the electrostatic generators. Building G was built as an adjunct to Building Z, to house a graphite block for the standardization of slow-neutron measurements. Less spectacular than the installation of the accelerating equipment but equally necessary was the setting up of equipment for the electronics laboratory, and as the groups concerned arrived, for photoneutron source work, for spontaneous fission investigation, for research on counter equipment, and for the Water Boiler.

6.7 The rationale of equipment and organization in this division is rather evident. Its program lay almost entirely in the field of neutron and fission physics. With the exception of spontaneous fission, the reactions to be studied were all of a type induced by neutrons of various energies. Together with photo-neutron sources, the cyclotron, the van de Graaffs and the Cockcroft-Walton gave neutrons of reasonably well-defined energies from the thermal region up to 3 Mev. The greatest uncertainties appeared in the 1 to 20 kev region. On the side of observation, all the experimental arrangements involved the use of fission, neutron, and radiation detectors, together with the necessary electronic equipment for registering data.

Preliminary Experiments

6.8 In the outline of the experimental physics program in Chapter I (1.57), it was stated that there were certain preliminary experiments which had to be done to prove conclusively that the atomic bomb was feasible. One of these was to measure the time delay in neutron emission after fission, the other was to confirm the theoretically plausible belief that the number of neutrons per fission was essentially independent of the energy of incident neutrons.

6.9 The average time for a neutron generated in a fissionable mass to produce its successors in the chain reaction is a factor of primary importance in determining the final bomb efficiency. This time consists of two periods: the time of flight and the emission time. The first is the time between the emission of a neutron after fission and a new fission caused by absorption of this neutron. It is of the order of 10^{-8} sec. The emission time consists of the lifetime of the compound nucleus plus the time between the splitting apart of the fission fragments and the emission of neutrons from them. From theoretical arguments both these times should be negligible, of the order of 10^{-15} sec, but it was imperative to confirm the theory experimentally since it was of critical importance that this time be in fact small.

6.10 The Cyclotron Group had begun the instrumentation for a "Baker experiment" to determine the emission time after fission, before leaving Princeton, and this was their first experiment at Los Alamos. This experiment takes advantage of the extremely high speed of the fission fragments to measure very short emission times. Thus after 10^{-8} sec, the fragments will be about 10 centimeters from the point of fission if there is no material in their path. As the experiment was performed, a foil of U^{235} was wrapped around a neutron counter, and two cases compared: one where the fragments were permitted to travel out from the counter, and the other where they were stopped in its vicinity. For geometrical reasons the chance of a neutron being counted falls off rapidly with the distance at which it is emitted. In the two cases the same neutron count was obtained within the limits of experimental error. It was thus possible to conclude that most neutrons were emitted in times less than 10^{-9} sec, and that the percentage emitted in more than 5 x 10^{-9} sec was negligible. This result was reported to the Governing Board in November 1943, and one doubt was removed. The same result was confirmed later by a different method, using apparatus constructed primarily for measurement of the neutron number (6.14). Somewhat later an experiment was carried out by the same group demonstrating that the fission time was also less than 10^{-9} sec.

6.11 The second unverified assumption, that the neutron number was the same for fissions from slow and fast neutrons, was not accurately tested until the fall of 1944 (12.3); the theoretical assurance here was quite strong. A more urgent confirming experiment was the measurement of the neutron number of Pu^{239}. At the beginning of the project it was not even experimentally certain that fission of this substance would produce neutrons. To some extent, therefore, the entire program of plutonium production was still a gamble.

6.12 Actually the first nuclear experiment completed at Los Alamos was the comparison of the neutron numbers of U^{235} and Pu^{239}, using a barely visible speck of plutonium, which was all that then existed. In this experiment, carried out in July 1943 by the Electrostatic Generator Group, neutrons emitted from known masses of uranium and plutonium were compared by counting the number of protons recoiling from fast neutrons in a thick paraffin layer surrounding the fissionable material. The fissions themselves are produced by somewhat less energetic neutrons. Ionization pulses from the proton recoils were observed with samples of normal uranium containing U^{235}, with Pu^{239}, and without any fissionable material. The numbers were made comparable by simultaneously recording the fission rates in a monitor chamber. To determine the relative number of neutrons per fission from the relative number per microgram, it was necessary to measure their relative fission cross sections for the particular energy spectrum of the neutrons used. This was done by comparing the two materials in a double fission chamber.

6.13 Another experiment was carried out simultaneously by the same group, as a check upon the first. This used a thorium fission detector, and the primary neutrons used to cause fission had energies well below the fission threshold of thorium. Despite the small amount of plutonium available for these experiments, they showed that the neutron number for plutonium was somewhat greater than that for uranium, and gave a value for the ratio of these numbers which was not materially improved by later measurements.

The Neutron Number

6.14 Other relative and absolute measurements of neutron numbers were carried out in this period. At about the date of the first experiment described above, the ratio of the neutron numbers of plutonium and uranium was roughly checked by the Cyclotron Group, and somewhat later a precision determination was carried out.

6.15 As was stated above, the assumption that the neutron number is independent of the energy of the neutrons initiating fission was in need of experimental confirmation. In the spring of 1944 the Cyclotron Group and the Electrostatic Generator Group compared U^{235} fissions from thermal neutrons with those from 300 kv neutrons, and found the ratio of neutron numbers to be unity within rather wide limits of experimental error. Later this ratio was remeasured with smaller experimental errors, and the value of 1.0 confirmed for both U^{235} and Pu^{239}.

6.16 The neutron-number measurements described above (6.12-6.15) are all relative, i.e., they involve comparison of one neutron number with another. The only absolute measurement was that which had been made at Chicago by Fermi. This value was in doubt (1.58), and one of the early problems was to check its value. The graphite block at the cyclotron gave a strong flux of thermal neutrons to produce fissions in the sample. The number of fast neutrons (from fission) was measured by measuring the resonance activity acquired by indium foils and calibrating this measurement by comparison with the activity induced by a radon-beryllium source of known output. The number of fissions was counted simultaneously, and the number of neutrons per fission thus obtained for both U^{235} and Pu^{239}. The radon-beryllium source used was calibrated by the Standards Subgroup of the D-D Group. Even without standardization the ratios for the neutron numbers of U^{235} and Pu^{239} gave a check of previous relative measurements.

6.17 In Chicago, meanwhile, Fermi was also checking the absolute neutron number of U^{235} by two methods, obtaining 2.14 and 2.18 neutrons per fission, a result that agreed with that obtained at Los Alamos.

Spontaneous Fission Measurements

6.18 Before coming to Los Alamos in the summer of 1943, the Radioactivity Group had been making spontaneous fission measurements in Berkeley.

The practical importance of these measurements derived from the need to minimize the neutron background in the bomb material. In particular, the neutrons from spontaneous fissions would set a lower limit to this background, below which it would be useless to reduce the background from (α, n) reactions in light-element impurities. In these experiments the size of the samples that could be investigated was limited by the need to avoid spurious counts in the ionization chamber caused by the coincidence of several alpha pulses, simulating the large pulse of a single fission. Since spontaneous fission decay is a very slow process, the result was that the data had to be taken over long periods of time, with consequent great care in the design and operation of equipment.

6.19 After coming to Los Alamos in June 1943, the Radioactivity Group constructed new ionization chambers and designed new amplifiers in order to make use of the larger samples of material that were becoming available. The Pajarito Canyon Field Station was set up several miles from Los Alamos in order to get away from the high radiation background associated with the Laboratory, and which would have masked completely the low counting rate from spontaneous fission.

6.20 In the fall of 1943 the Laboratory received a report to the effect that Joliot had found neutrons associated with the alpha radioactivity of polonium, a characteristic presumably of alpha emission as such. Because the difficulties of purifying polonium were already well known at Los Alamos, it was generally believed that Joliot must have overestimated the purity of his material, and that his neutrons were really from the (α, n) reaction of light element impurities. Such a "Joliot effect," if real, might materially effect the program of the Laboratory. Plutonium, as an alpha emitter, might have a neutron background that could not be brought down to tolerance by chemical purification. And as such, a polonium-beryllium initiator might be unusable because of neutrons associated with the polonium alpha radiation.

6.21 The alpha emitter that could be most easily purified was radon. Accordingly, a radon plant was constructed by the Radiochemistry Group, and 2 grams of radium procured for "milking." Investigation failed to reveal any spontaneous neutrons. This work was dropped when polonium purification and the direct measurement of spontaneous neutrons from plutonium were achieved (12.8).

6.22 In December 1943 came indications that some of the fissions in the 235 isotope were probably not spontaneous, but caused by cosmic ray neutrons. The evidence for this was that while the fission rates as determined at Berkeley and Los Alamos showed fair agreement for U^{238}, the Los Alamos rate was considerably higher for U^{235}. Since a large percentage of the cosmic ray neutrons are too slow to cause fissions in the former substance but do cause them in the latter, the results would be explained by the higher cosmic ray intensity at the Los Alamos elevation of 7300 feet compared to sea level at Berkeley.

6.23 The early estimate of 2000 feet per second as the minimum velocity of assembly for the 235 gun method was based upon the Berkeley spontaneous fission measurements, which indicated about two neutrons per gram per second from this source. After the discrepancies had been observed, it was found at the Pajarito station that a boron-paraffin screen reduced very considerably the number of "spontaneous" fissions observed, in both U^{235} and Pu^{239}. In order to estimate the spontaneous fission rate of Pu^{239} in a reasonable time, a new system was constructed in the spring of 1944 which permitted measurement from 5 milligrams of plutonium. In July 1944 it was found that there was a significant difference between the spontaneous fission rates of plutonium from cyclotron irradiation and from the much heavier irradiation of the Clinton pile. At this time it was suggested by Fermi that the higher spontaneous fission rate in the latter material might be caused by Pu^{240}, resulting from the (n, γ) reaction in the pile. A re-irradiated sample gave still higher spontaneous fission counts. These observations constituted, in fact, the first direct observation of the existence of the new isotope.

6.24 Since for economic operation the Hanford plutonium would be heavily irradiated, the neutron background in this material was predictably too high for the use of gun assembly. It was this fact that forced the abandonment of the plutonium gun assembly program, and made necessary the success of the implosion. The further consequences of this development are traced in other sections.

Energy Spectrum of Fission Neutrons

6.25 Previous to Los Alamos, some work had been done to investigate the energy distribution of the neutrons emitted by the fission process. It appeared that the mean energy was about 2 Mev, but that an appreciable fraction of the neutrons had energies less than one million volts and so would be incapable of causing fission in U^{238}. The cloud chamber data from Rice Institute (1.61) involved big corrections; the data of ion chamber pulse size distribution from Stanford (1.61) looked reasonable theoretically. These results showed neutrons tailing off from one million volts, agreed with older experiments on range and effective energy as obtained from slowing-down and from hydrogen cross section measurements. The photographic emulsion technique used at Liverpool showed a much sharper maximum at about 2 Mev (1.61). All the above measurements suffered from having been made with large masses of dilute material, which gave a good chance that neutrons would lose energy from inelastic scattering before being measured.

6.26 Another of the early problems was therefore the more accurate determination of the fission spectrum. The photographic emulsion technique appeared to be the most promising method for covering a wide range of neutron energies in one run and for keeping the scattering material to a minimum. It was straightforward and involved no appreciable corrections if carefully executed. Plates were exposed at the University of Minnesota by the Electrostatic Generator Group; one of the early tasks, when they came to Los Alamos, was to set up equipment and train personnel to read the plates. Early results showed that in shape the high energy end of the spectrum agreed with the British and Stanford results, but on the low energy side it disagreed with both. The plates were calibrated with the D-D source and electrostatic generator Li(p, n) source. Measurements agreed, on the whole, with the former cloud chamber measurements at Rice Institute. Meanwhile at Stanford the method used there was carefully reviewed since both the maximum and mean energies were considerably lower than those obtained from cloud chamber and photographic plate data. This could be explained by inelastic scattering and consequent distortion toward lower energies. The final Stanford report was written in the summer of 1943 and personnel of the group came to Los Alamos.

6.27 At Los Alamos a detailed comparative study of the advantages, difficulties, and limitations of the various schemes for neutron spectroscopy were made. Several additional experiments were made by the Electrostatic Generator Group and the Detector Group, and converging results were finally obtained.

6.28 As a corollary to the effort to obtain quantitative knowledge of the fission spectrum, much effort was put first by members of the Detector Group, and later by the Electrostatic Generator Group, into the design of mock-fission sources, i.e., neutron sources with neutron spectrum comparable to the fission spectrum. Such sources were later used in semi-integral experiments to measure average cross sections under conditions closely resembling those in an actual fission bomb. A satisfactory mock-fission source was finally achieved in May 1944, by allowing the alpha particles from a strong polonium source to fall on a mixture of neutron-producing substances, the mixture being in such proportions that a reasonable reproduction of the fission spectrum was obtained. A series of photographic plate determinations of the spectrum from various mixtures indicated that $NaBF_4$ gave an excellent mock spectrum.

Fission Cross Sections

6.29 The critical mass and efficiency of the bomb depends upon the cross sections for fission, capture, and for elastic and inelastic scattering at all energies for which there are appreciable number of fission neutrons. Previous work in this field has been reviewed (1.61-1.63). As stated there, further work was required both in determining the absolute cross sections at various energies, and in measuring their variation as a function of the energy of incident neutrons. The emphasis in fission cross section measurements was early influenced by interest in the uranium hydride bomb. The theory of this bomb is explained more fully in Chapter V. Suffice it to say that the practicability of this type of weapon depended on the hypothesis that the slowing down of neutrons by hydrogen was compensated in its delaying effect by a corresponding increase in the fission cross section with decreasing neutron energy. If this hypothesis were true, the rate at which the explosion takes place would remain the same as in a metal bomb, while the critical mass would be considerably decreased. Evidence for the inverse dependence of cross section on neutron velocity was the early work at Wisconsin (1.62) which showed approximately 1/v dependence from 0.4 Mev down to 100 Mev. The same law of dependence was also verified between thermal velocities and 2 ev. On the other hand when the latter dependence was extrapolated to higher energies, and the high energy curve to low energies, the two failed to cross. In fact, between 2 ev and 100 kev there was found a 12-fold increase in the coefficient of 1/v to be accounted for. Since the practicability of the hydride bomb depended upon the actual shape of the curve in this region, it was of great importance to know approximately where the break occurred.

6.30 In this connection it was found from boron absorption measurements made by the Electrostatic Generator Group in August 1943 that the break occurred between 25 and 40 ev. This was the first indication that fission cross sections do not follow a simple law in the epithermal region. Because the break occurred at this low energy, the possibility of a hydride bomb was not yet excluded.

6.31 A more extended sequence of measurements followed by which the relative fission cross sections of Pu^{239} and U^{235} were measured as functions of neutron energy. At this time the properties of the former material were not well known, and it was of direct interest to learn how its fission cross section compared with that of U^{235}, which was known to be good bomb material. Experiments in which one cross section was to be compared with another were relatively easy to perform, requiring only the simultaneous counting of reactions occurring when two or more foils of known masses were immersed in the same neutron flux. In this way all the cross sections for fissionable materials available at the time were measured relative to the U^{235} cross section. The latter was, however, itself in some question, both as to its absolute value and its change with energy. Such relative measurements were made for U^{238}, thorium, ionium, Pu^{239}, protoactinium, neptunium^{237}, and also for the (n, α) reaction in boron and lithium; they were carried out over an energy range from about 100 kev to 2 Mev.

6.32 Apart from the importance of knowing the absolute cross sections for the primary materials, the other cross sections were useful as tools for neutron detection. Those elements, such as U^{238}, protoactinium, and Th^{232}, which had fission thresholds at high energies, were useful where a particular fraction of the neutron spectrum was to be examined. The elements boron and lithium proved to have approximately 1/v absorption cross sections and were useful for measurements in the 1 to 20 kev gap left by Los Alamos accelerating equipment. Further fast neutron reaction cross sections were measured of elements such as gold, phosphorus, sulfur, indium, etc. Even at this early date it was realized that reactions leading to radioactive isotopes would provide useful experimental information if the energy responses were known, since there would be many experimental arrangements where bulky detection chambers could not be used.

6.33 There were two difficult problems associated with cross section measurements, one of which was not yet completely solved by the end of the war period. These were (1) The absolute measurement of neutron flux over a wide energy range, so that some easily detectable reaction products, such as those from U^{235} fissions, could be established in terms of an absolute and accurate flux standard; and (2) the production of monoergic neutrons of energies from 1 kev to 50 kev with sufficiently high yields to perform necessary experiments and with good energy resolution.

6.34 The first problem was finally solved for the range between 400 kev and 3 Mev by the careful work of the Detector Group, who used the Li(p, n) and D(d, n) neutron sources and an electron-collection parallel-plate ionization chamber, with which the number of recoil protons from a thin tristearin film could be accurately counted. The success of this experiment depended in part on the accurate determination of the (n, p) scattering cross section, carried out earlier at Minnesota. It also depended upon the theoretical interpretation of the differential bias curves obtained in electron collection.

6.35 It should be mentioned in this connection that high counting rates, large alpha background in chambers with Pu^{239} and other types of background, had led to the development in the Detector Group and the Electronics Group, respectively, of new counting techniques involving electron collection, and new fast amplifiers (6.85). This caused a minor revolution in the counting techniques and electronic equipment used by the Physics Division.

6.36 The second problem was partially solved when early in 1944 the short electrostatic generator rebuilding program was completed (6.4). High currents and energy regulation to within 1.5 kev incorporated into this machine made it possible to utilize the back-angle neutrons from the Li(p, n) reaction down to less than 5 kev. Development of new counters - the so-called long counters - indicated the possibility of bringing the absolute fission cross section measurements down to the region of a few kev, where they were still extremely uncertain. This apparently simple experiment became long and involved because of difficulties in interpreting the counter data obtained. Checks by independent methods became necessary, one of which gave considerably lower cross section values in the 30 kev region than had first been obtained. If this lower value of the cross section were correct, it would reduce somewhat the potentialities of the hydride bomb. After considerable further investigation of counters and the construction of an antimony-beryllium source of 25 kev neutrons, the lower value was finally confirmed. The principal result of these efforts was another blow to the hydride gun program.

6.37 Absolute fission cross section measurements at several energies in the range 250 kev to 2.5 Mev were undertaken by the Detector Group in collaboration with the Electrostatic Generator Group and the D-D Group. These measurements, as stated above (6.34) were based upon the comparison of fission cross sections with hydrogen scattering cross sections. The results provided a reliable standard for other measurements, in which the relative values were more reliable than the absolute.

6.38 The fission cross section of U^{235} in the region below 1 kev was measured by the Cyclotron Group, early in 1944, monoergic neutrons being obtained by the "velocity selector." In this method, the neutrons are separated into velocity groups depending on the time of flight between source and detector, over a path several meters long. The velocity selector equipment had been built before Los Alamos, at Cornell, and was extensively rebuilt before cross section measurements were obtained.

6.39 A few other fission cross section measurements were made during this period at isolated energies, notably by the Radioactivity Group using photo-neutrons.

Capture Cross Sections, the "Branching Ratio"

6.40 The earliest measurement of capture cross sections was primarily the work of the Radioactivity Group. The principal method was the measurement of radioactivity induced by neutron capture. Of interest were the capture cross sections of fissionable materials, of possible tamper materials, and of other materials that might be present in the bomb assembly.

6.41 Capture cross sections were measured in a wide range of potential tamper materials, some of them very rare by ordinary standards, but cheap in comparison with active material. Platinum, iridium, and gold were among the substances investigated, as was the very rare element rhenium. The Experimental Physics progress report for April 1944 gives a summary of nearly two dozen elements and isotopes whose capture cross sections had been measured by the Radioactivity Group.

6.42 A very extensive series of capture cross section measurements was carried out in the Electrostatic Generator Group. A photo-neutron source was surrounded by spheres of potential tamper material, and the attenuation of neutrons measured. This method has the advantage over the measurement of induced radioactivity that it does not require an absolute flux determination or interpretation of induced activity, and that the resultant nucleus does not have to be radioactive. It has, however, the disadvantage that the spheres must be large, allowing a considerable degradation of the energy of the neutrons through inelastic scattering, and that it requires a knowledge of the transport mean free path. The long counter (6.84) was used in these experiments, since its sensitivity is nearly independent of neutron energy. By August 1944, a large number of substances had been examined, and preparations were being made to check the data obtained, a job that was not completed until the spring of 1945 (12.26).

6.43 The capture cross sections for active materials were subject to intensive investigation when it was observed, from two independent sources, that radiative capture might be an important process competitive with fission. One source was the outcome of the low energy fission cross section measurements of the Cyclotron Group. Here rather sharp resonances were discovered, i.e., relatively narrow energy bands in which the fission cross section increased because of resonance. This result implied that the relatively well-defined character of the resonant energy would be associated with a complementary uncertainty in the duration of the state. This duration might be long enough to permit radiative energy loss as a significant alternative to fission. The second source was the measurement of neutron-induced radioactivity by the Radioactivity Group. A number of activation cross sections were measured relative to the fission cross section of U^{235}. Consistently higher results were obtained than by other methods, when the known absorption (capture-plus-fission) cross section of U^{235} was used. It was pointed out that these difficulties would be removed if one could show the existence of a competing process, such as the (n, γ) reaction, with a probability not small compared to that of fission. The ratio of these probabilities is called the "branching ratio" of radiative capture to fission.

6.44 Experiments to measure the branching ratio were begun in the Electrostatic Generator Group early in 1944 for U^{235}. The method used by this group was to measure the ratio of the boron and lithium absorption cross sections to the fission cross section. Since the capture in boron and lithium results only in nonradiative disintegrations, no radiative capture cross section is involved in the experiment. In previous Chicago measurements the ratio of the boron absorption cross section to the U^{235} absorption cross section has been measured. Hence the ratio of the Chicago and Los Alamos figures would give the ratio of absorption to fission, a quantity whose difference from one would be the desired branching ratio. A value of 0.16 was obtained for thermal neutrons in U^{235}, indicating considerable radiative capture. This apparently unfavorable result was in fact an advantage, if one made the theoretically plausible assumption that the branching ratio decreased at the high energies predominant in an exploding bomb. The advantage arose from the fact that no appreciable energy dependence of the neutron number had as yet been detected, and since all of the Los Alamos neutron number measurements were relative to the Chicago measurement of neutrons emitted per neutron absorbed, a finite radiative capture implied a somewhat higher ratio of neutrons emitted per fission. One had therefore underestimated the high-energy effective neutron number. In order to test the expected behavior of the branching ratio with energy, the experiment was immediately extended to the fast neutron region, and the ratio of the boron to fission cross sections measured over a considerable energy region. A definite value for the high energy branching ratio could not be obtained, however, until better fission and absorption cross sections were available in the relevant neutron energy region, the extension of the experiment resulted principally in determining the boron - and lithium - to fission cross section ratios.

6.45 An independent measurement of the branching ratio was made in the Radioactivity Group, by the comparison of two cross section ratios. These were the ratio of the fission cross section of U^{235} to the capture cross section of gold (and manganese), and the ratio of the absorption cross section of U^{235} to the absorption cross section of gold (and manganese). The branching ratio calculated from these data gave about the same value as that obtained by the Electrostatic Generator Group.

6.46 A third measurement of the branching ratio was incidental to the measurements of the neutron number carried out by the Cyclotron Group in early summer of 1944 (6.14). The value obtained for U^{235} was in reasonable agreement with those from the experiments described above. A value was also obtained by them for the branching ratio of Pu^{239}, which indicated that it was much larger than for U^{235}, in fact about 0.5. This result indicated a large gain in the effective neutron number for high energies, if the branching ratio fell off with high energies as expected.

6.47 Another measurement by the same group gave the branching ratio of Pu^{239} as a function of the branching ratio of U^{235}, which was again consistent with earlier data.

6.48 No successful measurement of the branching ratio as a function of energy was made during this period; it was in fact only measured indirectly at a much later date (12.21). There was, however, a measurement of the branching ratio in U^{238} at high energies by the Radioactivity Group. Its purpose was to determine the neutron absorption by U^{238} remaining in the separated U^{235}.

Scattering Experiments, Choice of Tamper

6.49 At the beginning very little was known about the scattering properties of potential tamper materials. As an important factor in ultimate bomb design, the choice of a tamper had to be made as soon as possible. The notion prevailed for some time that inelastic scattering (i.e. scattering in which the neutrons, although not captured by the tamper nuclei, lose part of their energy to them by excitation) would play an unimportant role, since it would probably reduce neutrons to a very low energy where they would not contribute materially to the explosive chain reaction. Very little was known, moreover, about the variation of scattering with neutron energy. It was thought, at the time, that the most important part of the fission spectrum lay at high energies, near 2 Mev. It was felt that to a first approximation the usefulness of a tamper would be determined by the number of neutrons reflected backward to the core. It was therefore decided that the most rapid collection of pertinent information could be made by comparing the back scattering of trial tamper materials for D(d, n) neutrons from the Cockcroft-Walton. This could be done using either a nondirectional detector with a paraffin "shadow cone" to reduce direct beam, or with a directional detector. The shadow cone method greatly reduced the range of scattering angle measurable. It was thought that a directional detector could give an average over the angles from 120° to 180° in the geometry possible.

6.50 The D-D Group undertook these measurements by both methods in August 1943. The first directional detector was a spherical ionization chamber with a large directionality factor. The first scatterers measured were discs 1 inch thick and 10 inches in diameter of lead, iron, gold, and platinum. The latter two, vulgar wonders in an atomic bomb laboratory, brought a great stream of visitors from other groups. Lead showed up best per unit weight, but because of its relatively low density was not much better than gold or platinum.

6.51 Although the geometry used for back-scattering covered the angles 120° to 180°, it was discovered - as theory caught up with experiment - that a very small range of angle near 137° was weighted predominantly. Hence if back scattering were not uniform, the data obtained could be quite misleading. Several councils of war about this state of affairs resulted in an extension of the measurements.

6.52 This incident was of some importance in the growth of the Laboratory. An essential part of the design of such an experiment as this is sufficient preliminary analysis and calculation to show that from a given experimental arrangement the data sought can actually be got. For efficiency most of the elaborate calculations of this sort were delegated to members of the Theoretical Division because of their special skill. In this case the liaison between the latter and the D-D Group had been inadequate and a need for closer liaison was recognized.

6.53 By the end of October 1943, back-scattering measurements had been completed for a large list of substances, and a number of instrumental improvements had been made. After the first survey, the list of possible tamper materials was restricted to tungsten, carbon, uranium, beryllium oxide, and lead. At about this time, also, measurements of the fission spectrum indicated that the important energy range was nearer 1 Mev than 2 Mev. Results of the first experiments indicated, moreover, that earlier ideas about inelastic scattering were incorrect, and that inelastically scattered neutrons could play an appreciable role in the functioning of a tamper. Recognition of their possible importance was made easier, also, by the current concern of the Laboratory with the uranium hydride bomb. The same increase in cross section with decreasing energy that made this bomb seem feasible also suggested that neutrons slowed by inelastic scattering might still make a considerable contribution to an explosive chain reaction.

6.54 For these reasons preparations were made for the study of scattering as a function of energy and scattering angle, taking account of inelastically scattered neutrons. This work was done cooperatively by the D-D and Electrostatic Generator Groups, beginning in November 1943. Back-scattering data were obtained at 1.5 Mev and 0.6 Mev, as well as 3 Mev. In addition to over-all back-scattering measurements, an experiment was performed to give specific information on the degraded neutrons as a function of primary neutron energy for the elements still in the running as scatterers.

6.55 Materials studied in these experiments were carbon, lead, uranium, beryllium oxide, and tungsten. These are listed in the order in which they appeared promising. During May, June, and July 1944, this series of experiments had been extended to uranium nitride, lead dioxide, cobalt, manganese, nickel, and tantalum at several energies.

6.56 One further scattering experiment was begun in this period, an integral experiment which would attempt to obtain information about the hydride bomb. The D-D source was to be surrounded by a modifying sphere mocking the hydride core as nearly as possible; integral tamper properties would be investigated around this core as well as neutron distribution in tamper and core. One instrumental development that occurred in this connection was a new fission detector. This was a spiral ionization chamber with a spiral of depleted U^{238} so wound as to give a large surface area in a small volume. This counter operated at efficiencies as high as 85 per cent.

Water Boiler

6.57 The first chain reacting unit built at Los Alamos was the Water Boiler, a low-power pile fueled by uranium enriched in U^{235}. It was the first pile built with enriched material, the so-called alpha stage material containing about 14 per cent U^{235}. The necessary slowing down or moderation of fission neutrons is provided in this system by the hydrogen in ordinary water: the active mixture is a solution of uranyl sulfate in water solution. The tamper chosen was beryllium oxide.

6.58 The purpose of this undertaking was partly to provide a strong neutron source for experimental purposes, and partly to serve as a trial run in the art of designing, building, and operating such units. In was an integral experiment to test a theory similar in some respects to that involved in the design of a bomb. It was the first of a series of steps from the slow reaction first produced in the Chicago pile to the fast reaction in a sphere of active metal. It laid the foundation of instrumental and manipulatory techniques required in the later and more exacting steps of the series.

Unfortunately, the experimenters at Los Alamos did not have the full benefit of experience gained by those at Chicago; the result was some unnecessary delay before the first chain-reaction was started.

6.59 In the first months the Water Boiler calculations absorbed a fair part of the time of the Theoretical Division. Calculation of the critical mass depended upon the application of diffusion theory to a complex system consisting of active solution, container, and tamper. For economy of material it was important to find the optimum concentration of the solution. The number of hydrogen nuclei had to be large enough to slow down the neutrons to thermal energies, and small enough not to capture too many of them.

6.60 One of the first problems associated with the Water Boiler was the choice of a site for its location. It was located in an isolated region, primarily for reasons of safety. The boiler was first planned for 10,000 watts operation. The radioactivity of fission fragments from intermittent operation was estimated at 3,000 curies; the minimum safe distance from unprotected people was calculated on the assumption that a mild explosion could disperse this activity into the atmosphere. It was also desirable to operate in an isolated location because of the possibility of high instantaneous radiation in case of an uncontrolled chain-reaction.

6.61 During the month of September 1943, while design of the boiler and of the building to house it was still in a preliminary stage, Fermi and Allison came to Los Alamos from Chicago to discuss the problems connected with such a unit. They pointed out a large number of difficulties connected with operation of the boiler as a high-power neutron source. Some of these difficulties had already been anticipated but their acuteness had not in all cases been fully appreciated. One was a considerable gas evolution which would cause unsteadiness of operation. Decomposition of the uranium salt and consequent precipitation would result from the large amount of radiation to which it would be subjected. Heavier shielding than had been planned would be necessary.

6.62 As a result of these discussions, it was decided to omit from the plans for immediate construction all features necessary for high power operation, and go ahead with the design of a boiler for low power operation. Provisions were made in the plans, however, for the later installation of equipment necessary for high power operation. The main omission was equipment for chemical decontamination, unnecessary when the boiler was operated at trivial power outputs.

6.63 The reasons for going ahead with these modified plans were two. Although the boiler could no longer be used as an intense neutron source, it would make possible the investigation of a chain-reacting system with a very much higher U^{235} enrichment than had previous piles. The second and main reason, however, was again that of gaining experience in the operation of such a system and of preparing personnel and equipment for the later critical experiments.

6.64 Construction of the building to house the boiler and associated laboratories was begun at the Los Alamos Canyon or Omega site in October 1943, (see Appendix 3) and completed in February 1944. This building was intended not only for the boiler, but also for later critical assemblies (15.4). By the time of completion, detailed plans for the boiler were ready and construction was begun.

6.65 Design problems included safety features in the building (e.g., a heavy concrete wall separating the boiler from remote-control equipment), a thermostated enclosure to maintain constant boiler temperature, recording and monitoring equipment, including ionization chambers and amplifiers, control rods and their associated mechanisms, a supporting structure for the tamper and container, the container itself together with means for putting in and removing solution, design of beryllia bricks for ease in fabricating and stacking the tamper. Specifications for the tamper and active solution were worked out in conjunction with the Radiochemistry Group (8.62–8.68) and the Powder Metallurgy Group (8.49–8.51), the original choice of material and size specification having been made by the theorists.

6.66 Between the completion of the building in February 1944, and the first operation of the Water Boiler as a divergent chain reactor early in May 1944, the Water Boiler Group was engaged in the construction and installation of equipment, the discussion of experiments and instrumentation for such experiments, and with tests of the equipment.

6.67 Late in April 1944 a series of tests were begun to test the fluid handling equipment of the boiler, and the counting equipment. These tests ended with the use of normal uranyl sulfate solution. By this time enough enriched material had arrived, and after some purification and preparation of solutions by the chemists, the first tests were made to determine the critical mass. The successful operation of the Water Boiler as a divergent chain reactor marked a small but not unimportant step in the development of the art, a step toward the controlled use of nuclear energy from separated U^{235} or plutonium.

6.68 A number of experiments were undertaken during this period by the Water Boiler Group, prior to the general reorganization of the Laboratory in August 1944.

6.69 The operation of the Water Boiler, like that of other controlled reactors, depends upon the very small percentage of delayed neutrons; these make it possible to keep the system below critical for prompt neutrons and in the neighborhood of critical for all, including the delayed neutrons. Although the delayed neutrons are only about 1 per cent of the total, in the region near critical the time dependence of the system - its rate of rise or fall - is of the order of the delay period; prompt chains die out constantly, to be reinstated only because of the delayed neutrons.

6.70 One experiment planned and carried out with the Water Boiler (as well as with later assemblies) was the experiment proposed by Rossi and bearing his name. It was an experiment to determine the prompt period. This period depends on the time after fission that is taken for the neutrons to be emitted, on the fission spectrum, and on the scattering and absorption characteristics of core and tamper. It was essential to measure the prompt period in a metal assembly as accurately as possible. Its measurement in a hydrogenous assembly would not give direct information relevant to efficiency calculation, but would provide experience and instrumental development, and would also be a check on theoretical predictions.

6.71 The Rossi experiment counts neutron coincidences. The presence of a prompt chain in the reactor is presumed whenever a neutron is counted. A time analyzing system then records the numbers of neutrons counted in short intervals of time immediately after the first count. This gives a direct measure of the prompt period.

6.72 Another method, which gave less interpretable results, was rapidly changing the degree of criticality by means of a motor-driven cadmium control vane.

6.73 A third experiment was the measurement of the spatial distribution of neutrons in the boiler solution and tamper. This was carried out by means of small counters placed in various positions in the boiler and tamper, and served as a check against calculations from neutron diffusion theory.

6.74 A fourth experiment, related to the Rossi experiment, was the measurement of fluctuations in the neutron level in the boiler. This measurement was of interest in connection with the variation of the neutron number from fission to fission, a variation in turn connected with the statistical aspect of the chain reaction in the bomb, in particular the predetonation probability. The first measurement gave a value for the fluctuation of counts in a counter, relative to the average number of counts. This gave information about the fluctuation of the neutron number as soon as another quantity, namely the effective number of delayed neutrons, was measured.

6.75 Toward the end of the first period of the Laboratory, plans were under way in the Water Boiler Group to make critical assemblies with uranium hydride, and to rebuild the water boiler for higher-power operation. Both of these projects carry us over into the next period, when the work of the group was divided between two new groups; this further work is therefore reported in later sections (13.25 ff, 15.4 ff).

Miscellaneous Experiments

6.76 In addition to the nuclear properties and processes described above, certain other processes were investigated because of their relation to fission or to the interpretation of experiments.

6.77 A number of special investigations were made by members of the Radioactivity Group, in connection with the fission process. Measurements were made of range of fission fragments in heavy and light materials, and also of the energies and number of long-range alpha particles discovered in the fission process. Gamma rays emitted in fission were investigated since knowledge of the number and quantum energies of these rays was of possible importance in understanding the fission process itself, and also because these rays might be used in experiments designed to test the bomb. Gamma ray measurements were in fact made at the Trinity test (18.28).

6.78 As was already mentioned, a subgroup of the D-D Group was given the responsibility for calibrating the neutron emission from various natural sources. A graphite column was built, the diffusion length of the graphite was measured, the pile was standardized for indium resonance neutrons, and work was begun on the standardization of various natural sources. In addition to this work, the members of this subgroup measured gold and indium capture cross sections for column neutrons, and also the indium half-life. They conducted various experiments connected with safety in the handling and transportation of active material. They also worked constantly to improve the standardization of sources, upon which depended the accuracy of such experiments as the neutron number measurements of the Cyclotron Group.

6.79 Another program carried out within the Experimental Physics Division was the development of provisions for isotopic analysis. In the last months of 1943 it became increasingly evident that there was considerable uncertainty as to the amount of isotope U^{235} in the enriched samples which were being received at Los Alamos. Enriched samples were made up in normal form and also diluted with various amounts of normal uranium. The samples so produced were then divided into two parts. One set of samples was sent to Berkeley to be assayed by the neutron assay method, the other to New York for mass spectrograph analysis. The results were in disagreement by almost 10 per cent. The Berkeley method was carefully examined by Segrè, who could find no explanation for the discrepancy. The mass spectrographic method was examined by Bainbridge, who likewise could find no explanation. As a result provisions were made to set up both methods at Los Alamos under the Radioactivity Group. While equipment was being set up, the Chicago and New York Laboratories made three independent isotopic determinations in a certain sample known as E-10. Close agreement was obtained, and this sample was thereafter used as a secondary standard at Los Alamos with the neutron assay method. This method proved of great value in assaying the active parts of the gun assembly, and later the Pu^{239} assemblies.

6.80 By May 1944 a study of uranium isotopes had been made in the mass spectrometer as a first test, and the resolution was satisfactory. Analysis of normal material and of sample E-10 gave excellent agreement.

6.81 In the late summer of 1944 the Pu^{239} mass spectrometer was set up and preparations were made to test the sample of Pu^{239} re-irradiated at Site X, to assay its Pu^{240} content. This work was actually carried out in the Research Division, after its creation in August 1944. When the sample arrived and was examined, it showed a peak at the 240 position and the relative abundance of Pu^{240} to Pu^{239} was in good agreement with the figure that could be calculated from the value of the branching ratio and the rather uncertain irradiation. Thus the discovery of Pu^{240}, following from spontaneous fission measurements, was confirmed.

Instrumentation

6.82 In writing such a history as this, the impression is easily created that terminal work, work which enters directly into the main course of development, is the most important, difficult, and time consuming. Yet every successful (or unsuccessful) experiment implies a degree of instrumental development and construction that is not easily appreciated by an outsider. One deals in experimental nuclear physics with the realm of the small and the fast. Both the time scale and the amplitude of the phenomena studied must be transformed to make them susceptible to direct control and observation.

6.83 Although a considerable amount of modulation and control equipment for the accelerators was built at Los Alamos, developments on this side were less novel and extensive than on the side of observation and measurement. With the exception of photographic plate and cloud chamber techniques, all experiments involve the counting of ionization chamber pulses. In a typical experiment we may distinguish at a minimum four distinct steps: the counter or detector, the amplifier, the discriminator and scaler, and finally the mechanical recorder. The ions produced by the particles being studied are moved to collecting electrodes by an electrical potential applied across them. This registers as a minute electrical pulse of the order of microvolts. This pulse is then amplified to the order of volts, and fed into the counting system. The discriminator is a means of selecting only pulses of a certain type which are of interest to the experimenter. Since usually their frequency is too high to be directly recorded by mechanical means, a further electronic step is inserted, the scaler. The scaler "demultiplies" the frequency of the incoming pulses, so that, for example, it gives one pulse for every sixty-four incoming pulses. The pulses coming out of the scaler are then recorded mechanically.

6.84 Certain developments in the first step, the counter or detector, have already been mentioned in previous sections of this chapter, for example, fission detectors, in which ions are produced in an ionization chamber by fission fragments from a sample of fissionable material. These include threshold detectors, making use of the materials which only fission for neutrons above a certain energy. Another development of importance already mentioned was the long counter, developed by members of the Electrostatic Generator Group, which possessed the virtue of a very flat response to neutrons of different energies. Still another important instrument developed, which made use of the well-established hydrogen cross section, was the proportional hydrogen recoil counter. Finally, mention should be made of the boron trifluoride proportional neutron counter; this is an ionization chamber filled with boron trifluoride. $Boron^{10}$, about 18 per cent of normal boron, undergoes an (n, α) reaction, producing Li^7. The number of alpha particles counted corresponds directly to the number of neutrons absorbed. Such counters are surrounded with paraffin, to slow down the neutrons to energies with high reaction cross sections. High efficiency of these counters was obtained by high pressure counters, by purification of the trifluoride, and by using the separated B^{10} isotope when it became available.

6.85 The most extensive development in counting technique was that of very fast detectors and amplifiers. With proper design of counters and with certain gas mixtures, collection times for electrons were reduced as low as 0.2 μsec. This work, along with other counter development work, was mostly carried out by the Detector Group. To make use of electron collection required very fast amplifiers; these were developed by the Detector Group and the Electronics Group. The amplifiers developed had rise-times between 0.05 and 0.5 μsec.

6.86 A large amount of work was done in development of discriminators; for example, differential discriminators were developed which would select pulses of a given height - multi-channel discriminators which made possible the classification and simultaneous recording of pulses of different heights. A new scaling circuit was developed which increased the reliability of scaling, and thereafter became standard in the Laboratory.

6.87 Apart from this work on general counting equipment, a number of electronic techniques were developed for special purposes. One of these was in the field of timing circuits. Many experiments involved the measurement or control of phenomena occurring at specified time intervals; for example, the Rossi experiment, the measurement of the length of stay of neutrons in tamper, or the velocity selector for selecting monoergic neutrons from the cyclotron. Another type of timing circuit, developed extensively in connection with the study of the implosion, made possible the measurement of velocities in explosives. In connection with the use of oscillographs in recording data on the implosion, there was extensive development of amplifiers, circuits for printing timing marks on film, sweep circuits, and circuits to delay the starting of a sweep for a specified time.

6.88 Another important job carried out by the Electronics Group was the production of portable counters and other health instruments.

6.89 The work of producing thin foils of fissionable and other materials was largely the work of the Radiochemistry Group, and is reported in Chapter VIII (8.56 ff); but the Radioactivity Group and others from the Experimental Physics Division also collaborated in this work.

Chapter VII

ORDNANCE DIVISION

Organization and Liaison

7.1 The detailed history of ordnance and engineering activities at Los Alamos to the time of its extensive reorganization in August 1944 has been divided into the following six sections: (1) Gun design, proving, interior ballistics; (2) Projectile, target and initiator design; (3) arming and fusing; (4) engineering; (5) implosion studies; and (6) delivery. As of August 1944 this work was being carried out under the following groups:

E-1	Proving Ground	Lt. Comdr. A. F. Birch
E-2	Instrumentation	L. G. Parratt
E-3	Fuse Development	R. B. Brode
E-4	Projectile, Target, and Source	C. L. Critchfield
E-5	Implosion Experimentation	S. H. Neddermeyer
E-6	Engineering	L. D. Bonbrake
E-7	Delivery	N. F. Ramsey
E-8	Interior Ballistics	J. O. Hirschfelder
E-9	High Explosive Development	K. T. Bainbridge
E-10	S Site	Major W. A. Stevens
E-11	RaLa and Electric Detonator	L. W. Alvarez

7.2 Prior to its formal organization in June 1943, the germ of the Ordnance Engineering Division occupied two or three small rooms in Building U. The small staff assigned to it preliminarily was concerned with procurement, gun design, and instrumentation, but the main activity of this period consisted in the discussion and analysis of the work that lay ahead, labeling and organizing the elements of the new field into an accepted general scheme.

7.3 In May Capt. W. S. Parsons, USN, came to the Site for a preliminary visit. His transfer to be head of the ordnance engineering work at Los

Alamos was then arranged at the request of General Groves, on the recommendation of Conant and Bush and with the approval of the Governing Board. Capt. Parsons returned in June as Division Leader of The Ordnance Division.

7.4 The original groups of the new division were the first five listed above, under the leadership, respectively, of E. M. McMillan, K. T. Bainbridge, R. B. Brode, C. L. Critchfield, and S. H. Neddermeyer.

7.5 After Parsons' first visit in May he investigated the possibilities of obtaining a competent chief engineer to head group E-6. The man chosen by Parsons was George Chadwick, for 20 years Head Engineer of the Navy Bureau of Ordnance. Although Chadwick never resided at Los Alamos, he functioned from June to September 1943 as prospective head of this work. During this period he worked with the Bureau of Ordnance and the Navy Gun Factory on the design and fabrication of the first experimental guns, consulted at Los Alamos on the design of the Anchor Ranch Proving Ground, and in August was asked to assist in the procurement in the Detroit area of machinists and draftsmen. At this time Chadwick decided not to take the Los Alamos position. The connection with Chadwick in Detroit remained, however, and is discussed later in this section (7.12).

7.6 After a brief interval in which the Engineering Group was under the direction of J. L. Hittell, this position was given to P. Esterline in December, and was held by him until his resignation in April 1944. Late in May the position was given to L. D. Bonbrake, having been held on an interim basis by R. Cornog after Esterline's departure. The general reasons for the difficulty in finding the right person for the position of Chief Engineer are discussed in detail later (7.40-7.49).

7.7 In the fall of 1943 Groups E-7 under Ramsey and E-8 under Hirschfelder were added to the division.

7.8 Early in 1944 with the rapid expansion of the ordnance and particularly the implosion program, the administrative work of the division was subdivided under two Deputy Division Leaders: E. M. McMillan for the gun program, and G. B. Kistiakowsky for the implosion program. McMillan's place as Group Leader of E-1 was taken by Lt. Comdr. A. F. Birch. A new group (E-9) was added by Kistiakowsky under K. T. Bainbridge for the investigation and design of full scale high explosive assemblies and the preparation for a full scale test with active material. Bainbridge's place as Group Leader of E-2 was taken by L. G. Parratt. Each of the two branches of the division acted with the advice and assistance of a steering committee. Although formally equivalent, the two new subdivisions were of quite unequal significance organizationally. The gun program was proceeding smoothly and at a constant level of activity. The implosion program, on the contrary, was beset at this time with serious organizational and technical problems, springing from its rapid increase in size and importance.

7.9 In late June and early July there was further extensive reorganization of the implosion project. Neddermeyer became the chairman of the implosion steering committee, Kistiakowsky became acting group leader of E-5, and two new groups were formed. The first of these was E-10 under Major W. A. Stevens. The functions of this group were maintenance and construction for the implosion project, and the operation of the S Site plant. The second was E-11 under L. W. Alvarez. This group was engaged in the development of the RaLa tests, and in the investigation of electric detonators.

7.10 When Parsons returned to Washington after his first Los Alamos trip, he arranged that all his connections with the Navy Department would be handled through Lt. Comdr. Hudson Moore of the Research and Development Section of the BuOrd. The most important activities of the latter was with the Naval Gun Factory and concerned the fabrication of experimental guns. Moore also handled procurement of miscellaneous ordnance materials from Navy stores, and liaison with the Navy Proving Ground at Dahlgren, Va.

7.11 At the same time Parsons arranged for security reasons that all Navy equipment would be shipped to E. J. Workman, head of Section T, OSRD, Project at the University of New Mexico, Albuquerque.

7.12 The other principal liaison of the Ordnance Division was the "Detroit Office." As mentioned above, Chadwick was asked in August to assist in personnel procurement. In order to try out design engineers hired by Chadwick and to pay machinists employed before new shop facilities were ready for use, Chadwick set up an office in Detroit.

7.13 In August, Bush had approved the use of the Section T, OSRD Project at the University of Michigan for the development of radio proximity fuses for the bomb (7.36). Section T funds so used were to be replenished by transfer of funds from the Navy Department to OSRD.

7.14 In October, it was decided that models for flight tests would be fabricated under the procurement set-up of the University of Michigan using Section T funds. The orders would be placed by H. R. Crane who was the head of the Michigan Project. It was not contemplated, however, that the University of Michigan would act in any capacity beyond general supervision and accounting. Chadwick's office in Detroit, rather than Crane, would be responsible for inspection and follow-up work. This arrangement was not wholly satisfactory since Crane had a considerable interest in these models; fuse units designed and fabricated by his project were to be incorporated in them.

7.15 The financing of the Detroit office was arranged by contract between Chadwick and the University of California until November 10, later extended to March 1944. Chadwick was appointed OSRD representative to facilitate his work on fabrication contracts let by the University of Michigan. In May 1944, the financing of the Detroit office was taken over by the University of Michigan. In June, Lt. Col. R. W. Lockridge assumed charge of the Detroit office. In July he was appointed OSRD representative. Col. Lockridge's appointment was a means of unifying the Detroit-University of Michigan relationship and of bringing the activities of the Detroit office more closely under Manhattan District control.

7.16 Other ordnance liaisons are discussed in the appropriate sections (7.26, 7.37, 7.52, 7.69 ff).

Gun Design, Proving, Interior Ballistics

7.17 During the first six months of the Laboratory, the only method of assembly that was considered sound enough in principle to warrant an extensive proving and engineering program was the gun method. In particular the proving facilities, manufacture of guns, and bomb design were focussed on the use of a single gun to fire active material into a target. In this same period, however, there was an intensive study of alternative possibilities of the gun type. Notably, the use of two or more guns on the same target and the possibilities of jet propulsion received some consideration, and preliminary designs were made on double gun systems. The possible use of high explosive to replace slow burning propellant in multiple guns was explored to a certain extent. None of these alternative schemes proved attractive enough on paper to be taken as serious competitors to the single gun method, and in the course of six months the full attention of the gun group was given to the latter.

7.18 The state of knowledge of the physical and nuclear properties of the active materials was such in April 1943 that only very rough estimates could be made as to how much material had to be fired from a gun, how fast it must be fired, and in the case of plutonium, how much acceleration the material could stand. It was assumed that the material could be made as strong as need be through alloying and that the density of plutonium was essentially the same as that of uranium. Then, from the existing knowledge of nuclear cross sections, the sizes of critical assemblies were computed, and from the purification standards, the required velocity of assembly determined.

7.19 The gun performance thus required is in the range of standard ballistic experience. In fact, the velocity, 3000 feet per second, for a plutonium projectile was chosen as a practicable upper limit, since, obviously, much higher velocities would be desirable. Otherwise, the gun design problem bore little resemblance to standard ordnance problems. There was no concern, in the assembly of critical masses, about the usual questions of stability in flight of the projectile, absorbing the energy of recoil, erosion of the tube or muzzle pressure. Instead, the requirements were for as light weight tubes and as reliable interior ballistics as possible. About all that standard ordnance could contribute to this problem were its general formulae for gun strengths and its theory of propellants, both of which had to be used far outside the range of accumulated experience. The situation was a little disturbing at the start of the program of engineering the 3000 feet per second gun because the standard piece that came closest to its performance had proved to be an unreliable gun.

7.20 The seriousness of the problem of getting these fantastic guns made and proved called for a great expansion of personnel, facilities, and liaison in the Ordnance Division. This expansion was instituted by Captain Parsons upon his assignment to the project in May 1943. At this time, the attention of the division was centered immediately upon the practical problems of getting the 3000 feet per second gun made and proved. The reason for this specialization was, simply, that the proposed design of this gun was farthest removed from standard practice. The principal departures from standard design were: (1) this gun tube should weigh only one ton instead of the five tons usually characteristic of the same muzzle energy; (2) consequently, it must be made of highly alloyed steel; (3) the maximum pressure at the breech should be as high as practicable (75,000 pounds per square inch was decided upon), i.e., the gun should be as short as possible, and (4) it should have three independently operated primers.

7.21 The Naval Gun Design Section undertook the practical problems of engineering the proposed design in July 1943. Pressure-travel curves were obtained from the NDRC through R. C. Tolman. These were computed by the ballistics group at Section 1 of the Geophysical Laboratory under the supervision of J. O. Hirschfelder who subsequently joined the staff at Site Y and continued to supervise the work of the Interior Ballistics Group. The curves were drawn for maximum breech pressures of 50,000, 75,000, and 100,000 pounds per square inch and submitted to the Bureau of Ordnance, Navy Department.

7.22 As stated above, this was a unique problem involving special steel and its radial expansion, design and breech, primers and mushrooms for extra high pressures, insertion of multiple primers, and many smaller details. The absence of rifling and special recoil mechanisms were the only details in which this gun could be considered simpler than standard guns. Nevertheless, the drawings were completed and approved, in a very short time, and the forgings required were ordered in September. Some delay was occasioned in the preparation of the steel because of difficulty in meeting the physical specifications. The fabrication of guns was done at the Naval Gun Factory, and required about four months at high priority. The first two tubes, and attachments, were actually received at Site Y on March 10, 1944.

