Chapter IV

TECHNICAL REVIEW TO AUGUST 1944

Introduction

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

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

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

Nuclear Specifications of the Bomb

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

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

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

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

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

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

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

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

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

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

The Gun Method

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

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

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

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

The Implosion

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

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

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

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

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

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

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

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

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

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

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

Metallurgy

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

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

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

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

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

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

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

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

Chemistry

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

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

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

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

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

The Discovery of Pu^{240}

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

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

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

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

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

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

The Water Boiler

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

Chapter V

THEORETICAL DIVISION

Organization

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

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

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

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

Diffusion Problems

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

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

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

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

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

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

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

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

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

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

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

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

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

Water Boiler

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

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

The Gun

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

The Implosion

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

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

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

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

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

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

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

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

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

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

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

Efficiency

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

The Super

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Damage

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

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

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

Experiments

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

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

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

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

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

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

Chapter VI

EXPERIMENTAL PHYSICS DIVISION

Organization

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

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

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

Equipment

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

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

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

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

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

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

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

Preliminary Experiments

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

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

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

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

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

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

The Neutron Number

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

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

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

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

Spontaneous Fission Measurements

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

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

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

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

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

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

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

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

Energy Spectrum of Fission Neutrons

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

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

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

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

Fission Cross Sections

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

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

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

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

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

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

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

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

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

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

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

Capture Cross Sections, the "Branching Ratio"

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

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

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

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

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

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

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

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

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

Scattering Experiments, Choice of Tamper

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

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

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

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

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

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

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

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

Water Boiler

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Miscellaneous Experiments

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

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

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

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

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

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

Instrumentation

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

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

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

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

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

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

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

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

Chapter VII

ORDNANCE DIVISION

Organization and Liaison

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Gun Design, Proving, Interior Ballistics

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

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

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

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

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

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

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

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

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

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

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

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

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

Projectile and Target Design

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

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

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

Arming and Fusing

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

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

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

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

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

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

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

Engineering

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

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

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

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

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

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

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

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

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

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

Research on Implosion

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Delivery

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

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

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

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

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

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

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

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

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

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

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

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