

## 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. Weissman, 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  $\text{Pu}^{240}$ , 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  $\text{Pu}^{239}$  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  $\text{U}^{235}$ , 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  $\text{U}^{235}$  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-radio-lanthanum 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 ammonium 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 dozen 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  $\sim 630^\circ C$ . 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 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)  $UCl_3 + Li, Ca, \text{ or } Ba$ . 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 radio-assay 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 originally 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  $\text{Pu}(\text{IO}_3)_4$  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  $\text{PuO}_2$  (for carbon reduction). Plutinous 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  $\text{CCl}_4$  or sodium sulfite extraction for the  $\text{I}_2$ . Following this, liner and slag were dissolved in hydrochloric or nitric acid, followed by a precipitation of  $\text{Pu}(\text{OH})_4$  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  $\text{PuF}_4$  and  $\text{PuCl}_3$  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  $\text{PuCl}_3$  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  $\text{PuO}_2$  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  $\text{PuCl}_3$  in a solvent containing 48 per cent  $\text{BaCl}_2$ , 31 per cent  $\text{KCl}$ , and 21 per cent by weight  $\text{NaCl}$ . The metal obtained was in the form of droplets and usually contained small amounts of the cathode element. With the discovery of  $\text{Pu}^{240}$ , 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  $\text{PuCl}_3$  or  $\text{PuF}_4$  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  $\text{PuF}_4$  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  $\text{Pu}^{240}$  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 ( $\alpha$ , 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  $\text{PuO}_2$  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 micro-gas 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 war-time 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. Giaque 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 re-organization 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  $\text{Pu}^{240}$  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  $\alpha$ -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 an 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  $\text{Pu}^{240}$  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  $4 \times 10^6$  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 on 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 outgoing 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  $\mu$ sec long. Time resolution of 0.06  $\mu$ sec 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  $\text{Pu}^{240}$  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  $\text{U}^{233}$ , 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  $5 \times 10^{10}$  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  $(\gamma, n)$  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<sup>235</sup>. 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.