7.23 The tubes received in March were of two types. Both had adaptor tubes surrounding them in order that the recoil could be absorbed in a standard single Naval gun mount. On the type A gun this adaptor made no contribution to the strength of the tube and was fitted to the gun proper only at the breech. On type B, the adaptor did support the gun tube so that it was much stronger than the bare tube would be. The purpose of type A was to allow tests of the wall strength and deformation in the high alloy gun tube, and the purpose of type B was to make specifically interior ballistic studies.

7.24 While these guns were being procured, intensive effort was put into installations, acquiring personnel and perfecting techniques for testing the guns, and in establishing the necessary channels of procurement for accessories such as propellants, primers, cartridge cases, rigging gear, and the like. The early plan was to install a proving ground, along more or less established lines, with centralized control of all operations on explosives research. The proving work was done by the Proving Ground Group, and the operation, loading, and care of the guns was under the direction of an experienced ordnance man from the Naval Proving Ground at Dahlgren, T. H. Olmstead. Although this plan for a proving ground became impractical for the work on high explosives when the latter work became more elaborate, the gun work was adequately implemented at the original proving ground at Anchor Ranch. The buildings at Anchor Ranch included the usual gun emplacements, sand butts, and bombproof magazines, control room, and shop. Novel features were incorporated in recognition of the special nature of the proving problem. For one, the fact that it was by no means certain that high alloy tubes would not fragment when overloaded, plus the program for eventually firing the tubes in free recoil, increased the hazards of proving above the ordinary. To cope with this possibility the ground level of the gun emplacements was put above the roof of the bomb proofs, which were installed in a ravine. Also, to protect the guns, targets, etc., from public view, as well as to permit instrumentation on these units in all kinds of weather, the guns were provided with shelters that could be rolled away for the period of actual firing. Construction was started on the proving ground in June 1943 and continued at high priority. It was virtually completed in September. The first shots were fired from emplacement No. 1 on September 17, 1943, at 4:11 p.m. and 4:55 p.m. A second emplacement was completed by the following March in anticipation of receiving the special guns.

7.25 The proof firing between September and March was done chiefly with a 3"/50 Naval A.A. gun equipped with unrifled tubes. The purposes of these rounds were primarily to test the behavior of various propellants, to study elements of projectile and target design on 3 inch scale, and to smooth out instrumentation of the studies generally. The instrumentation was under the direction of K. T. Bainbridge. Most of the standard proving ground techniques were adapted to this work and some new ones were developed. Thus, the familiar photographic methods, microflash, fastax, and NPG projectile cameras proved extremely useful in studying the condition of the projectile as it left the bore and in detecting "blow by." Muzzle velocities were determined from the projectile camera records, as well as by magnetic coil and Potter chronograph. A photoelectric system was also developed for this measurement. Copper crusher gauges and piezo-electric gauges were applied to the determination of powder pressure. Electric-strain gauges were used on the barrel of the gun and on certain targets. And there were occasions for use of many other standard ordnance methods such as star gauging, terminal observations (on recovered projectiles), and yaw cards.

7.26 The success of application of these standard methods was usually above standard, particularly in photography. One nonstandard technique that was developed specifically for the interior ballistic problem was the following of the projectile, during its acceleration in the tube, by continuous microwaves. By the time that the type A and B guns arrived, the proving ground routine, the techniques of instrumentation, and the performance of propellants were well established, at least for work at 3 inch scale. In this time interval, the burning of propellants at very high pressure was being studied upon request from Los Alamos at the Explosives Research Laboratory at Bruceton, Pa., thus adding to the preparation for the special gun.

7.27 In February, the direction of Anchor Ranch was assumed by Comdr. F. Birch, with McMillan as Capt. Parsons' Deputy for the Gun. In March, the proving work swung over to testing the type B gun for interior ballistic behavior (first round March 17, 1944). By this time, however, the specifications for a lower velocity gun, to be used with U^{235}, became clear. These specifications were considerably less exacting than for the original gun envisioned for this purpose as they called for a muzzle velocity of only 1000 feet per second. Three of these guns were ordered from the Naval Gun Factory in March. Some of them were to be radially expanded, and a special gun mount had to be designed for them. In spite of this, they presented a much simpler problem to the Bureau of Ordnance, and no anxiety was felt for their operation.

7.28 By reason of the well-prepared experimental background, the testing went smoothly and rapidly. It was found that "WM slotted tube cordite" was the most satisfactory form of propellant at the high pressures involved. Other propellants were tried, but proved inferior. In particular, the 5"/50 Navy powder behaved erratically, as it had done before, and this was traced to worm holing of translucent grains. The Mark XV primers proved to stand over 80,000 pounds per square inch. The propellant performed properly at -50°C. The interior ballistic problem was solved, but the tube was eroded so badly that it had to be returned to the Gun Factory in April. Attention was then given to mechanical strength and deformation of the type A gun. By this time, the proving ground was working at very high efficiency. The installation of a drum camera greatly facilitated record taking, and many measurements of pressures, strains, velocities, and time intervals were made on one round. By early July, the soundness of the design was thoroughly proved, and only by running the maximum breech pressure up to 90,000 pounds per square inch was it finally possible permanently to deform the gun.

7.29 By early July, however, it became clear that the 3000 feet per second gun would never be used. The necessary presence of Pu^{240} in the Hanford plutonium (4.46) decreased the minimum time of assembly of this material far below what was possible by gun-assembly methods.

Projectile and Target Design

7.30 Although the development of designs of projectiles and targets for the gun assembly should be capable of moving more rapidly than the development of the gun itself, the uncertainties surrounding the problem were relatively more serious. Not only were the physical properties of plutonium entirely unknown, but the whole problem of producing an assembly of projectile and target that would start the chain reaction under favorable conditions, i.e., at the right time and in a compact geometry, was entirely new. Accumulated experience on armor penetration greatly discouraged early suggestions that the projectile be stopped by the target. The conception of a gun assembly, as of April 1943, was rather one in which the projectile passed through the target freely.

7.31 Before any work was started on these developments, the plan was complicated by the further uncertainty in the amount of active material that could be safely disposed in the projectile alone, or in the target. This was particularly important in the case of the hypothetical uranium hydride gun; for here the critical mass would be small, while for effectiveness a large number of critical masses would have to be assembled. Although planned primarily for the hydride gun, the critical mass calculations for odd metal shapes were not at the time accurate enough to rule out a possible need for such methods in the metal gun model. The development of these mechanisms was a difficult undertaking which remained uppermost in the efforts of the groups concerned until February 1944, by which time the hydride gun had been abandoned.

7.32 The design development of projectiles and targets was centered in the Projectile and Target Group. Very active interest and vital assistance in this work were contributed by other groups, notably the Proving Ground Group, and the Metallurgy Groups concerned with heat-treating of steel. Parallel to the policy adopted for the gun itself, all effort was concentrated on the problems pertinent to a 3000 feet per second velocity of assembly.

Arming and Fusing

7.33 The arming and fusing of the atomic weapons could not be done satisfactorily by straightforward application of the established art. Part of the reason for this is to be found in the enormous investment represented by a single bomb. Triggering devices that fail only 1 per cent of the time, on the average, were hardly acceptable. On the other hand, the great value of the single bomb dwarfed the expense of multiple triggering by very fine equipment which would be forbidding in a more commonplace weapon. The second reason is that, from the early beginning of the effort at Los Alamos, it was thought desirable to detonate the bomb many hundreds of feet from the ground and no fusing equipment had been developed explicitly for this purpose since the requirement is unique to the size of the explosion. Just how high above the ground the bomb should explode for maximum total damage was not known. The determination of this height had to be made mathematically from an extension of the theory of damage by blast plus a knowledge of the expected size of the explosion. None of this theory was available in April 1943.

7.34 The development of arming and fusing devices was begun in May 1943 in Group E-3 (R. B. Brode). The plan for fusing systems proper was the same for the gun type and for the implosion type bomb. It called for a guarantee of performance that allowed less than one chance in ten thousand of failure to fire within a hundred feet or so of the desired altitude. Two general lines of development were started. The one centered on the possible use of barometric switches for firing the bomb. The second line of attack was to adapt the newly developed electronic techniques to the fusing problem. In particular, the radio proximity fuse, radio altimeters and tail-warning devices performed, in some measure, the desired function of detecting distant objects, and the suitability of these devices for use in a bomb had to be determined. A third possibility, the use of clocks, was considered to be a last resort because their operation would require careful setting just before the bombing run, and the chances for human error are great under these circumstances.

7.35 The barometric switches had the advantage of being simple mechanical devices, whereas the various electronic systems were highly complex. On the other hand, it was by no means certain that a reliable barometric indication could be obtained in a falling bomb. Thus the work of the group included not only the design of a sensitive and sturdy barometric switch, which could be put into production, but also the proof of these switches in action. The latter effort proved to be the most extensive. It was necessary to fit model bombs with radio transmitters ("informers") whose signals were modulated by the action of the barometer, drop these from airplanes, and follow the flight of the bomb photographically as well as with the radio receiver. With the proper cross checks in timing, this procedure leads to the correlation between the recording of the barometer and the actual elevation. This work was started at small scale in December, in cooperation with the NPG at Dahlgren. Full scale bombs dropped from full height were subsequently used in the continuation of these tests at Muroc, beginning March 1944. By that time it seemed probable, however, that the pressure distribution on the surface of the bomb was not so sensitive to absolute elevation as would be desired, and that barometric firing should be used only if the electronic devices could not be developed. The development of the barometric switch thus became secondary, but the field experience in proving these units was of primary importance to the development of the weapon. It was through this early effort and cooperation with the Instrumentation Group, E-2, that the problems of instrumentation in the field, liaison with the Air Forces, and operations at distant air bases were solved and reduced to the routine that was so necessary for the successful proof of the completed bombs (completed, that is, except for active material).

7.36 In the period preceding February 1944, there remained, as stated above, considerable uncertainty as to the height above ground at which the bomb should be fired. The early estimates were below 500 feet, more specifically 150 feet. In this range, the amplitude-operated radio proximity fuse was a feasible device. Brode had had a major part in the development of these fuses for projectiles and began at once on the ground work for adapting them to the bombs. It was deemed undesirable to set up a radio proximity fuse laboratory, with the large increase of personnel necessary, at Los Alamos. Accordingly, the design development, manufacture, and tests of radio proximity fuses and "informers" were undertaken in liaison with Section T, OSRD. The work was to be done at the University of Michigan under the supervision of H. R. Crane. Field tests in this program were to be made at Dahlgren. This program was instituted in the summer of 1943 and was entering a major proof phase in February 1944 when theoretical work predicted that if the efficiency of the bomb were high enough the desirable height of detonation might be as high as 3000 feet. Since the amplitude-operated sets would not function properly at these elevations, it was immediately necessary to follow a new line of electronic development. The liaison with the University of Michigan Laboratory was continued, however, to follow up the radio proximity fuse development in case the eventual decision would be for several hundred feet, to continue in the production of "informers," and to assist in the new lines of attack.

7.37 The new types of electronic devices that were considered in February were the radio altimeters. It was decided to follow up both the frequency modulated "AYD" and the pulse type "718." The program for modification of AYD was assigned to the Norden Laboratories Corporation under an OSRD contract through Section T, with the approval of the Bureau of Ordnance, Navy Department. The 718 was just getting into production but the development group at the Radio Corporation of America was still operative. It was intended to negotiate with RCA to employ this group in the fuse development. Before the negotiations were started, however, it was learned that they also had under development for the Air Corps a tail warning device that should be readily adaptable to the fusing problem. The production of these "APS/13" units was just being started, but through the cooperation of the Signal Corps, a substantial part of the pilot production was made available for Brode's work. In fact, the third such unit to be made was delivered to Los Alamos in April. This set was tested in May by diving an AT-11 plane, and proved very encouraging. Two full scale drop tests in June strengthened the conviction that the APS/13, now nicknamed "Archie," was the answer to the electronic fusing problem. The modified AYD had persisted in showing difficulties that discouraged its use, even below 1000 feet, where it had been made to work. Field tests were continued on a more and more extensive scale through June and July, and included the final work on barometric devices as well as the preliminary study of the electronic sets.

7.38 Concurrent with the field tests, work in the Laboratory was given to the implementation and analysis of these tests as well as to design research and proof of service units. The latter effort involved establishment of vibration and temperature tests for clocks, switches, batteries, and electronic equipment in more or less standard procedures for the acceptance of airborne equipment. In April 1944, preparation of the over-all design of the arming and fusing system was undertaken. This system included pull-switches, banks of clocks and barometric switches for arming, and four modified APS/13 units operating independently to initiate the firing circuit at the desired altitude. The selection of this system was made on the basis of the preliminary field tests of these units and on general considerations of elimination of as many uncertain elements as possible. The field proof of operation of the system as a whole began in August 1944 and constituted a major part of the work of the following year. The scarcity of tail warning units in the summer of 1944, however, forbade wholesale use of these in bomb drops. Accordingly, the modification development of these units was assisted by the use of barrage balloons for testing the units (as assembled in models of the bombs). This phase of the work was carried out at Warren Grove, New Jersey.

7.39 In addition to the primary development of a high elevation triggering mechanism, some attention was given to underwater detonation. The goal was to detonate 1 minute after impact with the surface. This program hardly got underway, however, before theoretical considerations, based on model tests, predicted that shallow underwater delivery was ineffective. Full attention was then given to the air blast bomb. For the latter, a propeller-activated arming switch was also developed but was discarded as mechanically unreliable in the presence of ice or from misalignment. The only propeller arming actually used was in the four Navy standard nose impact bomb fuses A.N. 219 in the forward end of the Fat Man. The purpose of these was to get good self-destruction (at least) in the event of failure of the primary fusing system.

Engineering

7.40 In the original organization of the ordnance work, the primary responsibility for integrating the weapon was provided by setting up an Engineering Group, E-6. This was conceived as a group of competent design engineers who would reduce to accepted fabrication practice the specifications on performance as these specifications became clear. They would include the mechanical construction, ballistic and aerodynamic behavior, electrical wiring and incorporation of the arming and fusing equipment, and also the special provisions required for safing and handling the assembly mechanism and active material. In addition to this primary responsibility the group was to provide design service incidental to the various experimental programs in ordnance, procure special materials and shop services from outside industry, and supervise the Ordnance Machine Shop (C Shop).

7.41 As already mentioned (7.5), Capt. Parsons was assisted in setting up this group by George Chadwick, whose Detroit office was the center of initial personnel procurement. Operation of the design groups actually started at about the time of the completion of the ordnance building, A building. Since the research groups were just getting started also, there was very little notion of what the specifications for the weapon would be, except in the matter of aerodynamic performance. Thus the primary activity of the Design Group was centered on the designing of bomb models and the procurement of dummy bombs for test drops.

7.42 In addition to this work, the secondary responsibilities for C shop and for outside procurement of materials and machine work were growing daily. Demands for shop work were perpetually overloading the facilities at Los Alamos, even with the procurement services in Detroit. The manufacture of hemispheres to be used in implosion experimentation was one of the great problems; arrangements were made to procure these from Detroit shops through the Detroit office. This arrangement was supplemented in June 1944 by procurement of hemispheres from the Los Angeles area.

7.43 In spite of the lack of detailed specifications for the weapon, certain preliminary designs of mechanical and high explosives assembly, fuse assembly, and molds for charges were made during the first winter. The situation with the gun model was more satisfactory because the details were less tentative. It had been anticipated that the details on the implosion model would get more definite with time. Instead of this, as experimental information on the implosion increased, the possible specifications grew less and less definite, and more and more complex, in the sense of an increasing number of alternatives and additional elements. The picture was changing so rapidly and the contributions to design sprang from so many different divisions of the Laboratory that the original organization for engineering development was rapidly becoming inadequate.

7.44 It was evident that the level of coordination needed for making a weapon of the implosion system had risen above that represented by a single operational group. This was pointed out by Esterline when he resigned as group leader in April 1944. His successor, R. Cornog, made every effort to rectify the lack of coordination within the structure of the old organization. There were other organizational plans afoot, however, and these led eventually to coordination at the level of the Director, by the Weapons Committee (9.10). Meantime the engineering problems relating specifically to the internal structure of the implosion bomb were taken over by the new group under K. T. Bainbridge. This group, formed in March 1944, combined design and certain kinds of experimentation on the implosion system. (Design work, in particular, was under the supervision of R. W. Henderson). One of the responsibilities of this group was the full scale test of the bomb, and the history of this part appears elsewhere (Chapter XVIII). This reorganization of the engineering effort placed the implosion design in closer touch with implosion research. The work on the gun and on the external bomb assembly continued in the Engineering Group, but in close cooperation with the Explosives Development Group.

7.45 The new group was able to detail developments in such things as boosters, primacord branching, and detonation systems in the light of current research on these components. There was, however, little activity on the design of the active core and tamper, since research on these was still in the differential stage and there was, as yet, no acceptable plan for distributing the active material. Although some preliminary thought was given to this question, the active design work and the coordination with experiments on the nuclear physics of the bomb was done in G Division after the August 1944 reorganization (Chapter XV).

7.46 Another development that called for special organization was the design and manufacture of lens molds for high explosives. Here again the need for coordinating the efforts of theory, experimentation, casting practice, design, machining, and procurement went beyond the scope of any one operating group. Accordingly a Molds Committee was formed in the summer of 1944 and a Mold Design Section was organized under the administration of V shop. This work continued under the subsequent organization, and its story properly belongs to the later period (16.14).

7.47 The difficulties peculiar to the engineering function at Los Alamos were not all connected with the persistent uncertainty of final specifications. One basic source of difficulty was the developmental character of almost all Los Alamos engineering. Such engineering requires flexibility in meeting the constant change of specifications incident to new experimentation, and as a part of this, the settling of general design principles as a framework within which more detailed specification may later be fitted. In production engineering, on the contrary, emphasis is all on the details of design, and problems of tooling and mass production. It was the misfortune of Los Alamos and its engineers that they were drawn primarily from industry and were accustomed to larger and less complex operations than they found here. With differing degrees of directness in different cases, it was this difficulty that was responsible for the rather large turn-over of the engineering staff. And as the history shows, this problem was solved by a type of coordination quite unfamiliar to production plants.

7.48 Another difficulty was the combination of design and service functions within the Engineering Group. Although purely an organizational difficulty, it reflected also the inappropriateness of production methods to a research and development laboratory. The degree of procedural formality necessary in the preparation of detailed drawings for mass production is at the same time unnecessary and burdensome if applied in development work. Operation on this basis was a frequent source of difficulty, and tended by overloading the group with service problems to impair its principal function (3.109 ff).

7.49 Last among the difficulties to be recounted were the isolation of the site and the elaborate precautions required as security measures. The security policy was blamed more than once for misunderstandings on details of machine work being procured from outside shops. In any case, it is to be admitted that liaisons were generally so round about that they easily led to difficulties. The isolation of the place, over forty miles from a railroad, also contributed its bit to delay, particularly in handling heavy equipment. As the project approached the final phases of its work, the handling and working of full scale targets, bombs, guns, and high explosive systems became a greater and greater part of the work. This required not only the equipment for handling the material but the plants and tools for making and assembling the objects themselves. Since the provision for this heavy work is incidental to more obvious achievements, it is easy to overlook the important part played by making these provisions in the allotted time and on top of an isolated mountain.

Research on Implosion

7.50 The program of implosion research grew from its initial position as the concern of one small group into the major problem of the Laboratory, occupying the attention of two full divisions. The program was started during the conferences of April 1943 with the specific proposals made then by Neddermeyer (1.78). Neddermeyer had developed an elementary theory of high explosives assembly. There was, however, no established art that could be applied even to part of the mechanical problem. In this respect the implosion research differed from the gun research, where many mechanical and engineering features and methods of proof were at least relatively standard.

Coupled with this undeveloped state of the art of execution was a backwardness in the art of conception. As a result one cannot make a well-dated chronology of the appearance of "ideas." Development was rather in the form of a spiral. Rough conceptions that appeared quite early are reintroduced later with greater concreteness and in an altered context. Many possible developments and proof techniques were dreamed of in the spring and summer of 1943, to become even partial realities only a year later. For example, the possibility of using explosive lenses for focussing the converging shock wave, the possibility of a type of electric detonation, and the conceivable benefits of compression of active material were all considered in this period, but because of the lack of development very little could be done.

7.51 Another factor affecting the implosion program was that it began as a dark horse and did not immediately win in the Laboratory a degree of support commensurate with the difficulties that had to be overcome.

7.52 After the April conference Neddermeyer visited the Explosives Research Laboratory at Bruceton to become acquainted with experimental techniques as applied to the study of high explosives. Certain types of equipment and installations used at Bruceton were considered desirable for the early implosion work, and plans were made for including these at the Anchor Ranch Proving Ground. While at Bruceton, Neddermeyer had his first implosion test fired and found encouragement in the result.

7.53 The need for personnel with experience in handling and experimenting with high explosives became urgent at once and, because of continual expansion, remained an unfulfilled requirement throughout the life of the Laboratory during this period. There was enough general experience in the Implosion Group, however, to get started on a firing program as soon as the first explosives arrived. The first implosion tests at Los Alamos were made in an arroyo on the mesa just south of the Laboratory on July 4, 1943. These were shots using tamped TNT surrounding hollow steel cylinders.

7.54 Interest in the implosion remained secondary to that in the gun assembly. There was some consideration of the possibility of using larger amounts of explosive to increase the velocity. But the impossibility of recovery and the currently incomplete instrumentation kept such things in the "idea" stage for several months. The decisive change in this picture of the implosion occurred with the visit of J. von Neumann in the fall of 1943. Von Neumann had had previous experience with the use of shaped charges for armor penetration. Von Neumann and Parsons first advocated a shaped charge assembly, by which active material in the slug following the jet would be converted from a hollow cone shape to a spherical shape having a lower critical mass value. He was soon persuaded, however, that focussing effects similar to those which are responsible for the high velocity of Monroe jets would operate within an imploding sphere.

7.55 For the development of an adequate HE production plant and research program as well as for general assistance to the research in implosion dynamics, the consulting services of G. B. Kistiakowsky were acquired by the Laboratory in the fall of 1943. In February 1944, Kistiakowsky joined the staff as Capt. Parsons' deputy for the implosion. In April he assumed full direction of the rapidly increasing administrative problems of this work.

7.56 The period preceding February 1944 was spent in vigorous development of experimental techniques. The art of recovery from weak charges was the most rapidly exploited technique, since it required no elaborate instrumentation. Procurement of the spheres required was beyond the capacity of the Laboratory shops; outside procurement was arranged through the offices at Detroit and Los Angeles. Although the test conditions were admittedly far from those in a fast implosion, the recovery technique proved useful in the interpretation of the process and in revealing the importance of and possible difficulties in obtaining symmetry. By February, evidence had been obtained of possible trouble from the interaction of detonation waves and from the spread of detonation times in multipoint detonation. These were not yet, to be sure, thought of as basic defects lying outside the range of existing experimental techniques to correct.

7.57 The other techniques that were inaugurated in this period were chiefly in a stage of being perfected to the point where they would be of quantitative usefulness to the investigation. The difficulties lay principally in the necessity for recording events inside an explosive and for timing these events within an uncertainty of the order of 1 microsecond. In November a program for photographing the interior of imploding cylinders by high explosive flash light (a method developed at Bruceton) was started. Some qualitative results were soon obtained, but refinement of the method and elimination of secondary blast effects required until spring to achieve. Practically the same history applies to the flash X-ray method of studying small spheres. The principal problem presented by this method was the precision of time correlation between the implosion and the X-ray discharge. This problem was solved, in cooperation with the Ordnance Instrumentation Group, by extensive modification of the commercial X-ray machines. The Instrumentation Group had also designed and had constructed rotating prism cameras making use of ultra-centrifuge techniques. These were adapted to proving ground use for taking rapidly repeated photographs of cylindrical implosions in December and January, but were never used effectively for their intended purpose. Much later they were used sucessfully in lens investigations

(16.25). Also in December, field preparations were started for taking electronic records of objects imploded in a magnetic field. The first shot of this type, the "magnetic method," was fired January 4, 1944, and the results were encouraging. The magnetic method was designed to take advantage of the fact that the motion of metal in a magnetic field alters the field. Thus the inward motion of imploding metal would induce a current in a surrounding coil, and the proper interpretation of this current would give information on the velocity and other characteristics of the implosion. Considerable perfection of the electronic records was needed, however, and this held up final proof of the method until spring.

7.58 Quantitative data from the X-ray, high explosive flash, and rotating prism camera techniques showed the usefulness of these methods for determining velocities and symmetry at small scale, but also indicated a necessity for controlled quality of high explosive castings and boostering systems, as well as improved simultaneity of detonation. Programs were instituted for the improvement of these services; this involved the production of castings of uniform density and composition, and the institution of quality control, including X-ray examination and density measurement of charges. In view of the impending large scale production of heavy charges, development work was also undertaken on methods of casting and examining such charges for controlled quality.

7.59 Whereas the original Anchor Ranch Range had been designed to accommodate both the gun and implosion programs, the expansion of the latter soon crowded the Anchor Ranch facilities. In particular the casting and detonation of large charges required a large casting plant and several widely separated test sites. The largest of these units, the casting plant, was begun in the winter of 1943. It included an office building, steam plant, a casting house, facilities for trimming and shaping high explosive castings, and magazines for storage of high explosive and finished castings.

7.60 This S Site (sawmill site) was one of the most difficult undertakings of the Laboratory from an administrative point of view. To find men with experience in high explosive casting work, or even with general experience in handling explosives, proved for the most part impossible. Supervisory personnel were equally difficult to obtain. Almost the only available channel was the army; the S Site Group was staffed almost entirely by men in the SED. Among these a few had appropriate industrial backgrounds, a larger number were young soldiers with some scientific training, usually in chemistry, and the rest were relatively unskilled hands. Originally scheduled for completion in February and full operation in April 1944, steady operation on a reasonable scale did not actually get under way until August. Because of increasing demand and the unavoidable lag in S Site expansion, it was early in 1945 before the small original Anchor Ranch casting room was fully replaced as a source of supply for experimental charges.

7.61 The first of the new experimental methods to be successfully adapted to work at larger scale was the high explosive flash technique, which was used not only with cylinders but also with hemispheres. In the early months of 1944 attention was being given to extending flash X-ray methods to larger scale and using a grid of small ion chambers instead of photographic recording. This work was, however, only begun in the fall (15.17 ff). The "RaLa" method, that of including a strong source of gamma radiation in the imploding sphere and measuring the transmitted intensity as a function of time, was being discussed, and active development was gotten underway, including electronic instrumentation and preparation for handling the highly radioactive radio-lanthanum (RaLa) to be used. The possible use of the betatron to produce penetrating radiation for work with large spheres was discussed at this time; but it was not decided to use the technique until the beginning of the second period (15.23 ff). Lastly, the possibility of making a full-scale, active test in a closed vessel was also being considered and model tests were started and procurement possibilities investigated. Use of such a containing vessel would permit recovery of active material in case of a complete fizzle. This later grew into the "Jumbo" program which, together with other engineering problems, was centralized under the Ordnance Instrumentation Group and later under a special group devoted to this program (16.32 ff).

7.62 Preparations for an implosion test with active material were begun in March 1944. The main problems were (1) the choice of a test site, (2) the investigation of methods to permit recovery of active material in case of failure of the nuclear explosion, and (3) the design of instrumentation to measure blast effects and nuclear effects of a successful explosion. Discussion of the third topic is referred to Chapter XVIII. Sites considered were in New Mexico, Colorado, Arizona, Utah, and California, as well as several island sites off the coasts of California and Texas. In making the choice the advantage of nearness to Los Alamos had to be weighed against possible biological effects even in sparsely populated areas of the Southwest. During the period before August 1944, a good deal of exploration was by map, automobile, and plane, and still more was planned. Investigation was begun, finally, of several aspects of the recovery problem. Recovery from a large containing vessel strong enough to withstand the shock of the high explosive alone was the principal means considered. Also investigated was the possibility of setting off the bomb inside a large sand pile which would prevent dispersal and permit recovery of active material in case of failure. Other methods were investigated, but during the period in question the main problem was that of designing "Jumbo," the containing vessel.

7.63 The main activities of the spring and early summer months may be summarized as follows: (1) the preliminary development of new methods, such as RaLa, the magnetic method, the counter X-ray method, which should be useful with larger scale implosions; (2) increasing production and quality control of cast explosives and detonation trains; (3) increased investigation of questions of simultaneity in detonation, including the preliminary investigation of electrical detonation systems; and (4) the exploitation of techniques established during the winter for studying the implosion. Implosion studies only reached the stage of giving regular and reliable results during the summer of 1944; being concerned with end results, they were the object of great attention from the rest of the Laboratory and particularly from the Theoretical Division. In fact, this early work laid the foundation of a new branch of dynamics, the physics of implosion. It had been firmly established that the earlier results on implosion velocities were essentially correct, and considerably lower than theory predicted for normal impact by a detonation wave. The dynamics of confluent materials had also been thoroughly investigated.

7.64 In July 1944 the Laboratory faced the fact that the gun method could not be used for the assembly of plutonium. Hence at that time there was not a single experimental result that gave good reason to believe that a plutonium bomb could be made at all. There was, however, a large investment in plants and proving grounds and a wide background of experience in improving explosives and timing, which made it possible to launch an even more ambitious investigation of the implosion. The new development was to be centered on the possible use of explosive "lenses" which could be designed to convert a multiple point detonation into a converging spherical detonation wave and thus eliminate the troublesome interaction lines. Preliminary studies of such systems had been made in England and at Bruceton, and the work of adapting them to the implosion problem became the principal objective of the implosion groups. The requirement for experimental lens-mold design was the most difficult initial step and this occupied some months. Meanwhile, the effort to eliminate the interaction jets from nonlens implosion continued.

7.65 The decision to go into the study and use of explosive lenses entailed expansion of research. Furthermore, the eventual production of these special explosive systems had to be provided for. To be prepared for the use of the first quantities of plutonium all this had to be accomplished well within a year. The major portion of the Laboratory was accordingly reorganized so as to concentrate manpower and facilities on the implosion problem.

7.66 Division X was formed, under Kistiakowsky, for the purpose of experimentation with explosive systems and their method of fabrication and for setting up an adequate production system for all special charges (9.1 ff, Ch. XV). Also under Division X were put the more or less established methods of implosion investigation, such as the small scale X-ray and the high explosive flash methods. The development of new or as yet unproved techniques for the investigation of implosion dynamics, and the responsibility for design development of the active core of the implosion was made the objective of G Division under Bacher (9.1 ff, Ch. XV). The urgency of the directives of these divisions is readily appreciated when it is considered that there was no approved design of either explosive or mechanical systems at the time of the reorganization, August 14, 1944.

Delivery

7.67 The work of the Delivery Group covered everything from the completed bomb (gun or implosion model) to its final use as a practical airborne military weapon. In the nature of the case there was considerable overlapping between its responsibilities and those of the groups engaged in final bomb-design; particularly with the Fuse Development Group (7.33 ff), the Engineering Group (7.40 ff), and other groups responsible for bomb design. Even in these cases, however, it was the especial responsibility of the Delivery Group to see that cooperating groups functioned smoothly together as a team with an eye to their eventual collaboration in combat delivery. In addition to this responsibility and to its own proper functions in design and procurement, the Delivery Group was responsible for liaison with Air Forces activities including the choice and modification of aircraft and the supervision of field tests with dummy bombs. In the second part of this history it will be seen that the activities of the Delivery Group (then expanded into what was called Project A) were extended to include the planning and establishment of the advance base where the bombs were assembled, the assembly and loading of the bombs, and the testing and arming of the bombs in flight (Chapter XIX).

7.68 The first activities of the delivery program began in June 1943 when N. F. Ramsey (then still working with the Air Forces) undertook to investigate available planes with respect to their bomb-carrying capacity. At this time the main possible weapon was the plutonium 3000 feet per second gun model with an over-all length of 17 feet. The only plane which would fit this requirement was the B-29, which even so could carry the bomb only by joining the bomb bays. The possibility of wing-carriage by other planes had been considered and rejected. At this time it appeared that there might be considerable difficulty in obtaining a test B-29. Another plane capable of carrying the bomb was the British Lancaster, and some investigation was made of the possibility of using this plane. In terms of standardization of maintenance, however, this plane would have been difficult to operate from American bases; it was therefore decided that the B-29 would have to be used.

7.69 Preliminary ballistic tests were made in August 1943 at the Naval Proving Ground at Dahlgren, ostensibly for the Air Corps. The dummies dropped were scale models (14/23) of the "Long Thin Man," the 3000 feet per second plutonium gun referred to above. These models consisted of a long 14" pipe welded into the middle of a split standard 500 pound bomb. They showed, on testing, extremely bad flight characteristics. In subsequent months further tests of scale models were made at Dahlgren, and models were developed which had much better flight characteristics. During this period preliminary models of a proximity fuse developed at the University of Michigan (7.36) were also tested.

7.70 On the occasion of Ramsey's first visit to Los Alamos in September 1943, implosion was just being urged by von Neumann. From this model a preliminary estimate was made of a 9000 pound bomb with a diameter of 59 inches. On the basis of these estimates the Bureau of Standards bomb group was asked, through the Bureau of Ordnance, to have wind-tunnel tests made to determine the proper fairing and stabilizing fins for such a bomb.

7.71 In the fall of 1943 plans for full scale tests were gotten under way. For the purpose of B-29 modification, two external shapes and weights were selected as representative of current plans at Los Alamos. These were, respectively, 204 and 111 inches long, and 23 and 59 inches in diameter, the "Thin Man" (gun) and "Fat Man" (implosion). In November 1943 Ramsey and General Groves met with Colonel R. C. Wilson of the Army Air Forces, and plans were discussed for the first modified B-29. In December the first full scale models were ordered through the Detroit Office, and Ramsey and Capt. Parsons visited the Muroc Airbase to make the necessary test station plans.

7.72 Tests were begun at Muroc early in March 1944. The purpose of these tests was to determine the suitability of the fusing equipment, the stability and ballistic characteristics of the bombs, and the functioning of the aircraft and bomb release mechanism. The flight characteristics of the Thin Man model proved stable, while those of the Fat Man were under-damped, which caused a violent yaw and rotation. When the fuses were tested, the

Michigan proximity fuses (7.36), failed almost completely. The release mechanism proved inadequate for the Thin Man. Four models "hung up" with delays of several seconds. The last model tested released itself prematurely while the plane was still climbing for altitude. This bomb dropped on to the bomb bay doors, which had to be opened to release the bomb and were seriously damaged. This accident ended the test pending repair of the plane and revision of the bomb release mechanism.

7.73 Tests at Muroc were not resumed until June, pending plane repair and modification. The intervening time at Los Alamos was devoted to a number of activities: the analysis of the first Muroc data, the planning of a functional mock-up of the plane and bomb-suspension for handling, loading, shaking, and cold tests, and construction of a site (V Site) for this work; investigation of the need and possibility of heating equipment for the B-29 bomb bay.

7.74 In addition, effort was devoted to the design and procurement of 23/59 scale Fat Man models for possible B-24 flight tests. Negotiations were started to obtain the use of the high-velocity Moffett Wind Tunnel for bomb ballistic experiments on the Fat Man. Although stable and statistically reliable Fat Man models were subsequently designed without this, only ballistic experiments under controlled conditions would have yielded definite information on the safety factors involved in these models. The necessary arrangements for this testing program proved difficult to make, and it was in the end deemed unnecessary.

7.75 During this period also two new bomb models were designed. One was the case for the 1000 feet per second U^{235} gun assembly, which by contrast with the much larger Thin Man was given the code name "Little Boy." The length of this model was so reduced that it could be carried in a single B-29 bomb bay. When in midsummer the plutonium gun assembly was abandoned, it became unnecessary to join the bomb bays of the plane. The second model was the "1222" Fat Man model. This consisted of twelve pentagonal sections of dural bolted together to form a sphere, and surrounded by an armor steel shell of icosahedral structure, with stabilizing shell attached. Mechanical assembly of this device required the insertion of some 1500 bolts.

7.76 The chief contribution of the Muroc tests in June was that although the Fat Man model tested was still unsatisfactory in its flight characteristics, field modifications, resulting from a suggestion by Capt. David Semple, USAAF (dcd), to increase the drag, gave a stable model. This modification involved the welding of angularly disposed trapezoidal drag-plates into the box tail of the bomb. No release failures occurred, and the fusing mechanisms tested proved to have great promise.

7.77 In the period between these tests and the next held in October, design, procurement and testing work continued at Los Alamos. When by midsummer it became certain that the plutonium gun assembly would not be used, the remaining models were the Little Boy and the Fat Man. A new model Fat Man was developed in this period, to improve flight characteristics and simplify mechanical assembly. This was the "1561." It consisted of a spherical shell made up of two polar caps and five equatorial zone segments, machined from dural castings. The assembled sphere was enveloped by an ellipsoidal shell of armor attached at the equator. The tail was bolted to the ellipsoid. The electrical detonating and fusing equipment was mounted on the sphere in the space between the sphere and the outer ellipsoid.

Chapter VIII

CHEMISTRY AND METALLURGY

Introduction

8.1 The basic problems of the Chemistry and Metallurgy Division were the purification and fabrication of active, tamper, and initiator materials of the bomb. These problems ramified in many directions, and to the ramifications were added a number of activities of service to the rest of the Laboratory. In relation to the rest of the Laboratory the activities of the Division were largely determined rather than determining. This was true not because the work was routine or subordinate, but because it was successful. The record of the chemists and metallurgists at Los Alamos is one of wide-ranging exploration of techniques combined with extraordinary cleverness in meeting or avoiding technical problems, sometimes on short notice.

8.2 Prior to April 1944 the Chemistry and Metallurgy Division had only a loose group structure, with groups designated as Purification, Radiochemistry, Analysis and Metallurgy, headed respectively by C. S. Garner, R. W. Dodson, S. I. Wiessman, and C. S. Smith. At that time the administration of the division was extensively reorganized. J. W. Kennedy, who had served from the beginning as Acting Division Leader, became Division Leader. C. S. Smith became Associate Division Leader in charge of metallurgy. The group subdivision was as follows:

CM-1	Health and Safety, Special Services	R. H. Dunlap
CM-2	Heat Treating and Metallography	F. Stroke
CM-3	Gas Tamper and Gas Liquefaction	E. A. Long
CM-4	Radiochemistry	R. W. Dodson
CM-5	Uranium and Plutonium Purification	C. S. Garner
CM-6	High Vacuum Research	S. I. Weissman
CM-7	Miscellaneous Metallurgy	C. C. Balke

CM-8	Uranium and Plutonium Metallurgy	E. R. Jette
CM-9	Analysis	H. A. Potratz
CM-10	Recovery	R. B. Duffield

In June 1944 Group CM-11 was formed under A. U. Seybolt and was concerned with carrying on previous work on problems of uranium metallurgy.

8.3 It was stated earlier that the program of the Division could not be defined completely until the division of labor between Los Alamos and other Manhattan laboratories was decided. The metallurgy program, however, was clear from the beginning, as was the necessity for setting up analytical methods for refereeing all questions of chemical purity, whether purification occurred here or at some other Laboratory. In addition there were several special service functions, such as the preparation of thin film targets of various materials for the experimental physicists, the purification of thorium for threshold fission detectors, and the fabrication of metal parts for apparatus and experimental work to be used by other groups.

8.4 The recommendation of the special reviewing committee (1.86) had favored the location of purification work at Los Alamos. In May 1943 this recommendation was adopted and the necessary planning undertaken. The headquarters of the purification work would be at Los Alamos, and the necessary facilities would be built there, including a large dust-free laboratory building. The plan was that after this building was completed and an adequate staff was on hand, a major part of the purification research and later all of the final purification would be done at Los Alamos. In the meantime this research would be carried out at the Metallurgical Laboratory, at the University of California at Berkeley, and at Iowa State College. In order to maintain the advantages of Los Alamos control and responsibility for purification and yet minimize the expansion which might be required by reason of such a program, it was evident that a coordinator would have to be found to establish the proper lines of demarcation between the work of this site and the others involved. Late in May 1943, C. A. Thomas, Research Director of Monsanto Chemical Company, visited Los Alamos to consider the requirements and the position of coordinator.

8.5 At the end of July, Thomas accepted the position. His job was not one of coordinating the research programs of the various projects but simply one of establishing communication between otherwise isolated laboratories and adjudicating their conflicting requirements for scarce materials. At about this time the planned new building was designed by Brazier with the advice of Thomas and members of his staff, and erected. In spite of the fact that this building was constructed of the same temporary materials as other Los Alamos buildings, it was remarkable in that it embodied the features of being both dustproof and air-conditioned. It was largely completed and staff members were moving in by December 1943.

8.6 Immediately upon undertaking his duties, Thomas set up a program for the extraction of polonium, either from lead dioxide residues that had been located or from bismuth which could be irradiated in the piles at Clinton or Hanford. Research on the former problem was undertaken at the Monsanto Laboratories and on the latter at Berkeley.

8.7 As already noted, a division of labor in many problems continued under Thomas' direction. For example, in the case of the investigation of plutonium chemistry as distinguished from purification proper, a Berkeley group provided information on the oxidation and valence states of plutonium, while the earliest reports on density and crystal structure of the metal came from the Metallurgical Laboratory. It might be noted, relative to the last mentioned work, that the measurements at the Metallurgical Laboratory were made before it was definitely established by investigations conducted at Los Alamos that there was more than one allotropic form of the metal (8.38). However, it was suggested in February of 1944 that the difference in structure in barium- and calcium-reduced plutonium, reported by Chicago workers, might be caused by the existence of at least two such forms.

8.8 Further instances of co-extensive programs at various sites occurred in the work of the bomb method of plutonium reduction (8.41-8.43) by both the Metallurgical Laboratory and the Los Alamos group, although the work at the former was only on a small scale. The simultaneous development was undertaken at these two laboratories of methods of spectrographic analysis for many elements, in particular the cupferron-chloroform extraction method with copper spark analysis (8.76). As to the latter, work on the method continued at Chicago with the final development being done at Los Alamos.

8.9 Thomas further arranged in the course of the liaison work that the Metallurgical Laboratory should be primarily responsible for the procurement of two groups of materials for the entire project, reagents of much higher purity than those commercially obtainable and refractories for use by the many metallurgical groups. The problem of securing an adequate supply of satisfactory refractories became increasingly important with the expansion of work by the Los Alamos metallurgists. These difficulties had been magnified by the fact that initial arrangements for procurement were not satisfactory. Under Thomas' auspices, however, arrangements for the development and production of these refractories were initiated in January 1944, and it was eventually decided that a group under F. H. Norton at the Massachusetts Institute of Technology was to undertake the research problems involved. The technical problems considered will be discussed later (8.52). It should be noted that arrangements were also made about this time to carry out research on the use of cerium sulfide, principally at the University of California. Cerium metal was produced at the Iowa State College, with the bulk of the output being sent to M.I.T. Some subsidiary work was also done at Brown University.

8.10 Despite the most careful liaison efforts, work by the Los Alamos metallurgists was sometimes delayed because of the time lag between changes in requirements for refractories and corresponding changes in the output by the fabrication groups at other sites. In order to overcome this time lag, the local refractory research group was enlarged during April 1944, and production of standard refractories undertaken. Subsequently, at a meeting of the chemistry and metallurgy groups at Chicago in June 1944, it was decided to send the production of Berkeley, Ames, and M.I.T. to Los Alamos in an effort to meet the sharp rise in demand for refractories there. Despite all these efforts the problem of procuring a sufficient number of the proper types of refractories continued throughout the period covered by this report.

8.11 With the discovery of Pu^{240}, there was no further need for coordination of purification work. The discovery came at a time when it had become clear that the chemical purification of Pu^{239} could be accomplished, although still with great difficulty. The division of labor between the various sites, moreover, was at that time well worked out.

8.12 The chemistry of U^{235}, and its attendant liaison, presented much simpler questions than plutonium. There were two main problems to be examined by workers at Los Alamos: The processing of the tetrafluoride for experimental work in the laboratory and for the production of weapons; and problems concerning the Water Boiler, such as the decontamination of solutions. The purification of U^{235} to the tolerance limits specified by the Los Alamos Laboratory was undertaken by Tennessee Eastman at Oak Ridge. Los Alamos chemists were interested in knowing the processing which the material had undergone before shipment and the nature of the analysis done at Oak Ridge. They also specified the chemical form in which the material was to be shipped, for example, as the sulfate, nitrate, or tetrafluoride. Other questions which arose were connected with isotopic concentration, mixing of lots with different concentrations, methods of assay and the like. One special item of liaison was the cooperation between Los Alamos and the Clinton Laboratories at Oak Ridge on the production of radiobarium-radiolanthanum for the implosion studies (17.42). In the course of the work in connection with the Water Boiler and particularly the decontamination of

Water Boiler solutions, Los Alamos chemists leaned heavily on the corrosion experts at the Metallurgical Laboratory and at Clinton, while DuPont was of material assistance in obtaining stainless steel for the apparatus.

8.13 The scheduling of work to be done at the Los Alamos laboratories, and particularly the concentration of purification work at this Laboratory, involved a necessary growth of personnel. From a group of about twenty in June 1943, the Chemistry and Metallurgy Division grew until at its peak in 1945 it employed about 400 staff members and technicians. Progress was slow and the procurement of personnel difficult because many of the most suitable men were employed in other branches of the project. In the absence of an over-all supervisor whose decision as to the allocation of these men would be binding, the difficulties became almost insurmountable. The inadequacy of the metallurgical staff was particularly serious since metallurgical work for ordnance experimentation could not be done elsewhere.

8.14 From the completion of the chemistry building in December 1943 to April 1944, about twenty men came to Los Alamos from Berkeley, Chicago, and Ames where they had been doing research on the purification problem. In the early fall a group of four men came from California Institute of Technology after the completion of an unrelated project there. These additions, together with the results of intensive efforts to recruit qualified personnel through Army facilities, helped carry the division past the crucial stages of its growth. The history of the Chemistry and Metallurgy Division as developed in the following sections is set forth under the following headings: Uranium Purification, Uranium Metallurgy, Plutonium Purification, Plutonium Metallurgy, Miscellaneous Metallurgy, Radiochemistry and Analysis Work.

Uranium Purification

8.15 Since in terms of the gun assembly method for producing a large scale explosion the purity requirements for U^{235} were three orders of magnitude less exacting than for plutonium, it was the general policy of the chemists to concentrate their efforts on the more difficult of the two problems. For this reason, and because some work had been done prior to the project, relatively little work on uranium purification was done in the first months of the Laboratory's existence. Furthermore, it seemed entirely possible that a purification procedure for uranium might be merely a by-product of that for plutonium, since a complete investigation of the chemistry of the latter had not yet been effected.

8.16 It was, in fact, primarily the role of uranium as a stand-in for plutonium that was responsible for the first work in uranium purification. During the first half of December 1943, it was decided to curtail, if not completely eliminate, the very exacting microchemical investigations of plutonium purification then going on. This decision was based on the prospect of gram amounts of plutonium from the Clinton pile within two or three months. Under the circumstances, it was believed that microchemical experience with such a stand-in as uranium would be more useful. This work was carried out by the Uranium and Plutonium Purification Group in cooperation with the metallurgists, and aimed at plutonium - rather than uranium - standards of purity.

8.17 Several methods of uranium purification were investigated during the course of the stand-in work. These methods all entailed a series of "wet" and "dry" chemistry steps. For example, the originally adopted procedure provided a carbonate precipitation (with ammonium carbonate), a diuranate precipitation (with amonium hydroxide) and a +6 oxalate precipitation as the "wet" purification steps. Igniting the resulting uranyl oxalate to the oxide U_3O_8, reduction to UO_2 with hydrogen, and conversion to the tetrafluoride by heating in the presence of hydrogen fluoride constituted the "dry" part of the process.

8.18 This basic procedure underwent extended investigation devoted chiefly to variation in the conditions and parameters by the Purification Group (CM-5) until about August 1944. However, during the early months of the same year the Radiochemistry Group (CM-4) also engaged in uranium purification research in the course of its work in supplying the physicists with enriched uranium in small quantities for isotopic analysis. The wet purification procedure outlined above was departed from in many respects, such as the employment of a peroxide precipitation step, precipitation of the acetone-sulphate complex of uranium, electrolytic methods, and the +4 oxalate precipitation. The success of these variations was overshadowed by considerations of large scale production. Thus, for example, although the peroxide precipitation step yielded excellent results, the bulkiness of the precipitate militated against the employment of the step in large scale operations. On the other hand, the ether extraction of uranyl nitrate plus nitric acid, also studied in the course of this work, later came into extensive use.

Uranium Metallurgy

HYDRIDES

8.19 After the formation of the Uranium and Plutonium Metallurgy Group in April 1944, the work described below was done primarily in that group, and was placed in a separate group in June 1944. The first work in uranium metallurgy at Los Alamos was the preparation and powder metallurgy of its hydride. This compound had been successfully produced on the project by Spedding's group at Ames, and the existence of the possibility of large scale, controlled production was learned of at Los Alamos in April 1943. The employment of the hydride in a bomb was still being seriously considered (4.14). Consequently, metallurgical investigations concerning uranium hydride were in order. The early literature identified the compound as UH_4 but primary work in the formation of the hydride indicated that UH_3 was closer to the true formula. That this was so was verified independently by the chemists.

8.20 The metallurgical work was modified by bomb requirements with the result that methods of producing hydride in high density form and the elimination of the pyrophoric characteristic became important problems. Compacting of the hydride by cold pressing and hot pressing methods was attempted as well as the possibility of hydride formation under high pressures applied externally to the massive material being treated. This work generally led to the establishment of many control factors in the hydride formation process.

8.21 The work on the pressure bomb method of producing high density hydride compacts was curtailed when success was achieved with the formation of uranium-plastic compacts. The research on the latter began during February 1944, the objectives being to prepare compacts in desired geometric shapes in which the hydrogen-to-uranium ratio varied. This feature could readily be accomplished by the employment of uranium powder and a suitable hydrogenous binding agent. It was also possible largely to eliminate the employment of the hydride and thus reduce the number of fires. In the early days of this work, a half doxen small fires a week were not unusual. The plastic bonding agents employed, among others, were methyl methacrylate, polyethylene and polystyrene. Compacts were thus made with uranium-hydrogen compositions corresponding to UH_3, UH_4, UH_6, UH_{10} and UH_{30} which were used for various experiments by the physicists.

URANIUM REDUCTION

8.22 The problem of preparing uranium metal of high purity was undertaken with two objectives in mind. The objectives of the problem were: (a) the development of small-scale methods (0.5 to 1000 grams of metal) for the preparation of uranium metal of high purity which could be applied to enriched uranium metal when it becomes available; and (b) the use of uranium as a stand-in element for the development of reduction techniques which might be applied to the preparation of plutonium metal. Two general methods of metal preparation were investigated, the electrolytic process and the metallothermic process. The latter process was divided into two methods, the centrifuge method and the so-called stationary bomb method.

8.23 Electrolytic Process. The only successful electrolytic reduction process available for uranium at the time this work was initiated was the Westinghouse process which employed UF_4 and UO_2 dissolved in a fused mixture of sodium and calcium chlorides. The product produced by this process was a fine powder containing considerable oxide and, therefore, required washing, pressing, and melting for purification. These steps involve losses which are excessive considering the value of the metal to be produced (enriched uranium). Investigations here showed that uranium could be deposited above its melting point from solutions of UF_4 in fluorides and chlorides, but at high temperatures the purity was likely to be low. Accordingly, the most extensive investigations were limited to lower temperatures, and in order to simplify the container problem, to electrolytes containing no fluorides. Electrolytic methods were developed for the preparation of uranium metal on the 50 mg (of metal) scale and the 200 to 300 grams (of metal) scale. The electrolyte used consisted of 25 to 30 per cent uranium trichloride in a solvent containing 48 per cent $BaCl_2$, 31 per cent KCl, and 21 per cent by weight NaCl. The operating temperature was ~630°C. The high purity metal produced in both scales was in the form of dendrites which could be pressed and melted into one coherent piece. The recovery yields for the small scale method were 40 to 70 per cent and for the larger scale were 80 to 90 per cent.

8.24 Metallothermic Reduction Methods

Centrifuge Method. The purpose of this method was to reduce uranium metal on the small scale (50 mg to 1 gram of metal) by taking advantage of of the increased g-value for the collection of the small amounts of metal. The method consisted of reducing a uranium halide with either Ca or Li metal in a sealed bomb. The bomb was placed in a graphite rotor which was rotated while being heated in an induction coil. Successful reductions were made using the following mixtures: (1) UF_4 + Ca + I_2, (2) UF_4 + Li, (3) UF_3 + Li, (4) UCl_3 + Li, Ca, or Ba. The metal produced in (1) was brittle and contained considerable amounts of entrapped slag. The metal produced using the other mixtures was malleable but usually contained some entrapped slag which decreased the purity of the metal. Very good yields were obtained in all cases.

Stationary Bomb Method. At the time this problem was started (August 1943) only the large-scale (25 pounds of metal) reduction technique as developed at Iowa State College and the possible use of iodine as a booster were known. This large-scale method was not applicable to small-scale work where high yields and high purity were needed. The problem here involved the development of refractory crucibles for the reaction, the design of suitable bombs, the investigation of raw materials for the reaction, and the development of techniques for each scale of reduction studied. Methods of handling the very valuable enriched uranium without danger of loss were also worked out. Successful bomb techniques were developed for the reduction of uranium tetrafluoride and uranium trichloride with calcium metal on the 0.5, 1, 10, 25, 250, 500, and 1000 gram (of metal) scales. Most of the work was done on the tetrafluoride because of the hygroscopic nature and the more difficult preparation of the trichloride. Experiments on the 10 gram scale also showed that UI_4 could be reduced with calcium metal using the same procedures as were used for the fluoride and chloride. Argon was used as an inert atmosphere in the bomb. The amount of iodine used and the heating cycles varied with each scale of reduction. It was found that magnesium oxide crucibles were the most satisfactory. Methods for the preparation of the several types of MgO crucibles were developed, the methods later being used by MIT for the routine preparation of the large scale crucibles.

URANIUM ALLOYS

8.25 Alloys were sought which would have better physical properties for fabrication than the unalloyed metal. Beginning in November 1943, an intensive program was undertaken on the preparation of uranium alloys in various percentage compositions. Mixtures of uranium with molybdenum, zirconium, columbium and rhenium were obtained which indicated that many desirable properties could be produced in such alloys. In particular, extended investigation of the uranium-molybdenum system showed that it had a much higher yield strength than pure uranium. The emphasis on this work, however, was not maintained and in the fall of 1944 most of the alloy research was dropped, as ordinary uranium was found to have adequate physical properties.

Plutonium Purification

8.26 The plutonium purification procedure, as distinguished from the more general chemistry of plutonium, was primarily the work of the Uranium and Plutonium Purification Group at Los Alamos. Early in October 1943, the first small quantities of plutonium arrived at the Laboratory and shortly thereafter intensive work on stand-ins was initiated to permit the members of the Purification Group to determine and improve their techniques. The stand-ins used included uranium, cerium, lanthanum, zirconium and thorium. Uranium was used principally in the investigation of ether extraction methods; thorium and cerium were employed to test solubilities and purification by various precipitations. In metallurgical work, cerium trichloride was used as a stand-in for plutonium trichloride and in other small scale work cerium tetrafluoride was used as a stand-in for the corresponding plutonium salt.

8.27 The development of plutonium purification procedures is readily divisible into three parts: wet processing, dry processing and, by reason of the cost of the material, a recovery process. The intensity with which the division attacked the difficult problem of plutonium chemistry was rewarded by the complete development in the first period of the wet chemistry procedure finally used in large scale work. By August 1944, however, the dry process and recovery procedures had not been completely determined. The former was still in the formative stage of development and the latter, while satisfactory, was greatly simplified by later research (17.18 to 17.21).

THE WET PROCESS

8.28 The plutonium output from Clinton and later from Hanford was received at Los Alamos as a highly viscous mixture of decontaminated and partially purified nitrates. These nitrates consisted of about 50% +4 plutonium and 50% +6 plutonium. This material had to be dissolved out of its stainless steel shipping container, diluted, and a sample removed for radioassay purposes. This preliminary work generally required between three and four days and when completed permitted further processing by the wet purification steps. A wide variety of purification procedures were investigated. Early in 1944 the first tentative procedure involved a double sodium plutonyl acetate precipitation followed by a double ether extraction employing sodium nitrate as a salting-out agent. Potassium dichromate was orignally used to go from +4 to +6 plutonium ion, but this was soon superseded by sodium bromate plus nitric acid. For selective reduction from +6 to +3 plutonium, hydrogen iodide or potassium iodide in acid solution were used throughout the work.

8.29 The first major difficulty encountered was the need of a process to separate small amounts of uranium impurity from large amounts of plutonium. Various compounds such as carbonate, peroxide, fluoride, iodate, and oxalate were investigated. The iodate $Pu(IO_3)_4$ was found to give 99.5 per cent removal in two precipitations with a selective reduction step, but it proved extremely difficult to convert this compound to sodium plutonyl acetate. Finally, the precipitation of the +3 oxalate provided the solution to the problem and became an important part of all future processes.

8.30 The procedure as outlined gradually changed. Two oxalate precipitation steps were incorporated. An ether extraction and a plutonyl acetate step were dropped. By July 1944, completely enclosed 1-gram and 8-gram apparatus was being set up, and the process known as the "A" process had taken form. This involved reduction to +3 oxidation state, oxalate precipitation, oxidation to +6 oxidation state, sodium plutonyl acetate precipitation, ether extraction from nitric acid and ammonium nitrate solution, reduction to +3 oxidation state, and a final oxalate precipitation. The process gave yields of about 95 per cent. The product was then turned over to the dry chemists as an oxalate slurry. The residue supernatants were returned for recovery.

8.31 The reason for the development of enclosed apparatus was primarily the plutonium health hazard (9.30).

THE DRY PROCESS

8.32 Since the conversion of the wet oxalates to the dry halide of plutonium led to a product which the metallurgists had to reduce to metal, there was collaboration between these groups in the attempt to settle on a suitable compound for reduction. The tetrafluoride was decided upon in July 1944. The preceding investigations covered the range of most of the halides of plutonium (for calcium bomb reduction) and PuO_2 (for carbon reduction). Plutonous chloride and bromide were rejected because they were highly hygroscopic. The carbon reduction of the oxide was also dropped.

8.33 The preparation of the tetrafluoride underwent continual development and improvement during the course of the work of the Laboratory. It was prepared variously from the nitrate, oxalate, and oxide by the use of anhydrous hydrogen fluoride. The conversion of the nitrate was poor, and research concentrated on the conversion of the oxalate and oxide. The final choice of the oxide occurred early in 1945, as did the final development of production methods (17.21).

RECOVERY OF PLUTONIUM

8.34 Except for the peroxide recovery method (17.22) all procedures were developed by the Recovery Group before August 1944. Recovery was necessary from the supernatants of plutonium purification and from liners and slags of the metallurgists. From the supernatants the procedure involved concentration of the plutonium with subsequent purification. Reduction was made with sulphur dioxide followed by a precipitation with sodium hydroxide. Treatment with aluminum hydroxide as carrier brought down further amounts of plutonium. About 1 mg per liter remained in solution, and these secondary supernatants were stored. Purification steps originally involved iodide reduction oxalate precipitation, oxidation ether extraction, sodium plutonyl acetate precipitation, iodide reduction, and a final oxalate precipitation. After these steps had been carried out, the purified product went directly to the dry chemists.

8.35 Work on liner and slag recovery showed quite early that complete solution would be necessary for good recovery. The major difficulty was to remove iodine and iodide ion before solution. The first method developed was one of CCl_4 or sodium sulfite extraction for the I_2. Following this, liner and slag were dissolved in hydrochloric or nitric acid, followed by a precipitation of $Pu(OH)_4$ from a solution almost saturated with ammonium nitrate. This precipitation was carried out at a high enough hydrogen ion concentration to leave most of the magnesium (from the magnesia liner) in solution.

Plutonium Metallurgy

8.36 In March 1944 the first bomb reductions of PuF_4 and $PuCl_3$ were undertaken by the Uranium and Plutonium Metallurgy Group. This was the first direct metallurgical work with plutonium, although previous work had been done with stand-ins (8.22). Emphasis was placed at this time on the electrolytic method and chloride reduction.

8.37 Research on the physical properties of plutonium metal began in April 1944 and was to prove of major importance because of the unique physical properties of the metal. By May 1944 metal yields were over 80 per cent by a number of methods, and the shift of interest to the stationary bomb reduction method began. Extensive work on remelting, as a final purification step, centered interest on the use of refractories which would not contain light element contaminants. This eliminated the usual refractory materials. One of the principal refractory materials investigated in these studies was cerium sulfide (8.52).

8.38 Discrepancies found in the density of various metal samples produced the first hint of the existence of plutonium allotropes. By June, metal obtained from $PuCl_3$ using calcium as a reducing agent and subsequent remelting in cerium sulfide crucibles came within a factor of 10 in meeting the prevailing purity specifications. The alpha and beta allotropes of plutonium were definitely established at this time, and the PuO_2 plus carbon reduction method developed in the High Vacuum Research Group came into temporary prominence.

PLUTONIUM REDUCTION

8.39 As with uranium, two general methods of metal preparation were investigated; the electrolytic process and the metallothermic process. The latter process was again divided into two methods; the centrifuge method and the so-called stationary bomb method. An additional method was also studied in which the oxide of plutonium was reduced and the metal distilled. The process finally adopted for the preparation of uranium metal was the reduction of the tetrafluoride in the stationary bomb using calcium metal as the reductant with iodine as a booster. The reasons for this selection were the same as in the case of uranium (8.22).

8.40 Electrolytic Process. Investigations of the electrolytic reduction of plutonium on the 50 mg to 1 gram (of metal) scale gave recovery yields of ~50 per cent. The bath consisted of 24 per cent $PuCl_3$ in a solvent containing 48 per cent $BaCl_2$, 31 per cent KCl, and 21 per cent by weight NaCl. The metal obtained was in the form of droplets and usually contained small amounts of the cathode element. With the discovery of Pu^{240}, the work on the electrolytic process was stopped in favor of the metallothermic process. Details of the electrolytic process are given in LA-148.

8.41 Centrifuge Reduction Method. As in the case of uranium (8.24), the purpose of this method was to prepare plutonium metal on the small scale (50 mg-1 gram of metal) by taking advantage of the increased g value for the collection of the small amounts of metal. Successful reductions of plutonium were made using $PuCl_3$ or PuF_4 with Li as a reductant. Calcium reductions of these halides using iodine as a booster were not as successful. The plutonium metal prepared by the centrifuge method was the first plutonium metal prepared on any scale larger than a few micrograms. With the development of the 0.5 gram scale stationary bomb method for the preparation of plutonium, the centrifuge method was abandoned. However, the centrifuge served its purpose at a time when it was needed most.

8.42 Stationary Bomb Method. The work on this problem was started in March 1944; however, much preliminary work had already been done using uranium as a stand-in. As in the case of uranium, the problem of plutonium reduction involved the development of refractory crucibles for the reaction, the design of suitable bombs, the investigation of raw materials for the reaction, and the development of techniques of each scale of reduction studied. During the research on plutonium, cerium and lanthanum were also used as stand-in elements, and techniques were developed to prepare both metals from their chlorides and fluorides on all the scales given below. The chloride of plutonium was used for the first successful reductions of plutonium by the stationary bomb method. These reductions were done on the 0.5, 1, and 10 gram scale. The fluoride was then investigated and found to be more satisfactory because of its nonhygroscopic nature and greater ease of preparation. Successful techniques were developed for the reduction of plutonium on the 0.5, 1, 10, 25, 160, 320, and 480 gram scales. The average yields in a single button of clean metal ranged from 95 per cent for 0.5 gram, to 99 per cent for the 320 and 480 gram scales. The bromide was also reduced on the 1 gram scale, but with lower yields. The methods developed here from the reduction of PuF_4 are now used for the routine production of pure plutonium metal.

8.43 The oxide reduction method involved the reduction of plutonium oxide with carbon or silicon and distilling the resulting metal onto a cold finger. This method gave yields on a small scale (5 gram) of 30 to 90 per cent of spectroscopically pure plutonium. The discovery of Pu^{240} called a halt to this ultra-high purity method.

8.44 In addition to reduction techniques, remelting techniques were also investigated. A large part of this work was in the choice of crucible materials. Remelting was important because of the need for metal with uniform physical properties and because further impurities (e.g., magnesium) were removed in the process.

8.45 Extensive work was carried out by the Los Alamos metallurgists on the physical properties of plutonium metal. Early results yielded inconsistent data from measurements on different metal samples. In July 1944 these inconsistencies were partially explained by proof of the existence of alpha and beta allotropes, with a transition from the room-temperature alpha phase to the beta phase at between 100 to 150°C.

Miscellaneous Metallurgy

8.46 A good deal of outstanding metallurgical work was done at Los Alamos outside the narrow field of uranium and plutonium, principally by the Miscellaneous Metallurgy Group. Many of the jobs undertaken were more or less routine, but these routines had to be developed through the solution of difficult minor problems. The development of uranium hydride compacts has been discussed in connection with uranium metallurgy (8.19 to 8.21). Work on the compacting of boron neutron-absorbers was undertaken in August 1943. Development of beryllia tamper material for the Water Boiler was begun in the same month. The formation of high-density beryllia bricks for this became a production job in December 1943 and was completed in February 1944. In May, because of the difficulties encountered in obtaining refractories from other sources, magnesia liners and cerium sulfide crucibles were developed for the plutonium metallurgy program.

BORON COMPACTS

8.47 The remarkable properties of B^{10} as a neutron absorber gave this material several uses in the laboratory. The potential importance of B^{10} was such that its procurement was undertaken quite early, and studies were begun of means of compacting it. The oxide, the carbide, and the element were used as starting materials.

BERYLLIA COMPACTS

8.48 One of the accomplishments of the Miscellaneous Metallurgy Group was the development and production of beryllia bricks for the Water Boiler tamper and scattering experiments. Since these bricks were to be used as tampers, high density was desirable. Beryllium metal would have been the best tamper material. Use of the metal at that time, however, would have virtually exhausted the country's supply.

8.49 Various methods of obtaining high density were tried, among them impregnation with magnesium fluoride, but the fluoride was undesirable from a nuclear point of view. A method of impregnation with beryllium nitrate followed by ignition proved rather poor. The method finally chosen was a hot pressing technique, somewhat unusual for a refractory material.

8.50 Experimentally, the bricks were prepressed in a steel mold, then hot pressed in graphite at 1700°C at pressures in the neighborhood of 1000 pounds per square inch for 5 to 20 minutes. Fifty-three bricks were made for the Water Boiler tamper, shaped to fit around the 12-1/16 inch sphere of the boiler. For this production job the method was a variation of the method described above. The density averaged 2.76.

CRUCIBLE AND REFRACTORY RESEARCH

8.51 The purpose of this important work was to find materials for crucibles and liners which would not introduce contaminants into purified uranium and plutonium. Wetting, sticking, and thermal sensitivity had also to be considered. In this program a great many substances were investigated including cerium sulfides, calcium oxide, magnesium oxide, tantalum, graphite, a tantalum-thorium nitride mixture, zirconium nitride, thorium sulfides, beryllia, uranium nitride, thoria, tungsten carbide, tantalum carbide, titanium nitride, and many others. Cerium sulfide was one of the really hopeful materials found during this period and effort was concentrated on trying to improve the fabricated material's resistance to thermal shock, its main weakness.

MISCELLANEOUS SERVICE ACTIVITIES

8.52 The metallurgists prepared a great variety of materials for physics and ordnance experiments. These involved machining, heat treating, metallographic studies, casting of various metals, electroplating, miscellaneous plastic preparations, and powder metallurgy. Metallographic methods for uranium and plutonium studies were essentially new. This work was done mainly by the Heat Treating and Metallography Group, and the Miscellaneous Metallurgy Group.

Radiochemistry

8.53 Prior to August 1944 the work of the Radiochemistry Group fell into the following categories: foil preparation, boron trifluoride preparation, development of sensitive methods of neutron detection, the chemistry of initiators, the chemistry of the Water Boiler, and the planning of remote control methods for the handling of radio-lanthanum.

FOIL PREPARATION

8.54 The preparation of thin foils for physical experiments was a service activity but, as such, involved a great deal of arduous and delicate work and continued research on methods. Foils of a large number of different substances were made with emphasis on the oxides of uranium and plutonium. Among other substances were boron, protoactinium, uranium 233, neptunium 237, and thorium.

8.55 The principal methods of foil preparation used were evaporation, electrodeposition, and the "lacquer" method -- the last so-called because in it an alcohol metal salt solution is mixed with a nitrocellulose lacquer, spread in a thin film, and ignited to oxide.

8.56 Boric oxide foils were prepared by the lacquer method. Aluminum boride foils were prepared by heating aluminum foils in boron trifluoride. Boron was deposited on tantalum and tungsten foils by thermal decomposition of diborane. This work with boron was exceptionally difficult, requiring the production of very thin foils with accurately known mass. It was developed by Horace Russell, Jr. Deposits of the oxides of thorium, uranium, and plutonium were prepared by the lacquer technique, as well as by electrolytic methods.

8.57 The virtuosity of the chemists engaged in this work was remarkable. They turned out large numbers of foils that accurately met the physicists' specifications, including unusual geometries. In many cases the data supplied with the foils by the chemists were as important in interpreting physical experiments as any of the physical measurements made with them.

CHEMISTRY OF INITIATORS

8.58 It was assumed from the beginning that a neutron initiator would be used with the bomb to provide a strong neutron source that would operate at the instant of optimal assembly. Naturally, the first initiators were designed for the gun. The type of initiator, if any, to be used with the implosion was not settled until after August 1944. The principal mechanism adopted for the initiator was the properly timed mixing of alpha-radioactive material with a substance that would support the (α, n) reaction.

SENSITIVE COUNTERS

8.59 The radiochemists developed a neutron counter based upon the Szilard-Chalmers reaction. In this reaction a nucleus absorbs a neutron. It then loses energy by gamma emission and the recoil of the atom frees it from its chemical bonds. This dissociation permits the chemical separation of the reaction product and the measurement of its induced radioactivity. The sensitivity of this reaction as a neutron counter is high because it permits the absorption of neutrons in a large volume of material. Until early 1944 ethylene bromide was used as the basis of the procedure. At that time work began with potassium permanganate. A detection efficiency of about 10 per cent was eventually obtained.

8.60 Sensitive boron trifluoride counters were developed cooperatively by the Radiochemistry Group and the Radioactivity Group. It was the job of the radiochemists to prepare this substance in an extremely pure form. High purity of the product made possible effective operation at high boron trifluoride pressures. The first such counter, the "bucket chamber," had a 1 or 2 per cent efficiency. Later, the radiochemists developed another counter themselves (17.31).

WATER BOILER CHEMISTRY

8.61 Since the Water Boiler contained active material in aqueous solution, there were a number of chemical problems associated with the physical ones. The choice of a compound to use, the original purification of the material, the prevention of corrosion of the containing sphere, and methods of decontamination and analysis were the main matters requiring investigation.

8.62 In the original boiler it was decided to use uranyl sulfate for the following reasons: the solubility and solution density were higher than that of the nitrate; there was, moreover, some saving in critical mass because the neutron capture cross section is smaller for sulfur than for nitrogen.

8.63 Purity requirements were not strenuous except for two or three light elements. They were calculated by the rule that no impurity in the solution should absorb more slow neutrons than the sulfur in the sulfate.

8.64 Work on corrosion determined that stainless steels were suitable for boiler container and piping. The effects of working, welding, and annealing were studied and it was shown that weight loss dropped to zero after a few days time. Boiler parts were therefore pretreated with normal isotopic uranium sulfate solution and corrosion difficulties were substantially eliminated.

8.65 The hydrates of uranyl sulfate were investigated in order to predict volume changes from final additions of active material to the boiler "soup." A stable hemi-hydrate was found with less water than the normal precipitate from saturated solutions.

8.66 The refractive index of uranyl sulfate solutions was investigated to develop a rapid method of keeping track of amounts of uranium in solution. With monochromatic light through pure solutions, concentration would be measured to 0.1 per cent.

8.67 In the actual setting up of the boiler in May 1944, the chemists made all additions and removals of "soup," keeping accurate records of concentration by the refractive index and by the gravimetric analysis method. When the activity of the boiler reached the critical point, the concentration was measured by refractrimetry and checked by other methods. The control rod was calibrated by adding small weighed increments of sulfate and determining the critical setting of the control rod for each increment.

RADIOLANTHANUM

8.68 Before August 1944 no test shots were fired in the RaLa program (7.61). Design of a "mechanical chemist" for remote control work with this highly radioactive material began, however, as early as May 1944. By August the apparatus, at the Bayo Canyon site, was almost complete. Further account of this work is found in Chapter XVII.

Analytical Methods

8.69 The high purity analytical program was organized on the basis of theoretical considerations already reported (1.72). The plan to use plutonium originally demanded an extremely low rate of neutron emission, estimated to be 3 neutrons per minute per gram. Tolerance limits for each element were calculated by polonium alpha-particle bombardment of element targets and calculation of the neutron yield, thus obtaining the amount of element necessary to give 3 neutrons per minute.

8.70 The tolerance limits for light elements, not counting the rare earths, were found to be extremely low. In addition, effects are additive. It was generally agreed that the sensitivity of analytical methods should be one tenth of tolerance. Since early experimental production would be very small, and analytical samples might well be no larger than 1 milligram, it was evident that research on new submicro-analytical methods was necessary.

8.71 In general, high sensitivity was sought rather than high precision. The analytical chemist's greatest difficulty was to identify and determine approximately the interfering elements. When this was done the purification procedure could be modified to eliminate such elements or at least cut them down considerably.

8.72 Unusual factors entered into such submicro work. Reagents had to be unbelievably purified in order that the presence of a particular impurity should not become the limiting factor of the method. Contamination was probably the major difficulty, since most of the worst elements are prevalent in any ordinary experimental environment (atmospheric dust and fumes, floor scuffings, etc.). The laboratories were equipped with precipitrons. Floors and walls were kept very clean. A special sub-group of the CM-1 Service Group was devoted to light element contamination control and investigation. Theirs was the job of making certain that dust in laboratory air was at a minimum and that laboratory personnel were not unconsciously causing some significant contamination. Control tests were run on dust deposition in the laboratories; the humidifying system was found to bring in contamination and was stopped. Shoe covers were adopted to avoid floor scuffing, and methods of cleaning floors were improved.

8.73 Some of the analytical methods used at Los Alamos were developed at the Chicago Metallurgical Laboratory. Rather close liaison existed between the two projects in this particular field. One of the outstanding analytical developments at Los Alamos was the vacuum method for carbon and oxygen analysis.

8.74 The analytical methods involved in the early work at Los Alamos are outlined below, with discussion in the succeeding paragraphs.

1. Spectrochemical Methods
   A. Plutonium

(1) The cupferron and gallic acid methods--trace analysis for light element impurities
(2) The direct copper-spark method

B. Uranium
(1) The gallium-oxide-pyroelectric method
(2) Determination of rare earths in uranium
(3) Cupferron precipitable refractories in uranium

C. Miscellaneous
(1) Impurities in graphite by gallium-oxide-pyroelectric method
(2) Determination of fluorine in uranium and calcium (the strontium fluoride band method)

2. Colorimetric Methods
   A. Determination of phosphorus in uranium and plutonium
   B. Determination of microgram quantities of acid soluble sulfide sulfur
   C. Determination of iron in plutonium
   D. Determination of submicrogram quantities of boron in calcium, uranium and plutonium
3. Gravimetric Methods
   A. Determination of molybdenum in uranium-molybdenum alloys
   B. Determination of carbon in uranium tetrafluoride
4. Assay Methods
   A. Radioassay
   B. Photometric assay
5. Gasometric Methods
   A. Oxygen
   B. Carbon

SPECTROCHEMICAL METHODS

8.75 These methods were developed by the Analysis Group. Copper electrodes for use with spark excitation had been used at Berkeley in the first spectrochemical analysis of plutonium. The direct copper spark method as used at Los Alamos was a Chicago development. It had been shown at Chicago that plutonium could be extracted by cupferron and chloroform. The method of making this separation before sparking was conceived at Chicago but developed at Los Alamos. The pyroelectric-gallium-oxide method was developed at Los Alamos.

A. Plutonium Analysis

8.76 The cupferron and gallic acid methods were first used (early

1944) as co-methods in developing routines for trace analysis of the light elements. The former was chosen as standard. When in August 1944 purity standards were relaxed, the need for very sensitive methods disappeared and further research on their improvement ended. The cupferron method is discussed in Chapter XVII.

8.77 The direct copper spark method was used throughout the Laboratory history. In this procedure plutonium is evaporated on copper electrodes and the spark-spectrum photographed in the 2500-5000 angstrom range. The quantities of impurities are estimated by measurement of spectral line densities. By the end of this history, this was the only method available for the determination of thorium and zirconium. It was used for preliminary determination of impurities in incoming Hanford plutonium solutions.

B. Uranium Analysis

8.78 Except for a short time early in 1944 when the gallic acid method of analysis was tried for a while, the pyro-electric method was the means of over-all purity analysis of uranium. In this method the oxide mixed with gallium oxide is arced from a crater in a graphite electrode and estimates are made spectrographically. Volatilization of impurities along with gallium occurs in a manner analogous to steam distillations but the complex uranium spectrum does not appear. Volatile compounds lost in ignition to the oxide are not determinable.

8.79 Rare earth determination of high sensitivity was made possible by a method which removed other impurities, followed by examination of the spark spectrum.

8.80 Cupferron-precipitable refractories -- titanium, zirconium and iron -- were separated from other impurities by this method and examined in the copper spark.

C. Miscellaneous

8.81 Graphite purity analysis was developed as an adjunct of the PuO_2 graphite metal reduction (8.42) and dropped with the latter.

8.82 The strontium fluoride band method was the only successful method discovered for fluorine analysis. This method involved the absorption of fluorine in sodium hydroxide. The sodium fluoride is arced in the presence of excess strontium oxide and the amount of fluorine estimated by comparing strontium fluoride band head intensities with a standard. This method seems applicable to a number of materials but at Los Alamos has only been applied to uranium and calcium.

COLORIMETRIC METHODS

8.83 Phosphorus in uranium and plutonium was estimated by a colorimetric method depending on the formation of molybdenum blue from orthophosphate.

8.84 Microgram quantities of acid-soluble sulfide were estimated by a colorimetric method. This depends upon the conversion of hydrogen sulfide into methylene blue, which is determined spectrophotometrically.

8.85 Iron was determined spectrophotometrically in the presence of +3 plutonium after reduction to the ferrous state with hydroxylamine.

8.86 Boron in calcium, uranium tetrafluoride, and plutonium were determined by distillation as methyl borate from a special quartz still. The distillate was trapped in calcium hydroxide solution and the boron estimated by a colorimetric method.

GRAVIMETRIC METHODS

8.87 Gravimetric methods were used for the determination of molybdenum in uranium-molybdenum alloys and of carbon in uranium tetrafluoride.

ASSAY METHODS

8.88 Before August 1944 radioassay was the means of keeping track of plutonium quantities received, while a true analytical method, photometric assay, was being investigated. This method, however, did not yield encouraging results and was later proved quite untrustworthy. Hence, radioassay was continued. This method involved the determination of the quantity of an aliquot of the material by measurement of its alpha activity.

GASOMETRIC ANALYSIS

8.89 Description of the procedures involved in this work is limited by the extreme complexity of the apparatus used. The apparatus used for oxygen and for carbon microdeterminations can be classed among the most complicated analytical set-ups in the history of chemistry.

8.90 The oxygen method developed by the High Vacuum Research Group solved one of the most pressing analytical problems at Los Alamos — the development of a dependable micromethod for oxygen determination. The over-all method was not new but its application on a microscale, the accuracy obtained, and the furnace tube developed were quite new.

8.91 The procedure involved vacuum fusion of a sample in a graphite crucible and analysis of the gases evolved. Oxides react with graphite at high temperatures, giving carbon monoxide. Determination is made of this compound.

8.92 The apparatus was composed essentially of two parts, a high vacuum system (10^{-8} centimeters) and a somewhat revised Prescott microgas analyzer. Either one of these systems could be broken from the line independently. The sample size was about 50 milligrams and sensitivity of the method was about 10 p.p.m. In the original procedure the crucible and furnace tube were put in place, the sample was put in a dumper bucket, and the tube sealed. The sample was dropped into the crucible after the latter was suitably degassed. The gas was then collected for analysis.

8.93 The apparatus for carbon analysis in plutonium was simply a modification of the oxygen apparatus. The sample was burned in oxygen from mercuric oxide in a low-carbon platinum crucible. The gaseous products were then analyzed by the Prescott apparatus. In this case the sensitivity was 5 p.p.m. or less.

Cryogeny

8.94 At the very beginning it had appeared that the development of a deuterium super-bomb might prove feasible and necessary during the wartime course of the Laboratory. As the great difficulties attending this development became more apparent, and as the energy of the Laboratory was absorbed in the prior problem of the fission bomb, the experimental side of the Super project was gradually brought to a standstill. Except for new cross section measurements later made in F Division (13.22), this program was in fact limited from the beginning to investigation of the preparation and properties of liquid deuterium. Locally, it was virtually limited to the design and construction of a deuterium liquefier. This was a Joule-Thompson liquefier patterned after that built by W. F. Giauque at the University of California. It consisted of an ethane, a liquid air, and a liquid hydrogen (or deuterium) cycle. The first two cycles were completed by the beginning of 1944. The hydrogen cycle was completed in April 1944 and tested. Although the original design was for a capacity of 35 liters an hour, at the altitude of Los Alamos (7300 feet) it produced only 25 liters, a loss which could be compensated for by additional compression, if necessary.

8.95 Because of limitations of space and personnel, the physical investigations relevant to the problem of producing and storing liquid deuterium were carried out under contract by Prof. H. L. Johnston at Ohio State University. These included studies of the ortho-para conversion of liquid hydrogen and deuterium, of hydrogen-deuterium exchange problems, of the high-pressure low-temperature equation of state for hydrogen and deuterium, of the heat of vaporization of liquid deuterium, of the Joule-Thompson coefficients of hydrogen and deuterium. Experiments were made on the properties of thermal insulators at low temperatures. Studies were also made of the long-term operation of hydrogen liquefaction equipment.

8.96 Work was begun under the Ohio State contract in May 1943, and continued through the life of the project. The contract was renewed at 6 month intervals. As of September 1945, investigations still incomplete were planned for completion by the end of that year.

8.97 After the first test of hydrogen liquefaction in April 1944, no further developments in cryogenic work occurred at Los Alamos. Long and his group were assigned to other problems. On the basis of his assurance that with existing equipment he could produce amounts of the order of 100 liters of liquid deuterium in 2 months, and amounts of the order of 1000 liters in 8 months, cryogenic work was formally suspended in September 1944.

Chapter IX

THE PERIOD AUGUST 1944 TO AUGUST 1945 - GENERAL REVIEW

Reorganization

9.1 The second period of the Los Alamos Laboratory's existence begins with the general administrative reorganization which occurred in August 1944 (see graph No. 5). Measurement of the spontaneous fission rate of Clinton plutonium done by the Radioactivity Group (4.42-4.46) in the summer of 1944 ended all hope of making a gun assembly bomb out of this material. The Laboratory had originally been organized around the problem of making guns. Its organization had been stretched, but not broken, by the early implosion program. Now, however, it was evident that a reorientation was required. Work on the U^{235} gun, which had proved a relatively simple problem, proceeded as before. But while up to this time implosion had been considered a difficult if rewarding alternative to the gun, it now became an absolute necessity if the Hanford plutonium production was to be of any use. A complete reorganization of the Laboratory was indicated. Two entire divisions - G (Weapon Physics) and X (Explosives) - were created to study the problem of implosion dynamics.

9.2 G Division, under Bacher, included several groups which had worked under him in the Experimental Physics Division, as well as several groups from the Ordnance Engineering Division. X Division, under Kistiakowsky, included several groups formerly in Ordnance Engineering. Experimental Physics, renamed R (Research) Division, was organized under Wilson with those groups not transferred to G. The ordnance or O Division remained under Captain Parsons with those groups not transferred to G or X. CM (Chemistry and Metallurgy) Division and T (Theoretical) Division remained unchanged administratively, although the work of several of their groups changed considerably. In A (Administrative) Division, the principal change was the organization of a new group under Long which included C and V Shops, formerly the machine shops serving Ordnance Engineering and Experimental Physics, together with many of the miscellaneous shops of other divisions. Enrico Fermi arrived from the Metallurgical Laboratory early in September, and became leader of F Division, which included originally the Water Boiler and Super bomb groups. As part of the reorganization, Parsons and Fermi became Associate Directors of the Laboratory, and Mitchell and Shane Assistant Directors. Parsons was to have special responsibility for all aspects of the work having to do with ordnance, assembly, delivery, and engineering, and Fermi was to have responsibility for the research and theoretical divisions and for all nuclear physics problems.

9.3 Because of the complexity and urgency of the problem, the plan for the Laboratory's reorganization involved much interlocking of responsibilities and jurisdictions. G and X Divisions had to collaborate in the closest possible way, since they were working on separate phases of the same problem, and had to share facilities, equipment, and occasionally personnel. Since O Division was responsible for items to be fabricated away from the Laboratory, and for the design of the final weapon, it had to confer regularly and systematically with X and G Divisions. It was necessary to see that all plans and specifications of these Divisions could be incorporated into the final weapon design. If a plan was proposed in O Division to simplify fabrication, it had to be proposed to X and G Divisions to see whether their requirements were satisfied by it. R Division had to cooperate closely with G in carrying out nuclear measurements that would assist G in interpreting integral experiments and predicting the behavior of an implosion bomb. From the point of view of the implosion program, T, CM, and R Divisions considered themselves as service divisions. Perhaps the most thoroughly organized of these was T Division, which drew up a plan for assigning theoretical groups to service work for experimental groups. Members of T Division kept informed of the activities of the groups to which they were assigned, attended meetings of these groups, and were prepared to advise them when consulted.

9.4 Shortly before the general reorganization of the Laboratory, Oppenheimer outlined a plan to replace the Governing Board by two separate boards. The Governing Board had served as a policy making body attempting to handle general administrative problems and technical policies and serving as a medium for communicating technical developments. By the middle of 1944 it was seriously overburdened. The new plan divided the functions of the Governing Board between an Administrative and a Technical Board. Both of these bodies were advisory to the Director. The members of the Administrative Board appointed in July 1944 included Lt. Col. Ashbridge (Commanding Officer), Bacher, Bethe, Dow, Kennedy, Kistiakowsky, Mitchell, Parsons, and Shane; those of the Technical Board, Alvarez, Bacher, Bainbridge, Bethe, Chadwick, Fermi, Kennedy, Kistiakowsky, McMillan, Neddermeyer, Captain Parsons, Rabi, Ramsey, Smith, Teller, and Wilson. The Administrative Board was organized informally; members were urged to raise any questions concerning administrative problems and could invite other members of the Laboratory to discuss specific topics. The Technical Board meetings consisted of prepared discussions on some subject of immediate technical concern and also of brief reports on recent progress or problems of great urgency arising between meetings. Such reports were made by members of the board itself, by interdivisional committees, by division leaders, or by other members of the Laboratory who might have special contributions to make to the subject under discussion.

Conferences and Committees

9.5 As the implosion program developed and the time schedule tightened, the Technical Board proved inadequate to handle the many technical problems of the Laboratory. It was never formally dissolved but simply stopped meeting as its functions were taken over by various interdivisional committees and conferences. Among the most important of these were the Intermediate Scheduling Conference under Captain Parsons, the Technical and Scheduling Conference, and the "Cowpuncher" Committee. Both of the last named committees were under the chairmanship of S. K. Allison, former Director of the Metallurgical Laboratory, who arrived at Los Alamos in November 1944. In this shift from the single Technical Board to the more flexible structure of specialized committees, the Director had the advice not only of these committees, but also of certain senior consultants, notably Niels Bohr (2.5-2.8), I. I. Rabi (1.26), and C. C. Lauritsen (9.17), who served in the capacity of Elder Statesmen to the Laboratory in the guidance of its later program. Another important consultant of the Laboratory was Hartley Rowe, Chief Engineer of the United Fruit Company and former Technical Adviser to Gen. Eisenhower. Rowe came to the Laboratory in November 1944 and assumed responsibility for the transition from "bread board" models to production. He later established a new Division for this purpose (9.13), and was of great assistance in solving some of the bottle-neck problems of the Laboratory, for example, procurement of the firing unit for the implosion bomb (16.38, 19.8), and procurement of machinists (9.38).

9.6 The Intermediate Scheduling Conference was an interdivisional committee which began meeting in August 1944 to coordinate the activities, plans, and schedules of groups more or less directly concerned with the design and testing of the implosion bomb. The committee was formalized in November with Capt. Parsons as chairman, Ashworth (19.3), Bacher, Bainbridge, Brode, Galloway, Henderson, Kistiakowsky, Lockridge, and Ramsey as permanent members and Alvarez, Bradbury, Doll, and Warner as alternates. The conference scheduled topics in advance and invited to its meetings other members of the Laboratory when occasion arose. Eventually the conference was concerned with both the gun assembly and implosion bombs. The agenda of its meetings included chiefly procurement arrangements for items needed for the final weapons, the test program carried out in cooperation with the Air Forces, and details of the packaging and assembly of the bomb parts for overseas shipment. Although originally planned to handle both administrative and technical aspects of the design and testing of bombs, this conference became almost exclusively administrative in its function, and the technical problems were handled by the Weapons Committee formed in March 1945.

9.7 The Technical and Scheduling Conference was organized in December 1944 shortly after Allison's arrival, and assumed responsibility for scheduling experiments, shop time, and the use of active material in accordance with the requirements of the Laboratory's program. Each conference was called to discuss some particular subject such as the explosive lens program or the program of multiplication experiments on U^{235} metal spheres. Such subjects were announced in advance, with several persons requested to make short reports on various phases of the problem under consideration. The personnel of the conference was not fixed, but varied according to the subject to be discussed. In a large measure this conference took the place of the Technical Board, and was concerned primarily with the solution of technical problems. It became more a technical than a scheduling conference.

9.8 The intricate problems of scheduling the implosion program became the task of the Cowpuncher Committee, composed of Allison, Bacher, Kistiakowsky, C. C. Lauritsen, Parsons, and Rowe. It was organized "to ride herd on" the implosion program, i.e., to provide over-all executive direction for carrying it out. The committee held its first meeting early in March 1945. This group met often and published semimonthly a report called the Los Alamos Implosion Program which presented in detail the current status of the work. This included the progress of experiments in each group concerned in the program, the scheduling of work in the various shops, and the progress of procurement.

9.9 In April with the freezing of the implosion bomb design, the directive for G Division was amended and amplified to include responsibility for the so-called tamper assembly. G Division had to specify the design, obtain designs drawn to these specifications, and procure all parts of the first two complete tamper assemblies. This assignment involved consultation with many sections of the project, and to carry it out Bacher appointed M. Holloway and P. Morrison as G Division Project Engineers. They maintained close relations with the metallurgists, with various groups of the Explosives Division responsible for the design of the outer parts of the bomb, with the Weapons Committee on conditions of transport and storage, and with the Cowpuncher Committee for an over-all check of their work.

9.10 Among other interdivisional committees was the Weapons Committee, organized in March 1945. It assumed to a large extent the technical responsibilities originally assigned to the Intermediate Scheduling Conference, which became primarily an administrative group. The Weapons Committee was directly responsible to Capt. Parsons and was organized with Ramsey as chairman and Warner as executive secretary. It included eventually Comdr. Birch, Brode, Bradbury, Fussell, G. Fowler, and Morrison. This committee was asked to assume responsibility for planning all phases of the work peculiar to combat delivery and later became part of Project A (Chapter XIX).

9.11 A Detonator Committee composed of Alvarez, Bainbridge, and Lockridge was appointed in October 1944 to decide all questions connected with the external procurement of electric detonators. Bacher, Fermi, and Wilson composed a committee for the detailed planning and scheduling of experiments with U^{235} metal in order to save time and make the experimental program as fruitful and illuminating as possible. In February 1945, Oppenheimer appointed Bethe, Christy, and Fermi as an advisory committee on the design and development of implosion initiators. Niels Bohr met with this committee when he visited Los Alamos, and members of the committee kept in close touch with the Initiator Group and with the radiochemists.

9.12 Early in March 1945 two new organizations were created, with the status of divisions - the Trinity Project, and the Alberta Project - one to be responsible for the test firing of an implosion bomb at Trinity, and the other to be responsible for integrating and directing all activities concerned with the combat delivery of both types of bombs. The Trinity Project was led by Bainbridge with Penney and Weisskopf as consultants. Project A was led by Captain Parsons with Ramsey and Bradbury as technical deputies. The work of both of these projects is discussed in later chapters (XVIII and XIX).

9.13 The last division created almost at the end of this second period of the Laboratory's history was Z Division under J. R. Zacharias, who came to Los Alamos from the MIT Radiation Laboratory in July 1945. The new division was intended to carry out an engineering and production program, chiefly concerned with airplane and ballistic problems, to replace the program which had been carried out at Wendover Field, Utah. The project had by this time acquired a small airfield of its own near Albuquerque, formerly an army base called Sandia, was to be assigned its own planes, and also had the use of the large army base at Kirtland Field near Albuquerque. Although planned at a time when a prolonged program of manufacture was thought necessary, the new division was barely organized before the war ended.

Liaison

9.14 Many of the problems of liaison which had proved so difficult in the first period of the Laboratory's history (3.12 ff) had been solved or were no longer major problems by the time of the general reorganization. Liaison with the Army and Navy became increasingly important as designs were frozen, actual airborne tests became necessary, and preparations were made for combat delivery. Details of this will be discussed later (Chapter XIX).

9.15 The principal liaison problem which existed during the second period of the Laboratory's history was that with the Camel Project at the California Institute of Technology. The Camel project was created as the last of a series of expansions of the Laboratory in its transition from research through development engineering to final bomb production. In the fall of 1944, Oppenheimer learned that the Caltech rocket project had almost completed its research and development program and was entering the stage of production. The group at the Caltech project combined high professional scientific ability with practical wartime experience in weapon engineering, and moreover had their own procurement, laboratory, and field facilities. Since both manpower and facilities were becoming badly overstrained at Los Alamos, in November 1944 Oppenheimer discussed with C. C. Lauritsen, head of the Caltech rocket group, the possibilities of collaboration between the two projects. The matter was discussed further in correspondence with Bush, Conant, and Groves, and after some negotiations about contracts, the Camel Project was formed.

9.16 The character of the work done at Camel was determined by the facilities existing there, by the experience of the staff, and by the stage of the work at Los Alamos at the time the Camel Project began. Thus the Camel staff did no work on nuclear physics or the nuclear specifications of engineering; their work was confined to problems associated with the bomb assembly mechanism and its combat delivery. Specifically, their work can be classified under the two heads of implosion design and delivery. Under both categories wide use was made of Camel procurement facilities. Under the first category they did research and engineering of special components of the implosion assembly, detonators, lens mold design, impact and proximity fuzes, and high explosives components. In addition to these special programs carried out as a division of labor with Y, Camel had its own general implosion program. This was set up at the time of the final "freeze" at Los Alamos in April 1945, when a shortening time schedule forced the abandonment of alternative lines of implosion development. At that time the multiple lens bomb was adopted as final by Los Alamos, while it was decided that Camel would carry out a standby program. Camel work on weapon delivery covered the production of implosion bomb mock-ups, of "pumpkins" (bomb mock-ups loaded with high explosive, and intended for eventual practice bombing of enemy targets) with special impact fuzes, and included a special program of drop tests. The drop test program paralleled the Los Alamos program at Wendover and Sandy Beach (14.17), and provided data, for example, on bomb ballistics.

9.17 The main line of liaison between the two projects was between Oppenheimer and Lauritsen; reports were exchanged and personnel made numerous trips for consultation on specific subjects. Lauritsen spent part of his time at Los Alamos where he was a member of the Cowpuncher Committee, whose responsibility it was to push through the many-sided implosion program on schedule. In March and April 1945 there were extensive discussions of the joint Camel-Los Alamos implosion program. In addition to clarifying the technical aspects of the program, it was agreed that there was a mutual lack of understanding of the nature of the responsibilities of personnel at both sites, that provisions for exchanging information were inadequate, and that better liaison was needed. As a result of these discussions a CIT liaison office was established as part of the Director's office at Los Alamos with McMillan specially responsible for coordination. Mail service was improved, teletype connections were established, and eventually regular airplane schedules established for freight and passengers between "Kingman" (Wendover Field, 19.2), Los Angeles, Inyokern (Camel's field site), Santa Fe, Sandy Beach (14.17), and Albuquerque.

Administration

9.18 In July 1945 the administration of the Laboratory was organized into the following groups:

A-1	Office of Director	D. Dow
A-2	Personnel Office	C. D. Shane (Assistant Director)
A-3	Business Office	J. A. D. Muncy
A-4	Procurement Office	D. P. Mitchell (Assistant Director)
A-5	Library, Document Room	C. Serber
A-6	Health Group	Dr. L. H. Hempelmann
A-7	(absorbed in Groups A-1 and A-9)	
A-8	Shops	E. A. Long
A-9	Maintenance	J. H. Williams
A-10	Editor	D. R. Inglis
A-11	Patent Office	Major R. C. Smith
A-12	Safety Office	S. Kershaw

OFFICE OF DIRECTOR

9.19 The office of Dow, Assistant to the Director, handled a variety of administrative duties of a nontechnical sort. One of the most important of these continued to be that of construction liaison between the using technical groups and the Post Operation Division which handled construction (3.122). During the fall of 1944 increased consumption caused a number of power failures, and solution of this problem was the responsibility of Dow's office. Eventually power was increased by tying-in with the Albuquerque line. Another responsibility of this office - shared with the Personnel Office - was the preparation of employment contracts for staff members on leave of absence from academic institutions. The first of a series of these was prepared in September 1944, covering six months and extending to the beginning of the next academic year if the project terminated. Dow's office also cooperated with the Business Manager's office in securing insurance policies for personnel. One of the latest of these, made available for purchase by University of California employees in July 1945, was an accident policy issued by the Indemnity Insurance Co. of North America, insuring "against bodily injuries caused by accidents and arising out of and in the course of the insured's duties in connection with war research undertaken by or on behalf of the contractor." Unlike previous Manhattan District Master Policies (3.68), this one insured against certain aviation hazards, which were important because of the expanding test program.

PERSONNEL

9.20 Abandonment of the Plutonium gun program in the general reorganization of the Laboratory (9.1) released a number of chemists and physicists, but these were readily absorbed into the newly strengthened implosion program. In fact, a general expansion was necessary and Shane, Bacher, and Long went on recruiting drives to the other projects of the District. As a result of their efforts a considerable group of civilian scientific personnel was secured from the Metallurgical Laboratory and from Oak Ridge, and a number of technical military personnel from the SAM Laboratories in New York and from Oak Ridge.

9.21 Personnel procurement was always hampered by the housing shortage (3.28), and the situation grew worse as the Laboratory continued to expand at a rapid rate. The third section of the housing area was completed by McKee in December 1944 (3.121), and it was tentatively determined that no additional multiple-unit housing would be constructed. A policy had to be established that employees should be housed whenever as a class they could not be procured without housing - specifically this included machinists, scientific personnel, essential administrative personnel, and sixteen technical maintenance men (3.119). An effort was made to relieve the shortage by encouraging machinists to come without their families, in exchange for a bonus payment. A number of additional dormitories were built, but the solution was not an adequate one (9.46).

9.22 Salary policy remained one of the Personnel Office's principal difficulties. Although a working agreement was reached with the Contracting Officer in July 1944, it was not a final one. The agreement provided that salary increases be limited to 15 per cent of the minimum range per year, and that not more than 25 per cent of all employees hired within a year might be hired at salaries in excess of the minimum of the applicable salary scale. There were to be no increases in salaries over $400 per month. In January 1945 Shane made an effort to remove the $400 restriction and proposed semi-automatic merit increases for persons in this category. After much correspondence a certain number of individual increases of this sort were granted, and eventually the policy was changed to permit such increases as a regular thing. Also in January 1945 Shane requested that the project be granted an exemption from the 25 per cent hiring provision because of the special conditions of employment prevailing there. He pointed out that in determining the salary ranges of the original job classifications it was thought desirable to use low minima and large ranges in order to permit employment of personnel of the varied qualifications needed by the project. In the shops, especially, the nature of the work required a greater proportion of highly skilled workers than an ordinary production shop, and the assignment of relatively young and inexperienced enlisted personnel made it necessary to hire principally highly skilled civilians to fill the responsible positions. The Contracting Officer agreed to make certain exceptions, especially in the case of shop personnel. In July 1945 the salary situation reached another critical point, this time concerning approval of salary increases. Since March the Personnel Office had had difficulties in reaching agreements with the Contracting Officer on salary changes, although they were following the same rules which had been acceptable since the agreement of July 1944. Shane requested that a conference be held and policies changed by mutual agreement. Such a conference was held late in July 1945. Although agreement was reached on a number of minor points, the main issues were not settled, and with the end of the war Shane resigned from his position as Personnel Director.

PROCUREMENT

9.23 The Procurement Office was not directly affected by the Laboratory's reorganization, except that the volume of its work increased and continued to do so until shortly before the Trinity test (see graph No. 7).

9.24 In October 1944 the Property Inventory Section was established with Capt. W. A. Farina in charge. Capt. Farina was responsible for making a physical inventory of the Laboratory, for revising the Procurement Office's record system to make it compatible with War Department regulations, and for advising the University concerning government property policy. The necessity for having an inventory made and for having someone at the site responsible for the accountability of material had been discussed since the early days of the project but always postponed because of more urgent work.

9.25 By the end of 1944 the Ordnance Division had established its own special Procurement Group under Lockridge, and to avoid confusion it was necessary to outline the responsibilities of each procurement group. Mitchell continued to be responsible for all stock catalogue items. Lockridge was responsible for all fabrication jobs involving machine shop work and mechanical assembly, and either Mitchell or Lockridge could place orders for fabrication jobs involving chemical and metallurgical techniques, plastics, and electrical work. In the last case, to avoid duplication, the office making out the requisition would notify the other office. Much of Lockridge's purchasing was done through special channels rather than through the University Purchasing Office in Los Angeles, but he worked in close cooperation with the office of Col. Stewart. A considerable proportion of Lockridge's ordering was done from the CIT project, and also from G. Chadwick of the Detroit Office.

9.26 In April 1945, the time schedule for the Trinity test had become exacting, and the number of urgent purchase requests increased rapidly, so rapidly that it became necessary to inflate the urgency ratings which had been in use. Up to that time the Procurement Office had used four ratings - X, A, B, and C, in order of decreasing priority. Early in May Mitchell announced that the super rating of urgency X would be subdivided into three - XX, X1, and X2. XX priority could be used only if failure to obtain the material would produce a setback of major importance in the over-all program of the Laboratory, and authorized the Procurement Office, through the Washington Liaison Office, to have recourse to the highest authority of the WPB and of all government agencies, and to use a special dispatch or cargo plane from anywhere in the United States for delivery.

9.27 Delayed deliveries on a number of urgent requests led Oppenheimer to call a meeting in May to review the procurement situation. One of the principal reasons for the delays was found to be the shortage of personnel in the Los Angeles, New York, and Chicago Purchasing Offices. Although the number of requisitions had increased greatly, there had been no increase in the number of buyers since January 1944. The Contractor's representative Underhill blamed the lack of adequate personnel on salary restrictions. As the result of this meeting, additional personnel were secured for all three Purchasing Offices, the Contracting Officer agreed to permit salary adjustments, direct communication was established between the project and the New York and Chicago offices, and project members were requested to submit improved drawings and specifications. There was a considerable effort to improve service as a result of this meeting.

9.28 While the number of purchase requests reached a peak in May, the amount of goods handled by the main warehouse of the Procurement Office reached its peak in June. Some notion of the quantities involved may be had from the following figures: During May, the Warehouse handled an average of 35 tons per day (89% incoming and 11% outgoing); during June the daily average rose to 54 tons (87% incoming and 13% outgoing); and during the first half of July it was 40 tons per day (80% incoming and 20% outgoing). Outgoing goods, chiefly for Trinity and overseas shipments, were handled by the Shipping Group which was organized in the spring of 1945.

HEALTH

9.29 During the second period of the Laboratory's history, the problems of the Health Group became progressively more numerous and more complex, as the number of persons exposed to radiation and radioactive materials increased.

9.30 In August 1944, following an accident involving plutonium (3.97), members of the Health Group and the Chemistry and Metallurgy Division expressed the dissatisfaction which they had felt for some time with the progress of biological studies on plutonium at other projects. Permission was obtained from the Director to undertake a research program at Los Alamos to study the biological problems of special interest to this project. This program was begun by a group of chemists under the direction of a steering committee consisting of Kennedy, Wahl, and Hempelmann, with the primary purpose of developing tests for detecting overdosage of personnel with plutonium. Up to this time it had been necessary to rely on "nose counts" (filter paper swipes of the nostrils) to indicate exposure of personnel and these gave only a qualitative idea of the amount of material inhaled. For a more quantitative test it was thought necessary to determine the amount of plutonium excreted daily in the urine, and also to determine the amount present in the lungs. A satisfactory urine test was difficult to develop because of the small quantities of plutonium involved ($\sim 10^{-10}$ micrograms per liter of urine), and because of the difficulty of collecting specimens free of alpha contamination. A successful method of analyzing urine was developed in January 1945, but was not used as a routine test until after the first human tracer experiment had been performed in April. Because of the difficult time-consuming nature of the urine test, it was impossible to do frequent examinations for any individual, and a system was worked out by which the persons most heavily exposed, as indicated by nose counts, had the most frequent examinations. A satisfactory method of detecting plutonium in the lungs was not developed.

9.31 Lack of adequate monitoring equipment continued to be a problem for some time. Instruments supplied by the Chicago Laboratory did not at first meet specifications of this Laboratory, and the development of equipment, begun in May 1944, by the Electronics Group of the Physics Division, continued for several months. A proportional counter of adequate sensitivity was developed by this group.

9.32 With the reorganization of the Laboratory in August 1944, the Monitoring and Decontamination Section of the Chemistry and Metallurgy Service Group was reorganized and part of its personnel and part of its function transferred to the Health Group. The division of responsibility between the Health Group and the Monitoring and Decontamination Section did not prove satisfactory, and in January 1945 a new group was organized which would have full responsibility for the entire alpha contamination problem of the Chemistry and Metallurgy Division. William Hinch, formerly of the Metallurgical Laboratory, became leader of this group in April 1945, and assumed responsibility for developing new methods of monitoring and decontamination, arranged to procure monitoring instruments from Chicago, and added an electronics section to maintain existing instruments. The large quantities of plutonium produced at Hanford began to arrive during April, and one of the new group's most important functions was that of adapting existing facilities for processing plutonium to meet safely the increased demand upon them. Except for a short period early in July the facilities proved adequate. At that time the Plutonium Recovery Group handled excessive amounts of plutonium, and urine analyses showed that four persons had in their bodies more than the 1 microgram of plutonium considered safe. As large amounts of material arrived and people began to worry about the accidental bringing together of critical amounts, a policy of quantity control was inaugurated in which any transfer of material from group to group had to go through a record office. Eventually DP site was built (17.59 ff) 1-1/2 miles from the rest of the Technical Area to minimize the many dangers of this work, especially that of fire.

9.33 The polonium hazard, though parallel in many ways to the plutonium hazard, never became as serious a problem for the Health Group. No research was done on the subject at Los Alamos, but routine urine tests were done on all exposed personnel in accordance with the standards of the Manhattan District Medical Section. Polonium is not so dangerous as plutonium per unit of radioactivity even though it spreads around a laboratory very readily. Health group records indicate that only two people exceeded temporarily the tolerance limit for polonium excretion. The typical costume of a worker with plutonium or polonium included coveralls or laboratory smock, rubber gloves, cap, respirator, shoe covers, and often a face shield. All of these items were worn only once and then laundered. The following figures give some notion of the magnitude of the decontamination problem. In July 1945 when personnel in CM Division approached 400, 3550 rooms were monitored, 17,000 pieces of clothing were laundered, 630 respirators were decontaminated, and also 9000 pairs of gloves, of which 60 per cent were discarded. In June 1945 decontamination of laboratories was made the responsibility of the laboratory workers themselves. To this end they were instructed in cleaning procedures and methods of detection.

9.34 The hazards of external radiation which had been negligible and confined largely to accelerating equipment and radioactive sources in the early period of the project became more critical in the fall of 1944. At that time three potential sources of danger appeared - the Water Boiler and later the power boiler, the implosion studies of the RaLa Group and critical assembly experiments. Operation of the power boiler resulted in several instances of mild overexposure to radiation caused by leaks in the exhaust gas line and one serious exposure of several chemists during decontamination of active material. The implosion studies of the RaLa Group which used large amounts of radioactive barium and lanthanum brought a serious situation which the health group monitored constantly. A series of accidents and equipment failures caused considerable overexposure of the chemists in this group. This condition persisted for about six months until the system of remote control operation was finally perfected (17.41). The most serious potential radiation hazard was that of the critical assembly experiments, and here the Health Group had no responsibility, except that being sure that the men were aware of the dangers involved. These experiments were especially dangerous because there is no absolute way of anticipating the dangers of any particular experiment, and because the experiments seem so safe when properly carried out that they lead to a feeling of overconfidence on the part of the experimenter. Two serious accidents resulted from the critical assembly work during this period of the project's history - one of them resulted in the acute exposure of four individuals to a large amount of radiation and the other resulted in the death of one person.

9.35 The Health Group made extensive reports of the radiation hazards caused by the Trinity test, and these are discussed in a later chapter (Chapter XVIII).

9.36 With the rapid expansion of the Laboratory that began in the fall of 1944, the Health Group found itself understaffed and unable to maintain personal contact with all the individuals engaged in technical work. Consequently its records of external radiation dosage to personnel became less accurate. This was particularly true in the cases where the radiation hazards were not serious and did not change frequently, and where experiments using natural sources were performed after transactions which involved transfer of these sources from one person to another without the knowledge of the Health Group. This was not true of groups where exposure to external radiation was prolonged or severe. There are also instances where blood counts of exposed personnel were not made or were made less frequently than desirable, largely because of poor cooperation of personnel. Complete radiation and hematology records are valuable chiefly as legal evidence in case of future claims against the project. It was the policy of the Health Group, in cases where lack of trained personnel did not permit meeting all of its obligations, to do jobs in the following order of priority: 1. Procedures which actively protected personnel against industrial hazards. 2. Accident reports or termination records for persons leaving the project. 3. Records or reports of routine exposure, hazards, etc.

SAFETY

9.37 The safety problems of the project were handled entirely by the Safety Committee until early in 1945 when the committee advised the Administrative Board of the need for a full-time safety engineer to execute the policies devised by the committee. At about this time Mitchell resigned as chairman of the committee because of his increasing responsibilities as Procurement Division Leader and was replaced by David Lipkin. At the end of February the project hired Stanley Kershaw of the National Safety Council to be full-time Safety Engineer, and on March 1 established the Safety Group to parallel the function of the Health Group. The Safety Committee continued to meet regularly as an advisory body to guide in the formulation of the project's health and safety policy. A conference held in May established a division of responsibility between the Post Safety Section and the Laboratory Safety Group. The Safety Committee recommended safe procedures and the Safety Group assisted in carrying them out, but basically the group and division leaders were responsible for the safety of the work done under their supervision. Neither the Health Group nor the Safety Group was a "police" agency, but relied largely on the cooperation of technical employees. A safety manual was issued in July as a guide for accident and fire prevention regulations for the project in accordance with standards approved by the various division leaders.

SHOPS

9.38 At the time of the general Laboratory reorganization in August 1944, both C and V shops were combined under the supervision of Long and Schultz. Peters was promoted to be superintendent of C Shop, and Henry Brockman became superintendent of V Shop. The reorganization of the Laboratory was coupled with a rapid expansion of the new divisions to several times their initial size, and entailed a corresponding increase in the volume of shop work. Additions to C Shop of 8500 and 3300 square feet had been built in May and July, but the recruiting of competent personnel was going very slowly. The personnel problems of the shop, particularly those of salary adjustment, had been presented to Shane on his arrival in June as Personnel Director. Shane reported to the Governing Board in August the results of an extensive survey of the shop situation. He had found a bad morale situation in the shop resulting from salary inconsistencies, and proposed to remedy this by salary adjustments and by releasing certain men whose work was below standard. In order to fill these vacancies and increase shop capacity rapidly, he proposed an extensive recruiting drive by army and shop representatives. Such a recruiting drive was undertaken on a large scale, early in November, after one unsuccessful attempt to obtain the needed machinists by less drastic methods. This drive was carried out simultaneously by six teams of army and shop representatives; its results can be seen from the rapid increase of the number of man-hours per month in Graph No. 9.

9.39 During November and December the shop situation was improved considerably as the result of a more consistent salary adjustment and employment policy. For the month of December 1944, a peak was reached, for C Shop, of 25,000 man-hours.

9.40 On January 18, 1945, part of the roof of C Shop was destroyed by fire. This fire started in the heat-treating shop where a large piece of metal ignited the tank of quenching oil in which it was being dipped. The fire traveled rapidly and spread to about half of the shop roof before it could be controlled.

9.41 The loss in shop hours is indicated in Graph No. 11. Part of this loss was absorbed by increasing the load in V Shop. Estimates at the time were that not more than a week was lost. Some machines were in operation within two days after the fire, and major construction repairs were completed within a month of the fire. The longest delay occurred in the repair of heat-treating equipment; because of difficulty in obtaining heavy crane parts from the manufacturer, this equipment was not in operation until the end of March.

9.42 In February a second recruiting drive took place, more successful in the calibre of men obtained. The recruiters had more experience than in the first drive, and found a better labor market. As a result of this drive the number of machinists rapidly approached the limiting figure determined by shop capacity; the actual peak was not reached until the end of June, when the two main shops employed, on a two shift basis, 446 men.

9.43 The rapid increase of shop loads in the fall of 1944 and spring of 1945 reflects the transition of the Laboratory from research to development and production, and its rapid expansion after the formation of G and X Divisions.

9.44 Some mention has been made above of morale problems in the Shops. In addition to the difficulties arising out of salary inequalities, the shop situation was complicated by the mixture of enlisted and civilian machinists, and by the quality of housing and community facilities which the civilian recruits met on their arrival at Los Alamos.

9.45 Machinists and toolmakers already in the army were secured through the SED beginning in June 1944, when the shops were falling behind and civilian machinists became increasingly hard to find. As a group these men were excellent, perhaps slightly superior on the average to the civilian group. They should be especially credited for the quality of their work, in view of several obvious sources of irritation. These men worked the 54 hour week which prevailed in the shops, whereas other enlisted men worked the 48 hour week standard in the Laboratory generally. The disparity of income between enlisted and civilian co-workers was greater in the shops than in other parts of the Laboratory. The spirit of cooperation in research, possible in other parts of the Laboratory, was largely missing here. It was only here that any obvious cases of personal friction developed; these were not of a serious nature, but they occurred.

9.46 Among the civilian machinists recruited in November and February there was a serious morale problem. The housing of these men with their families would have represented a major investment in housing at a time when the project had presumably not long to run. It was therefore decided by the military authorities that the men recruited would have to be housed in dormitories, leaving their families behind. For this they were paid $100 per month above their salaries, and promised return expenses if they remained for more than six months. The dormitories constructed to house these men were less comfortable and attractive than other dormitories. Complaints centered about housing, about mess facilities, and recreation facilities. Although Col. Tyler, the Commanding Officer, made great effort to improve this situation, it could not be radically altered. After a short time a number of trailer houses became available, to which, however, only a small percentage of the men could be assigned.

9.47 A tabulation of resignations and dismissals was made in January 1945. Of 219 men recruited in November and December, one-third had left by this time. Of these well over half had resigned ostensibly because of the conditions of life at Los Alamos. There is no doubt that some of the annoyances were considerably magnified. As a group these men were brought only into the periphery of the community. They viewed the project as one among many possible war jobs, and had little reason to do otherwise. The majority, who did their work well and remained with the project, nevertheless felt with some justification that they were discriminated against by the Contractor and the community.

9.48 From the second recruiting drive in February 1945, there were definitely fewer casualties. Men were more carefully selected and facilities at Los Alamos had been improved. To help ease the work load, shop facilities at the Metallurgical Laboratory were used to some extent beginning in the spring of 1945.

9.49 During this period several difficult technical problems were encountered. One had to do with the machining of full scale explosives castings (16.14). Responsibility for the design of tools and fixtures for this work was assumed by Long and Schultz in the spring of 1945. They were responsible also for the accurate gauging of full-scale castings.

9.50 Another example was the construction and use of molds for high-explosive lens casting. (16.24). Outside firms to which this work was first assigned were unable to carry it out. The shops suggested changes in design, worked out the techniques for producing molds, and sent representatives to outside producers to teach them the necessary techniques.

9.51 Several difficulties of a technical nature were encountered, at Los Alamos as at other sites, in the machining of uranium. It constituted in the first place a minor health hazard, that of normal heavy-metal poisoning. Uranium machining was carried out from the beginning in a special shop, under the direct supervision of Schultz. It was moved from a small annex to the Cryogeny Building in the spring of 1944, to a special enclosed region in C Shop. In the spring of 1945 it was moved to a new building of its own.

Chapter X

THE PERIOD AUGUST 1944 TO AUGUST 1945-TECHNICAL REVIEW

Introduction

10.1 As the previous chapter will testify, the growth of the Laboratory plant and program during the period reviewed was determined primarily by the urgency and difficulty of the implosion program. The status of that program at the end of the previous period has been outlined in Chapter IV (4.40-4.47). By comparison of this situation with that existing in July 1945, when the first implosion bomb was tested and found successful, it is clear that by the latter date a major technological victory had been won. The period reviewed, however, is not completely defined by that victory. The success of the Trinity test was possible only in the flowing together of several parts of the Laboratory's work. The success of the combat missions over Japan, moreover, presupposed that of the entire delivery program. Finally, it must not be forgotten that the first atomic weapon was the "Little Boy" gun assembly, developed during this period largely by a single group. Because it was relatively smaller and more straightforward than the implosion, its accomplishments will be reviewed first.

Gun Program

10.2 During this last year of the war the gun program was consolidated under one group, the Gun Group of the Ordnance Division. This group completed the design of the U^{235} gun assembly, tested its components at reduced and full scale, undertook their final engineering and procurement, and after an elaborate program of final field and drop tests produced a weapon more certain of high-order operation, without having been tested, than the more radical implosion design tested at Trinity. During the earlier period of the Laboratory the possibility of a U^{235} implosion bomb had not been ruled out. With the acquisition of accurate means of calculation and reliable cross section data, it became evident that such an implosion would be considerably less efficient than the plutonium implosion. This fact, added to the uncertainties of the whole implosion program, made it seem desirable to plan for the use of U^{235} by the gun method alone. Toward the end of the war the possibility of composite (U^{235} + Pu^{239}) implosion bombs was considered (11.2, 20.2). By the time of the "freeze" of the Laboratory program in February 1945 (11.10), the decision was final to use U^{235} only in the gun model.

10.3 This quiet and efficient group continued at the center of an affiliated program in the Research Division, the Theoretical Division, other groups of the Ordnance Division, and in the Alberta Project (Ch. XIX). From the Research Division, Group T-2 was able to obtain information on the nuclear properties of U^{235} sufficient to provide accurate data for critical mass calculations and calculation of the amount of material that could be safely used. From the sphere multiplication experiments of R Division, a still more accurate calculation of the critical mass could be obtained, by extrapolation. The gun was "mocked" by the model experiment in the same division, and this provided an integral check of the calculations of the performance of the weapon, including predetonation probability. The finished projectile and target, finally, were brought to the critical point by the Critical Assemblies Group of G Division shortly before shipment to Tinian for combat use. This assembly was a final check of the accuracy of predictions as to the point at which the system would become supercritical. Reliable efficiency calculation was made possible by theoretical and experimental estimation of the initial multiplication rate of the fully assembled bomb.

10.4 The fabrication of the projectile and target was the responsibility of members of Groups CM-2, -7, and -11 of the Chemistry and Metallurgy Division. Fabrication included the forming of the active material into pieces of proper shape and purity, and the steel casing that housed the target. The final design of the outer case, originally the responsibility of the Engineering Group of the Ordnance Division, was almost entirely transferred to the Gun Group during this period. Responsibility for the fusing and detonating system remained with the Fuse Group of the Ordnance Division. The Gun Group and the Fuse Group collaborated in the drop tests of the Little Boy carried out as part of the program of Project Alberta.

The Plutonium Bomb

10.5 At the beginning of the period under discussion, the hope for a successful implosion was so low that F Division was given the responsibility soon after its creation, of investigating even the slim possibility that as an alternative, an autocatalytic system of assembly utilizing plutonium might be found meritorious. The desirability of such systems was not immediately evident and in the meantime the Weapon Physics Division, the Explosives Division, and the Theoretical Division were preparing themselves for a direct attack on the implosion problem.

10.6 Prediction based on the analysis of Clinton plutonium led to the expectation that the Hanford plutonium would produce a large number of neutrons per second, in bomb-amounts of plutonium, from the spontaneous fission of Pu^{240} alone. Light impurities would produce additional neutrons, but purification would keep this contribution small compared to that from spontaneous fission. Only the implosion would be fast enough to assemble the plutonium in a time short enough to avoid predetonation.

10.7 The "direct attack" on the implosion problem included the continuation of small scale implosion studies in the new X Division, with particular emphasis on interpreting the causes of jets and irregularities, including the careful investigation of the source of timing errors in multipoint detonation and their contribution to asymmetries. The first lens test shot was fired in November 1944. In the meantime G Division was getting under way its many-sided effort to examine the implosion experimentally, was beginning work on electric detonators, and was planning the hydride critical experiments as a step to eventual critical assemblies of active metal. At the same time the Theoretical Division was completing its studies of the "ideal" implosion (which began with a spherically converging shock-wave), and was turning its attention to the theoretical interpretation of the jets and asymmetries that had been found in less-than-ideal experimental implosions.

10.8 In the Explosives Division means for preventing the development of irregularities were under investigation. Early results from the lens program in X Division, meanwhile, showed that a converging spherical detonation wave could be approximated by a lens system, provided a sufficient degree of simultaneity could be obtained for all lenses. Thus although there was as yet no sure path to success, hopeful directions of development had been marked out.

10.9 At the end of February 1945, a conference was held at Los Alamos, with General Groves present, at which it was decided that the time had come to freeze the program of the Laboratory in order to meet the July deadline for the first bomb test. At this conference it was decided to concentrate all further work on the lens implosion with a modulated nuclear initiator.

10.10 At the same time a detailed schedule of all implosion work was decided upon. By April 2 full scale lens molds had to be delivered and ready for full scale casting. Full scale lens shots had to be ready by April 15, to test the timing of multipoint electric detonation. Hemisphere shots had to be ready by April 25. The detonator had to come into routine production between March 15 and April 15. By the latter date large scale production of lenses for engineering tests had to be begun. A full scale test, by the magnetic method, had to be made between April 15 and May 1. Full scale plutonium spheres had to be fabricated and tested for their degree of criticality between May 15 and June 15. By June 4 the fabrication of highest-quality lenses for the Trinity test had to be under way. The Trinity sphere fabrication and assembly should begin by July 4.

10.11 To meet the stringent requirements of this program, the Cowpuncher Committee (9.8) was set up March 1, to "ride herd" on it. The feasibility of a modulated initiation was accepted April 27. Full-scale lens molds were completed in May, after innumerable procurement delays. Timing measurements of lenses were made with successful results shortly thereafter, but also delayed. By June 12 two full scale plutonium hemispheres were tested for neutron multiplication, something over two weeks late. The delays referred to arose primarily because of the difficulty of procuring good lens molds on schedule. The result was a shortened time for final engineering tests. But the Trinity test was made, actually four days ahead of the target date - July 20, 1945 - assumed in making the above schedule.

Theoretical and Experimental Physics

10.12 During the period reviewed, the Theoretical Division was able to bring to a successful close its earlier investigation of the techniques for solving neutron diffusion problems with accuracy, reliability, and speed. The division developed these techniques and similar refinements in the means of treating the other theoretical problems involved in the implosion and the nuclear explosion. It was therefore able to give realistic guidance to the last phase of the experimental program, to the final weapon design, and to the preparations for the Trinity Test. In F Division the investigation of the

Super continued at relatively low priority until the end of the war, when the freeing of men from other work made it possible to bring this work to partial completion. The results obtained indicated, in a convincing but not decisive way, that such a weapon is indeed feasible.

10.13 The experimental program in nuclear physics was continued by Divisions R and F. In R Division the original program of differential measurements was brought to completion: Neutron number measurements, spontaneous fission measurements, measurement of the fission spectrum and of fission cross sections, and scattering cross sections. Increasing emphasis was put on integral experiments, including tamper measurements and integral multiplication experiments, using solid spheres of U^{235}. In F Division, meanwhile, new measurements were made of the deuterium and tritium cross sections, which indicated a materially lower ignition temperature for tritium-deuterium mixtures than had been first obtained. The development of the high-power Water Boiler was carried through, and the instrument put into routine operation as a neutron source. Other work of the division included cooperation with R Division on sphere multiplication experiments and the preparation for radiochemical measurements at the Trinity test.

10.14 In G Division the Critical Assemblies Group carried through a program of critical assemblies with uranium hydrides of various compositions, and finally undertook the task of making metal critical assemblies with active materials to be used in the first bombs.

Chemistry and Metallurgy

10.15 The period under review saw, in the Chemistry and Metallurgy Division, the bringing to completion of full scale process and plant design, and full scale plant operation for production of active and tamper materials of both the U^{235} gun model and the Pu^{239} implosion model bombs. In addition to this the processing and handling of highly radioactive materials was carried out, both for the RaLa implosion test program and the neutron initiator program. As already pointed out the basic research program, plutonium purification, had already been solved on the research level by the time of the discovery of Pu^{240} and the relaxation of purity requirements entailed by that discovery. The development of an efficient purification scheme was completed in the present period, as were the necessary techniques of metal reduction and metal forming.

10.16 The work of the Chemistry and Metallurgy Division, which in addition to the main program outlined above continued to do a great variety of service work for other divisions, was hampered by serious hazards to personnel from plutonium and polonium toxicity, and developed an elaborate system of monitoring buildings and examining employees. This health control work was hampered by increased crowding of facilities, until new plant facilities were made available.

The Trinity Test

10.17 The planning of the Trinity test became a high priority program in March, 1945. The Jornada del Muerto site for the test, called Trinity, had been selected in the previous Fall, necessary road and building construction had been undertaken, and a Military Police detachment had taken up residence at "Base Camp" in December 1944. Although most parts of the Trinity program were under way in March, there was a vast amount of work still to be done, in planning and instrumenting the great variety of mechanical, optical, and nuclear records that had to be obtained. Several square miles of desert became the habitat of a complex laboratory, tied together in one vast system by thousands of miles of electrical wiring.

10.18 The first aim of the Trinity project was the rehearsal shot of 100 tons of high explosive that took place early in May. This provided a test of the organization for the final shot, and gave data for the calibration of blast measurements.

10.19 The information gained from this trial run proved valuable for the larger job ahead. As the July deadline of the test approached, larger and larger contingents of personnel arrived from Los Alamos to make ready the equipment for firing the bomb, for recording measurements of blast and shock, of spectographic and photographic information, and of nuclear data. The two hemispheres of plutonium were delivered from Los Alamos on July 11, and assembly of the high explosives began July 13.

10.20 The first atomic explosion was set off on the morning of July 16 after weeks of intensive preparation and hours of tense waiting to see if the weather, which had turned bad the night before, would clear. The event can hardly be summarized more concisely than in Sections 18.25 - 18.27.

10.21 With the impressive success of the Trinity test--whose yield certainly exceeded the expectations of most people, and was soon estimated to lie in the range 15,000 to 40,000 tons of TNT--the machinery of Project

Alberta, the overseas mission, began to operate. Since the history of this project is largely an account of its test program, its liaison arrangements and general administration, the present technical summary ends with the successful completion of the Trinity program.

Chapter XI

THE THEORETICAL DIVISION

Introduction

11.1 During the second period, from August 1944 to August 1945, the Theoretical Division took part in the general expansion of the Laboratory to the extent of increasing the size of its groups and adding three new groups. In comparison with other divisions it had relatively little administrative history. It was not seriously involved with the general Laboratory problems of personnel, construction, transportation, nor was it involved except in an advisory way with the complicated procurement and scheduling operations of the Trinity test and Project Alberta. It was therefore able to administer itself and do its work rather unobtrusively. Nevertheless it was an essential part of the final development program. As it gathered power from its earlier work, it was able to handle more realistic and complex problems with increasing efficiency, and to gain increased understanding of the difficult hydrodynamical questions involved in the implosion and the nuclear explosion, to refine its earlier calculations concerning critical masses and efficiencies, and to provide reliable interpretation of many integral experiments.

11.2 The Group Structure of the division by August 1945 was as follows:

T-1	Implosion Dynamics	R. E. Peierls
T-2	Diffusion Theory	Robert Serber
T-3	Efficiency Theory	V. F. Weisskopf
T-4	Diffusion Problems	R. P. Feynman
T-5	Computations	D. A. Flanders
T-6	IBM Computations	E. Nelson
T-7	Damage	J. O. Hirschfelder
T-8	Composite Weapon	G. Placzek

11.3 Group T-6 was added in September 1944 to operate the IBM machines, under S. Frankel and E. Nelson. Frankel left this group in January 1945 to join the Theoretical Group of F Division. Group T-7 was formed in November 1944 by a change of name. It was the former O-5 Group, already for practical purposes a part of the Theoretical Division. At the time of this formal change of status, however, the group was given the responsibility for completing earlier investigations of damage and of the general phenomenology of a nuclear explosion. Group T-8 was added in May 1945 upon the arrival from Montreal of G. Placzek. The responsibility of this group was to investigate future fission bomb possibilities, specifically the composite core implosion, intended to use U^{235} (with Pu^{239}) more efficiently than would be possible by gun assembly.

Diffusion Problems

11.4 Although by August 1944 the essential difficulties of the one-velocity diffusion problem had been overcome, even the most economical method (expansion of the neutron distribution in spherical harmonics) was still rather expensive. A very great simplification of these calculations was accomplished by Group T-2 in the fall of 1944, when an analytical expression was developed which by comparison with previously computed critical radii gave accuracies within 1 to 2%. This method made use of simple solutions for the shape of the neutron distribution far from boundaries (such as the boundary between core and tamper), and then fitted these solutions discontinuously at the boundary in such a way that the critical radius was given. From this time on, solutions for a great variety of critical radius or mass problems were proliferated extensively, and even reduced to nomographic form, permitting very rapid calculation.

11.5 Throughout the period under review various groups in the Theoretical Division, but particularly T-2, were concerned with special problems arising out of sphere multiplication experiments carried out in R Division. These calculations had to take into account the variation of the average cross sections after the initial and each following collision of neutrons emerging from a central source. The number of neutrons coming out of the sphere as a function of the number of source neutrons was calculated for various size spheres and for various dispositions of the source. These calculations agreed very closely with the measured values; and as larger spheres of U^{235} became available, it was possible to extrapolate to the critical mass with very high accuracy (12.18-12.23).

11.6 Critical mass calculations for the hydride were developed, by Group T-4, as a means of predicting the size of the hydride critical assemblies carried out by Group G-1 of the Weapon Physics Division. When the first assemblies were made, a sizable discrepancy between the actual and the predicted critical size was found, of the order of 50%. Efforts to track down the cause of this discrepancy were partially successful; when newer values for the fission spectrum were used, the discrepancy was materially reduced. The conclusion was reached that the discrepancy arose from the sensitivity of the calculations to experimental errors in nuclear constants employed, and not from errors in the theory. This, however, was not certain and some doubt was cast upon the adequacy of the methods employed. Hydride calculations, fortunately, were much more complicated than those for metal assemblies, and here the theorists had already demonstrated their ability to make accurate predictions.

The Gun

11.7 During the period under review relatively little new work had to be done on problems associated with the gun assembly. Group T-2 remained in charge of this work and prepared final calculations of the expected efficiency, including the predetonation probability. In the latter calculations use was made of the integral experimental data obtained by Group R-1 from the gun model experiment (12.24).

The Implosion

11.8 By the beginning of the period discussed in this chapter, most of the calculations relating to the ideal implosion (starting with a converging symmetrical shockwave at the outer edge of the tamper) had been completed, and it was possible to say that this part of the subject was almost completely understood. Implosion studies continued to be primarily the work of Group T-1.

11.9 Several new problems were set for the Theoretical Division. The first of these was to determine the effect of temperature on the course of the implosion. The second was the question of proving the stability of convergent shock wave. Since it would be very difficult to get experimental information on the irregularities produced inside a solid core, it would be necessary to rely entirely on indirect evidence and on theory. The third theoretical problem of major importance was to provide specifications and to help design a modulated neutron initiator.

11.10 On the first of these problems, the effect of shock-heating on the course of the implosion, calculation of equations of state for uranium was completed by April, and IBM calculations of the implosion thereafter included the effect of temperature. As a result efficiencies were decreased by a small amount, but less than had been anticipated.

11.11 On the question of stability of shocks, it was finally proven that plane shocks were stable, and the decay rate of irregularities was found. In the absence of a complete theory for convergent shocks, it was nevertheless possible to make rough estimates of the effects of instability.

11.12 In the design of the initiator, specifications were provided as to the initial neutron intensity required from the sudden mixing of α-n materials. By April 1945 the design for the initiator had been frozen.

11.13 As the time of the Trinity test approached, the main effort of Group T-1 went into an attempt to explain certain discrepancies between the experimental data obtained from implosion studies in G Division and the results of the latest and most comprehensive IBM calculations. Densities measured by the betatron and RaLa methods were somewhat lower than the theoretical values. Measured shock velocities were also lower than theoretical values. Material velocities measured by the electric pin method were in agreement with theory, while those measured by the magnetic method were lower than predicted. A thorough canvassing of all the assumptions used in calculation, and examination of possible experimental errors, led finally to the conclusion that the theoretical calculations should be revised downward somewhat, but not by an amount sufficient to alter the expected performance of the weapon very significantly.

Efficiency

11.14 During the period discussed in the present chapter the study of efficiencies, like that of other theoretical problems, moved from exploratory analysis intended to insure an essentially complete understanding of bomb physics, to final calculations of weapon design and performance, by the most reliable methods known. Efficiency calculations, in particular, carried the full responsibility of assuring the Laboratory that it was working toward an effective weapon, and of predicting as accurately as could be the efficiency of the bomb finally developed. In the nature of the case there could be no experimental verification of efficiency theory as a whole until the first test.

11.15 Several undertakings begun in the previous period were carried over and completed in the present period. The first of these was to examine carefully the effect of mixing between core and tamper, expected because of the Taylor instability at the interface.

11.16 Another type of calculation considerably improved upon was the prediction of the initial multiplication rate for supercritical assemblies. Allowance for several groups of neutron velocities, in particular, introduced a transient time-dependence of this rate in the initial stages of multiplication. The comparison of this theory with experiments of Groups R-1 and G-1 was not satisfactory and required some re-examination of theory and experiments before good agreement was reached.

11.17 During this period, also, the qualitative understanding of the effect of radiation on the course of the explosion was brought to completion; but reliable quantitative calculations remained impossible. Just before the Trinity test, therefore, the final efficiency prediction ignored this effect which, it was understood, might be responsible for a very much higher efficiency than that predicted.

11.18 Final efficiency calculations, published in the Theoretical Division Progress Report for June, gave a prediction that the yield at the Trinity test would lie between 5,000 and 13,000 tons of TNT. The actual yield of the Trinity explosion was certainly in excess of this upper limit. One quite successful measurement made during the test was the initial multiplication rate of the bomb as it exploded. Using this value and omitting some pessimistic assumptions made in arriving at the yield range quoted above, recalculation gave a value near 17,000 tons. Both because of the uncertainties in measuring the yield and also because of the large theoretical uncertainty introduced by ignoring the effects of radiation, the close agreement between this figure and the "official" yield of 17,000 tons conceals several unsolved problems. The first of these is the effect of radiation. The second is the proportion of the energy released converted into blast energy. Uncertainty about the latter relations makes it necessary to distinguish between the "nuclear" efficiency (fission energy released) and the "blast" efficiency, derived from the measurement of the blast wave. In view of the complexity of the efficiency problem and the unknown factors entering into it, the correspondence between theoretical and measured values was remarkably good.

11.19 There was somewhat less close agreement between the very successful result of the Trinity test and the subjective anticipations of many members of the Laboratory, who had by one argument or another prepared themselves against disappointing results. Shortly after the Trinity test, therefore, there was some discussion of possible unanticipated effects that might have accounted for the unexpectedly high yield. The most familiar of these was radiation. Another was the suggestion that the short-lived U^{239} formed in the tamper by neutron capture might be a slow-neutron fissioner, like U^{233}, U^{235}, or Pu^{239}. In the very high neutron flux present during the explosion, double neutron captures would be quite common, so that the tamper would act essentially as an explosive "breeder." Whether or not such a reaction is possible depends upon the fission cross section of U^{239}, which is difficult to measure because of the extremely short life of this element, and which has not been measured up to the time of writing.

Damage

11.20 Much more extensive investigation of the behavior and effects of a nuclear explosion were made during this period than had been possible before, tracing the history of the process from the initial expansion of the active material and tamper through the final stages. These investigations included the formation of the shock wave in air, the radiation history of the early stages of the explosion, the formation of the "ball of fire," the attenuation of the blast wave in air at greater distances, and the effects of blast and radiations of human beings and structures. Much of this information was of importance in making plans for the Trinity test. It was essential to know also the probable fate of plutonium and fission products in the ball of fire and the smoke cloud ascending out of it. These calculations, plus calculations of blast and radiation, were essential in planning experiments and observations at Trinity, and in planning for the protection of personnel. Theoretical studies of damage to structures and to personnel were, of course, made in anticipation of combat use. Extensive use in this connection was made of British data on damage to various kinds of structures caused by high explosive bombs. General responsibility for this work was given to Group T-7, with the advice and assistance of W. J. Penney.

Experiments

11.21 As in the earlier period reported (Ch. V), the consulting services of the Theoretical Division continued to occupy a considerable part of its time. Some of the more important lines of relationship have been discussed under the previous sections. Among the large number of neutron experiments on which the division gave advice or performed computations may be mentioned the Gun Model experiment of Group R-1, the scattering experiments of R-3, the integral tamper experiments of R-1 and R-3, and the multiplication experiments of R-1. For the Weapon Physics Division the theorists assisted with problems such as the design of RaLa experiments, the theory and analysis of magnetic pick-up data, and the focussing of x-rays for the x-ray method. Assistance was given, as before, in the design of various counters and detectors, and in calculating their efficiencies.

11.22 The most varied assistance was given to the operating groups engaged in the conduct of the Trinity experiment, all phases of which were under surveillance by the Theoretical Division which, as mentioned above, was responsible for preparing adequate order-of-magnitude calculations of the effects to be expected, and also for being sure that, in terms of these calculations, the experimental program was properly planned and coordinated.

11.23 In this place credit must be given to the computing group, T-5, for its essential services in obtaining numerical solutions, required in most theoretical investigations.

Chapter XII

RESEARCH DIVISION

Introduction

12.1 In the first period of this history the emphasis of the experimental physics program was placed on the measurement of quantities needed in determining the nuclear specifications of the bomb. By the end of this period two essential developments had occurred: the Laboratory had acquired a full view of the difficulties in the field of implosion dynamics, and was ready to begin the integral investigation of chain reacting systems. The first development was the most important organizing influence from this time on. A considerable number of experimental physicists went into the new G Division, to assist in the investigation of implosion dynamics. The second development also drew personnel out of the old Experimental Physics Division. A number of experimentalists went into G Division to work with hydride and later with metal critical assemblies. In addition, the Water Boiler Group and part of the Detector Group went into the new F Division. This left for the new experimental physics division, called the Research or R Division, four groups:

R-1	Cyclotron Group	R. R. Wilson
R-2	Electrostatic Generator Group	J. H. Williams
R-3	D-D Group	J. H. Manley
R-4	Radioactivity Group	E. Segrè

R. R. Wilson became Division Leader, while remaining Group Leader of the Cyclotron Group.

12.1 The program as well as the composition of the Research Division was affected by the new stage the Laboratory was entering. Some part of its work can be described as a continuation of the previous Experimental Physics

Division program. Increasingly, however, the experimental work reflected the maturing of the Laboratory program as a whole; differential experiments were carried out to investigate the finer points of the chain fission reaction, and an increasing number of semi-integral and integral experiments was made. Finally, in January 1945, Division R was asked to assist in preparation for the Trinity test scheduled for July 1945. With this all but the highest priority experiments were postponed, and the four groups began to develop instrumentation for the test. Their work on that project is described in Chapter XVIII. The account of the experimental physics resumed after the test (July 16) brings us close to the end of the period covered by the present history. The present chapter closes, therefore, with and account of work in progress in August 1945, or planned for the immediate future.

Neutron Number Measurements

12.3 One of the neutron measurements for which only preliminary data had been obtained in the first period was the comparison of neutron numbers from fissions induced by slow and fast neutrons. The Cyclotron Group, the Van de Graaff Group, and the D-D Group all made further comparisons. Of these experiments those of the Van de Graaff Group had the highest reliability; but all showed that there was no appreciable dependence of the neutron number on energy.

12.4 In September 1944 a sample of U^{233} was received and enough measurements made to show that it was a good potential bomb material. Its neutron number, in particular, was measured by the Cyclotron Group, and found to be slightly greater than that of U^{235}.

12.5 The only other direct measurement of a neutron number was that made by the Radioactivity Group, for spontaneous fission of Pu^{240}. Over a period of months enough data were gathered to show that the number of neutrons per fission was in the neighborhood of 2.5.

12.6 As incidental to the construction of apparatus to measure the decay constant of nearly critical assemblies of U^{235}, the Cyclotron Group measured the product of the neutron number times the fission cross section at high energies.

Spontaneous Fission Measurements

12.7 In the fall of 1944 the Radioactivity Group moved its spontaneous fission work from its old Pajarito Canyon Site to a new site, called the East Gate Laboratory. This change had the advantage of a much shorter commuting distance, and also of avoiding close contact with new high explosive firing sites, as the test area of the implosion program expanded toward Pajarito Canyon.

12.8 In the year between August 1944 and August 1945 spontaneous fission data were taken with a long list of heavy elements, including isotopes of thorium, protoactinium, uranium, neptunium, plutonium, and element 95. Careful measurements were made with a number of samples of plutonium, including early samples produced by cyclotron irradiation, later samples from the Clinton pile (including one re-irradiated sample), and still later samples of Hanford material. This work was necessary in order to know the neutron background coming from the spontaneous fission of Pu^{240} in the Hanford bomb material.

Fission Spectrum

12.9 The principal experiment performed during this period was a measurement by the Electrostatic Generator Group of the fission spectrum from fast neutrons impinging upon a plate of enriched uranium. These measurements were made by the photographic emulsion technique. They gave results in qualitative agreement with those obtained earlier by the same group using slow neutron initiation. The average energy of fission neutrons was somewhat higher; but it was not possible to decide whether this was a real difference, or caused only by difference in the experimental arrangements.

12.10 Work was begun by the Electrostatic Generator Group to investigate the low energy end of the fission spectrum, using the cloud chamber technique, a mock-fission source and a surrounding sphere of enriched uranium. Data were obtained which gave good agreement with extrapolations from earlier measurements. Completion of this experiment had to be postponed because of the pressure of Trinity work in March 1945. Measurements were completed, however, for the bare mock-fission source. In the low energy region its spectrum was relatively close to the fission spectrum.

12.11 Three mock-fission sources were built by the Radiochemistry

Group, using 2, 8, and 25 curies of polonium, the last giving 4×10^6 neutrons per second. These were used in the various sphere multiplication experiments (12.18-12.23).

Fission Cross Sections

12.12 One of the series of experiments completed in the last months of 1944 was a remeasurement of the fission cross sections of U^{235} and Pu^{239} as a function of energy. Earlier results had been accurate enough to show the existence of resonances at low energies and to indicate the general character of the energy dependence over the spectrum. This remeasurement was intended as a check on the earlier data and to obtain more accurate data in the intermediate energy region. As before, this work was done by the Cyclotron Group for thermal up to 1 kev neutrons, and by the Electrostatic Generator Group at high energies, collaborating with the D-D Group at the highest energies. The Cyclotron Group, in particular, measured both the fission and absorption cross sections. To obtain the latter they measured the transmission of neutrons, i.e., those not captured (or scattered) in passing through the fissionable material. Since absorption is fission plus radiative capture, this experiment gave additional data or radiative capture. An important physical result was that the ratio of radiation capture to fission was a sensitive function of energy.

12.13 These same groups also measured the fission cross section of U^{233} as a function of energy, as part of the program to estimate the virtues of this material. They found that at high energies its fission cross section was about twice that of U^{235}, placing it between U^{235} and Pu^{239} both in cross section and in neutron number.

12.14 As a continuation of investigation of fission thresholds, comparisons were made by the Electrostatic Generator Group of the fission cross sections of Np^{237} and U^{235}. The cross section of the former was measured from the threshold at about 350 to 400 kev up to 3 Mev.

Scattering Experiments

12.15 The principal scattering measurements in this period, as before, were the work of the D-D Group. Differential measurements of the type carried out earlier were continued. For this purpose a new directional proportional counter was developed with a higher directionality factor than the one previously used. Differential measurements were completed for some materials used in the bomb construction that had been incompletely measured before, namely, aluminum, cobalt, copper, and uranium. Finally, mention should be made of the measurement of scattering from the first amount of beta stage U^{235} large enough to provide data. Scattering turned out to be smaller than for normal uranium by an amount comparable to the difference of their average fission cross sections. This experiment was important negatively, in that if a large inelastic scattering cross section had been found in U^{235}, this would imply a longer time between fissions and hence smaller efficiency than expected.

12.16 The difficulty with differential scattering measurements was that they did not give reliable information on inelastic scattering as it would actually affect the operation of the bomb. A number of more nearly integral experiments were carried out by the D-D Group for this reason. In the principal series of these experiments, the technique used was to measure neutrons at the inner surface of a hollow spherical tamper, using a central D-D or photo-neutron source. By using source neutrons of different energies and detectors of different thresholds, it was possible to measure the integral energy degradation for various tampers.

12.17 Another means of attack on the same problem was the measurement of the "decay time" of tamper materials. A burst of neutrons sent into a tamper gives an intensity of reflected neutrons that falls off with time, and this can be measured by means of a time analyzer. What is measured here is again an integral effect, depending on the path and energy degradation of the scattered neutrons; but it is just this integral effect by which the time scale of the explosion is affected.

Multiplication Experiments

12.18 Multiplication experiments, using larger and larger spheres of U^{235} and Pu^{239} as these materials became available, were among the most important integral experiments made in this period. The technique common to the various experiments was the use of a mock fission source surrounded by a sphere of active material and in some cases tamper. These experiments were the work of the Electrostatic Generator Group and D-D Group. The interest of the latter group was largely the measurement of neutron distribution and inelastic scattering in core and tamper. Neutron multiplication was also measured in the process. More accurate measurement of neutron multiplication, plus the measurement of the average fission cross section for fission neutrons, plus data which set an upper limit to the value of the branching ratio for fission neutron energies, came from the experiments of the Electrostatic Generator Group. These experiments overlapped somewhat; for example, both groups measured the inelastic scattering in the spheres of active material, by comparing the spectrum of outcoming neutrons with that of the mock source and with earlier fission spectrum measurements, in which not enough material had been used to produce substantial energy degradation.

12.19 The ratio of neutron intensities with and without the surrounding sphere of fissionable material is a quantity which depends upon the average fission cross section, the neutron number, and the branching ratio. In order to set an upper limit to the last named quantity, the Electrostatic Generator Group measured the average fission cross section simultaneously with the multiplication. The method of doing this was to place foils or thin sheets of active material in the equatorial plane between the two hemispheres, so arranged that the area of exposed foil at a given radius was proportional to the volume of a shell of material at the same radius. In the untamped sphere measurements the hemispheres were separated by insulators and used as the two plates of a high pressure ionization chamber. One hemisphere with foil attached was kept at high voltage, while the other was used as the collecting electrode. In this way it was possible to count the fission fragments from the foil; the total number of fissions in the sphere was then this number multiplied by the ratio of sphere to foil masses.

12.20 Where a surrounding tamper was used the above technique was unwieldy; instead, thin plates of material were placed between the hemispheres, separated by cellophane "catchers." The number of fissions was estimated by measuring the radioactivity of fission fragments deposited on the cellophane.

12.21 One outcome of these experiments was the conclusion that the branching ratio averaged over fission neutron energies was small, as had been hoped on general theoretical grounds. This experiment measured the net increase of neutrons per neutron capture. Comparison of this with the net increase of neutrons per fission, known from earlier experiments to be essentially independent of energy, showed that the high energy branching ratio was quite small.

12.22 Another outcome was the possibility of extrapolating the sphere multiplications as a function of radius to the point where multiplication would become infinite, i.e., to the critical radius. These extrapolations gave, in fact, a prediction of the critical radius that was extremely close to the values obtained with the first metal critical assemblies (15.12).

12.23 Because of uncertainty as to the accuracy with which the mock fission source reproduced the true fission spectrum, the multiplication experiment was repeated by the Cyclotron Group for small spheres and by the Water Boiler Group in F Division for large spheres, using a true fission source. The results checked very closely (13.31).

Other Integral Experiments

12.24 In late 1944, an experiment was devised in the Cyclotron Group to measure the number of critical masses that could be disposed safely in the target and projectile of the gun assembly. In place of active material and tamper, a mock-up was used which for thermal neutrons imitated the absorbing and scattering properties of these materials. By this time it was possible accurately to calculate the critical mass of an untamped sphere. Hence a mock sphere was constructed which was equivalent to a just-critical metal sphere. The decay time of this sphere was measured with pulsed thermal neutrons. Once this measurement was made, comparison with odd-shaped tamped systems could be made, such as the gun projectile and target. This experiment not only determined approximately the number of critical masses that would be safe in the projectile and target, but also gave information on the change in degree of criticality as the projectile moved toward the target, and hence on the predetonation probability.

12.25 Another integral experiment made by the Cyclotron Group was the measurement of the multiplication rate as a function of the mass of active material. This experiment was performed by two methods, one devised by the RaLa Group of G Division (formerly the Detector Group of Experimental Physics), and one by the Cyclotron Group. The first was a Rossi type of experiment, in which the counting of a single neutron triggered the counting of further neutrons as a function of time. The second was a fast modulation experiment, in which the chain was started by a neutron pulse from the cyclotron and the decay of the burst measured as a function of time. The change in decay time for small changes in the degree of criticality of the system is thus measured in the near-critical region. According to theory the curve so obtained can be extrapolated into the supercritical position. Both U^{235} with a tamper and Pu^{239} with a tamper were measured in this experiment. The Pu^{239} sphere was used in the Trinity test shortly thereafter. The U^{235} measurement permitted extrapolation to the number of critical masses in the assembled Hiroshima bomb. This made possible a semi-empirical prediction of its efficiency. The equipment used in this experiment exemplified the counting techniques carried to a high point of development at Los

Alamos. The cyclotron beam was modulated to give pulses 0.1 μsec long. Time resolution of 0.06 μsec could be obtained in the counting channels.

Miscellaneous Experiments

12.26 Capture cross section measurements were continued. The Radioactivity Group made differential measurements at various energies, and the Electrostatic Generator Group measured average capture cross sections in spheres surrounding a mock-fission source. Neutron capture in tantalum, which seemed to show some anomalies, was investigated by both groups.

12.27 Mass spectrographic analysis and neutron assay of fissionable material was continued as a routine matter. The Pu^{240} content of new batches of plutonium was measured as they arrived, and searches were made for the rarer isotopes of uranium and plutonium. Mass spectrographic work was attached to the Electrostatic Generator Group, neutron assay to the Radioactivity Group.

12.28 As incidental to the investigation of fission properties of U^{233}, its half-life was measured by the Radioactivity Group. The same group continued its investigation of gamma radiation and alpha particles emitted with fission. They also measured the gamma radiation of radio-lanthanum, needed in connection with the RaLa measurements of the implosion. This group also measured neutron background from assembled initiators as these were constructed.

Chapter XIII

F DIVISION

Introduction

13.1 As part of the administrative reorganization of the Laboratory, F Division was formed in September 1944, shortly after Fermi's arrival from Chicago. As Associate Director, Fermi was given general responsibility for the theoretical and nuclear physics research of the Laboratory. As Division Leader of F Division he was given the directive to investigate potentially fruitful lines of development not included under the main program of the Laboratory. This responsibility included the Super in its theoretical and experimental aspects and means of fission bomb assembly alternative to the gun and the implosion. Because of Fermi's previous association with pile development, the Water Boiler Group was also placed in this division. The last group in the division was added in February 1945 to do experimental work with the high power Water Boiler as a neutron source, and to prepare for the measurement of fission fragments at the Trinity test. The work of F Division in the Trinity test is reported in Chapter XVIII.

13.2 The group organization was as follows:

F-1	The Super and General Theory	E. Teller
F-2	The Water Boiler	L. D. P. King
F-3	Super Experimentation	E. Bretscher
F-4	Fission Studies	H. L. Anderson

The Super

13.3 During June 1944 Teller's group had been separated from the Theoretical Physics Division and placed in an independent position, reporting to the Director. This separation was a recognition of the exploratory character of this group's work as contrasted with that of the Theoretical Division generally, which had, primarily, responsibility for obtaining design data for fission bombs. Again in September 1944 Teller's group became the theoretical branch of the new F Division, created under Fermi.

13.4 Theoretical work on the Super from this time was without essential surprises. The analysis of the thermonuclear reaction became more quantitative and concrete. Increasing attention was given to the theory of detonation mechanisms. Work reached its highest intensity in the spring of 1945 and continued for several months after the end of the war and the period covered by this report.

13.5 Various models were investigated. The end sought was a bomb burning about a cubic meter of liquid deuterium. For such a bomb the energy-release will be about ten million tons of TNT.

DAMAGE

13.6 No account of the Super development at Los Alamos can be complete without some account of estimates of damage. It must be emphasized that these considerations are essentially qualitative. In fact with energies of the order contemplated, the effects of explosions begin to enter a new range, which may make necessary some account of meteorological and geological phenomena normally beyond human control. Under these circumstances accurate calculation is less important than a thorough canvassing of the possibilities. The following account is highly tentative, both quantitatively and in degree of thoroughness.

13.7 The ten million ton Super described above would not be the largest explosion seen on the Earth. Volcanic explosions and the collision of large meteorites such as the Arizona or Siberian have undoubtedly produced larger blast energies, perhaps a thousand or ten thousand times larger. On the other hand these explosions were very cool compared to a thermonuclear explosion, and correspondingly more familiar in their effects.

13.8 The blast effects from a ten million ton Super can be scaled up from the known damage at Hiroshima and Nagasaki. Taking the destroyed area from a ten thousand ton bomb to be ten square miles, the Super should produce equal blast destruction over a thousand square mile area. This would be more than enough to saturate the largest metropolitan areas.

13.9 More widespread ground damage would perhaps result from an explosion underground or underwater near a continental shelf. Since it is estimated that a severe earthquake produces energies of the same order as the Super, the surface effects might be comparable. To produce these effects would require ignition at a very great depth, of the order of several miles.

13.10 This bomb begins to reach the upper limit for blast destruction that is possible from detonation in air. Just as a fission bomb exploded in shallow water will have its radius of destruction in water limited by the depth at which it is exploded (14.18), so with a Super in the atmosphere. It "blows a hole" in the atmosphere, so that the maximum radius of destruction is comparable to the depth of the atmosphere.

13.11 Neutrons and gamma rays from the Super would not be a significant part of its damage; their intensity falls off more rapidly with distance than the blast effects. Even at Hiroshima and Nagasaki they did not cause a large percentage of casualties. From a larger bomb their effects would be greater, but not proportionately greater.

13.12 The effects of visible radiation, on the other hand, fall off less rapidly than blast effects. This destruction can, in fact, be made directly proportional to the energy release. While blast damage can be increased a hundredfold, visible radiation damage can be increased a thousandfold. For the first purpose the bomb would be detonated about ten times higher than at Hiroshima and Nagasaki, for the second about thirty times higher. And the real point of the latter method is that there is no limit to the possibility of detonating larger bombs at higher altitudes. Thus a Super which burned a ten-meter cube of deuterium at a height of three hundred miles would equal in effect a thousand "ordinary" Supers detonated at ten-mile altitudes. In both cases the area of damage would be in the neighborhood of a million square miles. It should, of course, be emphasized that such a high altitude weapon is at the present time only a theoretical possibility.

13.13 It is difficult to estimate damage from visible radiation. In Hiroshima and Nagasaki the total effect was a composite of blast, gamma radiation, and visible radiation. The last was sufficiently intense to ignite wooden structures over an area of a square mile or so. Casualties from visible radiation alone would be considerably smaller, because of the protecting effect of clothing and walls. Effects from a Super would be comparable, and either more or less intense depending on the relative military importance of extensive versus intensive burning. The figures already given would correspond to an intensity about the same as that at Hiroshima and Nagasaki.

13.14 The most world-wide destruction could come from radioactive poisons. It has been estimated that the detonation of 10,000 to 100,000 fission bombs would bring the radioactive content of the Earth's atmosphere to a dangerously high level. If a Super were designed containing a large amount of U^{238} to catch its neutrons and add fission energy to that of the thermonuclear reaction, it would require only in the neighborhood of 10 to 100 Supers of this type to produce an equivalent atmospheric radioactivity. Presumably Supers of this type would not be used in warfare for just this reason. Without the uranium, poisonous radioactive elements could be produced only by absorption; for example C^{14} could be produced in the atmosphere; not, however, in dangerous amounts. Poisoning, moreover, would be obviated by detonation above the atmosphere, which is in any case the region in which the general destructive effects of the Super seem greatest.

Other Theoretical Topics

13.15 The gloomy prospects of the implosion in the fall and winter of 1944-45 made it desirable again to investigate autocatalytic and other possible methods of weapon assembly. This whole subject, which had been investigated earlier, had been given up because of the uniformly low efficiencies indicated. The operating mechanism of autocatalysis makes use of neutron absorbers which are removed in the course of the initial explosion. Thus, for example, one or more paraffin spheres coated with B^{10} may be placed inside the fissionable material, in such a way that the whole assembly is just subcritical. If by some means a chain reaction is started, the heating of the material will result in the compression of the boron "bubbles," the reduction of the neutron absorbing area, and a consequent increase in the degree of criticality. Thus in principle the progress of the explosion creates conditions favorable to its further progress. Unfortunately, the autocatalytic effect is not large enough to compensate for the poor initial conditions of this type of explosion, and the result is not impressive.

13.16 Another type of assembly mechanism examined was one that made use of shaped charges to attain much higher velocities for a slug of active material than would be possible with conventional gun mechanisms. This method also gave low efficiency when calculated for the high neutron background of Hanford plutonium.

13.17 Another topic of continuing interest was the possibility of various types of controlled or partially controlled nuclear explosions, which would bridge the gap between such experiments as the "dragon" (15.7) and the final weapon.

13.18 Some time was spent on safety calculations for the K-25 diffusion plant, principally on estimations of critical assemblies of enriched uranium hexafluoride under various conditions and degrees of enrichment.

13.19 A topic of interest in connection with the Trinity test was the formation of chemical compounds in air by the nuclear explosion. Such compounds as oxides of nitrogen and ozone are poisonous, and the quantity produced had to be estimated. It was also anticipated that they would effect the radiation history of the explosion, which was to be examined spectrographically.

Deuterium and Tritium Reaction Cross Sections

13.20 The low energy cross section of the T-D reaction was found to be higher than extrapolation from high energy data had indicated. This discovery, which considerably lowered the ignition temperature of T-D mixtures, was the result of work undertaken at the beginning of the period under review by Group F-3, the Super Experimentation Group.

13.21 Since both the T-D and the D-D cross sections at low energies were known only by rather dubious extrapolation, it was planned to measure them simultaneously.

13.22 The first series of measurements was made with a small (50 kev) Cockcroft-Walton accelerator constructed for the purpose at Los Alamos. With this equipment experiments were carried out in the region from 15 to 50 kev. The quantity measured was the total number of disintegrations as a function of the bombarding energy, from which the reaction cross sections could be derived. In both cases the target used was made of heavy ice cooled with liquid nitrogen. The D-D reaction was produced by a deuterium ion beam, and the protons produced in the reaction measured. In the case of the T-D reaction the procedure was analogous, except that special precautions had to be taken to conserve the small amount of tritium available as an ion source. In this case the alpha particles from the reaction were counted.

13.23 The result of these measurements was that the extrapolated values of the D-D cross section were shown to be approximately correct. The tritium cross section, however, was very much larger than had been anticipated at energies of interest.

13.24 These measurements were later (after the end of the period under review) extended to the 100 kev region, using a larger accelerator constructed for the purpose.

The Water Boiler

13.25 Upon the completion of the series of Water Boiler experiments described in Chapter VI, it was decided to develop a higher power boiler to be used as a strong neutron source for various experiments. A power of 5 kilowatts was chosen as a suitable value. The original 10 kilowatt design was modified considerably. The essential design features were completed in October and construction of concrete foundations and shields begun. The boiler was built and in operation in December 1944.

13.26 The power level for which the boiler was designed was chosen because this was attainable with the amount of enriched material available at the time, because the cooling requirements would be simple, and because the chance of trouble from frothing or large gas evolution caused by electrolysis of the solution would be small. Such a boiler was calculated to give a flux of 5×10^{10} neutrons per square centimeter per second.

13.27 A number of changes in design were made from that of the low power boiler, and some from the original 10 kilowatt design. The solution used was uranyl nitrate rather than uranyl sulfate. The main reason for this was the greater ease with which the nitrate could be decontaminated if that should prove necessary (17.37). Additional control rods were installed for increased flexibility of operation. Water cooling and air flushing systems were installed, the latter as a means for removing gaseous fission products. The boiler had, finally, to be carefully shielded because of gamma radiation and neutrons.

13.28 It turned out that decontamination of the boiler was unnecessary, even after 2500 kilowatt-hours of operation. This was caused in part by the success of the air flushing system, which removed some 30% of the fission products, and in part by the absence of corrosion of the stainless steel container.

13.29 The tamper of the high power boiler was chosen on the basis of tamper experiments performed with the low power boiler before it was torn down. Partly because of the difficulty of procuring the needed amount of beryllia, and partly because of the (γ, n) reaction in beryllium which it was desirable to avoid, the tamper chosen was only a core of beryllia bricks, surrounded by a layer of graphite.

13.30 The power boiler was equipped with a graphite block for thermalizing fission neutrons.

Neutron Physics Experiments

13.31 It has been mentioned in Chapter XII (12.23) that the important sphere multiplication experiments which were made first in the Electrostatic Generator Group were repeated and verified in F Division. These experiments were performed independently by the Water Boiler Group and the F-4 Group. In both experiments a source of fission neutrons was obtained by feeding a beam of thermal neutrons from the Water Boiler and graphite block on to a target of U^{235} in the center of the $3\frac{1}{2}$" and $4\frac{1}{2}$" U^{235} spheres. In the Water Boiler Group the fissions in the source and throughout the sphere were measured by a technique similar to that used by the Electrostatic Generator Group, catching the fission fragments on cellophane foils. In the experiments of F-4 the fissions produced were measured by means of a small fission chamber placed at various radial distances from the center. In these experiments the U^{235} target was itself a small fission chamber identical with that used to measure fissions in the sphere. Comparison of fissions in the source chamber with those in the detecting chamber at various distances gave the multiplication rate. Of these two experiments the first gave results closer to those of the Electrostatic Generator Group, and to the final empirically established values of the critical mass.

13.32 Several thermal cross section measurements for the various elements were made, using the high neutron flux from the boiler. One was the absorption cross section of U^{233}. The thermal scattering cross sections of U^{235} and Pu^{239} were measured, and in the course of these measurements cross sections were also obtained for a large number of other elements.

13.33 In order to make calibrations for the measurement of gamma ray and neutron intensities at the Trinity test, the Water Boiler Group made measurements of delayed neutron and gamma ray emission from samples of Pu^{239}, as a function of the delay time (i.e., the time after irradiation). These experiments made use of a rather spectacular technique, which was to shoot a slug of material with a pneumatic gun into a pipe through the middle of the boiler, and measure the decay of activity with time by means of an ionization chamber for gamma rays and a boron trifluoride counter for neutrons.

13.34 In addition to providing a strong neutron source for the experiments described above, the Water Boiler also was used to make neutron irradiations for other groups in the Laboratory.

Chapter XIV

ORDNANCE DIVISION

Introduction

14.1 As a result of the August 1944 reorganization of the Laboratory, three groups of the old Ordnance Division were transferred to the new Explosives Division: the Implosion Experimentation Group, the High Explosives Development Group, and the S Site Group. Two groups, the Instrumentation Group and the RaLa and Electric Detonator Group, became part of the new Weapon Physics Division. The remaining six groups (7.1) constituted the new Ordnance Division, but with the old Proving Ground, and Projectile, Target, and Source Groups combined as a single Gun Group. Added shortly were two new groups, one to investigate the possibilities of underwater explosion of the weapon and to compile bombing tables for the Little Boy and Fat Man, and one as a special ordnance procurement group. By the end of September the organization of the Ordnance Division was as follows:

O-1	The Gun Group	A. F. Birch
O-2	Delivery	N. F. Ramsey
O-3	Fuse Development	R. B. Brode
O-4	Engineering	G. Galloway
O-5	Calculations	J. O. Hirschfelder
O-6	Water Delivery, Exterior Ballistics	M. M. Shapiro
O-7	Procurement	Lt. Col. R. W. Lockridge

14.2 During the period under review the activities of the Ordnance Division followed two paths. One was the completion of its earlier research and design activities, and the other was its increasing weapon test program and preparation for final delivery. In March 1945 this second activity was formalized as Project Alberta. For the sake of continuity, however, the test and delivery programs of O Division are described in the chapter on the Alberta Project (Chapter XIX) for the entire period under review. This leaves for the present chapter the completion of certain topics continued from the earlier history of the Ordnance Division which, although increasingly connected with the work of the test program, deserve separate treatment. In the sections following, the account of the gun development program is completed, as is that of arming and fusing, bomb ballistics, and the study of surface and underwater explosions. The work of two groups is omitted. One is the Calculations Group, originally established to compute pressure-travel curves for the gun (7.21). By the beginning of the period now reviewed the work of this group was essentially complete. Not long after it became attached, on a part time basis, to the Theoretical Division as Group T-7 (11.3).

14.3 The second group whose activities are not separately discussed is the Engineering Group. Not long after the beginning of this period the main design of the outer case for both the Little Boy and the Fat Man had been frozen. Apart from the engineering service activities for the division, this group designed the many detailed modifications of the outer case and layout of the Fat Man that became necessary as design of inner components progressed and as the test program revealed weaknesses in earlier designs.

14.4 While the administrative difficulties of the earlier period did not entirely disappear, the Engineering Group was relieved of the burdens which had been responsible for most of its earlier troubles (7.40 ff). The general coordination of the weapon program was taken over by the Weapons Committee (9.10). The administration of shops was placed under the new Shop Group of the Administrative Division. It remained the responsibility of the Ordnance engineers to coordinate design of the Fat Man, under the general supervision of the Weapons Committee. George Galloway remained in charge of the group from the time he took over, in September 1944, to the end of this period. To this group must be credited the design of the outer components of the Fat Man. The corresponding elements of the Little Boy were designed primarily within the Gun Group itself.

Gun Assembly

14.5 With the abandonment of the high velocity gun project (for plutonium) in August 1944, the emphasis on gun work was put on making a weapon out of the gun for U^{235}. All work on guns, targets, projectiles, initiators, and bomb-assembly for the gun was then consolidated in one group under Comdr. F. Birch. Because of the current uncertainty as to how plutonium could be used, the objective of the group was to produce as reliable an assembled weapon as possible, for which field operations would be as simple as possible, so that at least the U^{235} that was being produced could be used effectively.

14.6 At this time there was still considerable uncertainty as to the isotopic concentration in which the uranium would be received and a consequent uncertainty as to the critical mass. The previous experimentation with the higher velocity, however, had abolished almost all other uncertainties of more fundamental nature. Thus there was no essential problem in projectile, target, and initiator design or in interior ballistics. The problems were, rather, how to make this unit serviceworthy and how to establish proof of the overall assembly. And for the part of this work that depended upon the mechanical properties of the active material, the normal uranium metal was a perfect substitute.

14.7 One lesson that had been learned from the previous year was that it required six months to procure new guns. Thus the guns that had been ordered in March arrived in October, and the special mount arrived somewhat later. It was December before active proof work on the new model could be started. The proof of tubes, as such, consisted of instrumented firing of each tube two or three times at 1000 feet per second with a 200 pound projectile. They were then greased and stored for future use. A few tubes were used in connection with other experimentation. Notable among other tests were the proof of full scale targets and the determination of the delay between the application of the firing current and the emergence of the projectile. In addition to these "live" barrels, a large number of dummies were procured for use in drop tests of the assembled bombs. These guns, which were made mostly from discarded Naval guns, were not meant for firing and required no "proof."

14.8 Although the gun presented no new ballistics problem, it was far from a conventional gun in appearance. It weighed only about half a ton, was 6 feet long, and had a large thread on its muzzle. Two types of these guns were originally designed and made: Type A, of high alloy steel, not radially expanded, and with three primers inserted radially; and Type B, of more ordinary steel, radially expanded, and with the primers inserted in the "mushroom" (nose of breech assembly). The same primers and same general type of propellant that had been proved in an earlier gun were adapted. The Type B gun was readily selected for further production because of its somewhat lighter weight, and particularly because the process of radial expansion is an excellent test of the quality of the forging.

14.9 In the interval between August and December, the proving work was done on targets, projectiles, and initiators at reduced scale. The laws of dimensional scaling work out quite well, and a large amount of design research was done on the 3"/50 and on the 20 millimeter Hispano guns. This was a particularly acceptable procedure for testing tampers. Many other parameters were varied, extensively in 20 millimeter tests, less extensively at 3 inch scale, and tested, using substitute materials. The result of this program was that, by the time the actual gun arrived, a reasonably firm design of the target and projectile components had been established.

14.10 From December on, practically all firing was done at full scale. This was done to determine whether the results at smaller scale were misleading, as they might be because of the inability to duplicate heat treatments at different linear scales. The target cases for full scale tests were impressive objects and difficult to handle, particularly to take apart for inspection after the shot. The latter difficulty was alleviated by development of a tapered assembly that could be pushed apart hydraulically. In this way, the very good outer cases of high alloy steel could be used again. In fact, a most amazing development in the history of the target cases is that the first case ever to be tried proved to be the best ever made. This case was used four times at Anchor Ranch Range and subsequently fitted to the bomb and dropped on Hiroshima. Certain failures in subsequent target cases of the same design emphasized the importance of careful heat-treating and led to slight modifications of design. In general, the pieces that were heat-treated at Site Y were far superior to those procured from industry. As in the case of guns, a large number of dummy targets had to be made for the drop tests of the assembled bomb, and these were not made very carefully. The fact that they shattered when the projectile seated, in those drops where a live gun was used, only aided in recovering the inner portions for study.

14.11 By far the most extensive program in this group was the engineering and proving of assembled units. This work consisted of ironing out the mechanical integration of the bomb in cooperation with the Fusing Group and the Delivery Group. Various stages of completeness in the assembly were required, depending upon the completeness of the test. Thus, the practice drops ranged from tests of fusing and informers and bomb ballistics in which dummy guns and targets were employed to drops of units that were complete except that ordinary uranium was used in place of the active variety. In the latter tests, the bomb was dug up for further study of the assembly.

14.12 There were no major changes in bomb design in this period. In fact, the design was frozen in February 1945. In order that no one outside the contractor would possess the complete design, the heavy fabrication was divided among three independent plants. The gun and breech were made at the Naval Gun Factory; the target case, its adaptor to the muzzle threads. and its suspension lug were made by the Naval Ordnance Plant at Centerline, Michigan; and the bomb tail, fairing, and various mounting brackets were made by the Expert Tool and Die Company, Detroit, Michigan. Contact with the latter firms was maintained through the Project's Engineering Office in Detroit. (7.12) Smaller components, such as the projectile, target inserts, and fuse elements were either made or modified for their ultimate use at Site Y.

14.13 The component parts were assembled at Wendover Field, Utah, in preparation for drop tests. Some assemblies were made at Site Y, however, both for preliminary experience and for instrumented ground tests. An assembled gun and target system was fired at Anchor Range in free recoil with entirely successful results. At Wendover, thirty-two successful drops were made, and in only one drop did the gun fail to fire. This was traced to a mistake in electrical connections.

14.14 The airborne tests led to one revision of design for the breech of the gun. It was desirable to be able to load the gun after take-off, or unload it before landing with an active unit. The original design did not permit this under flight conditions, so the breech was modified to permit loading and unloading of the powder bags by one man in the bomb bay of the plane.

Arming and Fusing

14.15 In the period under review the center of activities for the Fuse Group was the test program at Wendover Field, Utah. The final and main series of tests began in October 1944 and continued through May 1945. By the beginning of these tests the over-all design of the arming and fusing system, begun in April 1944, was completed. The following is a general description of the fusing system as finally developed. Its main component was the modified APS/13 tail warning device, called "Archie." This radar device would close a relay at a predetermined altitude above the target. Four such units were used in each fuse, with a network of relays so arranged that when any two of the units fired, the device would send a firing signal into the next stage. This stage consisted of a bank of clock-operated switches, started by arming wires which were pulled out of the clocks when the bomb dropped from the plane's bomb bay. These clock switches were not closed until 15 seconds after the bomb was released. Their purpose was to prevent detonation in case the A units were fired by signals reflected from the plane. A second arming device was a pressure switch, which did not close until subject to a pressure corresponding to 7000 feet altitude. In the gun weapon, the firing signal went directly to the gun primers; in the implosion weapon, this signal actuated the electronic switch which closed the high voltage firing circuit.

14.16 Alternative to the Archie, but with a lower altitude range, was another radar device developed at the University of Michigan, the PMR or "Amos" unit. This was stand-by equipment, or equipment that could be used if a change of strategy should favor lower altitude firing. It was also tested and found fairly satisfactory, but the tests were not extensive. The above is a very brief account of a complicated test program, involving tests of much subsidiary equipment connected with the firing circuit for the two bombs and radio informers in the bomb case to signal information to observers. As in all parts of Project Alberta, this program served to train combat crews.

Bomb Ballistics

14.17 One of the tasks carried out at Los Alamos was the construction of bombing tables for the Little Boy and Fat Man. For this work the necessary ballistic data were obtained from field measurements at Sandy Beach, the Salton Sea Naval Air Station, a small rocket testing station, used by Los Alamos because it afforded an approach over water nearly at sea level, simulating the conditions which would be encountered over Japan. Similar data were obtained for the blockbuster "pumpkin" program by the Camel project at Inyokern, and the two groups were in consultation on techniques of measurements and the data obtained. The work at Los Alamos was in the hands of Group O-6.

Surface and Underwater Explosions

14.18 As mentioned earlier, one of the tactical uses considered for the bomb was as a weapon against harbors, by surface or underwater detonation. At about the beginning of the period under review, it became clear that surface or shallow underwater detonation would expend a large part of the energy of the explosion in producing cavitation, and relatively little would go into shock wave in water. To maximize shock-wave damage it would be necessary to detonate much deeper than would be possible in harbors.

14.19 The effects of shallow explosions were investigated experimentally at a very small scale by the sudden withdrawal of an immersed cylinder which resulted in the creation, and sudden collapse, of a cylindrical cavity in water. Later experiments were carried out with amounts of explosive of a few ounces at a depth of one or two feet. The amplitudes of gravity waves produced in these tests could be scaled up to give rough agreement with existing data on underwater explosions of several hundred pounds of explosive. From this point the results were scaled up to explosions of the order of magnitude of interest to the Laboratory.

14.20 From the experimental data it was discovered, contrary to expectation, that a surface explosion produced larger gravity waves than a subsurface explosion of the same size. From a theoretical analysis, scaling laws were derived which made it possible to predict with some assurance the effects of the surface or near-surface detonation of atomic bombs. This program was the work of the Water Delivery and Exterior Ballistic Group, with the assistance of Penney and von Neumann. It had been begun at the end of the previous period by McMillan.

Chapter XV

WEAPON PHYSICS DIVISION

Introduction

15.1 At the time of its organization in August 1944, the Weapon Physics or G Division (G for gadget, code for weapon) was given a directive to carry out experiments on the critical assembly of active materials, to devise methods for the study of the implosion, and to exploit these methods to gain information about the implosion. In April 1945, the G Division directive was extended to include the responsibility for the design and procurement of the implosion tamper, as well as the active core. In addition to its primary work with critical assemblies and implosion studies, G Division undertook the design and testing of an implosion initiator and of electric detonators for the high explosive. The Electronics Group was transferred from the Experimental Physics Division to G Division, and the Photographic Section of the Ordnance Division became G Division's Photographic Group.

15.2 The initial organization of the division, unchanged during the year which this account covers, was as follows:

G-1	Critical Assemblies	O. R. Frisch
G-2	The X-Ray Method	L. W. Parratt
G-3	The Magnetic Method	E. W. McMillan
G-4	Electronics	W. A. Higginbotham
G-5	The Betatron Method	S. H. Neddermeyer
G-6	The RaLa Method	B. Rossi
G-7	Electric Detonators	L. W. Alvarez
G-8	The Electric Method	D. K. Froman
G-9	(Absorbed in Group G-1)	
G-10	Initiator Group	C. L. Critchfield
G-11	Optics	J. E. Mack

15.3 For the work of G Division a large new laboratory building was constructed, Gamma Building. New firing sites were established, with small laboratory buildings associated with them (see map Appendix No. 3). Most of the work of G Division occupied new office, laboratory, and field facilities; despite this, its work was well under way by the beginning of October 1944.

Critical Assemblies

15.4 The work of the Critical Assemblies Group was carried out at Omega Site, (6.64 ff) where it shared space with the Water Boiler Group. Its main work was to carry out experiments with critical amounts of active materials, including both hydrides and metals. It was given the further responsibility of investigating the necessary precautions to be observed in the handling and fabrication of active materials at Los Alamos, to be certain that in these operations no uncontrolled nuclear reactions could occur. When G Division acquired the definite responsibility of designing and preparing the core and tamper - the "pit assembly" - of the Trinity and subsequent implosion bombs, members of the Critical Assemblies Group were given this responsibility.

15.5 During the early period of this group's existence, a large number of critical assemblies were made with various uranium hydride mixtures. A relatively large amount of effort was spent in investigating these assemblies for two reasons. The first was that there was not yet enough material for a metal critical assembly without hydrogen. The second was that by successively lowering the hydrogen content of the material as more U^{235} became available, experience was gained with faster and faster reactions. It was also still not ruled out, at this time, that hydride bombs using small amounts of material might be built.

15.6 By November 1944 enough hydride-plastic cubes of composition UH_{10} had been accumulated to make a cubical reacting assembly in the beryllia tamper, if the effective composition was reduced to UH_{80} by stacking seven polythene cubes for each cube of UH_{10} plastic. Further experiments were made with less hydrogen and other tampers. In February 1944 this hydride was sent back to the chemists and metallurgists for recovery and conversion to metal, and the program of hydride critical assemblies was ended.

15.7 The most spectacular experiments performed with the hydride were those in which a slug of UH_{30} was dropped through the center of an almost critical assembly of UH_{30}, so that for a short time the assembly was supercritical for prompt neutrons alone. This experiment was called "tickling the dragon's tail," or simply the "dragon." The velocity of the falling slug was measured electrically. Before the experiment was actually performed a number of tests were made to prove that it was safe, for example that the plastic would not expand under strong neutron irradiation, thus causing the slug to stick and cause an explosion. On January 18, 1945, strong neutron bursts were obtained, of the order of 10^{12} neutrons.

15.8 These experiments gave direct evidence of an explosive chain reaction. They gave an energy production up to twenty million watts, with a temperature rise in the hydride up to 2°C per millisecond. The strongest burst obtained produced 10^{15} neutrons. The dragon is of historical importance. It was the first controlled nuclear reaction which was supercritical with prompt neutrons alone.

15.9 Because of the intensity and short duration of the bursts obtained, better measurements of delayed neutrons were possible than had been made previously. Several short periods of delayed emission were found, down to about 10 milliseconds, that had not been reported before. These experiments suggested a promising future method for producing modulated bursts of fast neutrons.

15.10 The Critical Assemblies Group made large numbers of safety tests for other groups. Most of these involved placing various amounts of enriched uranium with various geometries in water, in order to determine the conditions under which the accidental flooding of active material might be dangerous. During the course of these tests the first accident with critical materials occurred. A large amount of enriched uranium, surrounded by polythene, had been placed in a container to which water was being slowly admitted. The critical condition was reached sooner than expected, and before the water level could be sufficiently lowered the reaction became quite intense. No ill effects were felt by the men involved, although one lost a little of the hair on his head. The material was so radioactive for several days that experiments planned for those days had to be postponed.

15.11 Similar safety tests were made on models of the gun assembly. These tests were made because water immersion of the bomb might occur accidentally in transporting the bomb or in jettisoning it from aircraft. Immersion could also be thought of as the limiting case of wetting from other possible sources.

15.12 The number of tests made with U^{235} metal assemblies was much larger than those with Pu^{239}, since sufficient plutonium was available only rather late. The first critical assembly of the latter material, which was of a water solution of Pu^{239} with a beryllia tamper, was made in April 1945. By fabricating larger and larger spheres of plutonium and inserting these in a bomb mock-up, the critical mass in a bomb was determined.

15.13 In April 1945 when G Division was given the definite responsibility of designing the pit assembly of the bomb, the "G Engineers," Morrison and Holloway, were taken for this work from the Critical Assemblies Group. The G Engineers worked with the design staffs of X and O Divisions in the final detailed design of the implosion bomb.

Implosion Studies

THE X-RAY METHOD

15.14 The X-raying of small spherical charges was developed in the Ordnance Division and was in successful use in August 1944 (7.57). At this time the small scale work was placed in X Division, and the G Division X-ray Group was formed to extend this method of implosion study to larger scale. In addition to this development work the G Group continued a number of service activities, such as the servicing and improvement of the X-ray tubes for the X Division work. Other demands made upon them included the radiography of explosive charges for the RaLa Group. Radiographic examination of the final weapon tamper was also a problem for this group near the end of this period, but the work was done with radioactive sources of gamma rays rather than with X-rays.

15.15 In the early part of the new program the chief objective in the development work was to adapt X-ray techniques to large scale implosions (up to 200 pounds of high explosives). It was proposed actually to follow the course of the implosion by detecting the incidence of the X-rays as a function of time, using a grid of small Geiger counters. These counters were to be either of 1 or 3 millimeters in diameter, and disposed in the form of a cross in the X-ray shadow of the object. The principal aims of the projected program were (1) reliable action of the counters, and (2) reduction of the amount of scattered radiation that would be recorded. These objectives were pursued relentlessly, but because of the great technical difficulties involved, with little real success. The program was dropped in March 1945. It had become clear that, although the difficulties might not be insurmountable, there was not sufficient justification for retaining the highly trained personnel required for this work. At this time also it had become clear that essentially the same type of information was obtainable by repeated exposures with the betatron (15.23 ff).

15.16 Although the development of the counters and their electronic circuits and the reduction of the scattered radiation were uncertain of success in the new X-ray program, preparation of a field site where the technique could be tested was completed by early fall 1944. This site was called P Site. The protection required for X-ray equipment near exploding charges was designed so that it would be useful in other X-ray experiments as well. As a preparation for the final use of the X-ray and counter technique, the problems of using the magnetic method (15.18) in conjunction with the X-ray method were solved at P Site. The first combined X-ray and magnetic record was obtained in late January 1945. The technique was directly applicable to the combination of betatron and magnetic records (15.26).

15.17 As emphasis on lens design increased, flash X-ray photography was used to study the detonation waves. These studies paralleled those of the X-ray section of the Implosion Studies Group X-1. In April 1945 a second X-ray team was placed on the initiator problem. Altogether, a large amount of important experimental work on initiators was accomplished. The use of P Site for the initiator program branched out into various recovery experiments carried out there, and finally to the installation of the alpha counting experiment (15.38).

THE MAGNETIC METHOD

15.18 By August 1944 the magnetic method had been established as a practical way of determining the velocity of the external metal surface of an imploding sphere (7.57). By integration the average compression could also be obtained. The development of the method was directed along three main lines: (1) the improvement of the instrumentation and adaption of the method to large charges with electric detonation; (2) cooperation with RaLa, X-ray and betatron experiments to get coordinated records; and (3) the development of new methods.

15.19 The first part of the program started with the construction of a separate proving ground in the Pajarito Canyon (see Site Map). This was completed in December 1944. Meanwhile a great deal of work was done in the laboratory in improving the circuits and developing shielding techniques. When the field work got under way it was found that the main problem was to protect the magnetic record against spurious signals caused by the electric detonators and by the static charges developed in the explosion. New results were obtained when it became possible to "purify" the magnetic records and interpret their details. It was found that several reflected shock waves from the metal core could be recognized. The intersection of detonation waves also produced reliable signals.

15.20 The magnetic techniques were adapted to larger charges, more detonation points, and finally to the electric detonation of lenses. The increase of surface velocity with number of detonation points was demonstrated in this way.

15.21 A unique property of the magnetic method was the fact that it was the only experimental method that could be applied to a full scale implosion assembly. The electric method (15.31ff) could be applied at full scale, but not to complete spheres. One of the chief objectives of the magnetic program was to prepare for the investigation of full scale shots. Unfortunately the method could not be used at Trinity because it was desirable to fire that shot with the same type of metal case as used with the bomb assembly, and this case made it impossible to obtain magnetic records.

15.22 As technical difficulties were surmounted the method was applied successfully at various tamper diameters. At these scales the method was used alone as well as in coordination with the RaLa and betatron methods. The timing results were particularly useful in these cooperative tests. As a method by itself its main value lay in following the dynamic sequence in the implosion and giving data as to the way this changes with increasing size of the implosion.

THE BETATRON

15.23 The use of the betatron as an instrument to study the implosion had been proposed earlier, but was not decided upon until the time of the discussions which led to the formation of G Division. For this work the 15 million volt betatron of the University of Illinois was obtained, after expert analysis of its capabilities for this work by its inventor, D. W. Kerst, and of the possible use of a vertical cloud chamber for recording by Neddermeyer. These analyses gave sufficient promise of success to justify undertaking the development of the method. Experimental work was begun in the early fall of 1944, on the performance (rise and burst times) of the betatron at Illinois, and also on the refinement of shadow recording by means of flash photography of the cloud chamber.

15.24 Construction of the betatron site (K Site) was begun concurrently. As at the X-ray site, the test implosion was detonated between two closely spaced bomb-proof buildings, one containing the high voltage gamma ray source, the other containing the cloud chamber and recording equipment. Equipment was protected from the blast by aluminum nose pieces over the exit and entry ports through which the radiation passed, and by shock-mounting all equipment which could be damaged by the shock wave. Construction and installation required until the first of 1945 for completion.

15.25 In the meantime the cloud chamber technique was improved by finding means for trapping the low energy tracks to obtain better definition of the image in the cloud chamber, and by an intricate and ingenious sequence circuit developed in the laboratory. This circuit had to correlate the firing of the explosive charge, the gamma ray burst, and the expansion and photographing of the cloud chamber.

15.26 By the time the first test shots had been fired, the importance of results from the betatron had become crucial because of the likely abandonment of the counter X-ray method. The adaptation of the field installations, the electronic circuits, and the cloud chamber to their special purposes proceeded so rapidly that by April 1945 a systematic study of compression under scaled lens shots was started. The results of the early work showed an unexpected irregularity, but nonetheless gave evidence of definite compression of the uranium core by an amount not very different from that predicted theoretically. By June, the spread of data had been reduced enough to provide valuable experimental information for the correction of the constants used in the theory of the bomb. Cooperative data provided by the magnetic method (15.18 ff) indicated that the spread in experimental results was possibly caused by variation in the behavior of the high explosive.

15.27 The betatron program was one of the few that maintained a single purpose and function from its inception to the production of final results. If for this reason its history can be written briefly, the technical achievements and ultimate importance of this work are among the most impressive of the several such achievements at Los Alamos.

THE RaLa METHOD

15.28 The RaLa program (7.61) resembles the betatron effort in that it was a single-purpose, more or less direct, adaptation of a known radiographic technique to the study of implosions. Both methods had to be adapted to microsecond time resolutions. For RaLa this meant the development of unprecedented performance with ionization chambers as well as unheard of sources of gamma radiation. As has been said elsewhere (17.42), the source of radioactivity was the radiobarium from fission products of the chain reacting pile. By the fall of 1944, the extraction of radiolanthanum had been put on a working basis. The production of sufficiently fast ionization chambers had also been achieved.

15.29 The RaLa firing program itself got under way in Bayo Canyon (see Site Map) October 14, 1944. Because of the lack of knowledge as to how serious the contamination would be after imploding a source which was the equivalent of many hundred grams of radium, the permanent installations were kept at a minimum for these first trials. Sealed army tanks were used as observation stations. The contamination danger appeared so conveniently small, however, that permanent bomb proofs were subsequently installed (November 1944).

15.30 The early shots were fired by multipoint, primacord systems and the results were correspondingly erratic.

THE ELECTRIC METHOD

15.31 The principal technique of the electric method of investigating the implosion was that of recording electronically the electrical contacts formed between the imploding sphere and prearranged wires. This is a well-established method of investigation but, as always in adapting old methods to implosion work, it was necessary to sharpen the time resolution and meet rather more serious interference with the circuits on the part of the explosive than had previously been encountered.

15.32 The development of the electric method was undertaken in August 1944. Although many possible applications of electronic circuits suggested themselves, the first effort was to make oscillographic recordings of the position of a plate as a function of time, when the plate was accelerated by a high explosive charge. This was done by merely spacing small pins at intervals differing by about a millimeter from the surface to be accelerated. The first successful results were obtained in early October 1944. The technique was then rapidly developed and refined, so that by the early part of 1945 quantitative data on the acceleration of plates was being obtained. The method was then adapted to the more severe implosions of partial spheres, and finally used on lens systems in which only one lens was omitted. Information was obtained on velocities of material at various depths in the core, and on shock-wave velocities. This information was particularly valuable in supplying the Theoretical Division with direct, quantitative data on which to test its conclusions and base its predictions for the implosion bomb.

15.33 Variations of the technique described above were studied but not developed as fully as the contact method. These included the condenser microphone and resistance wire methods. The use of tourmaline crystals for timing signals, as well as for possible pressure recording, was pursued by the Initiator Group. In addition to the information of general theoretical interest on the properties of metals under compression, these methods were useful in many particular studies. Thus the Initiator Group was able to determine the pressure distribution in a detonation wave, and the Electric Method Group measured transit times relative to detonation times, uniformity of lens operation, and the location and velocities of spalls and jets. They also measured many velocities of interest in connection with initiator design, and the velocity of shock-operated jets formed in the thin crack between tamper halves.

15.34 Aside from its general usefulness for many research purposes of G Division, the Electric Method was--apart from the magnetic method--the only one which could be applied at very large scale. By June 1945, the technique was being applied regularly at large scale and to almost complete spheres of lens charges.

Initiators

15.35 The Initiator Group differed from those discussed above in that it was primarily concerned with the development of a component of the bomb rather than with a particular research method.

15.36 A proving ground was built in Sandia Canyon (see map) for initiator work. The nature of proof was not predetermined, however, and in any case could not be complete, because the performance requirements could be measured only by operation in the bomb itself. Many leads were followed, accordingly, in the attempt to simulate actual conditions as closely as possible and to learn as much as possible about the mechanisms that were supposed to make the various designs work.

15.37 The initiator program involved a great deal more than the attention of the Initiator Group. A large part of the problem was the procurement and radiochemical preparation of polonium. (17.32 ff) There was considerable theoretical investigation of proposed initiator mechanisms, and a large amount of experimental corroboration was produced by the X-ray, flash photography, and electric methods. The function of the Initiator Group was to coordinate these researches with its own and with trends in design.

15.38 By February 1945 it was pretty well decided that the initiator should be of an (α, n) type. Many designs were invented, and the work of selecting promising ones and testing them was started. All designs worked by some mechanism for mixing the α-n material, previously kept separated by some α-absorbing material under the impact of the incoming shock-wave. At this time the possibility of complete recovery of imploded spheres was discovered. The recovery of units with and without α-n materials in them formed an important part of the early work. Although the results thus obtained built up confidence in the feasibility of initiators, this work superseded by studies of specific mechanisms.

15.39 The deadline for a decision on the feasibility of an initiator was met, with a favorable report, on May 1, 1945.

15.40 In the course of the month after May 1 the acceptance specifications and fabrication procedures were established, and the first service unit was finished by the radiochemists early in June.

15.41 The Initiator Group set up handling procedures in cooperation with the G Engineers and the Radiochemists for production, surveillance, and recording the history of each unit. This plan had just been put in operation by the end of the war.

Electric Detonators

15.42 The need for a high degree of simultaneity in the multipoint detonation of the implosion had been realized from the beginning, as had the potential usefulness for this purpose of electric detonators. The early development of primacord branching systems, the investigation of timing errors associated with them, and the experimental and theoretical work on the importance of such errors were undertaken (7.63). Electric detonators were already under development and during August 1944 this work was transferred to the new G Division.

15.43 The degree of perfection desired was, of course, not built into the commercial electric detonators, and the problem of simultaneously firing many such units had not been faced. This required the development of an adequate high voltage power supply and of a simultaneous switching device. The high voltage supply could be made from commercial units, and was simply a bank of high voltage condensers. The switch problem, acute at first, was met by straightforward developments which led to several designs adequate for experimental purposes.

15.44 The switch finally developed for weapon use and the particular detonators used were proved in X Division (16.37). But with a firing circuit adequate for experimental purposes it soon became apparent that there was a serious lack of simultaneity and source of failure in the detonators themselves.

15.45 Facilities for rapid loading of experimental detonators were developed at Los Alamos, and this local supply became the primary one.

15.46 The criterion for acceptance was, of course, a small maximum time spread and percentage of failures. Tests were made by oscillographic methods and later by photographic observation, using the rotating mirror camera.

15.47 In the spring of 1945 the Detonator Group was burdened with the supervision of the preparation of thousands of detonators for field work in the implosion program. By the end of May the specifications for the combat service units had been completed, and responsibility for putting the units into service was taken by Group X-7 (16.37-16.38).

Photography

15.48 The Photographic and Optics Group, which before August 1944 had been a part of the Instrumentation Group of the Ordnance Division, was responsible in G Division for the development of optical instruments and for the operation of technical photographic facilities. As such it was partly a service group and partly an experimental group. It prepared photographic and spectrographic equipment for the Trinity test. Before that it had played a substantial part in the Wendover drop tests.

15.49 Besides procurement of photographic equipment and maintenance of a photographic stockroom, the Photographic Group designed and built cathode ray oscilloscope cameras, armored still cameras for various high explosives test sites, an armored stereoscopic camera for the flash photography of imploding hemispheres, (16.9) and a cloud chamber stereoscopic system for the betatron cloud chamber (15.25). The group built boresights and a photo velocity system for the 20 millimeter gun. It designed and developed the rotating prism and rotating mirror cameras (15.46). As with electronic, so with photographic recording of data, the Laboratory was able to reach a high level of perfection in accurate high speed work. Much of this work stands to the credit of the Photographic Group.

Electronics

15.50 Early in September 1944, the Electronics Group was greatly expanded because of the very heavy demands placed on it by the Laboratory. At this time many types of equipment were in production. Among the standard items made were scalers, power supplies, discriminators, and two types of amplifiers with rise times of 0.1 and 0.5 microseconds. Many new pieces of equipment were designed and built. A scaler with a resolving time of 0.5 microsecond per stage, amplifiers with less than 0.05 microsecond rise time, a ten-channel pulse height analyzer, and a ten-channel time analyzer were constructed. The group built most of the electronic equipment used on the betatron and much auxiliary equipment for the cyclotron, van de Graaffs and D-D sources. It designed and built new sweep circuits, delay circuits, calibrating circuits, and a host of other circuits needed by a laboratory as large and diversified as Los Alamos had become. If the above gives some indication of the variety of work carried out by the Electronics Group, its magnitude is indicated by the fact that the membership of the group averaged in the neighborhood of fifty. These ranged from wire men to designers of new equipment. The number of major construction items ranges over a thousand, and the number of service and repair items far beyond this figure, for the period covered.

15.51 The services of this group are not reflected directly in a single part of the weapons finally developed at Los Alamos and for this reason are likely to be slighted. But it is safe to say that without its services many of the experiments of G Division and the Laboratory would not have been done as well, they would have required more time, and the completion of the bomb itself would have been delayed.

Chapter XVI

EXPLOSIVES DIVISION

Organization and Liaison

16.1 The Explosives Division, organized in August 1944, consisted originally of the Explosives Experimentation, High Explosives, and S Site Groups of the old Ordnance Division. Its rapid growth from that time (see Graph No. 5) was accomplished by subdivision of groups into sections and by addition of new groups. Two organization lists are given below, for September 1944 and for August 1945:

September 1944

X-1	Implosion Research	Cmdr. N. E. Bradbury
1A	Photography with Flash X-Rays	K. Greisen
1B	Terminal Observations	H. Linschitz
1C	Flash Photography	W. Koski
1D	Rotating Prism Camera	J. Hoffman
1E	Charge Inspection	T/3 G. H. Tenney
X-2	Development, Engineering, Tests	K. T. Bainbridge
2A	Engineering	R. W. Henderson
2B	High Explosives	Lt. W. F. Schaffer
2C	Test Measurements	L. Fussell, Jr.
X-3	Explosives Development and Production	Capt. J. O. Ackerman
3A	Experimental Section	Lt. J. D. Hopper
3B	Production Section	J. B. Price

August 1945

X-1	Implosion Research	Cmdr. N. E. Bradbury
1B	Terminal Observations	H. Linschitz

1C	Flash Photography	W. Koski
1D	Rotating Prism Camera	J. Hoffman
1E	Charge Inspection	M/S G. H. Tenney
X-2	Engineering	R. W. Henderson
X-3	Explosives Development and Production	Maj. J. O. Ackerman
3A	Experimental Section	Lt. J. D. Hopper
3B	Special Research Problems	D. H. Gurinsky
3C	Production Section	R. A. Popham
3D	Engineering	B. Weidenbaum
3E	Maintenance and Service	Lt. G. C. Chappell
X-4	Mold Design, Engineering Service and Consulting	E. A. Long
X-5	Detonating Circuit	L. Fussell, Jr.
X-6	Assembly and Assembly Tests	Cmdr. N. E. Bradbury
X-7	Detonator Developments	K. Greisen

16.2 The following are the principal developments in the administrative history of X Division. The new division was headed by G. B. Kistiakowsky, whose previous status had been Deputy Division Leader of the old Ordnance Division, in charge of the Implosion Project. Kistiakowsky was assisted by Major W. A. Stevens for administration and construction. Group X-4 was created early in October under E. A. Long and J. W. Stout. It was charged with the engineering of molds for S Site, research on sintered and plastic bonded explosives, and miscellaneous services for the division. A section was added to this group in December 1944 under W. G. Marley, responsible for some aspects of lens research. In November 1944 a second research section under D. H. Gurinsky was added to Group X-3.

16.3 A major organizational change occurred in March 1945 when Group X-2 under Bainbridge was dissolved. Bainbridge was put in charge of the new Trinity Project, preparing for the Trinity test. Three new groups were formed to continue the work of X-2: an Engineering Group under R. W. Henderson, X-2; a Detonator Firing Circuit Group, X-5, under L. Fussell, Jr., whose section of X-2 had already acquired the responsibility for the design and development of a firing unit for the electric detonators; and an Assembly and Assembly Test Group, X-6, under N. E. Bradbury. In March 1945, M. F. Roy joined the Laboratory staff as Assistant to the Division Leader of X Division. In May 1945, Section X-1A was discontinued and a new group formed under K. Greisen, X-7, to carry through the final development of detonators.

16.4 The directive of the new division was, in essence, to develop the explosive components of the implosion bomb. In the same sense it was the directive of G Division to develop the active components. These directives did not mark out separate areas of work so much as they did specify the directions from which the two divisions should attack the central problem, which was to produce the optimum assembly of active material. X Division was specifically:

(a) To investigate methods of detonating the high explosive components.

(b) To develop methods for improving the quality of high explosive castings.

(c) To develop lens systems and the methods for fabricating and testing them.

(d) To develop engineering design for the explosive and detonating components of the actual weapon.

(e) To provide explosive charges for implosion studies in G and X Divisions.

(f) To specify and initiate the design of those parts of the final weapon for whose development it was responsible.

16.5 Since it was the joint responsibility of X and G Divisions to carry out the fundamental implosion development work, these two divisions worked in close cooperation. Both, for example, carried out studies of implosion dynamics. In these studies G Division was primarily concerned with the measurement of the assembly velocity and compression of the bomb pit while X Division was primarily concerned with the explosive techniques for achieving high assembly velocity and compression. But no real separation along these lines is possible. In practice the methods of implosion study which were already developed and known to be reliable remained in X Division, while G Division concentrated on the development of new methods.

16.6 The division of labor between X and O Divisions in relation to the implosion bomb can be described roughly by saying that the former was responsible for the explosive components, the latter for the case, including under the latter mainly what was necessary to convert such a bomb as was set off at the Trinity Test into a combat weapon.

16.7 The important outside connections of the Explosives Division were with the Explosives Research Laboratory at Bruceton, and later with the Camel Project of the California Institute of Technology. Work was done also at the Yorktown Naval Mine Depot and by the Hercules Powder Company. The Yorktown Naval Mine Depot supplied explosives and when a new type of explosive was under investigation supplied information on its physical and explosive properties. Hercules produced spark-gap detonators to Los Alamos specifications. The general liaison with the Camel Project is discussed in 9.15ff. Special items related to the work of X Division are discussed in the appropriate sections below.

16.8 The history of X Division, like that of the whole implosion project, is one of gradual development, usually in the direction of greater complexity of the operating mechanism. From the situation in the summer of 1944, described at the end of Chapter VII, it was a long time before there was any real assurance of success. Programs were complex. Alternatives had still to be worked on with fair priority. The pursuit of these various developments would, morever, have been useless without constantly improving techniques of experiment and observation, attention to which was therefore a large part of the division program. A good deal of theoretical effort went into the interpretation of implosion jets.

16.9 Three principal techniques were used in the implosion studies; rotating pyramid and rotating mirror photography; high explosive flash photography; and flash X-ray photography. Measurement at maximum compression was possible by careful timing of the X-ray flash and the explosive detonation. The rotating prism or mirror techniques and the high explosive flash technique gave shadow photography of imploding cylinders. By a device which gave a succession of high explosive flashes it was possible to obtain images on the same negative at different stages of collapse. Hemispherical implosions were observed by reflected high explosive flash light, and photographed stereoscopically. These observations made it possible to verify predictions about jetting based on two-dimensional cylinder results, and served to remove the last possible doubt that the cylinder jets might be optical illusions. Early in 1945, this hemisphere technique became standard, and the cylinder work was dropped.

6.10 Apart from timing studies and lens development work, discussed in the next sections, the remaining work of the Implosion Research Group also centered on the investigation of jet phenomena. Experimental data needed to test the theory that jets were caused by shock-wave interaction were obtained by so-called slab shots. Slabs of explosive were placed on top of metal slabs and detonated at two or more points. The effects of interaction between the shock waves were observed by flash photography and terminal observation. It was thus possible to study jetting as a function of the angle between detonation waves.

16.11 The need for special means of initiating the high explosive at several points "simultaneously" was recognized from the beginning. What was not recognized was the degree of simultaneity required.

Explosives Development and Production

16.12 In Chapter VII the early development of production facilities has been reviewed. After August 1944, the story is one of continual expansion, constant training of new personnel, and research on better and more reliable production methods. The size of the undertaking is indicated by the records of X Division, which show that in a period of eighteen months some twenty thousand castings of adequate experimental quality were produced, and a much larger number rejected through quality control tests. At its peak, S Site used something over 100,000 pounds of high explosives per month. Seven or eight different explosive materials were used in castings, in an enormous variety of shapes and sizes. The principal explosive used was Composition B. Others used in smaller quantity were Torpex, Pentolite, Baronal, and Baratol. Casting methods were used wherever possible. Some development was carried out at the Explosives Research Laboratory of precision pressing techniques, but without encouraging results.

16.13 It may perhaps be thought that in such a field as high explosives casting there was an existing art which could be made use of at Los Alamos. This unfortunately was not so. Military techniques for loading explosives are crude when measured by such standards as were needed in the implosion. Very little scientific work had ever been done in the field, and there had been very little incentive to regard high explosives as possible precision means for producing phenomena outside the ordinary range of experimental physical techniques. But just such an incentive was created by the requirements of the implosion program at Los Alamos. Hence many of the problems faced were new. Their solution was undertaken primarily by the Explosives Development and Production Group.

16.14 The machining of explosives, entailed by the use of risers and of overcasting techniques, became a well-developed art at S Site and was the greatest innovation introduced in the manufacture of explosive charges. By removing the top section of the casting (in which its imperfections had been concentrated), a charge meeting the necessary standards of quality was obtained. This machining program required close cooperation between the explosives groups concerned and the Shop Group (9.49). Holding jigs and cutters finally used represented several months of development and experimentation. The earliest molds were designed so as to minimize machining by the use of small risers. This kind of machining is normally considered very dangerous, and its development is considered as a revolutionary development in explosives manufacturing. By careful design and control of operating conditions all hazards were virtually eliminated, as is shown by the fact that more than

50,000 major machining operations had been conducted by the end of the war, without detonation of the explosive

16.15 During all its history, S Site was forced by the growing demands for explosives production to grow at a rate faster than the completion of facilities and arrival of personnel would easily allow. That S Site successfully fulfilled its objective is largely the result of faithful and efficient work by the soldiers who constituted more than 90 per cent of its staff.

16.16 S Site, or Sawmill Site as it was originally called, was placed in limited operation in May 1944 after a winter of construction and difficult equipment procurement. It was subsequently enlarged, and its equipment completely modified.

16.17 All early castings made in the large casting buildings at S Site were cylinders of Composition B, ranging in weight from 30 to 500 pounds. Small castings of Pentolite at that time were being cast at the Anchor Ranch casting room which had been placed in operation in October 1943. This small casting room was equipped with four kettles of two gallon size. It continued to furnish very small cylinders and special castings of Pentolite throughout the war.

16.18 Experience gained from an examination of the cylinder castings, made by the Standard methods of ordnance practice, showed that extensive research would be necessary to produce large castings with the required quality.

16.19 As a result, a Research and Development Section was set up in June 1944. It started with four men but was enlarged throughout the war as qualified men could be assigned to the work. Initially a building suitable for experimental casting research was not available. Building S-28, the trimming building, was used temporarily until the laboratory buildings were completed in September 1945.

16.20 Early research was confined to the determination of the nature of the explosives and to the problems of securing in castings a quality adequate for the testing program. Little time was available for the longer range program of making large castings of uniform quality.

16.21 Beginning in November 1944 the experimental work was divided into two fundamentally different parts; the first was the solving of problems encountered in making high quality large castings with slurries, and the second was the development of methods to manufacture explosive lenses.

16.22 The adoption of lenses imposed upon S Site a tremendous development program not only in the research for, but in the production of, many forms of lenses. No little part of the research work conducted in late 1944 and early 1945 was directed toward the production line manufacture of small-scale lens charges.

16.23 Incidental items of research included the development casting of special explosives such as Torpex and Baronal.

16.24 When the use of lenses became a certainty late in 1944, the designs for a series of lens molds were frozen. These molds covered the range of predictable explosives rates and lens sizes, from small to full scale. This freezing of design was a wise decision, as it permitted meeting schedules later. On the other hand, it greatly increased the burden on research, to make available molds work in spite of their known deficiencies.

16.25 Throughout most of 1944, production of charges in large quantity was not required. Late in 1944 the demand for various sizes and shapes of casting increased greatly as a result of the introduction of new methods of studying implosion and the general growth of the Laboratory staff. The new testing methods included the betatron, X-ray equipment, new mirror camera installations, RaLa, flash X-rays, the magnetic method, and the "pin" method. These tests stimulated requirements for lenses as well as solid charges.

16.26 As a result of these increased requirements a new small casting line, consisting of five buildings, was constructed between December 1944 and March 1945. This line was designed to meet the then predictable requirements for small castings. It later was called upon to meet more varied requirements in the lens program. It was in this line of buildings that the quantity production of small lenses used in the testing program was conducted.

16.27 The adoption of lenses and full machining of charges for the full scale unit called attention to the inadequacy of facilities for full scale work. Buildings S-25 and S-26 were, therefore, built for this purpose between March 1945 and June 1945.

16.28 Because of increased demands for full scale, high quality castings not only for Trinity but for combat use, manufacturing, research and control were intermingled during June and July 1945. New problems arose continuously, involving major changes in process and control. The requirements for inspection, both by physical measurement and by gamma and X-ray examination, increased so greatly as to make the success of this phase of engineering one of the primary accomplishments of the period.

16.29 Throughout the work at S Site, it was common to speak of "production casting." This was really a misnomer, as all castings were of tailored quality and were produced in reasonably small quantity in a large variety of molds. Such production differed from ordnance production not only in that its maximum monthly output of 100,000 pounds of castings would be one day's run of a standard ordnance line, but more significantly in that it consisted of thousands of high quality charges 1 pound to 120 pounds in size compared with a small number of large units weighing thousands of pounds each.

16.30 Because of the requirement of the site for men with scientific or explosives experience and willingness to work with explosives, each new request for assignment of enlisted men or civilians was followed by a long delay. The site was always hampered in its work by a shortage of manpower. On the other hand, the growth in manpower which did occur made the training of new men a continuous problem.

16.31 Similarly, the general shortage of personnel available to the construction contractor delayed construction at S Site. It was necessary always to reduce building requirements to the minimum in order to meet time schedules.

Recovery Program

16.32 The early work of the recovery program has been discussed in Chapter VII (7.61 ff). This was one of the branches of the Laboratory's activity which was dropped, after extensive research and development, for reasons extraneous to the program itself. It was argued, orginally, that the chances for a nuclear explosion at the first test might not be great, and that in this case it would be necessary to recover the active material. As the time approached for the test, however, two arguments were sufficient to justify the abandonment of recovery plans. One was that with the weapon finally designed the chances for no nuclear explosion became very small; the other was that the use of a containing sphere or "water baffle" would make it quite difficult to obtain information that would explain a partially successful nuclear explosion, by this time a more realistic worry than complete failure. Hence when the first test was made, Jumbo, which was such a magnificent piece of engineering, stood idly by, half a mile from the test tower. After the test Jumbo was unscathed; but its crumpled rigging tower was a preview of the damage to steel structures in Japan.

16.33 The specifications for Jumbo had been that it must, without rupture, contain the explosion of the implosion bomb's full complement of high explosive, and permit subsequent mechanical and chemical recovery of the active material.

16.34 The final design of Jumbo was the elongated elastic design described in Chapter VII. An order was placed for this container early in August 1944, with the Babcock and Wilcox Corporation. It was delivered to the test site in May 1945 and erected on its foundation. Because of its size (about 25' by 12') and weight (214 tons) Jumbo had to be transported to the nearest rail siding on a special car over an indirect route to provide adequate clearance. From the siding it was transported overland to the test site on a special 64-wheel trailer.

16.35 Jumbo was constructed from the design of R. W. Carlson. Numerous tests were made with scale model "Jumbinos" to determine their ability to contain charges without rupture.

16.36 The other recovery method investigated in this period was the "water baffle," in which fragments were stopped by a 50:1 ratio of water to high explosive mass. This work was dropped before conclusive results were obtained, but showed that high-percentage recovery would be difficult to achieve. For use in recovery experiments, a shallow concrete catch basin, 200 feet in diameter, was constructed at Los Alamos.

The Detonating System

16.37 The main development work on electric detonators was carried out in the Electric Detonator Group of G Division. But the detonators designed there were primarily intended for experimental work, either on detonators themselves or for firing experimental charges. Detonators for the weapon had to satisfy further requirements, such as durability, ruggedness, and reliability. Reliability was a particularly important consideration.

16.38 The firing of the electric detonators was accomplished by discharging a bank of high voltage condensers through a suitable low inductance switch. This problem was assigned to Section X-2C, later to become Group X-5, the Detonating Circuit Group. Mechanical switches were incapable of doing this. One type of switch extensively developed, and used in experimental work when accurate timing was required, was the so-called explosive switch. In this switch electrical contact was made by the detonation of an explosive charge which broke through a thin dielectric layer between two metal discs, between which the high voltage then passed. This switch was made double by the use of four semicircular discs. Its disadvantage was that it could not be tested before use. Because of this an alternative switch was finally adopted, an electronic device resembling the thyratron. This switch operated between two electrodes, the discharge being triggered by charging a third, "probe" electrode to a suitable high voltage. These switches were able to pass very high currents in the order of a microsecond. The firing units incorporating them were built by the Raytheon Company. Delays in their production made it possible to test only a few of them before the Nagasaki drop.

Engineering, Testing, Assembly

16.39 The division of labor between the Explosives Division and the Ordnance Division has been described above by saying that the former was responsible for that part of the completed weapon which would be tested in the Trinity test, the latter for its use as an airborne weapon. But those components for which X Division was responsible had to be developed to the point of adequacy in combat use. In addition to developing these elements, the engineers of X Division worked with the engineers of O Division in designing their assembly within the Fat Man case. Once this design was completed, it had to be given assembly tests, and tests for reliability and ruggedness under conditions of combat use. Assembly design was the work of the Engineering Group. Assembly tests were the work of the same group, until the formation for this purpose of a special group, X-6, in March 1945. The latter group also collaborated with other interested groups in the drop tests at Wendover Field (19.3).

16.40 Aside from miscellaneous engineering work for the division, the Engineering Group was at first responsible for the design of high explosives molds, particularly lens molds of various types and sizes. Mold development and procurement became such a serious bottleneck by October 1944 that the engineering of molds was made one of the main responsibilities of the new Engineering Service Group established at that time. A system was developed by which experimental molds were built in the Los Alamos shops, while the same designs were sent out for outside procurement. These outside orders were closely followed, with local shop experience as a guide. When the Camel Project came into existence, its engineering group gave valuable assistance in expediting procurement in the Los Angeles area. Even so, the bottleneck remained and it was only by a matter of days that enough final full scale lens molds were obtained for the Trinity test.

Chapter XVII

CHEMISTRY AND METALLURGY

Introduction

17.1 In August 1945 the Chemistry and Metallurgy Division had evolved to the following group organization:

CM-1	Service Group	R. H. Dunlap
CM-2	Heat Treatment and Metallography	G. L. Kehl
CM-4	Radiochemistry	L. Helmholtz
CM-5	Plutonium Purification	C. S. Garner
CM-6	High Vacuum Research	S. I. Weissman
CM-7	Miscellaneous Metallurgy	A. U. Seybolt
CM-8	Plutonium Metallurgy	E. R. Jette
CM-9	Analysis	H. A. Potratz
CM-11	Uranium Metallurgy	S. Marshall
CM-12	Health	W. H. Hinch
CM-13	DP Site	J. E. Burke
CM-14	RaLa Chemistry	G. Friedlander
CM-15	Polonium	I. B. Johns
CM-16	Uranium Chemistry	E. Wichers

This organization evolved by the following steps:

(a) In September 1944 Group CM-3, Gas Tamper and Gas Liquefaction, was transferred to the new Explosives Division.

(b) In April 1945 the following changes occurred: Uranium and plutonium chemistry, formerly concentrated in CM-5, was divided between CM-5 and a new group, CM-16. CM-1 was divided into a Service Group (CM-1) and a Health Group (CM-12). The old Radiochemistry Group (CM-4) was split into three groups, with CM-14 and CM-15 taking charge of RaLa and polonium work, respectively. R. W. Dodson, former group leader, became Associate Division Leader in charge of radiochemistry. C. C. Balke, former group leader of CM-7, left the Laboratory. Miscellaneous metallurgical services were transferred to this group with its new group leader, A. U. Seybolt. CM-11 remained as the Uranium Metallurgy Group. CM-10, the Recovery Group, was absorbed into the new DP Site Group, CM-13.

17.2 These changes were motivated primarily by the expansion of radiochemical work associated with the expanding RaLa implosion studies and the preparation of polonium for the implosion initiator program, and by the need to streamline the processing of uranium and plutonium, arriving in ever increasing quantities.

17.3 The transition to large scale operation involved a constant growth of personnel in the division, and a constant expansion of laboratory, shop and plant facilities. The main steps in this physical expansion were the completion in August 1944 of a large annex to D Building; the completion of the metallurgy building, Sigma Building, in October 1944; the construction of the RaLa Chemistry Building in Bayo Canyon in November 1944. The last and and greatest addition was the chemical and metallurgical production plant, DP Site. The first buildings of this site were completed and being occupied in the summer of 1945.

Uranium Purification and Recovery

17.4 The flow of beta stage enriched uranium received from the Y-12 plant was generally as follows: The material was received as a purified fluoride and reduced directly to metal. For hydride experiments the metal was converted to hydride and formed by plastic bonding. When hydride or metal experiments were completed, the material was returned for recovery, as in the meantime were crucibles, liners, and other containers that had been used in fabrication. Recovered solutions were converted to hexanitrate, extracted with ether, and precipitated as reduced oxalate. The oxalate was ignited to oxide and converted back to the original tetrafluoride.

17.5 The essential step of purification was the ether extraction method that had been developed at the end of the first period (8.18). This method was also applied by the radiochemists to the decontamination of Water Boiler solutions, and by the Recovery Group in experiments on test-shot recovery methods. In April 1945 uranium recovery and purification was concentrated in a new group, CM-16. Before that time extensive investigations had been made to determine the best reagents and ion concentrations for the extraction.

17.6 The dry purification step--reduction of UO_2 to UF_4--was developed to a point where fluoride yields were as high as 99.9%. Fluorination of the trioxide and oxalate were also investigated.

17.7 The studies of recovery from liners and slag revealed the necessity for complete dissolution of these materials prior to recovery. Ether extraction was then employed. Continuous extraction apparatus was designed and built by the Recovery Group, capable of extracting a large volume of solution per hour, and giving recovery yields averaging better than 99.9%. The average amount of uranium remaining in stripped solutions was not more than 60 micrograms per liter.

17.8 A very difficult research problem, undertaken by the Recovery Group, was that of recovering active material in case of a test shot failure. Recovery of uranium dissipated through a large volume of sand, sawdust, and similar materials was attempted. This was in imitation of the conditions that would obtain if the bomb material were scattered over a large area by high explosives. Recovery by this method was found to be very poor. Recovery from sealed containing spheres was entirely successful. A 3/4 inch sphere of uranium, scattered explosively inside of a 12 inch "Jumbino" containing sphere, was recovered about 99%.

Uranium Metallurgy

17.9 By August 1944 it was possible to obtain metallic uranium with a neutron count below tolerance. A number of developments after that date were essential, however, to the final U^{235} weapon. The principal topics are the stationary bomb reduction, uranium remelting, uranium forming, cladding and protection of surfaces, use of uranium sponges, and production of final weapon parts.

17.10 After the adoption of the stationary bomb reduction technique a large number of minor improvements were made. A better product was obtained by increasing the bulk density of the tetrafluoride to be reduced. A wide survey of liners was made, resulting in the final choice of two: (1) a magnesia liner developed by the Miscellaneous Metallurgy Group; (2) a magnesia plus silica liner developed at the Massachusetts Institute of Technology. Other investigations included the effect of impurities in the reductant, grain size, firing technique, and the use of inert gases in the bomb. The net result of this research was a very high yield of high purity, well-consolidated metal.

17.11 Work on uranium remelting was begun in June 1944. This process served the double purpose of driving off volatile impurities and preparing the metal for shaping. A great deal of trouble was encountered in obtaining crucibles that would not crack on cooling. Magnesia and beryllia were finally used, with special heating methods.

17.12 Techniques for forming uranium were intensively investigated after August 1944. Three main techniques used were casting, hot pressing, and rolling. Both magnesia and graphite molds were used successfully for casting. A serious difficulty was that of melting large amounts of uranium, but the trouble was overcome by resistance heating. Investment and centrifugal casting were tried, and the latter adopted as by far the best. Hot pressing was not used in preparing parts for the full-sized gun assembly; it gave no greater accuracy than casting. It was used, however, for smaller pieces, including hemispheres for sphere multiplication experiments and the preparation of slugs for rolling.

17.13 A variety of cladding techniques were investigated, including electroplates of gold, zinc, silver, and later nickel and chromium. Chromium gave the best, but still inadequate, protection. Evaporated metal techniques proved better.

17.14 Low density compacts of uranium were prepared by sintering metal powder obtained from decomposition of the hydride. These had densities ranging from 1 to 6. Protection against spontaneous combustion and corrosion was obtained by treatment with nitrogen, which formed a nitride coating on the metal.

17.15 The culminating work of the Uranium Metallurgy Group was the casting of the final parts for the Hiroshima bomb. This work had been scheduled more than a year before for completion on July 26, 1945. It was in fact completed July 24.

Plutonium Purification

17.16 The plutonium purification program was of course the most directly affected by the abandonment of the plutonium gun program in August 1944.

17.17 Concentration of effort in plutonium purification after this time was upon simplification, production routine, efficiency, and the plutonium health hazard.

WET PURIFICATION

17.18 Shortly after the beginning of this period the first completely enclosed full scale apparatus was completed. The full run of 160 grams required 24 hours, with about 60 liters of supernatant remaining for recovery. Aside from minor difficulties and improvements, this represented the completed form of the "A" process of wet purification, described in Chapter VIII (8.30).

17.19 Early in 1945 investigation and testing of a "B" and "C" wet process began. The "B" process, the one finally adopted for routine plutonium purification, was simpler than the "A" process, and gave higher yields and a smaller volume of supernatants. It involved only two steps: an ether extraction with calcium nitrate, and an oxalate precipitation. The process met purity requirements and gave a product satisfactory for further processing. In July 1945 the "A" process was dropped completely.

17.20 For a time some thought was given to an even simpler "C" process involving only an oxalate precipitation. Purification, however, was not sufficient. The chart on the following page gives the essential information on the "A" and "B" processes.

DRY CONVERSION

17.21 After it was decided to employ only fluoride metal reduction (8.43), effort was concentrated on the production of the fluoride. Three methods were investigated, involving nitrate, oxalate, and oxide hydrofluorination. The method finally chosen was the oxide method, which involved the conversion of the oxalate from wet purification to oxide by heating in oxygen, and introducing hydrogen fluoride at 325°C in the presence of oxygen. The process involved a 24 hour cycle, and gave yields of 92 to 99%.

RECOVERY

17.22 Aside from recovery methods developed earlier (8.34 ff), the principal development of this period was that of peroxide precipitation. Of the four steps first employed--oxalate precipitation, ether extraction, sodium plutonyl acetate precipitation, and a final oxalate precipitation--the ether step was eliminated, and the sodium plutonyl acetate step used only for rather heavily contaminated material.

17.23 The danger of plutonium to the health of operators was greatest

"A" Process

Nitrates from X or W

↓ reduction with dilute HNO_3 and HI

Pu^{+++}

↓ $H_2C_2O_4$

$Pu_2(C_2O_4)_3$, $9H_2O$

↓ oxidation with $NaBrO_3$ and HNO_3

PuO_2^{++}

↓

$NaPuO_2Ac_3$

↓ HNO_3

PuO_2^{++}

↓ ether extraction with NH_4NO_3 and HNO_3

PuO_2^{++}

↓ reduction with dilute HNO_3 and HI

Pu^{+++}

↓

$Pu_2(C_2O_4)_3$, $9H_2O$

Yield 95 per cent
supernatant 60 liters
16 - 24 hours run

"B" Process

Nitrates from X or W

↓ oxidation

PuO_2^{++}

↓ ether extraction $Ca(NO_3)_2$ and HNO_3

PuO_2^{++}

↓ reduction with dilute HNO_3 and HI

Pu^{+++}

↓ $H_2C_2O_4$

$Pu_2(C_2O_4)_3$, $9H_2O$

Yield near 100 per cent
supernatants 30 - 40 liters
10 - 11 hours run

in the recovery operations. The need to vary procedures to fit the type of contamination involved made the development of enclosed apparatus difficult. Such apparatus was, in fact, not developed until after the period covered by this history (November 1945 at DP Site). The main safety effort was perforce the careful monitoring of personnel; those who showed exposure in excess of body tolerances were taken away from further exposure until counts returned to normal (9.30).

Plutonium Metallurgy

17.24 When the new purity tolerances were established, all metal reduction methods were eliminated except the stationary bomb reduction of the tetrafluoride. Work continued as before in the field of crucible research for remelting. It became possible, however, to use magnesia since with increased tolerances the danger of magnesium impurities was less serious. A good deal of research was done on the physical properties of plutonium metal, since more than two allotropic phases were suspected, and this was of primary importance in forming operations. Work began on alloys, with the purpose of finding one that would keep a high temperature phase stable at room temperature. The stable room temperature phase, called the alpha phase, is brittle and difficult to work with. Fabrication operations were investigated, as were methods of surface cleaning and protection. Because plutonium is highly susceptible to corrosion, these were far more important topics than in the case of uranium.

17.25 The techniques of metal reduction and remelting were well established by August 1944. This work, of course, was on a very small scale, and the techniques had to be adapted to large scale operation as more plutonium became available.

17.26 Within the limited time available to them, the metallurgists made rather extensive studies of the physical properties of plutonium. The first transuranic element manufactured in kilogram amounts proved to have a remarkable physical structure. It exists in five distinct allotropic forms between room temperature and the melting point, labeled in the order of temperatures at which they are stable α, β, γ, δ, ϵ. It is very electropositive, but had the highest electrical resistivity of any metal. It is very corrosive in water and air.

17.27 Of all the phases the α, or room temperature phase, is the densest. Because this phase is brittle, and the δ and ϵ phases malleable, the material was pressed at δ phase temperatures. When a series of hemispheres were cast by this method for multiplication studies, warpage and cracks appeared after the metal stood a day or so at room temperature. Evidently higher temperature phases were being retained for a time at room temperature, the warping and cracks being caused by a delayed transition to the denser phase.

17.28 While cleaning and etching plutonium surfaces caused no serious problems, that of protective coating did. A large number of electroplated and evaporated metal coatings were tried, and electrodeposited silver was decided upon for the Trinity hemispheres. At the last minute, however, small pinholes were discovered in the coat as well as blistering caused by the retention of small amounts of plating solution under the coat. Since the scheduled test was only a few days away, it was decided to use the material in this condition, with the blisters polished down to restore the fit of the hemispheres.

Miscellaneous Metallurgy

17.29 The principal metallurgical work of this period, other than uranium and plutonium metallurgy, was that of the Miscellaneous Metallurgy Group in fabricating the gun tamper, beryllium crucibles and refractories, and some boron compacts.

17.30 In crucible research cerium sulfide continued in use for some time after the lowering of purity standards. The material finally adopted for all plutonium and uranium crucibles and liners was a vitrified magnesia developed by the Miscellaneous Metallurgy Group and manufactured at Los Alamos, at the Massachusetts Institute of Technology, and at Ames.

Radiochemistry

17.31 The principal developments in radiochemistry after August 1944 were the following: The implosion initiator program was gotten under way, and the staff of men in this program engaged in polonium research was increased. Radiolanthanum work, in colloaboration with the RaLa Group, was carried out at the Bayo Canyon Laboratory. These two groups were formally separated from the Radiochemistry Group in April 1945. Work began with the high-power Water Boiler, with its consequent problem of decontaminating highly irradiated uranium. Foil chemistry was continued. A new sensitive neutron detector was developed. A calorimeter was built for polonium work. A microtorsion balance was constructed for use in the determination of the mass purity of samples.

POLONIUM

17.32 The use of polonium for the implosion initiator represented a major technical achievement which, moreover, involved a good deal of basic research into the chemical and "metallurgical" properties of this element. Investigation of this element might be said to be as novel as that of plutonium.

17.33 The main problems were these: first, to prepare polonium of sufficient purity to meet the neutron background tolerance; second, to prepare high-density uniform foils; third, to coat these foils against polonium and alpha particle escape. It must also be said that this work is hazardous, both because of the high alpha activity of polonium, and because of its extreme mobility. It is virtually impossible to work with polonium and avoid entrance of the material into the human system. It is eliminated rapidly, however, and does not settle in dangerous concentrations in the bone, as do radium and plutonium. The full extent of the polonium hazard can only be determined with time. Pragmatic safety rules were intended to minimize and detect polonium absorption. Persons with more than a tolerance dose were removed from possible contact with the material until urine counts dropped below tolerance.

17.34 After the first half curie, which was recovered from residues from radon capsules, the polonium used at Los Alamos was obtained from bismuth irradiated in the Clinton pile. Polonium was separated from the bismuth at two plants of the Monsanto Chemical Company in Dayton, Ohio. This material was deposited on platinum foils and shipped to Los Alamos in sealed containers. Much trouble was encountered throughout by migration of the polonium off these foils, on to the walls of the container.

17.35 Polonium purification was primarily the responsibility of Monsanto, although some research on chemical purification and purification by distillation was done at Los Alamos.

17.36 Other work of the polonium chemists was chiefly the preparation of (α, n) neutron sources for the experimental physicists. Of these the most complicated were the mock-fission sources referred to in 12.11.

WATER BOILER CHEMISTRY

17.37 Work on the low power boiler ended in August 1944, and operation of the high power boiler was begun in December 1944. It was decided to use the nitrate in the second boiler, so the first job of the radiochemists was the repurification and conversion of the old material from sulfate to nitrate. The main reason for choice of the nitrate was that the nitrate had to be used in decontamination (ether extraction); the nitrate also was found to be slightly less corrosive than the sulfate.

17.38 It became necessary to build a "hot" chemistry laboratory and remote control decontamination apparatus. Research was carried out in collaboration with the Recovery Group on the use of the ether extraction method.

17.39 The remote control apparatus was placed behind a thick concrete wall. The irradiated material was run directly from the boiler into an underground tank. From there it was pumped up into the extraction column by means of air pressure. After extraction of the solution it could be run back into an underground vault, or let out into the "hot" laboratory if concentration was desired. Concentration was carried out behind a shield of lead bricks.

17.40 The heavy irradiation of the nitrate in the boiler caused decomposition and loss of nitrogen. This caused precipitation of the basic nitrate. To avoid this, frequent analyses of the boiler solution were made, and nitric acid was added to make up the deficit.

RADIOLANTHANUM

17.41 The first remote control apparatus mentioned in 8.70 was developed largely before August 1944, but its first use falls in the period after that date, and the entire subject is discussed in this place. The determining feature of the RaLa chemistry, essential to the RaLa method, was the enormous radioactivity involved. A single batch of material could represent up to 2300 curies of activity.

17.42 The isolation of radiobarium from the fission products of the Clinton pile was arranged in April 1944. The material received from Clinton consisted of a mixture of Ba^{140} and La^{140}. After arrival, the short-lived lanthanum had to be separated from its parent barium.

17.43 The first control apparatus and associated means for separating lanthanum from barium was designed as protection against about 200 curies of activity, and operated by the so-called phosphate method. In small scale tests, the separation of the lanthanum from about 100 times its mass of barium was found to be nearly quantitative when the phosphate was precipitated from an acid phosphate solution. But in full scale practice the method did not turn out well. Difficulties were encountered because of long filtration times and strong hydrogen ion dependence.

17.44 As the RaLa method was developed, moreover, the requirements on source strength and dimensions became more stringent. It became necessary to develop a new and better method. This method would have to provide shorter filtration time and precipitation on a smaller filter area, and would have to give good separation with higher yield. And the operators would have to be given protection against higher activities.

17.45 In March 1945 work began on a new method, with collaboration between the radiochemists and the Plutonium Chemistry Group and Recovery Group. For greater radiation protection the controls were removed to a distance of 90 feet. The separation method developed was a hydroxide-oxalate process. Lanthanum hydroxide was precipitated with sodium hydroxide, filtered on a platinum sponge filter, dissolved in nitric acid, and reprecipitated with oxalic acid; the oxalate was allowed to stand approximately 25 minutes, and then a small quantity of hydrogen fluoride was added. The resulting precipitate was crystalline, which could be filtered rapidly on a small area and was not affected by intense radiation.

17.46 Half-life measurements of carefully purified La^{140} gave a value of 40.4 hours.

INSTRUMENTS AND SERVICES

17.47 Mention has been made of boron trifluoride investigations for filling proportional neutron counters. The preparation of very pure trifluoride was investigated, as were methods of recovery of the costly isotope 10 from counters no longer needed. In addition to filling counters in large numbers as a service to the experimental physicists, the radiochemists developed a very sensitive counter, the "quadruple proportional counter," for quick measurement of weak neutron sources.

17.48 Among other important services a large number of foils were prepared for the experimental physicists. The essential problems and accomplishments of this work have already been described.

17.49 A calorimeter was built for use in connection with the measurement of the half-life of polonium. A microtorsion balance was constructed for weighing polonium samples. The comparison of weight and activity of samples was found to be a reliable method of purity analysis.

Analysis

17.50 In the early work of the Analysis Group, the main emphasis had been upon the determination of very small amounts of light element impurities. After the discovery of Pu^{240}, however, the need for strict contamination control was eliminated, as was that for further research on methods for determining these elements. Tolerance limits were easily determined by existing methods, and it became possible to turn attention to investigations which had previously been secondary, particularly in heavy element analysis. The Analysis Group devoted increasing effort to the problems of the metallurgists, to improvement of instrumental techniques and development of routing methods. The following is an outline of the principal analytical methods investigated after August 1944.

Spectrochemical methods
- a. Plutonium
  - (1) The cupferron method for heavy elements
  - (2) The gallium oxide pyroelectric method
- b. Uranium
  - (1) The determination of zirconium in uranium
  - (2) The determination of uranium in urine
- c. Miscellaneous
  - (1) Impurities in calcium and magnesium oxides

Volumetric methods
- a. Determination of acid soluble sulfide in U and Pu
- b. Determination of sulfate in Pu

Assay methods
- a. Radioassay
- b. Photometric assay
- c. Gravimetric assay
- d. Microvolumetric assay

SPECTROCHEMICAL METHODS

17.51 The cupferron procedure as applied to heavy element determination was essentially the same as that developed earlier for light element trace analysis. It eventually proved applicable to 39 element impurities. In addition to its use for plutonium analysis, it was applied in analyses of other elements, including uranium. The procedure consists in forming the acid-insoluble cupferride of plutonium, and extracting the compound from the impurities with chloroform. The aqueous impurity solution is then evaporated on copper electrodes, sparking of which gives the impurity spectrum. It may also be noted that this method was used by the Health Medical Group to determine plutonium in urine samples.

17.52 The pyroelectric gallium oxide method was developed in the first period for uranium analysis, but became one of the important methods for plutonium analysis as soon as such analysis became necessary. In this case the material was arced in a dry-box to reduce health hazards.

17.53 For certain elements, notably titanium, zirconium, thorium, columbium, and tantalum, no satisfactory spectrochemical method was discovered by the end of the period considered in this history. None of these elements, fortunately, was of crucial importance.

17.54 Some further investigation of uranium analysis was made in this period. At the end a method was being studied for the determination of zirconium in uranium. A process was being investigated for determining uranium in amounts of 0.1 to 1 microgram in urine samples. This method was being sought for use in health control work.

17.55 The analysis of impurities in calcium oxide and magnesium oxide became important with the adoption of these materials as crucibles and liners in plutonium metallurgy. The investigation was begun early in 1944, but was pursued more intensively after they were adopted for that purpose. The method was one of direct arc spectrography. Difficulties were encountered in obtaining consistent results, most of which were found to be caused by variation in the state of subdivision of the oxides. This was overcome by very fine grinding of the samples.

17.56 Several useful developments of spectrochemical techniques were made during this period. These included the double spectrograph, the double slit spectrograph and the dry box arc. The double spectrograph consists of two spectrographs aligned in opposition, and passes light emitted from the source through the slits of both instruments. The double slit spectrograph produces juxtaposed spectra of two wave length ranges on the same film. The advantages of these methods lay in halving the sample size in one case, and the time for a complete analysis in the other. They were important because of economy of valuable material, reduction of analysis time, and reduction of exposure to plutonium on the part of the operator. The dry box arc with outside controls was developed because of the extreme danger from arcing plutonium. Laboratory contamination is serious with ordinary arcing, and the danger to operators very great.

VOLUMETRIC METHODS

17.57 Acid soluble sulfide was determined by distillation of hydrogen sulfide, which was absorbed and determined volumetrically. The method was used for plutonium and uranium metals. Sulfate in plutonium was determined by reduction to sulfide, followed by volumetric measurement of absorbed hydrogen sulfide.

ASSAY METHODS

17.58 Photometric, volumetric, and gravimetric methods were investigated to establish a procedure for routine plutonium assay. Results from the photometric method were untrustworthy. The gravimetric was good but too slow. The volumetric method was the one finally adopted. After its development in early 1945, all Hanford material was assayed by this method.

DP Site

17.59 At the beginning of 1945 all plutonium production work had been planned for and was being carried out in the Chemistry Building, Building D. Three things contributed to the alteration of this plan. The first was the increasing realization of the seriousness of the plutonium health hazard. As has been pointed out before, Building D was originally planned with the idea in mind of preventing the contamination of plutonium by light element impurities. In fact, the most serious contamination problem was to prevent the contamination of personnel with plutonium. The building was not ideal from this point of view, and as larger amounts of plutonium began to arrive, adequate decontamination became increasingly difficult. The second factor was an increase in the expected rate of flow of the Hanford material, which would, at maximum production, tend to overstrain the resources of D Building. The third factor of importance in changing the plutonium production plan was a bad fire which occurred in C Shop on January 15, 1945 (9.40). This fire demonstrated vividly the possibility of fire in D Building. The consequences of such a fire, including the spread of contamination over a wide area of the Laboratory, indicated that it was imperative to build a new production plant, designed so that fire was unlikely, and so located that accidents would not retard the work of the Technical Area.

17.60 In February, consequently, a committee was appointed to design and expedite the construction of a new plant. The plans for this plant were enlarged so as to accommodate the processing of polonium, which in the meantime had shown itself to be a serious hazard, and which was inadequately housed in the Technical Area.

17.61 The new plant, the so-called DP Site, located on South Point, was divided into two areas. The first of these, the East Area, was designed for the processing of polonium and the production of initiators, under the supervision of Group CM-15, the Polonium Group. The second area, the so-called West Area, was designed for the processing of plutonium and the production of bomb cores. This area was under the control of Group CM-13.

BUILDING DESIGN

17.62 When building design work started, the final processes were not worked out. However, it was necessary to design buildings which would house any finally accepted process. The buildings consist of four identical working buildings, plus an office, in the West Area, and one working building plus an office in the East Area. The buildings are of entirely noncombustible construction, steel walls and roof, rock wool insulation, metal lathe and plaster lining. All rooms have smooth walls and rounded corners for easy cleaning. Each of the West Area operating buildings is 40 x 200 feet and contains two 30 x 30 feet operating rooms and two 40 x 50 feet operating rooms. The East Area operating building is 40 x 240 feet and is broken up into small rooms.

17.63 The chief feature of the buildings is the ventilating system. The air is withdrawn from the rooms through hoods at a rate of about 2 cubic feet per second. Where the hood capacity of the rooms is too small, additional exhaust ducts are furnished so that the air in every room is changed once every 2 minutes. The exhaust ducts assemble into a common duct. The exhaust air from each Area (East and West) is then passed through a bank of electrostatic filters to remove contamination, through a bank of paper emergency filters, and finally through a series of 50 foot stacks. The air is exhausted by four 50 horsepower blowers in the West Area, and by two 40 horsepower blowers in the East Area.

17.64 A few preliminary building drawings were made early in February and serious design work started about the first of March. About March 15 work was started on the East Area buildings. The latter set of buildings was essentially complete June 1. Installation of equipment was complete by July 15, and operations started shortly after that date. The West Area buildings were complete by July 15. The much more extensive process installation was essentially complete by October 1, but minor difficulties prevented plant operation until November 1, after the end of the period described in this history.

PROCESS DESIGN

17.65 When work started on design of a new plant, the operating procedures were not fully worked out. Subsequently, these processes were worked out by various groups in the CM Division. While these procedures were being used in D Building, a new plant committee supervised the redesign of the equipment for use at DP Site.

17.66 For safe operation with plutonium and polonium, all operations are carried out in closed systems. For easy plant operation, all operations are designed to be carried out in a routine fashion. Finally, to prevent chain reactions with plutonium, all equipment is designed so that no more than a safe amount can be charged into any piece of apparatus.

17.67 Improved protective furniture (hoods, and dry boxes--sealed boxes containing an inert atmosphere with inserted gloves) was designed for every operation. This equipment was all made out of stainless steel for corrosion and fire resistance and ease of decontamination. A total of about 20 carloads of this furniture was fabricated by the Kewaunee Manufacturing Company in about 100 days.

Chapter XVIII

PROJECT TRINITY

Pre-Trinity

18.1 Preparations for an experimental nuclear explosion were begun in March 1944 when it was decided by the Director and most of the group and division leaders of the Laboratory that such a test was essential. It was extremely difficult to plan integral experiments which would duplicate in any satisfactorily complete way the conditions of a bomb. The many questions about a practical bomb left unanswered by theory and differential and integral experiments could only be answered by an actual experiment with full instrumentation. Kistiakowsky, then Deputy Division Leader for the implosion program in the Engineering Division, formed E-9, the High Explosives Development Group, under Bainbridge, to investigate and design full scale HE assemblies and prepare for a full scale test with active material. Group E-9 became Group X-2 (Development, Engineering, Tests) during the general Laboratory reorganization.

18.2 The first systematic account of the test plan was made in a memorandum early in September 1944 by Fussell and Bainbridge, in which it was considered that the energy release might be from 200 tons to 10,000 tons TNT equivalent. These early plans were based on the assumption that Jumbo, a large steel vessel, would enclose the bomb so that the active material could be recovered in case of a complete failure. Among the tests planned at this time were the following:

Blast measurements - piezo electric gauges
paper diaphragm gauges
condenser blast gauges
Barnes' Boxes (not used)
condenser gauge blast measurement from plane

Ground shock measurements - geophones
seismographs
Neutron measurements - gold foil
fast ion chamber (not used in this form)
Gamma Rays - recording in plane, dropped "gauges" - (not used)
gamma ray sentinels
Nuclear efficiency
Photographic studies - fastax cameras at 800 yards
spectrographic studies, radiation characteristics
photometric
ball of fire studies
SCR-584 Radar
Meteorology

Additional nuclear measurements were considered in three reports by Moon, who anticipated some of the experiments later adopted.

18.3 One of the early problems of the test group was that of selecting a site. At one time eight different sites were considered - the Tularosa Basin; Jornada del Muerto; the Desert training area near Rice, California; San Nicolas Island off the coast of southern California; the lava region south of Grants, New Mexico; southwest of Cuba, New Mexico, and north of Thoreau; sand bars off the coast of south Texas; and the San Luis Valley region near the Great Sand Dunes National Monument in Colorado. There were a number of factors to be considered in making the selection. Scientific considerations required that the site be flat to minimize extraneous effects on blast and that the weather be good on the average because of the large amount of optical information desired. Safety precautions required that ranches and settlements be distant to avoid possible danger from the products of the bomb. Tight time scheduling required a minimum loss of time in travel of personnel and transportation of equipment between Project Y and the test site, while security required complete separation of the activities at the test site from the activities at Y. The choice finally narrowed to either the Jornada del Muerto region in the northwest corner of the Alamogordo Bombing Range, or the desert training area north of Rice, California. The final choice of the Jornada del Muerto was made, with General Groves' approval, after consultation with General Ent of the Second Air Force early in September 1944. The project secured the use of an area approximately 18 x 24 miles within the base, with the nearest habitation 12 miles away and the nearest town about 27 miles away.

18.4 Once a site had been selected it became extremely important to secure good maps of the region. Arrangements were made with the Second

Air Force for a 6 inch to the mile mosaic to be made of a strip 6 x 20 miles including point zero (the point of detonation) at the center. These aerial mosaics were extremely useful both for the early exploratory work and for final precise planning. A great deal of delay was occasioned because of an inadequate supply of maps, which had to be obtained through the Security Office in order not to reveal the Laboratory's interest in the regions in question. The maps finally used were obtained by devious channels and included all of the geodetic survey maps for New Mexico and southern California, all of the costal charts of the United States, and most of the grazing service and county maps for the state of New Mexico.

18.5 The original plans for construction of the base camp at the test site were drawn up by Capt. S. P. Davalos (Assistant Post Engineer), Bainbridge, and Fussell in October 1944, and provided for a maximum of 160 men. This was supported by a memorandum from Kistiakowsky outlining the plan and scope of the proposed operations and justifying construction requirements. These two documents were approved by Gen. Groves and contracts were let for the initial construction early in November. The camp was completed late in December, and a small detachment of Military Police under Lt. H. C. Bush took up residence. Laboratory personnel concerned with the Trinity tests agreed that the wise and efficient running of the Base Camp by Lt. Bush under extremely primitive conditions contributed greatly to the success of the tests.

18.6 With the concentration of the Laboratory on the implosion program beginning in August 1944, the test program lost in priority. The shortage of manpower for research and development work resulted in the members of the Development, Engineering, Tests Group devoting most of their time and effort to engineering problems and abandoning to a large extent their work on the test. Among the few accomplishments during this period were the layout of the test site, the design and construction of shelters, the collection of earth samples, the procurement of meteorological and blast gauge equipment, and a certain amount of planning for the measurement of nuclear radiations.

Trinity Organization

18.7 By March 1945 almost all the essential physics research for the bomb had been completed and Oppenheimer proposed the establishment of Project TR, an organization with division status, composed of personnel chiefly from the Research Division, which was to have full responsibility for a complete test. As originally organized, Project TR included the following:

Head	K. T. Bainbridge
Safety Committee	S. Kershaw
TR U.S.E.D.	Capt. S. P. Davalos
Security	Lt. R. A. Taylor
CO MP Detachment	Lt. H. C. Bush
Consultants	W. G. Penney
	V. F. Weisskopf
	P. B. Moon
TR-1 Services	J. H. Williams
TR-2 Shock and Blast	J. H. Manley
TR-3 Measurements	R. R. Wilson
TR-4 Meteorology	J. M. Hubbard
TR-5 Spectrographic and Photographic	J. E. Mack
TR-6 Airborne Measurements	B. Waldman

This organization expanded rapidly and by the time of the test involved about 250 technical men. Groups R-1, R-2, R-3, R-4, F-4, G-11, O-4, T-3, and T-7 worked full time on the Trinity Project, and various other groups gave a great deal of time to this work. Group G-4 manufactured the greater part of the electronic equipment.

18.8 In June 1945 Project TR included the following:

Head		K. T. Bainbridge
Aide		F. Oppenheimer
TR U.S.E.D.		Capt. S. P. Davalos
Security		Lt. R. A. Taylor
CO MP Detachment		Lt. H. C. Bush
Consultants	Structures	R. W. Carlson
	Meteorology	P. E. Church
	Physics	E. Fermi
	Damage	J. O. Hirschfelder
	Safety	S. Kershaw
	Earth Shock	L. D. Leet
	Blast and Shock	W. G. Penney
	Physics	V. F. Weisskopf
TR Assembly		Cmdr. N. E. Bradbury
		G. B. Kistiakowsky, Alternate
TR-1 Services		J. H. Williams
TR-2 Air Blast and Earth Shock		J. H. Manley

TR-3 Physics	R. R. Wilson
TR-4 Meteorology	J. M. Hubbard
TR-5 Spectrographic and Photographic Measurements	J. E. Mack
TR-6 Air Blast	B. Waldman
TR-7 Medical Group	Dr. L. H. Hempelmann

18.9 By the time Project TR was set up, all of the elaborate schemes for recovery of active material were virtually abandoned, including the use of Jumbo (16.53) and the use of large quantities of sand or of water. At the time recovery methods were considered seriously the supply of active material was extremely limited and there was a very strong feeling that the bomb might fail completely to explode. As confidence in the ultimate success of the bomb increased and adequate production of active materials seemed assured, the recovery program no longer seemed essential. Perhaps the most important deciding factor, however, was the fact that any effective recovery program would interfere seriously with securing information on the nature of the explosion, which was, after all, the principal reason for the test. Jumbo was taken to the site and erected at a point 800 yards from its originally planned location, since it was not to be used for this test.

Rehearsal Test

18.10 The first task of the new group was that of preparing a rehearsal shot known as the 100 ton shot. This had been proposed in the summer of 1944 both as a full dress administrative rehearsal and as a way of providing calibration of blast and earth shock equipment for the nuclear bomb test. It was finally scheduled to take place on May 5, 1945. The date was extended to May 7 to allow the installation of additional equipment, but several requests for an additional time extension had to be refused because any further delay would have delayed the final test, which was already very tightly scheduled.

18.11 The test was carried out early in the morning of May 7 with 100 tons of HE stacked on the platform of a 20 foot tower. Very little experimental work had ever been done on blast effects above a few tons of HE, and it was important to obtain blast and earth shock results in order to determine the proper structures to use to withstand these effects for the final shot. By using appropriate scale factors, the center of gravity of the 100 ton stack of HE was made 28 feet above the ground in scale with the 100 foot height for the 4000 to 5000 tons expected in the final test. The stack of HE was provided with tubes containing 1000 curies of fission products derived from a Hanford slug to simulate at a low level of activity the radioactive products expected from the nuclear explosion. Measurements of blast effect, earth shock, and damage to apparatus and apparatus shelters were made at distances in scale with the distances proposed for the final shot. Measurements to determine "cross-talk" between circuits and photographic observations were in general carried out at the full distances proposed for the final shot.

18.12 The test was successful as a trial run, and was useful chiefly for suggesting methods for improving procedures for the final test. The most critical administrative needs emphasized by the test were better transportation and communication facilities and more help on procurement. The chief purposes of the test were accomplished. Men who had worked in well-equipped laboratories became familiar with the difficulties of field work. Blast measurement and earth shock data were valuable in calibrating instruments and providing standards for the safe design of shock-proof instrument shelters. Measurement of the effects from the radioactive material inserted in the stack of HE was especially valuable in giving information on the probable amount and distribution of material which would be deposited on the ground. This information secured by the Fission Studies Group of F Division was essential for planning the recovery of equipment, the measurement of bomb efficiency, and protection of personnel for the final test. The high percentage of successful measurements in the final test may be attributed in large measure to the experience gained from the rehearsal shot practice.

Preparations for the Trinity Test

18.13 When Project Trinity was established, July 4 was set as the target date for the test, although it was doubtful that this date could be met. Preparations for the test continued at an increasingly rapid pace after the completion of the rehearsal shot. The breadth and intensity of the preparations which were necessary for the Trinity test cannot be overemphasized. The task was one of establishing under conditions of extreme secrecy and great pressure a complex scientific laboratory on a barren desert. The number of people available was very small in comparison with the amount of work to be done. Over 20 miles of black top road were laid, plus a paved area in the vicinity of the tower. All personnel and equipment had to be transported from Los Alamos, and after considerable effort the Trinity staff succeeded in securing about 75 vehicles. About 30 more were added during the last week by the monitoring and intelligence groups. A complete communications system had to be installed, including telephone lines, public address systems in all of the buildings, and FM radios in 18 of the cars. Miles of wires were used both for the communications system and in conjunction with the various experiments. A complete technical stockroom had to be established, and all of its varied contents transported from Los Alamos. The stockroom was officially known as "Fubar." Sanitary conditions were difficult to maintain, especially in the mess hall, because of the hardness of the water. Because of the extremely tight schedule for the test, any delay in the procurement or delivery of needed material meant that the group affected would have to redouble its efforts when the things finally did arrive. The combination of tight schedule and shortage of personnel meant that most of the people at Trinity, from mess attendants to group leaders, worked at fever pitch, especially during the last month. A 10 hour working day was considered normal, and often it stretched to 18 hours.

18.14 Among the most complex administrative problems associated with the test were those solved by the Services Group. This group undertook the very difficult task of providing the wiring, power, transportation, communication facilities, and construction needed. The construction schedule was especially tight and required a great deal of careful planning and hard work to complete successfully. For a month before the test there were nightly meetings to hear reports on field construction progress and to plan the assignment of men for the following day. Construction help was assigned on the basis of the priority of the experiments for which it was needed.

18.15 Considerable attention was paid to security and to the legal and safety aspects of the test. A great effort was made to dissociate the work at Trinity from that at Los Alamos. There was a great deal of discussion about what should be done about people in surrounding towns. This was finally settled by having a group of 160 enlisted men under the command of Major T. O. Palmer stationed north of the test area with enough vehicles to be able to evacuate ranches and towns if this was found necessary at the last moment. At least 20 men associated with Military Intelligence were stationed in neighboring towns and cities up to 100 miles away, and most of these men served a dual function by carrying recording barographs in order to get permanent records of blast and earth shock at remote points.

18.16 One minor source of excitement was the accidental bombing, with two dummy bombs, of the Trinity base camp by a plane from the Alamogordo Air Base early in May. The incidents were reported to the base commander through the Security officer and precautions were taken to prevent their recurrence.

18.17 Early in April Project TR secured the services of J. M. Hubbard, meteorology supervisor for the Manhattan District. He requested information from the various experimental groups on the particular weather conditions or surveys which they would find useful in their operations, and made an effort to find a time that would meet nearly every specification of the various groups. He secured the cooperation of the Weather Division of the Army Air Forces and was able to draw on information of a world-wide nature in making his surveys. The period which he selected as first choice for the final test was July 18 to 19, 20 to 21, with 12 to 14 as second choice, and July 16 only a possible date.

18.18 One of the most difficult problems faced by Project TR was that of scheduling. Weekly meetings were held, with consultants and responsible group and section leaders attending to consider new experiments and discuss detailed scheduling and progress reports. It was important to get as much information from the test as possible, but it was not possible because of the limitations of time and personnel to schedule every experiment proposed. In order to have a new experiment considered by the weekly scheduling meeting, the person making the proposal would have to prepare a detailed account of the objectives of the experiment, the accuracy expected, and the requirements for equipment, personnel, and machine shop and electronics shop time. On the basis of such information the Trinity scheduling group could decide whether a particular experiment was suitable and whether it had any possibility of being successfully completed.

18.19 The July 4 date accepted in March was soon found to be unrealistic. Delays in the delivery of full scale lens molds and the consequent delay in the development and production of full scale lenses, as well as the tight schedule in the production of active material, made it necessary to reconsider the date. The Cowpuncher Committee made an effort to schedule the pacing components in order to determine the time at which other components or other developments would have to be completed in order not to delay the test. By the middle of June, the Cowpuncher Committee agreed that July 13 was the earliest possible date, and the 23rd was a probable date. Because of the great pressure to have the test as early as possible, it would undoubtedly have to take place before all reasonable experiments, tests, and improvements could be made, but the July 13 date was fixed so that essential components would be ready at that time. On June 30 a review was made of all schedules by the Cowpuncher Committee, and the earliest date for Trinity was changed to July 16 in order to include some important experiments. Commitments had been made in Washington to have the test as soon after July 15 as possible, and these commitments were met by firing the shot early on the morning of July 16, as soon as weather conditions were at all suitable.

18.20 Four rehearsals were held on the 11th, 12th, 13th and 14th with all personnel cooperating. A "dry run" of the assembly of the HE component of the bomb was held early in July following a number of tests to study methods of loading and effects of transportation. Final assembly of the HE began on July 13, and of the active core on the same day. Nuclear tests and the assembly of the active component were carried out in McDonald's ranch house - a four room frame house about 3400 yards from the detonation point. The various pieces of apparatus employed were identical with those already shipped to Tinian, and the operation took on the character of a field test for the overseas expedition. On July 11 the active material was brought down by convoyed sedan from Los Alamos in a field carrying case designed for use overseas. The HE components were assembled at one of the outlying sites at Los Alamos and brought down by convoyed truck, arriving at Trinity on Friday, July 13.

18.21 Before the assembly started, a receipt for the active material was signed by Brig. Gen. T. F. Farrell, deputy for Gen. Groves, and handed to L. Slotin who was in charge of the nuclear assembly. The acceptance of this receipt signalized the formal transfer of the precious Pu^{239} from the scientists of Los Alamos to the Army to be expended in a test explosion. The final assembly took place on a canvas-enclosed flooring which had been built for the purpose within the base of the tower. Active material in large quantity was put within HE for the first time on this occasion. Although the people performing the operation and those watching it were outwardly calm, there was a great feeling of tension apparent. Only one difficulty was encountered which made the actual carrying out of the assembly anything more than a routine repetition of rehearsals. The heat of the desert together with the heat generated by the active material caused an interference between some of the parts, because a portion of the assembly had been completed the night before on the high mesa of Los Alamos and was cold to the touch. Differential expansion between these two parts was enough to cause interference. A delay of a minute or two occurred while the hot material was placed in contact with the cold material and cooled sufficiently to permit its entry as planned.

18.22 After the HE and nuclear components were completely assembled, the bomb was still without detonators. It was hauled to the top of its 100 foot tower where it rested in a specially constructed sheet steel house. On the 14th the Detonator Group installed the detonators and informers, and the Prompt Measurements Group and other test groups checked and completed the installation of apparatus for their experiments. Visits were made to the top of the tower every 6 hours by members of the Pit Assembly Group to withdraw the manganese wire whose induced radioactivity was a measure of the neutron background.

18.23 Elaborate plans were made for the evacuation of personnel in the event of any serious difficulty, with the Medical Officer to be in charge. The Arming Party, a small group responsible for final operations, also assumed responsibility for guarding the bomb against possible sabotage, and remained at the tower until the last possible moment. The weather seemed unfavorable early in the morning of the 16th, and not until shortly before 5:00 a.m. did the weather reports received from Hubbard begin to look satisfactory. As originally planned, the decision whether or not to run the test was to be made by Oppenheimer, Gen. Farrell, Hubbard, and Bainbridge, with one dissenting vote sufficient to call it off. The final decision was made and announced at 5:10 a.m. and the shot was scheduled to be fired at 5:30 a.m.

18.24 Nearest observation points were set up 10,000 yards from the tower with Base Camp located 17,000 yards from the tower. A number of distinguished visitors came for the test including Tolman, Bush, Conant, Gen. Groves, C. Lauritsen, Rabi, E. O. Lawrence, A. Compton, Taylor, Chadwick, Thomas, and von Neumann. All were instructed to lie on the ground, face downward, heads away from the direction of the blast. The control station, which was located at 10,000 yards, was connected to the various observation points by radio. From here periodic time announcements were made beginning at minus 20 minutes until minus 45 seconds. At that time automatic controls were switched on, setting off the explosion at 5:29 a.m. on Monday, July 16, 1945, just before dawn.

Trinity

18.25 There have been a great many descriptions of the explosion; one of the most graphic is that of Gen. Farrell who saw it from one of the 10,000 yard observation points. He said, in part: "The effects could well be called unprecedented, magnificent, beautiful, stupendous and terrifying. No man-made phenomenon of such tremendous power had ever occurred before. The lighting effects beggared description. The whole country was lighted by a searing light with the intensity many times that of the midday sun. It was golden, purple, violet, gray and blue. It lighted every peak, crevasse and ridge of the nearby mountain range with a clarity and beauty that cannot be described but must be seen to be imagined...." Several of the men stationed at Base Camp and members of the Coordinating Council of the Laboratory who watched the explosion from the hills about 20 miles away prepared eyewitness accounts of their experiences. All were deeply impressed by the intensity of the light, and also by the heat and the visible blue glow. Of the heat, one man said, "I felt a strong sensation of heat on the exposed skin of face and arms, lasting for several seconds and at least as intense as the direct noon sun." Of the blue glow, another reported, "Then I saw a reddish glowing smoke ball rising with a thick stem of dark brown color. This smoke ball was surrounded by a blue glow which clearly indicated a strong radioactivity and was certainly due to the gamma rays emitted by the cloud into the surrounding air. At that moment the cloud had about 1000 billions of curies of radioactivity whose radiation must have produced the blue glow." There were also many detailed accounts of the appearance of the now familiar mushroom-shaped cloud. It was several minutes before people noticed that Jumbo's steel tower had disappeared from view (16.32). At Los Alamos, over 200 miles away, a number of people who were not directly involved in the test and were not members of the Coordinating Council watched for a flash in the southern sky. As the shot had originally been scheduled for 4:00 a.m., many watchers grew impatient and gave up. A few did see it, however, and they reported a brief blinding flash of considerable intensity.

18.26 For many of the men who watched the test at Trinity, the immediate reaction was one of elation and relief, for the successful explosion of the first nuclear bomb represented years of difficult concentrated work. With this elation and relief came a feeling of awe and even of fear at the magnitude of what had been accomplished. For many the successful completion of the Trinity test marked the successful completion of the major part of their work for the Los Alamos Laboratory, and there was a general letdown and relaxation after the intensive efforts of the past months. For those men who were going overseas, however, there was no rest, and their preparations for Trinity were simply a rehearsal of their duties at Tinian.

18.27 Security, which always pervaded the work of the Laboratory, was not forgotten even in the hectic hours after Trinity. As the first cars of weary excited men stopped for food in the little town of Belen on their way back to Los Alamos, they spoke only of inconsequential things, and the occupants of one car did not recognize the occupants of another. In fact, the members of the coordinating council were required to return directly to Los Alamos in buses, avoiding any stops in New Mexico communities. Not until they reached the guarded gates of Los Alamos did the flood of talk burst loose. There was a great sale of Albuquerque newspapers the following day because in them was an account of an "explosives blast" at the Alamogordo Air Base. The story was credited to the Associated Press, but appeared in very few papers outside of New Mexico, and then only as a brief note about an unimportant accident.

Results of Trinity Experiments

18.28 To give some notion of the number and scope of the experiments done in connection with the Trinity test, the following summary is included. (See chart of location of Trinity experiments in Appendix 4). There were six chief groups of experiments: (1) Implosion; (2) Energy release by nuclear measurements; (3) Damage, blast and shock; (4) General phenomena; (5) Radiation measurements, and (6) Meteorology.

Implosion experiments included:

(a) Detonator asimultaneity measured with detonation wave-operated switches and fast scopes. These records were fogged by gamma rays.
(b) Shock wave transmission time measured by recording on a fast scope the interval from the firing of the detonators to the nuclear explosion.
(c) The multiplication factor (α) measurement was done by three methods - with electron multiplier chambers and a time expander; by the two chamber method; and with a single coaxial chamber, coaxial transformers, and a direct deflection high speed oscillograph.

The calculation of energy release by nuclear measurements included:

(a) Delayed gamma rays measured by ionization chambers, multiple amplifiers, and Heiland recorders from both ground and balloon sites.
(b) Delayed neutron measurements done in three ways - by the use of a cellophane catcher and U^{235} plates both on the ground and airborne, by the use of gold foil detectors to give an integrated flux, and by the use of sulphur threshold detectors. For the cellophane catcher method a record was obtained from the 600 meter station. With the gold foil method the number of neutrons per square centimeter per unit logarithmic energy interval was measured at 7 stations ranging from 300 to 1000 meters from the explosion. Of the sulphur threshold detectors only 2 of the 8 units used were recovered, and these gave the neutron flux for energies

of 3 Mev at 200 meters.

(c) The conversion of plutonium to fission products, measured by determining the ratio of fission products to Pu, gave a result equivalent to 18,600 tons of TNT. An attempt to collect fission products and plutonium on filters from planes at high altitude from the dust of the shot after it circled the world gave no results, although later some indications were obtained after the Hiroshima explosion by this method.

Damage, blast and shock experiments were divided into three groups: (a) blast, (b) earth shock, and (c) ignition of structural materials.

(a) Blast measurements included:

(1) Quartz piezo gauges - these gave no records since the traces were thrown off scale by radiation effects.

(2) Condenser gauges of the California Institute of Technology type were dropped from B-29 planes but no records were obtained because the shot had to be fired when the planes were out of position.

(3) The excess velocity of the shock wave in relation to sound velocity was measured with a moving coil loudspeaker pick-up, by the optical method with blast-operated switches and torpex flash bombs, and by the Schlieren method. By the moving coil loudspeaker method the velocity of sound was obtained for a small charge and then the excess velocity for the bomb; this measurement gave a yield of 10,000 tons and proved to be one of the most successful blast measuring methods.

(4) Peak pressure measurements were done with spring-loaded piston gauges at an intermediate pressure range of from 2.5 pounds to 10 pounds per square inch, with the same kind of gauges above ground and in slit trenches at a pressure range of from 20 to 150 pounds per square inch, with crusher type gauges, and with aluminum diaphragm "box" gauges at a range of from 1 to 6 pounds. The first of these methods gave blast pressure values which were low compared to all other methods, the crusher type gauges gave the highest pressure range, and the box gauges gave a TNT equivalent to 9900 ± 1000 tons. This last method was found to be inexpensive and reliable.

(5) Remote pressure barograph recorders gave results consistent with 10,000 tons. These were necessary for legal reasons.

(6) Impulse gauges - mechanically recording piston liquid and

orifice gauges - also gave results consistent with 10,000 tons.

(7) Mass velocity measurements were made by viewing with Fastax cameras suspended primacord and magnesium flash powder.

(8) Shock wave expansion measurements were made with Fastax cameras at 800 yard stations and gave a total yield of 19,000 tons.

(b) Earth shock measurements included:

(1) Geophone measurements with velocity-type moving coil strong motion geophones gave 7000 tons after extrapolation from a small charge and 100 ton data.

(2) Seismograph measurements done with Leet 3-component strong motion displacement seismographs gave results of approximately 15,000 tons. These were necessary for legal reasons.

(3) Permanent earth displacement measurements using steel stakes for level and vertical displacements gave results of 10,000 ± 5000 tons.

(4) Remote seismographic observations at Tucson, El Paso, and Denver showed no effect at these distances.

(c) The ignition of structural materials was observed using roofing materials, wood, and excelsior on stakes. Observations showed that the risk of fire produced by radiant energy is small for distances greater than 3200 feet. The risk of fire from direct radiation was likely to be much less than the risk of fire from stoves, etc., at the time of the explosion. These conclusions were confirmed at Hiroshima and Nagasaki.

The study of general phenomena consisted chiefly of photographic studies of the ball of fire and the column of blast cloud effects. This group of studies did include a radar study with 2 SCR-584 radars in which two plots of the cloud were obtained; radar reflection, however, was not found to be favorable. Photographic equipment used for these studies included Fastax cameras ranging from 800 to 8000 frames per second, standard 16 millimeter color cameras, a 24 frame per second Cine-Special, 100 frames per second Mitchell cameras, pinhole cameras, gamma ray cameras, Fairchild 9 x 9 inch aero view cameras at 10,000 yards and at 20 miles for stereo-photos. These photographic records were extremely valuable.

The rise of the column was followed with searchlight equipment and the first 18 miles of the main cloud path was obtained by triangulation. A part of this group of experiments was a number of spectrographic and photometric measurements and measurement of total radiation. Spectrographic measurements were done with Hilger and Bausch and Lomb high-time-resolution spectrographs, photometric measurements with moving film and filters and with photocells and filters recording on drum oscillographs, and total radiation measurements with thermocouples and recording equipment.

Post-shot radiation measurements included:

(a) Gamma-ray sentinels - these ionization chambers which recorded at 10,000 yard shelters were extremely valuable in giving the distribution of radioactive products immediately after the shot until safe stable conditions were assured.
(b) Portable chamber observations in the high gamma flux region were made from heavily shielded army tanks using portable ionization chambers of standard design about 4 hours after the shot, and ionization data from these chambers were radioed back to the control shelter.
(c) A dust-borne product survey was made by the Health Group with portable alpha and gamma ionization chambers and Geiger counters, both at the site of the explosion and at remote points up to 200 miles in order to measure dust-deposited fission products.
(d) Measurement of airborne products from B-29 planes equipped with special air filters was unsuccessful as noted above under blast measurements (2).
(e) A detailed crater survey was made with ionization chambers and amplifiers after 4 weeks and showed approximately 15 roentgens per hour at the edge of the crater and 0.02 roentgen per hour at 500 yards.

Weather information was obtained up to 45 minutes before the shot from the point of detonation to 20,000 feet and 25 minutes after the shot. Low level smoke studies were made to determine the spread of active material in case the nuclear explosion failed to occur. This information was vitally important for the success of the test.

Chapter XIX

PROJECT ALBERTA

Delivery Group

19.1 Although Project Alberta was not organized formally until March 1945, its work had been done by the Delivery Group of the Ordnance Division since June 1943 (7.67 ff) The group was responsible for the delivery of the bomb as a practical airborne military weapon, and during the first part of the Project's history participated in design of the final bomb, and undertook to act as liaison with the Air Force on such matters as the selection of aircraft and the supervision of field tests with mock bombs.

19.2 After the general reorganization of the Laboratory, when it was clear that the plutonium gun assembly method would not be used, three models remained - the Little Boy for the U^{235} gun assembly (7.71), the 1222 Fat Man model of the implosion assembly, and the model which became the finally adopted 1561 Fat Man. By September 1, 1944, it was decided to freeze the external shapes and aircraft requirements of the three models so that the Air Force could begin immediately to train a combat unit for the delivery of the bomb. A production lot of fifteen B-29's was modified at the Martin Nebraska plant under the guidance of S. Dike and M. Bolstad of Los Alamos. The first aircraft became available in October. Wendover Army Air Base in Utah, sometimes called by the code name Kingman, or the symbol W-47, was designated as the training and test center for the new Atomic Bomb Group, and Col. P. W. Tibbets was appointed commanding officer of the combat group known as the 509th Composite Group.

19.3 The first tests began at Wendover in October 1944, and continued up to the time of the first combat drop. A number of groups from O, X, and G Divisions, in addition to the Delivery Group, participated in the Wendover tests, including the Fusing Group, the Gun Group, the HE Assembly Group, the Electric Detonator Group, and the Ballistic Group. In November

1944, Cmdr. F. L. Ashworth, USN, assumed the responsibility of supervising these field operations. The long series of tests which had been begun with three tentative and later with the two final models included tests for ballistics information, for electrical fusing information, for flight performance of electrical detonators, for operation of the aircraft release mechanism, for vibration information, for assembly experience, and for temperature effects. Because the first lot of B-29's proved to have poor flying qualities and the special project modifications to have a number of weaknesses, a new lot of fifteen planes was obtained in the spring of 1945. These aircraft, which proved extremely satisfactory, had fuel injector engines, electrically controlled propellers, very rugged provisions for carrying the bomb, and all armament removed except the tail turret. In addition to the tests based at Wendover, a number of test drops were made at the Camel Project's field at Inyokern during 1945 (9.16). In connection with the Wendover tests, the Ballistics Group of O Division did some research on the problem of aircraft safety in delivery. They were concerned with such problems as the shock pressure that a B-29 could safely withstand, the maneuver that would carry the plane a maximum distance away from the target in a minimum time, and the use of special shock bracing for personnel.

19.4 During the fall of 1944 and winter of 1945, the Delivery Group at Los Alamos continued a program of design and production of mock bombs in an effort to achieve a final model. During this period the 1561 Fat Man was adopted in place of the 1222 model. In addition to the Wendover tests, numerous physics and engineering tests on complete units were made at one of the outlying sites at Los Alamos. The Delivery Group also began formulating plans for the establishment of an overseas operating base, known by the code word "Alberta."

Organization and Tests

19.5 In March 1945, Project Alberta or Project A was established to provide a more effective means of integrating the activities of the various Los Alamos groups working on problems of preparation and delivery of a combat bomb than the Delivery Group by itself had been able to offer. The new Project A was independent of any existing division and was organized as a loose coordinating body, with all specific work being done by groups of other divisions, and with Project A providing direction only insofar as preparations for combat delivery were concerned. Captain Parsons was the officer in charge of Project Alberta, with Ramsey and later Bradbury as deputies for scientific and technical matters. The organization included three groups - an administrative group known as the Headquarters Staff, a technical policy committee called the Weapons Committee (9.10) and a working group of representatives from other divisions. Cmdr. Ashworth was operations officer and military alternate for Capt. Parsons and served as chief of the Headquarters staff which eventually included Alvarez, Bolstad, S. Dike, G. Fowler, and S. J. Simmons. Simmons came to the Project in June from Massachusetts Institute of Technology Radiation Laboratory, where he had engaged in similar liaison activities with the Air Force. The Weapons Committee, of which Ramsey was Chairman until he went overseas and was succeeded by Bradbury, included Birch, Brode, G. Fowler, Fussell, Morrison, and Warner. Group representatives included:

Tests at Wendover	Cmdr. Ashworth
Tests at Wendover after June, 1945	Simmons
Measurements, airborne observations	Waldman and Alvarez
General Theory	Bethe
Gun Assembly	Birch
Aircraft	Bolstad and Dike
HE Assembly for implosion	Bradbury and Warner
Fusing	Brode
Electrical detonator system	Fussell
Engineering	Galloway
Supply	Lt. Col. Lockridge
Pit (active material and tamper of Fat Man)	Morrison and Holloway
Radiology	Capt. Nolan
Damage	Penney
Ballistics	Shapiro

19.6 Project Alberta was concerned chiefly with three problems: (1) the completion of design, procurement, and preliminary assembly of bomb units which would be complete in every way for use with active material; (2) continuation of the Wendover test program; and (3) preparation for overseas operations against the enemy.

19.7 Since the time schedule was becoming tighter, the major designs were of necessity continued with as few alterations as possible, although they were in many cases the result of a number of compromises and guesses made at a time when the problem was not well understood. The emphasis during this period was on supplying the many details necessary for successful operation and correcting faults which became apparent in tests. Examples of problems solved are such matters as the exact design of the tamper sphere, inclusion of a hypodermic tube between the HE blocks for monitoring purposes, and strengthening the Little Boy tail. Actually the Little Boy was far ahead of the Fat Man from the point of view of design and development, since the Gun Assembly group had a relatively long time to devote to such improvements. Members of the Weapons Committee were concerned with the need for starting work on an integrated design for the Fat Man based on current knowledge with no commitments to past production, but realized that such a program could not interfere with the primary job of patching up the existing model as quickly as possible. The job of redesigning the Fat Man from a sound engineering point of view eventually became the task of Z Division which was barely organized by the end of the war (9.13). Liaison problems in connection with the development of bombs were of great importance during this period and were handled primarily by Capt. Parsons and Cmdr. Ashworth. Among the military and semimilitary organizations and individuals involved in addition to the United States Engineers were the 20th Air Force, the Bureau of Ordnance, the Assistant Chief of Naval Operations for Material, Commander Western Sea Frontier, Commandant 12th Naval District, Commandant Navy Yard Mare Island, Bureau of Yards and Docks Navy Department, NOTS Inyokern, NMD Yorktown, and NAD McAlester. After Parsons and Ashworth went overseas much of this work was handled by Capt. R. R. Larkin, USN, who arrived at Los Alamos in June.

19.8 The Wendover test program under the supervision of Project Alberta continued at an increasing rate. The principal difficulty encountered in carrying out this program was the unfortunate failure of the company manufacturing Fat Man firing units, known as X-units, to meet its delivery schedule. In addition to reducing the number of tests possible on the X-Units, this failure prevented efficient over-all testing since many tests had to be repeated twice - once at an early date with all components except an X-unit, and once at a critically late date with an X-Unit. The tightness of schedule resulting can best be illustrated by the fact that it was not until the end of July that sufficient X-Units had been tested to confirm their safety with HE. The first live tests with the X-unit were not made until August 4 (Wendover) and August 8 (Tinian). Despite these difficulties a total of 155 test units were dropped at Wendover or Inyokern between October 1944 and the middle of August 1945. Much information was obtained from these tests and the corresponding changes incorporated into the design of the bombs.

Destination

19.9 Perhaps the most important function of Project Alberta was planning and preparing for overseas operations. As early as December 1944 the initial planning and procurement of some kits of tools and materials had begun, and these activities continued at an accelerated rate through July. In February Comdr. Ashworth was sent to Tinian to make a preliminary survey of the location and select a site for project activities. By March the construction needs for the Tinian Base, known as Destination, were frozen, and construction began in April. The buildings which were used by Project Alberta had all been especially constructed by the Seabees. Most of the buildings were located in the area assigned to the 1st Ordnance Squadron (Special) of the 509th Group, near the beach. These buildings included four airconditioned Quonset huts of the type normally used for bombsight repair, in which all the laboratory and instrument work was performed. These buildings were enclosed in a specially guarded area within the guarded working area of the group. In addition five warehouse buildings, a shop building, and an administration building were located here. About a mile away were three widely-spaced, barricaded and guarded, airconditioned assembly buildings. Ten magazines and two special loading pits equipped with hydraulic lifts for loading bombs into the aircraft were also constructed. A third such pit was constructed at Iwo Jima for possible emergency use. Materials for equipping the buildings and for handling heavy equipment in assembly, tools, scientific instruments, and general supplies were all included in special kits prepared by the various groups concerned. A kit for a central stockroom was also started, but the materials for the latter had not been shipped by the time the war ended. Beginning in May five batches of kit materials and of components for test and combat units were shipped by boat to Tinian, and a number of air shipments for critically needed items were made in five C-54 aircraft attached to the 509th Group. Project Alberta was able to beat its schedules largely because of the availability of these C-54's for emergency shipments.

19.10 As early as June 1944, the need had been considered for selecting personnel for field crews required in final delivery of the bomb and in the later stages of experimentation and testing prior to delivery. At that time it was agreed, however, that since the type of work might change and since there were many people anxious to volunteer, it would be wise to delay recruiting. Actually the personnel for the project teams at Tinian were selected early in May 1945, and were organized as follows;

Officer-in-Charge	Captain Parsons
Scientific and Technical Deputy	Ramsey
Operations Officer and Military Alternate	Comdr. Ashworth
Fat Man Assembly Team	Warner
Little Boy Assembly Team	Birch
Fusing Team	Doll
Electrical Detonator Team	Lt. Comdr. E. Stevenson
Pit Team	Morrison and Baker

Observation Team	Alvarez and Waldman
Aircraft Ordnance Team	S. Dike
Special Consultants	Serber, Penney, and Capt. J. F. Nolan

Team members included: H. Agnew, Ens. D. L. Anderson, T/5 B. Bederson, M. Bolstad, T/Sgt. R. Brin, T/Sgt. V. Caleca, M. Camac, T/Sgt. E. Carlson, T/4 A. Collins, T/Sgt. R. Dawson, T/Sgt F. Fortine, T/3 W. Goodman, T/3 D. Harms, Lt. J. D. Hopper, T/Sgt. J. Kupferberg, L. Johnston, L. Langer, T/Sgt. W. Larkin, H. Linschitz, A. Machen, Ens. D. Mastick, T/3 R. Matthews, Lt. (jg) V. Miller, T/3 L. Motichko, T/Sgt. W. Murphy, T/Sgt. E. Nooker, T. Olmstead, Ens. B. O'Keefe, T. Perlman, Ens. W. Prohs, Ens. G. Reynolds, H. Russ, R. Schreiber, T/Sgt. G. Thornton, Ens. Tucker, and T/4 F. Zimmerli.

19.11 The Los Alamos group formed part of what was known as the First Technical Service Detachment, and this army administrative organization provided housing and various services and established security regulations at Tinian. Also closely associated with the work of Project Alberta at Tinian were the members of the 509th Composite Group, whose duty it was to deliver the bombs to the enemy.

19.12 It was decided that Laboratory employees would remain on the Contractor's payroll. They were provided with per diem and uniform allowances in addition to their regular salaries and also with insurance policies. Each civilian was required to wear a uniform and received an assimilated army rank in accordance with his civilian salary classification.

19.13 Team leaders formed a Project Technical Committee under the chairmanship of Ramsey to coordinate technical matters and to recommend technical actions. Project personnel were responsible for providing and testing certain of the bomb components; for supervision and inspection during the assembly of bombs; for inspection prior to takeoff; for testing completed units; for over-all coordination of project activities, including the certification of the satisfactoriness of the unit; and finally for providing advice and recommendations about the use of the weapon.

19.14 Although preliminary construction at Tinian began in April, technical work did not begin to any great extent until July. The first half of July was occupied in establishing and installing all of the technical facilities needed for assembly and test work at Tinian. After completion of these technical preparations a series of four Little Boy tests was carried out with uniformly excellent results. The last of these included as part of the test a check of facilities at Iwo Jima for emergency reloading of the bomb into another aircraft. The first of three Fat Man tests was made on August 1 and showed essential components operating satisfactorily; the last of these tests on August 8 was conducted as a final rehearsal for delivery and used a unit that was complete except for active material.

19.15 The U^{235} projectile for the Little Boy was delivered at Tinian by the cruiser Indianapolis on July 26, only a few days before its tragic sinking off Peleliu. The Indianapolis had been especially held at San Francisco to wait for this cargo, and had then made a record run across the Pacific. The rest of the U^{235} components arrived on the 28th and 29th of July, as the only cargo of three Air Transport Command C-54's. Since the earliest date previously discussed for combat delivery was August 5 (at one time the official date was August 15). Parsons and Ramsey cabled Gen. Groves for permission to drop the first active unit as early as August 1. Although the active unit was completely ready, the weather was not, and the first four days of August were spent in impatient waiting. Finally on the morning of August 5 a report came that weather would be good the following day, and shortly afterwards official confirmation came from Maj. Gen. LeMay, Commanding General of the 20th Air Force, that the mission would take place on August 6. The Little Boy was loaded onto its transporting trailer the moment the official confirmation came through and was taken to the loading pit and loaded into the B-29. Final testing of the unit was completed and all was ready early in the evening. Between then and takeoff the aircraft was under continuous watch both from a military guard and from representatives of the key technical groups. Final briefing was at midnight, and shortly afterward the crews assembled at their aircraft under brilliant floodlights with swarms of photographers taking still and motion pictures. For this mission Col. P. W. Tibbets was pilot of the Enola Gay, the B-29 which carried the bomb. Maj. Thomas Ferebee was bombardier, Capt. Parsons was bomb commander, and Lt. Morris Jepson was electronics test officer for the bomb.

19.16 Only a few days before the scheduled drop it was decided by the technical group that it was not safe to take off with the bomb completely assembled, since a crash might mean tremendous destruction to men and materials on Tinian. Full safing could not be secured, but it was finally agreed that a partial safeguard would come if the cartridge which contained the propellant charge were inserted through the opening in the breech block during flight rather than on the ground. This scheme had been considered before (14.14) but was not finally adopted until this time. Capt. Parsons, who was already assigned to the crew as weaponeer, was given the job. This decision meant that Capt. Parsons had to be trained in a short time to perform the operation, and also that the bomb bay of the B-29 had to be modified to provide him with a convenient place to stand while completing the assembly. These things were done and the bomb was not completely assembled until the plane was safely in the air.

19.17 The progress of the mission is described in the log which Capt. Parsons kept during the flight:

6 August 1945

0245 Take Off
0300 Started final loading of gun
0315 Finished loading
0605 Headed for Empire from Iwo
0730 Red plugs in (these plugs armed the bomb so it would detonate if released)
0741 Started climb

Weather report received that weather over primary and tertiary targets was good but not over secondary target

0838 Leveled off at 32,700 feet
0847 All Archies (electronic fuses) tested to be OK
0904 Course west
0909 Target (Hiroshima) in sight
0915-1/2 Dropped bomb (Originally scheduled time was 0915)

Flash followed by two slaps on plane. Huge cloud

1000 Still in sight of cloud which must be over 40,000 feet high
1003 Fighter reported
1041 Lost sight of cloud 363 miles from Hiroshima with the aircraft being 26,000 feet high

The crews of the strike and observation aircraft reported that 5 minutes after release a low 3 mile diameter dark grey cloud hung over the center of Hiroshima, out of the center of this a white column of smoke rose to a height of 35,000 feet with the top of the cloud being considerably enlarged. Four hours after the strike, photo-reconnaissance planes found that most of the city of Hiroshima was still obscured by the cloud created by the explosion, although fires could be seen around the edges. Pictures were obtained the following day and showed 60 per cent of the city destroyed.

19.18 The active component of the Fat Man came by special C-54 transport. The HE components of two Fat Men arrived in two B-29's attached to the 509th Group, which had been retained at Albuquerque especially for this purpose. In all cases the active components were accompanied by special personnel to guard against accident and loss.

19.19 The first Fat Man was scheduled for dropping on August 11 (at one time the schedule called for August 20, but by August 7 it was apparent that the schedule could be advanced to August 10. When Parsons and Ramsey proposed this change to Tibbets he expressed regret that the schedule could not be advanced two days instead of only one, since good weather was forecast for August 9 and bad weather for the five succeeding days. It was finally agreed that Project Alberta would try to be ready for August 9, provided it was understood by all concerned that the advancement of the date by two full days introduced a large measure of uncertainty. All went well with the assembly, however, and the unit was loaded and fully checked late in the evening of August 8. The strike plane and two observing planes took off shortly before dawn on August 9. Maj. C. W. Sweeney was pilot of the strike ship Great Artiste, Capt. K. K. Beahan was bombardier, Comdr. Ashworth was bomb commander, and Lt. Philip Barnes was electronics test officer.

19.20 It was not possible to "safe" the Fat Man by leaving the assembly incomplete during takeoff in the same manner as the Little Boy. The technical staff realized that a crash during takeoff would mean a serious risk of contaminating a wide area on Tinian with plutonium scattered by an explosion of the HE, and even some risk of a high-order nuclear explosion which would do heavy damage to the island. These risks were pointed out to the military with the request that special guarding and evacuation precautions be taken during the takeoff. The Air Force officer in command decided that such special precautions were not necessary, and as it turned out the takeoff was made without incident. This mission was as eventful as the Hiroshima mission was operationally routine. Comdr. Ashworth's log for the trip is as follows:

0347 Take off
0400 Changed green plugs to red prior to pressurizing
0500 Charged detonator condensers to test leakage. Satisfactory.
0900 Arrived rendezvous point at Yakashima and circled awaiting accompanying aircraft.
0920 One B-29 sighted and joined in formation.
0950 Departed from Yakashima proceeding to primary target Kokura having failed to rendezvous with second B-29. The weather reports received by radio indicated good weather at Kokura (3/10 low clouds, no intermediate or high clouds, and forecast of improving conditions). The weather reports for Nagasaki were good but increasing cloudiness was forecast. For this reason the primary target was selected.
1044 Arrived initial point and started bombing runs on target. Target was obscured by heavy ground haze and smoke. Two additional

runs were made hoping that the target might be picked up after closer observations. However, at no time was the aiming point seen. It was then decided to proceed to Nagasaki after approximately 45 minutes spent in the target area.

1150 Arrived in Nagasaki target area. Approach to target was entirely by radar. At 1150 the bomb was dropped after a 20 second visual bombing run. The bomb functioned normally in all respects.

1205 Departed for Okinawa after having circled smoke column. Lack of available gasoline caused by an inoperative bomb bay tank booster pump forced decision to land at Okinawa before returning to Tinian.

1351 Landed at Yontan Field, Okinawa

1706 Departed Okinawa for Tinian

2245 Landed at Tinian

Because of bad weather good photo reconnaissance pictures were not obtained until almost a week after the Nagasaki mission. They showed 44 per cent of the city destroyed; the discrepancy in results between this mission and the first was explained by the unfavorable contours of the city.

19.21 Exchange of information between Tinian and Los Alamos was extremely unsatisfactory and caused considerable difficulty at each end. Necessarily tight security rules made direct communications impossible, and teletype messages were relayed from one place to the other through the Washington Liaison Office, using an elaborate table of codes prepared by Project Alberta. Late in July, the Laboratory sent Manley to the Washington Liaison Office in an attempt to make sure that there would be no friction in the regular channels of information, and that no information was being held up in Washington which would conceivably be of interest. The first news of the Hiroshima drop came to Los Alamos in a dramatic teletype prepared by Manley summarizing the messages sent by Parsons from the plane after the drop (see Appendix No. 2).

19.22 On the day following the Nagasaki mission, the Japanese initiated surrender negotiations and further activity in preparing active units was suspended. The entire project was maintained in a state of complete readiness for further assemblies in the event of a failure in the peace negotiations. It was planned to return all Project Alberta technical personnel to the United States on August 20, except for those assigned to the Farrell mission for investigating the results of the bombing in Japan. Because of the delays in surrender procedures, Gen. Groves requested all key personnel to remain at Tinian until the success of the occupation of Japan was assured. The scientific and technical personnel finally received authorization and left Tinian on September 7, except for Col. Kirkpatrick and Cmdr. Ashworth who remained to make final disposition of project property. With this departure the activities of Project Alberta were terminated.

19.23 The objective of Project Alberta was to assure the successful combat use of an atomic bomb at the earliest possible date after a field test of an atomic explosion and after the availability of the necessary nuclear material. This objective was accomplished. The first combat bomb was ready for use against the enemy within 17 days after the Trinity test, and almost all of the intervening time was spent in accumulating additional active material for making another bomb. The first atomic bomb was prepared for combat use against the enemy on August 2, within four days of the time of the delivery of all of the active material needed for that bomb. Actual combat use was delayed until August 6 only by bad weather over Japan. The second bomb was used in combat only three days after the first, although it was a completely different model and one much more difficult to assemble.

Chapter XX

CONCLUSION

20.1 After the end of the war the Laboratory experienced a sudden relaxation of activities. Everything had been aimed at a goal, and the goal had been reached. It was a time for evaluation and stock-taking. Plans for the future of Los Alamos and of nuclear research in general were widely discussed. Members of the Scientific Panel of the President's Interim Committee on Atomic Energy met at Los Alamos and prepared for the Committee an account of the technical possibilities then apparent in the atomic energy field. A series of lecture courses was organized, called the "Los Alamos University," to give the younger staff members the opportunity to make up for some of the studies they had missed during the war years.

20.2 While research projects that had been under way at the end of the war were being completed, plans for the period to follow were being formulated. Although their discussion leads beyond the period of the present report, one that may be mentioned was the outlining and writing of a Los Alamos Technical Series, under the editorship of H. A. Bethe, to set down a more systematic and polished record of the Laboratory's work than had been possible during the war. There was some concentration of effort to complete the theoretical investigations of the Super described in Chapter 13. Weapon production had to continue, and plans were made to finish the development work on the implosion bomb (11.2).

20.3 This history has been an account of problems and their solution, of work done. The other side of the history of Los Alamos, the reactions of these accomplishments upon the people who made them, is present only by implication. This account ends at a time, however, when these reactions assumed a sudden importance, and it is appropriate that it should end with some description of them.

20.4 For many members of the Laboratory the Trinity test marked the successful climax of years of intensive and uncertain effort. A new kind of weapon had been made, and the magnitude and qualitative features of its operation had been successfully predicted. Despite the fact, perhaps in part because of the fact, that the explosion occurred as expected, the sight of it was a stunning experience to its creators, an experience of satisfaction and of fear. A new force had been created, and would henceforth lead a life of its own, independent of the will of those who made it. Only at Trinity, perhaps, were its magnitude and unpredictable potentialities fully grasped and appreciated.

20.5 Four days after the first bomb was dropped over Hiroshima, the Japanese began surrender negotiations. The feelings that had marked the success of the Trinity test were evident once more. But now the Laboratory, experiencing the sudden slackening of effort that followed the end of the war, began to speak seriously of the bomb and its consequences for the future. The thoughts that were expressed were not new, but there had been no time before to express them. Since 1939, when the decision had been made to seek Government support for the new development, a uniformity of insight had grown up among the working scientists of the Manhattan District. They had come to realize that atomic warfare would prove unendurable. This was learned by the Japanese in the days of Hiroshima and Nagasaki, and soon all the world was saying it.

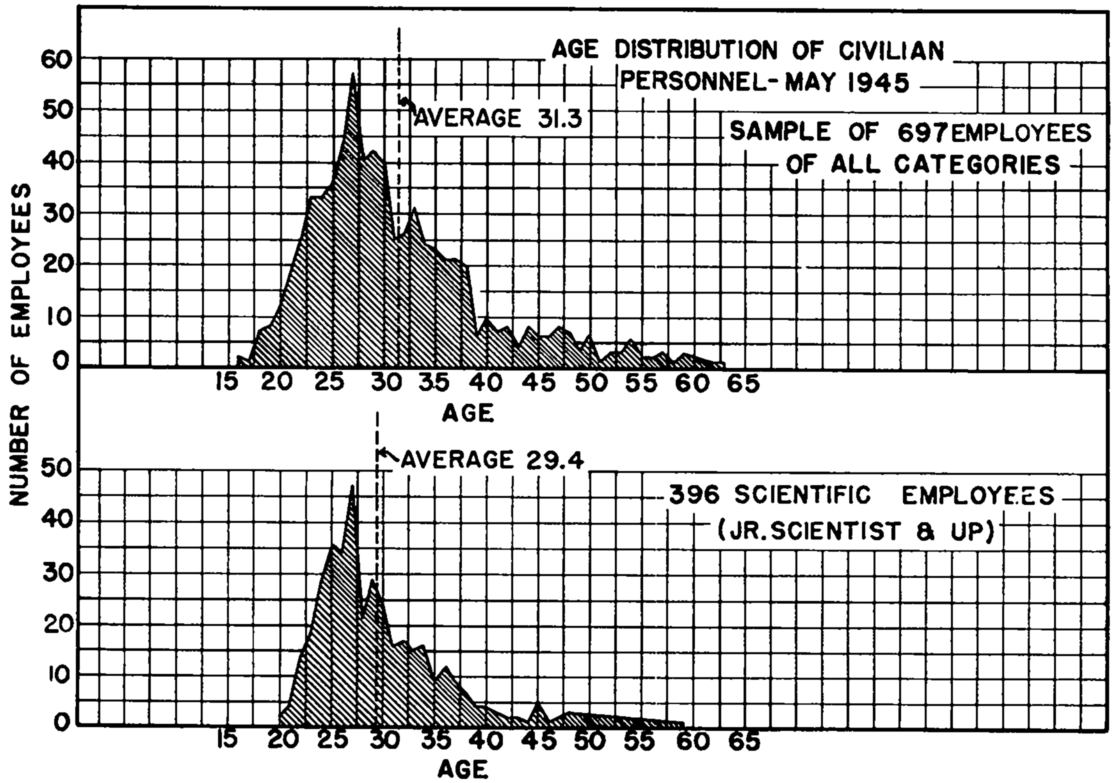
20.6 What the members of the Laboratory saw who joined in these discussions was more incisive than this. Atomic bombs were offensive or retaliatory weapons, their existence was a threat to the security of every nation which it could not venture, without the gravest risk, to meet on the military plane alone. The law of counterdevelopment, which has so uniformly in military affairs operated to produce new defenses against new weapons, could in this case operate to open channels of collaboration that have not previously existed among nations. The wartime scientific collaboration that had produced this weapon could, by its worldwide extension, be made uniquely the means for eliminating it from national armaments. Men of science, who had as a group never been concerned with the problems of society and of nations, felt responsible to tell the American public of the nature and implications of the new weapon, and to make clear the alternatives for the future that had arisen. This concern received perhaps its best and simplest expression in a speech by Oppenheimer, given on October 16, 1945, when the Laboratory was presented by General Groves with a certificate of Appreciation from the Secretary of War:

20.7 "It is with appreciation and gratitude that I accept from you this scroll for the Los Alamos Laboratory, for the men and women whose work and whose hearts have made it. It is our hope that in years to come we may look at this scroll, and all that it signifies, with pride.

"Today that pride must be tempered with a profound concern. If atomic bombs are to be added as new weapons to the arsenals of a warring world, or to the arsenals of nations preparing for war, then the time will come when mankind will curse the names of Los Alamos and Hiroshima.

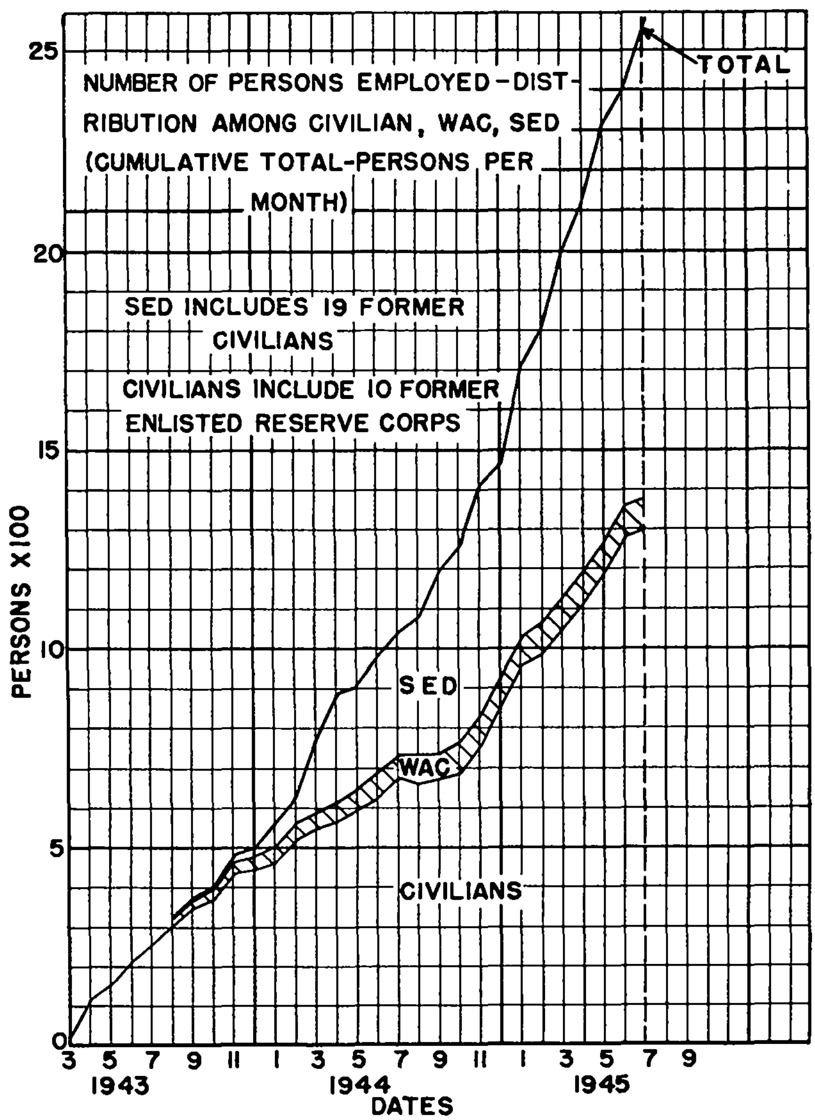
"The peoples of this world must unite, or they will perish. This war, that has ravaged so much of the earth, has written these words. The atomic bomb has spelled them out for all men to understand. Other men have spoken them, in other times, of other wars, of other weapons. They have not prevailed. There are some, misled by a false sense of human history, who hold that they will not prevail today. It is not for us to believe that. By our works we are committed, committed to a world united, before this common peril, in law, and in humanity."

GRAPHS



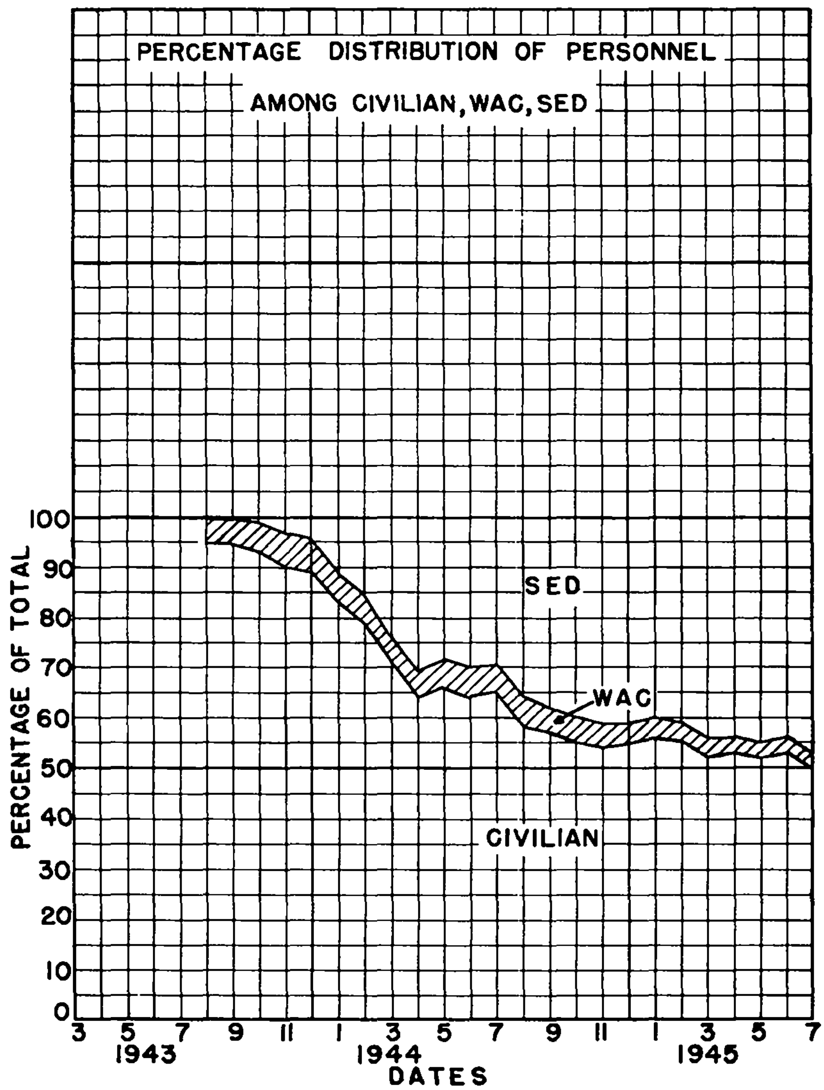
Graph Number 1. Age Distribution of Civilian Personnel - May 1945

Two curves are shown, one a sample of all employees, the other of scientific employees only. The averages for both are low - 29.4 for the scientists and 31.3 for the others - with 27 the most probable age for both. Actually there is only one man over 58 among the scientific employees. These figures emphasize the importance of the draft deferment problem. Information was obtained from the active card file of the Personnel Division in June 1945.



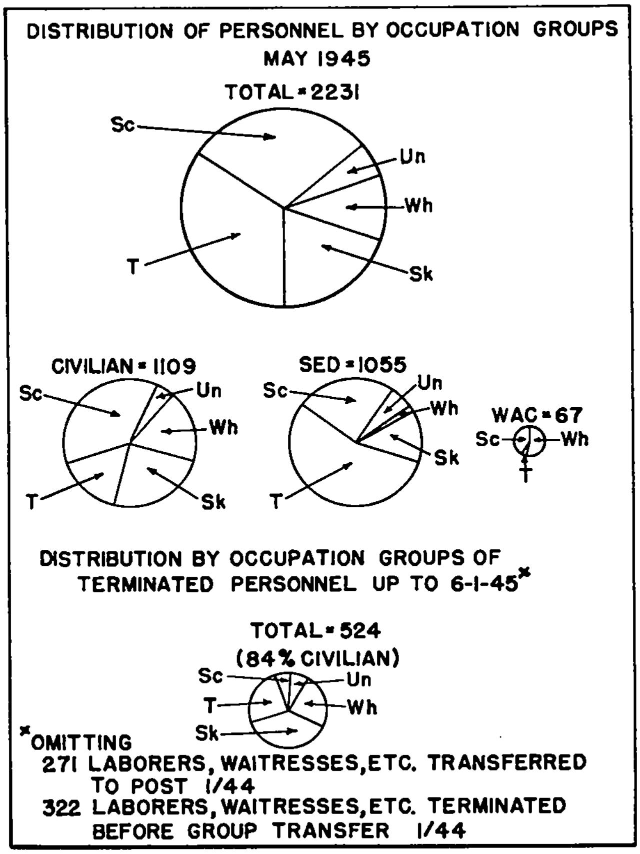
Graph Number 2. Number of Persons Employed - Distribution among Civilians, WAC, SED

Shows sharp and continuous increase of personnel from beginning of project. Civilians increase at a steady rate, WAC contingent remains about the same, and SED contingent increases very rapidly. Information was obtained from records of Technical Area and SED personnel offices.



Graph Number 3. Percentage Distribution of Personnel among Civilians, WAC, SED

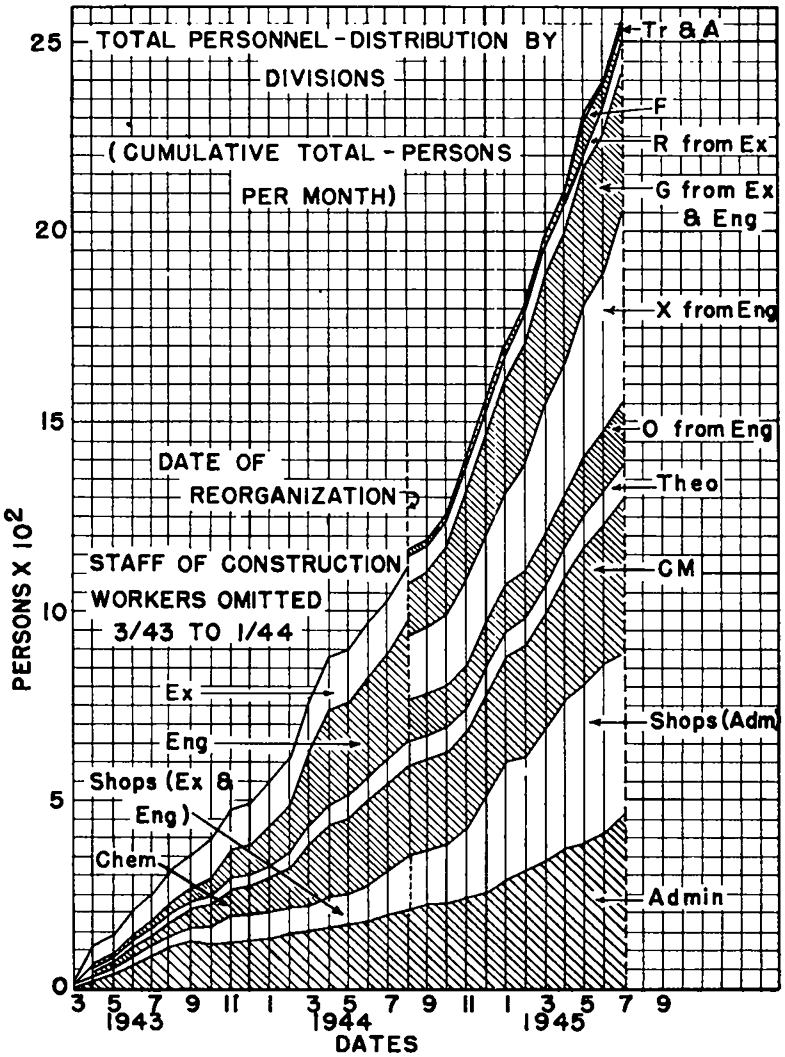
Data of previous graph replotted on percentage basis. Project changed from being 100% civilian during first five months to 50% civilian in July 1945.



Graph Number 4. Distribution of Personnel by Occupation Groups

Classification of personnel into five large categories, according to occupation, as of May 1945. Pie charts are proportional in diameter to number represented. In the chart for the total number one sees the preponderance of scientific and technical personnel; in the civilian chart the preponderance of scientific personnel; in the SED chart the preponderance of technical personnel. The chart of terminations shows the very small proportion of scientific personnel terminating and the relatively large proportion of skilled labor terminating. The latter fact reflects some of the difficulties encountered by the shops in retaining personnel, as well as a difference in motivation. Information was obtained from card files in Tech area and SED personnel offices

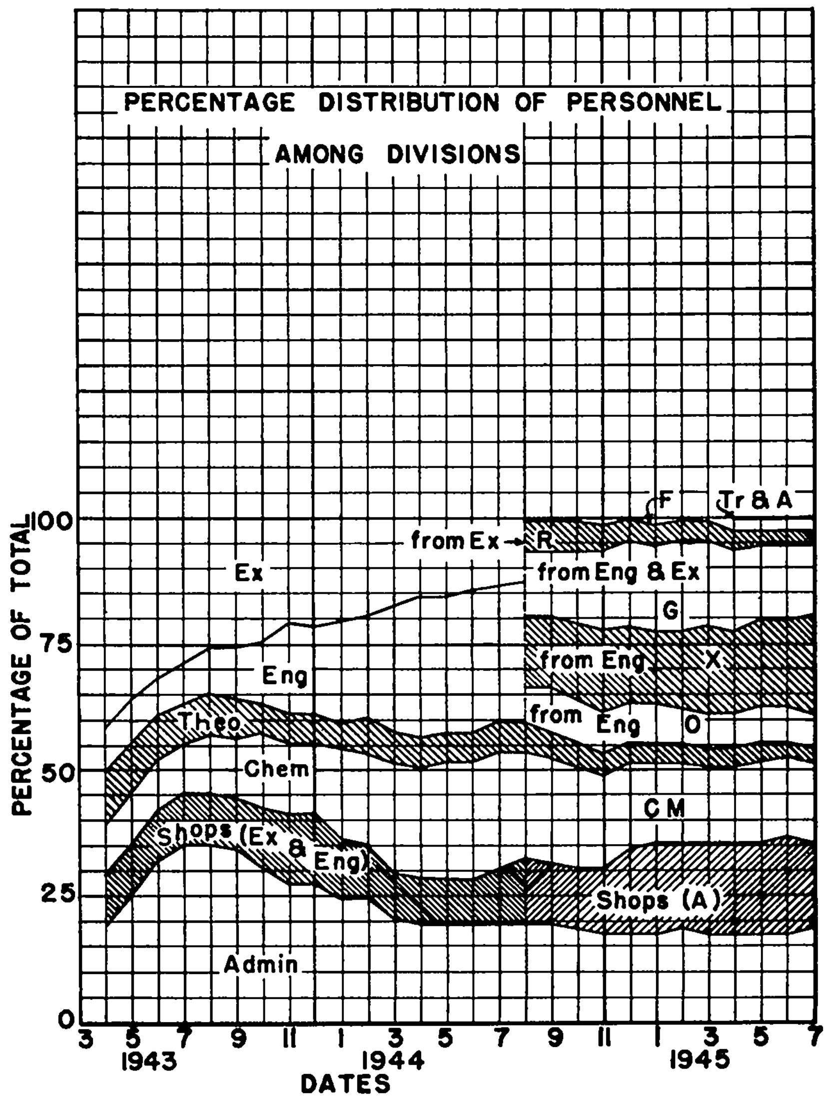
Un - Unskilled (Laborer, Messenger, Warehouse Ass't.)
Wh - White Collar (Clerk, Secretary, Nurse, Teacher)
Sk - Skilled (Machinist, Toolmaker, Glassblower)
T - Technical (Technician, Draftsman, Scientific Ass't.)
Sc - Scientific & Administrative (Jr. Scientist and up)



Graph Number 5. Total Personnel - Distribution by Divisions

Shows growth of various divisions, reflects change in emphasis from research to engineering, especially after reorganization in August 1944. Engineering divisions G, X, and O assume large proportions while research divisions R and T remain small. Information was obtained from group assignment records of Tech Area and SED personnel offices. Abbreviations and letters refer to various divisions:

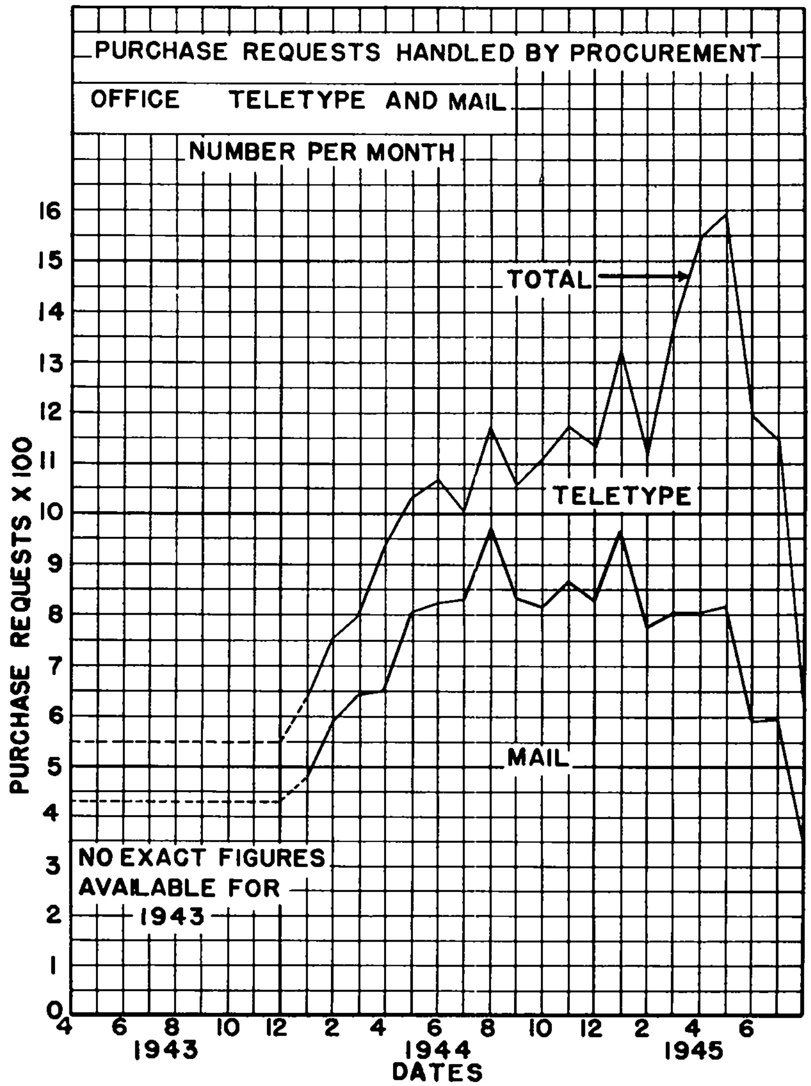
Exp.	Shops	R	O
Eng.	Admin.	G	T
Theo.	Tr & A	X	CM
Chem.	F		



Graph Number 6. Percentage Distribution of Personnel among Divisions

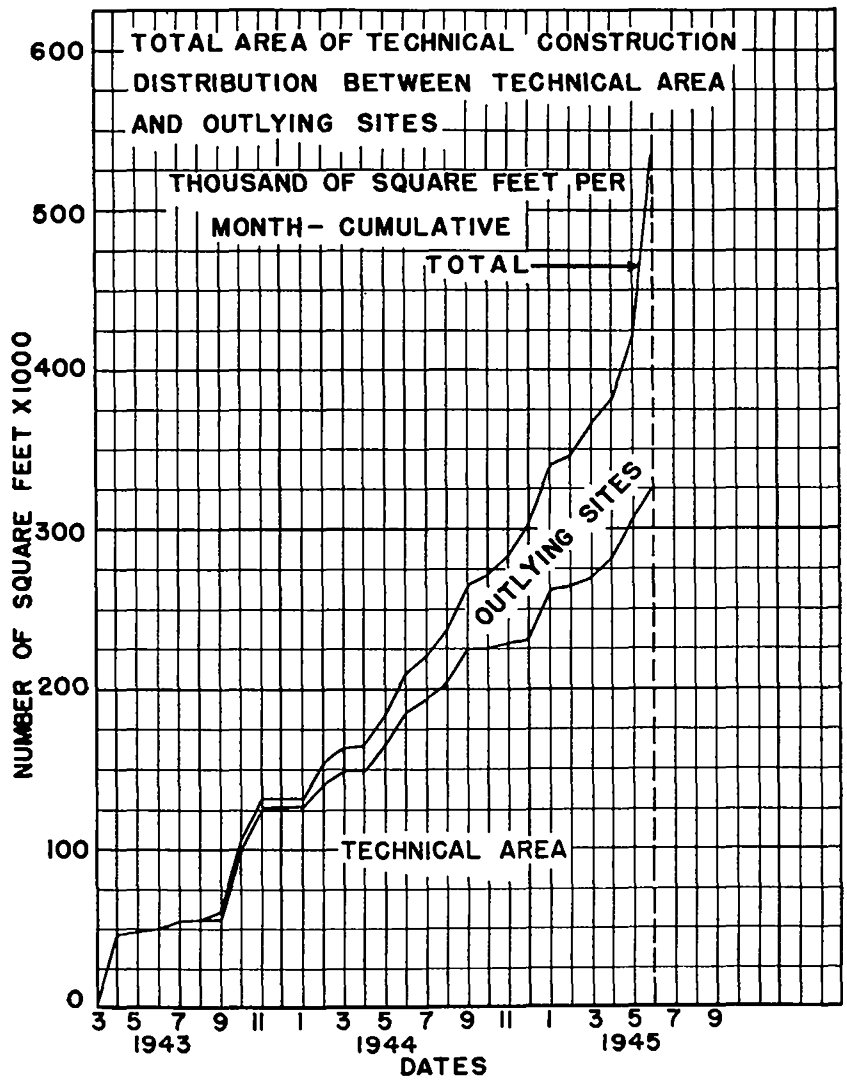
Data of previous graph replotted on percentage basis. Shops, G, X and O account for more than half of total personnel. Abbreviations and letters refer to various divisions:

Exp.	Shops	R	O
Eng.	Admin.	G	T
Theo.	Tr & A	X	CM
Chem.	F		



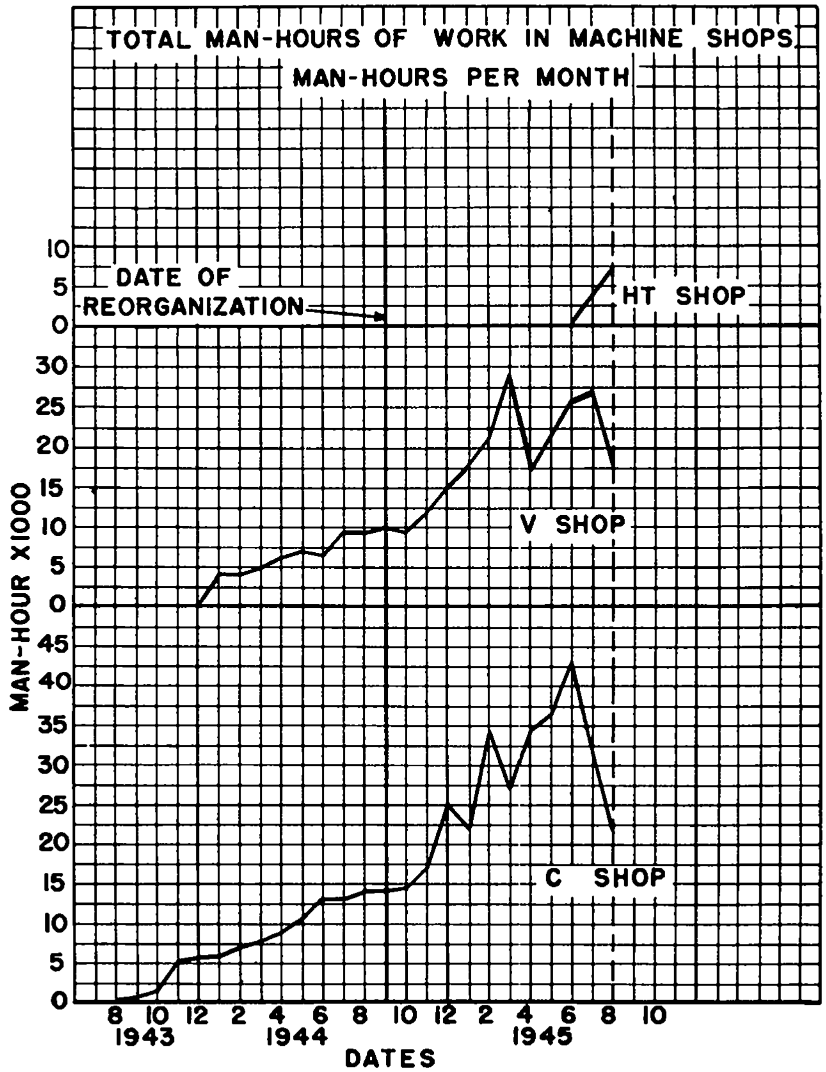
Graph Number 7. Purchase Requests Handled by Procurement Office - Teletype and Mail

Total number of requests handled each month by Procurement during 1944 and part of 1945. Mail requests represent bulk of routine business; teletype requests those items needed with special urgency. Peak month, especially for teletype requests, was May 1945, in preparation for Trinity. Note the sharp slump which follows. Each request involves at least 60 pieces of paper, according to Procurement records. Information was obtained from a monthly record of purchase requests kept in the request file section.



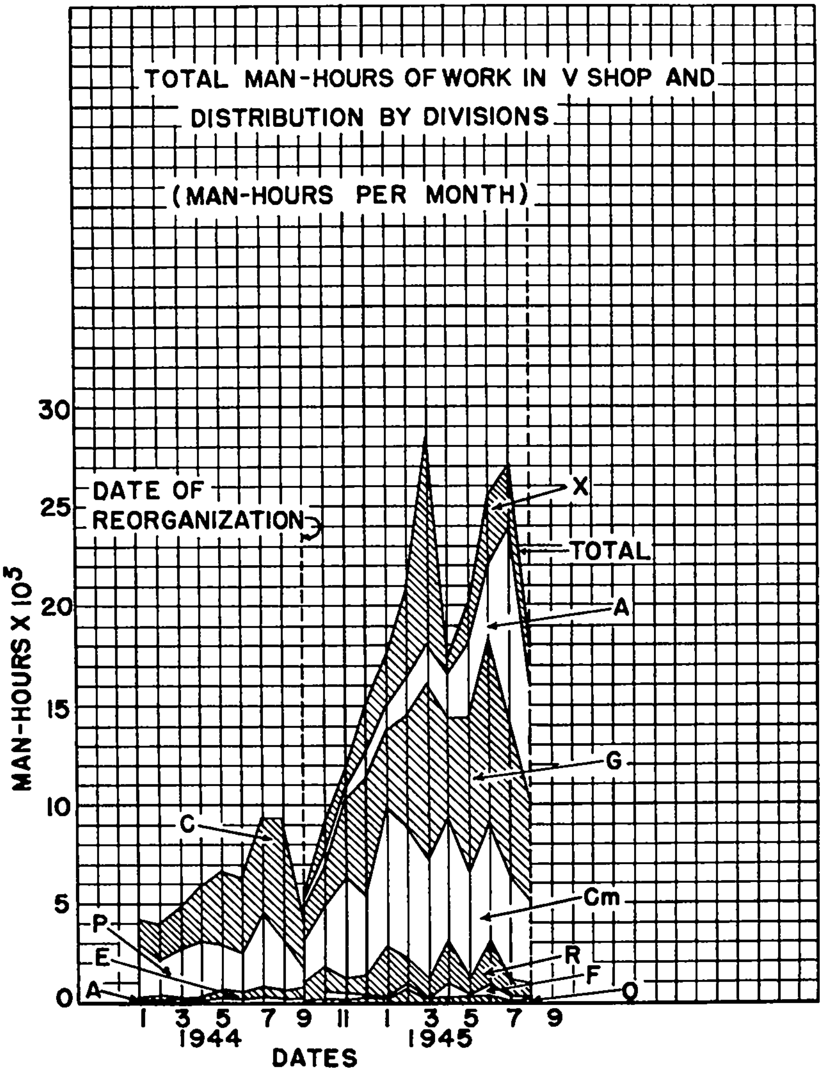
Graph Number 8. Total Area of Technical Construction - Technical Area and Outlying Sites

Shows steady growth of construction both in Technical Area and outlying sites. Sharp rate of increase in outlying site construction in June 1945 represents completion of first buildings at DP site. For more information see map of sites, Appendix 3. Information was obtained from files of D. Dow in Director's Office, files of Post Construction Officer, and files of W. C. Kruger, Project Architect.



Graph Number 9. Total Man-Hours of Work in Machine Shops

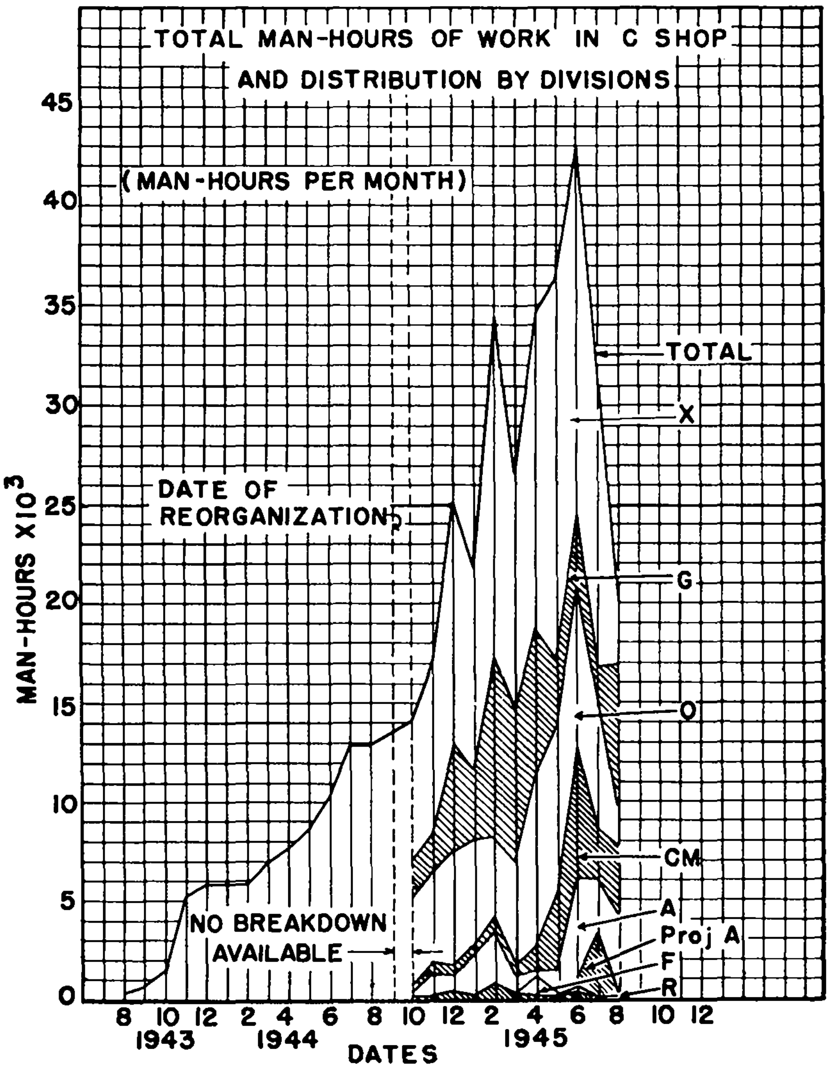
Shows rapid expansion of shops after reorganization. Slump in C Shop in January and sharp rise in February indicate results of fire. Peak of activity in C Shop in June preparatory to Trinity, followed by sharp decrease in activity; one month lag in peak for V Shop, but same sharp decrease follows. Information was obtained from weekly records kept in office of machine shops.



Graph Number 10. Total Man-Hours of Work in V Shop and Distribution by Divisions

Shows largest proportion of work done in V Shop for G and CM Divisions. Work done for A Division represents work done for shops themselves. Decrease in activity for all divisions except A after June 1945. Information was obtained from weekly records kept in machine shop office. Letters refer to various divisions:

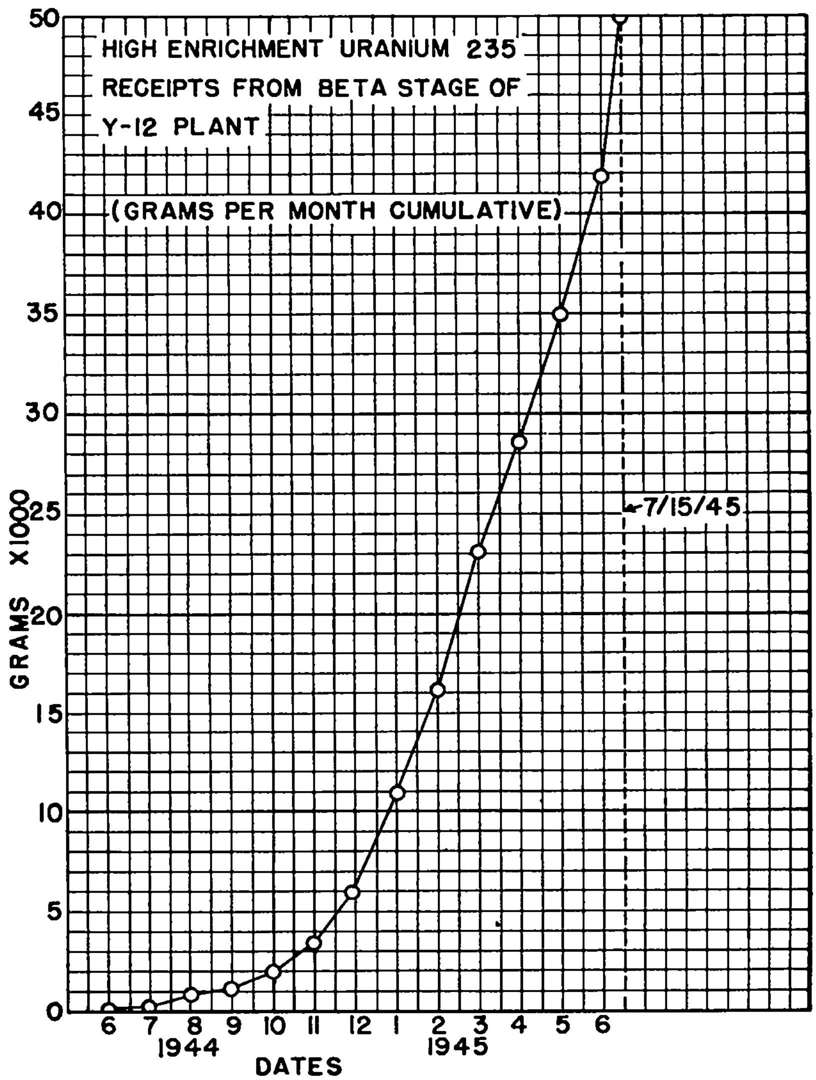
C	A	G	F
P	X	CM	O
E	A	R	



Graph Number 11. Total Man-Hours of Work in C Shop and Distribution by Divisions

Shows largest proportion of work done in C Shop for X Division. Fire accounts for slump in activity in January; no apparent reason for subsequent slump in March. Information was obtained from weekly records kept in machine shop office. Letters refer to various divisions:

X	CM	F
G	A	R
O	Project A	



Graph Number 12. High Enrichment U^{235} Receipts from Beta Stage of Y-12 Plant

Cumulative total of U^{235} received up to date of Trinity test. This represents all highly enriched U^{235} produced by the District. Such material was shipped after the final processing done in the beta stage of the Y-12 plant at Oak Ridge. Enrichment of U^{235} in tuballoy increased from 63% to 89%. Information was obtained from records of receipts in Director's Office, now filed with the Quantity Control Section of the Chemistry and Metallurgy Division.

APPENDIXES

APPENDIX NUMBER 1

GROVES-CONANT LETTER

This is the original directive of the Los Alamos Laboratory, referred to in Chapter I.

OFFICE FOR EMERGENCY MANAGEMENT

OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT

1530 P STREET NW.

WASHINGTON, D. C.

VANNEVAR BUSH
Director

February 25, 1943

Dr. J. R. Oppenheimer
University of California
Berkeley, California

Dear Dr. Oppenheimer:

We are addressing this letter to you as the Scientific Director of the special laboratory in New Mexico in order to confirm our many conversations on the matters of organization and responsibility. You are at liberty to show this letter to those with whom you are discussing the desirability of their joining the project with you; they of course realizing their responsibility as to secrecy, including the details of organization and personnel.

I. The laboratory will be concerned with the development and final manufacture of an instrument of war, which we may designate as Projectile S-1-T. To this end, the laboratory will be concerned with:

A. Certain experimental studies in science, engineering and ordnance; and

B. At a later date large-scale experiments involving difficult ordnance procedures and the handling of highly dangerous material.

The work of the laboratory will be divided into two periods in time: one, corresponding to the work mentioned in section A; the other, that mentioned in section B. During the first period, the laboratory will be on a strictly civilian basis, the personnel, procurement and other arrangements being carried on under a contract arranged between the War Department and the University of California. The conditions of this contract will be essentially similar to that of the usual OSRD contract. In such matters as draft deferment, the policy of the War Department and OSRD in regard to the personnel working under this contract will be practically identical. When the second division of the work is entered upon (mentioned in B), which will not be earlier than January 1, 1944, the scientific and engineering staff will be composed of commissioned officers. This is necessary because of the dangerous nature of the work and the need for special conditions of security. It is expected that many of those employed as civilians during the first period (A) will be offered commissions and become members of the commissioned staff during the second period (B), but there is no obligation on the part of anyone employed during period A to accept a commission at the end of that time.

II. The laboratory is part of a larger project which has been placed in a special category and assigned the highest priority by the President of the United States. By his order, the Secretary of War and certain other high officials have arranged that the control of this project shall be in the hands of a Military Policy Committee, composed of Dr. Vannevar Bush, Director of OSRD, as Chairman, Major General W. D. Styer, Chief of Staff, SOS, Rear Admiral W. R. Purnell, Assistant Chief of Staff to Admiral King; Dr. James B. Conant serves as Dr. Bush's deputy and alternate on this Committee, but attends all meetings and enters into all discussions. Brigadier General L. R. Groves of the Corps of Engineers has been given over-all executive responsibility for this project, working under the direction of the Military Policy Committee. He works in close cooperation with Dr. Conant, who is Chairman of the group of scientists who were in charge of the earlier phases of some aspects of the investigation.

III. Responsibilities of the Scientific Director.

1. He will be responsible for:

a. The conduct of the scientific work so that the desired goals as outlined by the Military Policy Committee are achieved at the earliest possible dates.

b. The maintenance of secrecy by the civilian personnel under his control as well as their families.

2. He will of course be guided in his determination of policies and courses of action by the advice of his scientific staff.

3. He will keep Dr. James B. Conant and General Groves informed to such extent as is necessary for them to carry on the work which falls in their respective spheres. Dr. Conant will be available at any time for consultation on general scientific problems as well as to assist in the determination of definite scientific policies and research programs. Through Dr. Conant complete access to the scientific world is guaranteed.

IV. Responsibilities of the Commanding Officer.

1. The Commanding Officer will report directly to General Groves.

2. He will be responsible for:

a. The work and conduct of all military personnel.

b. The maintenance of suitable living conditions for civilian personnel.

c. The prevention of trespassing on the site.

d. The performance of duty by such guards as may be established within the reservation for the purpose of maintaining the secrecy precautions deemed necessary by the Scientific Director.

V. Cooperation.

The closest cooperation is of course necessary between the Commanding Officer and the Scientific Director if each is to perform his function to the maximum benefit of the work. Such a cooperative attitude now exists on the part of Dr. Conant and General Groves and has so existed since General Groves first entered the project.

Very sincerely yours,

James B. Conant

Leslie R. Groves

APPENDIX NUMBER 2

HIROSHIMA TELETYPE

Copy of teletype announcing success of Hiroshima mission received at Los Alamos from Washington office, prepared by Manley (see Chapter XIX).

Note comments by teletype operators at end. They were T/3 Flora L. Little of Jackson, Mississippi, in the Washington office and T/3 Mildred Weiss of New Orleans, Louisiana, in the Los Alamos office.

NR 137

FROM WASH LIAISON OFC WASH DC AUG 45[illegible]0402

TO COMMANDING OFFICER CLEAR CREEK

FIVE PARTS - TART ONE

SW

KC

FLASHED FROM THE PLANE BY PARSONS ONE FIVE MINUTES AFTER RELEASE
AND RELAYED HERE WAS THIS INFORMATION QUOTE PAREN REF EIDM WL
TO OPPENHEIMER FROM GENERAL GROVES THIS RESUME OF MSSSAGES PREPARED
BY DOCTOR MANLEY PAREN CLEAJ CUT RESULTS COMMA IN ALL RESPECTS SUCCES
FUL PD EXCEEDED TR TEST IN VISIBLE EFFECTS PD NORMAL CONDITINXXXXXX
CONDITIONS OBTAINED IN AIRCRAFT AFTER DELIVERY WAS ACCOMPLISHED PD
VISUAL ATTACK ON HIROSHIMA AT ZERO FIVE TWO THREE ONE FIVE Z WITH
ONLY ONE TENTH CLOUD COVER PD FLACK AND FIGHTERS ABSENT UNQUOTE AFTER
RTXXXXX RETURN TO BASE AND GENERAL INTERROGATION FARRELL SENT THE
FOLLOWIGXXXX FOLLOWING INFORMATION QUOTE ALARGE OPENING IN CLOUD
COVER DIRECTLY OVER TARGET MADE BOMBING FAVORABLE PD EXCELLENT RECORD
REPORTED FROM FASTAX PD FILMS NOT YET PROCESSED BUT OTHER OBSERVING
[illegible]SOALSO ANTICIPATE GOOD TREXXXX RECORDS NXX PD NO APPRE

JQXD 2CFA

R NIL

K HOW MANY LINES DID U GET

R 12 LINWA

PLANES ALSO ANTICIPATE GOOD RCXXX RECORDS PD NO APPRECIABLE NOTICE OF
SOUND PD BRIGHT DAYLIGHT CAUSED FLASH TO BE LESS BLINDING THAN TRPXXX
TR PD A BALL OF FIRE CHANGED IN A FEW RECORDS TO PURPLE CLOUDS AND
BOILING AND UPWARD SWIRLING FLAMES PD TURN JUST COMPLETED WHEN FLASH
WAS AXXX OBSERVED PD INTENSLY BRIGHT LIGHT CONCEALED BY ALL AND RATE
OF RISE OF WHITE CLOUD FASTER THAN AT TR PD IT WAS ONE THIRD GREATER
IN DIAMETER REACHING THIRTY THOUSAND FEET IN THREE MINUTES PD MAXIMUM
ALTITUDE AT LEAST FORTY THOUSAND FEET WITH FLATTENED TOP AT THIS
LEVEL PD COMBAT AIRPLANE THREE HUNDRED SIXTY THREE MILES AWAY AT
[illegible]THOUSAND FEET OBSERVEDIT PD D

NIL AGN

.3 OK OPR WELL JUST HAVE TO KEEP TRING AS THESE MESSAGES AR IMP

MIN PLS

OPR U STARTED THIS MSG AS PART TWO ISNT IT PART OF PART ONE

M MIN OPR I TOLD U I WD START PART TWO WHERE PART ONE NILED
TS THAT CLEAR

BUT OPR I DIDNT GET PART ONE COMPLETE

AND THE I TOLD TO U TO SA START WITH 12 LINE

AND THE 12 LINE U L O WELL I THOT U MEANT U GOT 12 OK

M THIS IS A AWFUL MESS ISNT IT IT SH SURE IS DOU THINMI WNGEFG

MIN PLS

TRY ANOTHER MACHINE MAYBE IT WILL DO VETTER

OPR IT ISNT UG MACH AND I KNOW IT IT S MINE AND THERE ISNT
A THING CAN BE DONE AS THE REPAIR MAN SAYS THERE ISNT ANYTHING WRONG
WITH IT HES BEEN HERE ALL DAY AND THIS IS AS GOOD AS IT WAILL RUN
I HAVE LOADS TO GO UXX TO U TONIGHT BUT WELL HAVE TO DO IT THIS WAY
A FEW LINES AT A TIME MIN I WANT TO TALK TO THE LT A MIN

OK

OPR ILL CALL U BACK IN A BT 10 MINUTES

.4OK

APPENDIX NUMBER 3

SITE MAP

Scale - 1.8" = 1 mile, squares are 1/2 mi. × 1/2 mi.

Hard surfaced roads

Trails (foot)

▼ VI Site and Designation Number

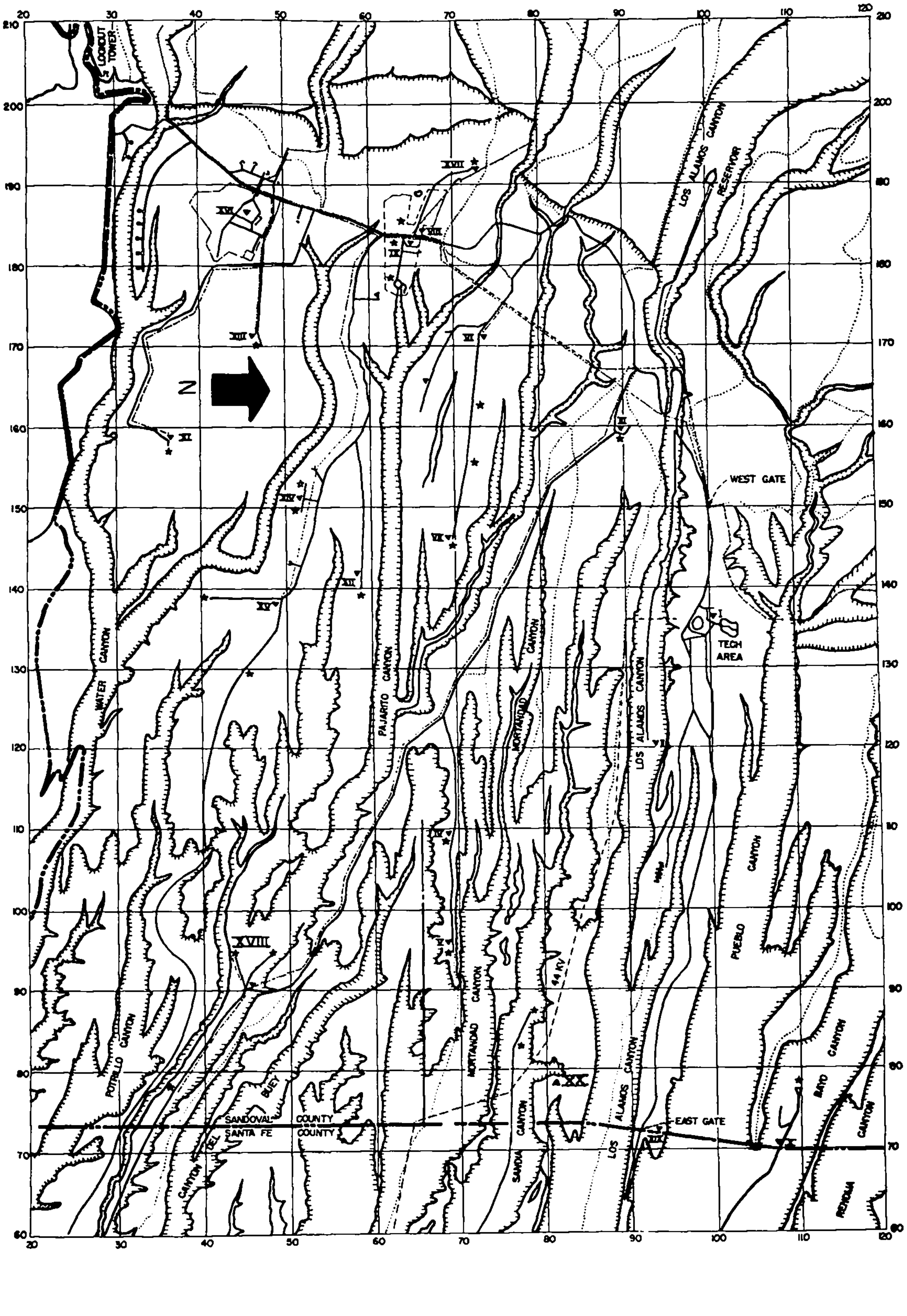
Water supply main

Power line

Firing sites

DP Site

Number	Site	Division	NS Coordinate	EW Coordinate
I	Post Tech Area		100	135
II	Omega	G	93	121
III	S. Mesa	G	89	158
IV	Alpha	G	68	108
V	Beta	G	69	94
VI	2-Mile Mesa - upper	X	74	171
VII	2-Mile Mesa - lower	Q	69	147
VIII	Anchor Gun Site	O	65	184
IX	Anchor HE	X	65	183
X	Bayo	G	107	71
XI	K	G	38	157
XII	L	X	59	139
XIII	P	G	47	171
XIV	Q	X	52	152
XV	R	X	49	138
XVI	S	X	46	187
XVII	X	G	72	192
XVIII	Pajarito	O-X	45	91
XIX	E. Gate Lab	R	93	72
XX	Sandia	G	77	82


LOOKOUT TOWER
N
WEST GATE
TECH AREA
LOS ALAMOS CANYON
RESERVOIR
WATER CANYON
PAJARITO CANYON
MORTANDAD CANYON
PUEBLO CANYON
POTRILLO CANYON
CANYON DEL BUEY
SANDOVAL COUNTY
SANTA FE COUNTY
SANDIA CANYON
44 KV
EAST GATE
BAYO CANYON
RENDIJA CANYON

APPENDIX NUMBER 4

TRINITY PROJECT DETAIL LOCATION PLAN

Station	Group Leader	Symbol
Piezo Gauge	Walker	×
Sentinel (Type A)	Moon	⊛
Sentinel (Type B)	Moon	✱
Geophone	Houghton	△
Paper Box Gauge	Hoogterp	⊡
Flash Bomb	Mack	◩
R 4 Ground Station	Segrè	⊠
R 4 Balloon Winch	Segrè	♀
E. D. G.	Moon	‡
Mack Slit Camera	Mack	ℛ
Impulse Meter	Jorgensen	◒
Condenser Gauge	Bright	▣
Excess Velocity Gauge	Barschall	⊕
Tank Range Poles	Anderson	⚠
Tank Flag Poles	Anderson	⚐
Primacord Station	Mack	♁
Metal Stake (Earth Disp)	Penney	o
Piezo Gauge Amplifier	Walker	◎
Balloon	Richards	♀
Balloon Winch	Richards	⊖
Ground Station	Richards	⌖

Roads ═══════════════════

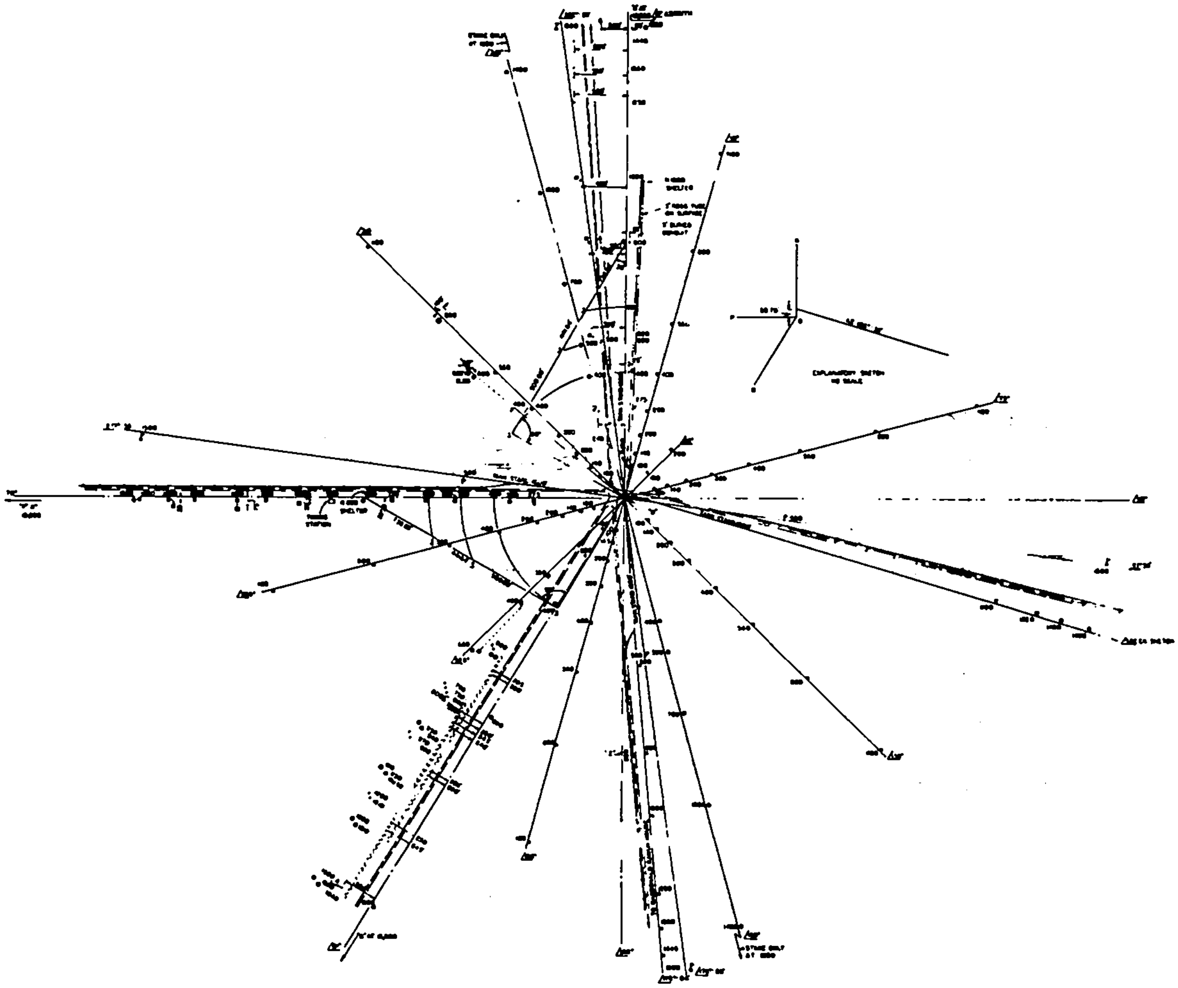
Buried Wires or Cables — — — — — — — — — — —

Center Lines - ——————— - ——————— - ——————— -

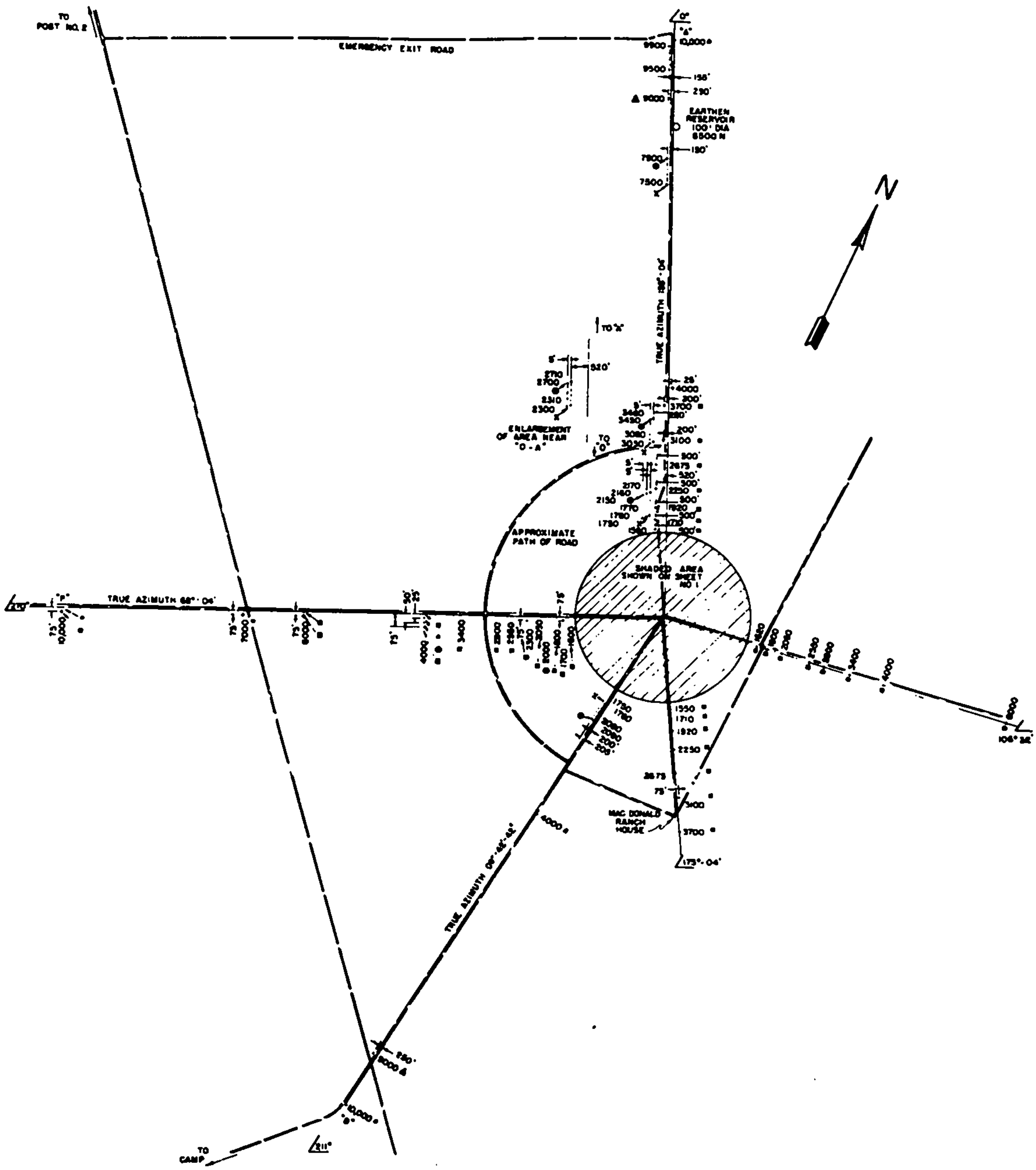
Tank Right of Way ┴┬┴┬┴┬┴┬┴┬┴┬┴

Note: Angles are Azimuths on "OA" Line
Distances thus (800) are Radial Yards from "O"
Distances thus (75') are Offsets from L of Roads and Center Lines.

Scale: 1500 Yard circle - 1" = 300 Yards. - Sheet 1
10,000 Yards - 1" = 2750 Yards. - Sheet A



Sheet 1


TO POST NO. 2
EMERGENCY EXIT ROAD
EARTHEN RESERVOIR 100' DIA
TRUE AZIMUTH
ENLARGEMENT OF AREA NEAR "0-A"
APPROXIMATE PATH OF ROAD
SHADED AREA SHOWN ON SHEET NO 1
TRUE AZIMUTH
MAC DONALD RANCH HOUSE
TRUE AZIMUTH
TO CAMP
N

Sheet A

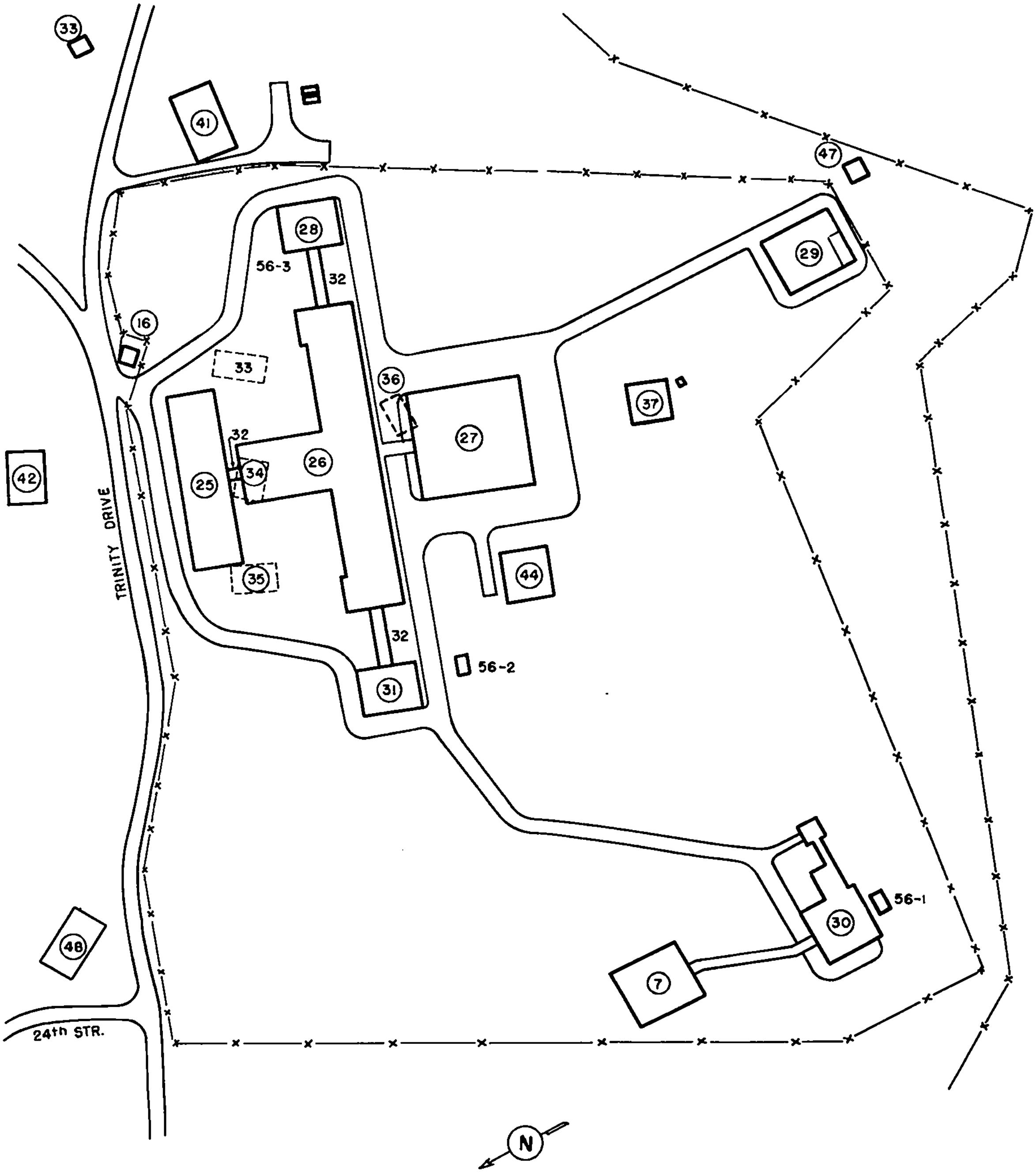
APPENDIX NUMBER 5

TECHNICAL AREA PLOT MAP

Map showing building layout of the Technical Area, as drafted in December 1942. Technical Buildings T, U, V, W, X, Y and Z were constructed as map indicates. Dashed lines show removed ranch houses.

TECHNICAL AREA AS OF DECEMBER 1942

Building No.	Designation
7	Infirmary
16	Gatehouse
25	T - Main Tech Building
26	U - Chem. and Phys. Labs
27	V - Shop (Machine)
28	W - Van de Graaff
29	Y - Cryogenics Lab
30	X - Cyclotron
31	Z - Cockcroft-Walton
32	Covered walk
33-36	Ranch houses
37	Chem. Stock
41	Warehouse
42	Icehouse
44	Boiler
47	Guard tower
48	Ranch house - PX
56	Cooling towers


33
41
47
28
56-3
32
29
16
33
36
37
27
32
26
42
25
34
TRINITY DRIVE
44
35
32
56-2
31
56-1
30
48
7
24th STR.
N

GLOSSARY OF TERMS

GLOSSARY OF TERMS

(α, n) Reaction. Any nuclear reaction in which an alpha particle (helium nucleus) is absorbed by a nucleus, with subsequent emission of a neutron.

Autocatalytic Assembly. Any method of assembling supercritical amounts of nuclear explosive, in which the initial stages of the explosion are made to assist the further assembly of the explosive. e.g., by expulsion or compression of neutron absorbers placed in the active material.

Baratol. A castable explosive mixture of barium nitrate and TNT.

Baronal. A castable explosive mixture of barium nitrate, TNT, and aluminum.

Betatron. Induction electron accelerator for generating electron beams of very great energies.

Branching Ratio. The ratio of the capture cross section to the fission cross section.

Cockcroft-Walton Accelerator. An accelerator using voltage multiplication of the rectified output of a high voltage transformer to obtain a high potential.

Composition B. A castable explosive mixture containing RDX, TNT, and wax in the proportion 60/40/1.

Critical Mass. That amount of fissionable material which, under the particular conditions, will produce fission neutrons at a rate just equal to the rate at which they are lost by absorption (without fission) or diffusion out of the mass.

Tamped Critical Mass. The critical mass when the active material is surrounded by a tamper.

Critical Radius. The radius of a spherical arrangement of fissionable material equal to one critical mass under existing conditions.

Cross Section. A quantitative measure of the probability per particle of the occurrence of a given nuclear reaction. It is defined as the number of nuclear reactions of a given type that occur, divided by the number of target nuclei per square centimeter and by the number of incident particles.

Absorption Cross Section. The cross section for the absorption of a neutron by a given nucleus.

Capture Cross Section. The cross section for the (n, γ) reaction, in which a neutron is absorbed by a nucleus, with subsequent emission of gamma radiation.

Fission Cross Section. The cross section for the absorption of a neutron, followed by fission.

Scattering Cross Section. The cross section for the scattering of a neutron by the nuclei of some target material. Since scattering is a quantitative matter, the definition is incomplete. The differential scattering cross section is the cross section for scattering at an angle between θ and $\theta + d\theta$. The transport cross section is an average or integral scattering cross section, so defined as to give the average scattering in the forward direction:

$$\sigma_T = 2\pi \int_0^{\pi} (1 - \sin\theta)\, \sigma_s(\theta) \sin\theta \, d\theta$$

where $\sigma_s(\theta)$ is the differential scattering cross section defined above.

Cyclotron. Magnetic resonance accelerator, used in investigating atomic structures.

D(d, n) Reaction. The nuclear reaction produced by bombarding deuterons with deuterons, producing high energy neutrons.

D-D Source. The above reaction used as a source of high energy neutrons. At Los Alamos, the Cockcroft-Walton accelerator was principally used for this purpose.

Deuterium. Heavy hydrogen, D_2 or H_2^2, the hydrogen isotope of mass two.

Deuteron. A nucleus of deuterium or heavy hydrogen.

Electron Volt. An electron volt is the energy acquired by an electron falling through a potential of 1 volt. One electron volt is about 1.6×10^{-12} ergs. In thermodynamic units, 1 electron volt corresponds to a temperature of about 12,000 degrees absolute. Thus a fortieth of a volt per particle corresponds to "room temperature." Energies of this order are called "thermal." One million electron volts corresponded to a temperature of 1.2×10^{10} degrees absolute.

Fission Spectrum. The spectrum, or energy distribution, of neutrons emitted in the fission process.

Inelastic Scattering. The scattering of neutrons in which energy is lost to excitation of target nuclei.

Li(p, n) Reaction. The nuclear reaction in which neutrons are produced by bombardment of lithium by protons.

Neutron Number. The number of neutrons emitted per fission. This number is statistically variable; the expression refers therefore to the average number per fission.

(n, γ) reaction. A nuclear reaction in which a neutron is captured by a nucleus, with subsequent emission of gamma radiation.

PETN. Pentaerythritol tetranitrate.

RDX. Cyclotrimethylenetrinitramine.

Thermonuclear reaction. A mass nuclear reaction induced by thermal agitation of the reactant nuclei. The reaction is self-sustaining if the energy release is sufficient to counter-balance the energy losses that may be involved.

Tamper. A neutron reflector placed around a mass of fissionable material to decrease the neutron loss rate.

Taylor Instability. A hydrodynamical principle which states that when a light material pushes against a heavy one, the interface between them is unstable, and that when a heavy material pushes against a light one, the interface is stable.

Tritium. The hydrogen isotope of mass three. This isotope was discovered in the Cavendish Laboratory by Oliphant in 1934. It was there produced by deuterium-deuterium bombardment. Tritium is a radioactive gas with a half-life of about twenty years.

Triton. A nucleus of tritium.

Thermal Neutrons. Neutrons of thermal energy - see Electron Volt.

T-D Reaction. The nuclear reaction of tritons with deuterons.

Torpex. A castable explosive mixture of RDX, TNT, and aluminum.

Van de Graaff Generator. An accelerator using the electrostatic charge collected on a mechanically driven belt to obtain a high potential.

INDEXES

NAME INDEX[1]

[1]References are to paragraph and exhibit numbers.

SUBJECT INDEX[1]

[1]References are to paragraph and exhibit numbers.