Appendix

B

David Hawkins' *Manhattan District History:* Development of the Mark II.

David Hawkins' *Manhattan District History* of the Los Alamos Project is not conveniently available to most interested readers, so I have concatenated pertinent extracts from that *History* which report development of the autocatalytic uranium hydride lateral implosion experimental device which, beginning on 4 July 1944, was named the Mark II by James Conant. Hawkins does not refer to the bombs and bomb designs in development at Los Alamos by the "Mark" designations, which are identified and used in the text chapters of *The Last Wave from Port Chicago.*

I have also included in this Appendix paragraphs from Hawkins' *History* that report the activities of other persons at Los Alamos who are mentioned in the text chapters of *The Last Wave from Port Chicago:* Captain Williams S. Parsons, USN; Commander Frederick L. Ashworth, USN; Dr. Maurice M. Shapiro; Ensign George T. Reynolds, USNR, etc.

I was privileged to have several conversations with University of Colorado Distinguished Professor Emeritus of Philosophy David Hawkins from 1982 until the year preceding his death on 24 February 2002 at age 88. Primarily our discussions centered on aspects and elements of the *Manhattan District History.* Professor Hawkins' *History* is constructed primarily from the extensive notes he compiled from verbal reports and briefings that he received from those persons foremostly involved in the work at Los Alamos, where Hawkins was resident from spring 1943 though the end of the war. He wrote the *History* during 1946 and 1947.

The *History* was CLASSIFIED until 1 December 1961 when it was distributed as report LAMS-2532 (Vol. I) by Los Alamos Scientific Laboratory of the University of California, and is Volume I of the two-volume *Manhattan District History, Project Y, the Los Alamos Project.* Volume I, the Hawkins' history, is titled *Inception until August 1945.* Chapters III - VIII of Volume I report the period of Los Alamos history from April 1943 to August 1944; Chapters IX - XIX report the period August 1944 to August 1945. Volume II, titled *August 1945 through December 1946,* was written by Edith C. Truslow and Ralph Carlisle Smith; Volume II reports the period of Los Alamos history from August 1945 through December 1946.

The original two-volume *Manhattan District History, Project Y, the Los Alamos Project* was republished in one volume by Tomash Publishers, Los Angeles, California, 1983, as Volume 2 of the series *History of Modern Physics, 1800-1950,* with the title *Project Y, the Los Alamos Story*. The Tomash edition includes a new introduction and a bibliography; the original text has been edited and includes modifications, additions and deletions of the original text. At the time of this writing, 24 July 2002, one copy of the Tomash edition is offered for sale on the Internet at $89.00 (U.S.).

David Hawkins was born at El Paso, Texas, and was raised in New Mexico. His knowledge of the terrain and topography of New Mexico contributed to the selection of Trinity Site for the 16 July 1945 test of the Mark IV spherical plutonium implosion gadget. He received his undergraduate degree (1934) and Masters of Arts degree (Philosophy, 1936) from Stanford University. He completed his doctorate in probability theory at the University of California, Berkeley, in 1941 and joined the faculty there. In 1943 his friend and faculty colleague at Berkeley, J. Robert Oppenheimer, invited him to join the project at Los Alamos where Hawkins was designated official historian of the project.

In 1947 Hawkins joined the faculty of the University of Colorado, Boulder, where he taught philosophy and the physical sciences. After

World War II, he publicly criticized the Manhattan Project and lobbied in Washington for international controls on the development of nuclear weapons. Al Bartlett, a University of Colorado professor who worked with Hawkins on the Manhattan Project, called him "one of the greatest intellects I've ever known." His widow, Frances Pockman Hawkins (Stanford, 1935), has described David Hawkins as a pacifist. In addition to his curriculum duties in philosophy and the physical sciences at the University of Colorado, Professor Hawkins trained math and science teachers in the education of children and wrote about the philosophy of science.

Professor Hawkins' *Manhattan Project History* is a work he compiled and wrote from his accumulated notes and daily logbooks in which he recorded his participation in and observation of Los Alamos activities from spring of 1943 through the end of the war; conversations, verbal reports and briefings that he received as the designated Project Y historian were additional grist to his intellectual mill. Prior to declassification and public release, Hawkins' *History* had been substantially edited by Los Alamos to delete all classified information. Hawkins' *History* is not a document-derived history, and few primary documents are cited in the *History*.

Hawkins told me he had been unaware of the "Mark" designations of the bombs and bomb designs in development at Los Alamos. He said he had been unaware that the autocatalytic uranium hydride lateral implosion experimental device (Mark II) had been so completely developed by 4 July 1944 that James Conant was able to instruct General Groves on that date that the Mark II, with a nominal 1,000 tons TNT equivalent energy yield, was available to the Joint Chiefs of Staff for the purposes of operational planning.

He was unaware that by 4 July 1944 Conant had instructed General Groves that the Joint Chiefs of Staff should be informed the Mark II would necessarily be proof fired once before the design could be available for use against the enemy. He was, he said, only incidentally aware of the 17 July 1944 Port Chicago explosion, which is not mentioned in his *History*. He was unaware that on 17 August 1944 James Conant reported to General Groves the decision made at Los

Alamos that the Mark II should be put on the shelf and that the Mark II could be developed for combat use in 3 or 4 months time from 17 August 1944. Hawkins was unaware that on 17 August 1944 the upper limit of effectiveness of the Mark II was known, and that Los Alamos expected the nominal 1,000 tons TNT equivalent Mark II could be somewhat improved.

Typographical errors and apparently incorrect or inaccurate text in the original I have recognized editorially with the notation, [sic]; other typographical errors that may occur are my own. All text in boldface type is text to which I have added that emphasis to highlight germane occurrences of subjects and names.

Extracts from: Manhattan District History. Project Y, the Los Alamos Project. Volume I. Inception until August 1945.

Text occurrences by Subject and Name

- Ashworth, USN, Commander Frederick L.
- autocatalysis
- autocatalytic
- autocatalytic bomb
- autocatalytic methods
- autocatalytic methods of assembly
- B^{10}
- ball of fire
- bomb of uranium hydride
- bomb made of hydride
- bomb made of uranium hydride
- boron
- compression or expulsion of neutron absorbers
- Hirschfelder, Joseph O.
- hollow steel cylinders
- hydride bomb
- hydride bombs
- hydride calculations
- hydride compacts
- hydride core
- hydride critical assemblies
- hydride critical masses
- hydride in a bomb
- hydride program
- hydride gun
- hydride gun program
- hydride of uranium
- hydride-plastic cubes of composition UH_{10}
- hydride problem
- hydride program
- hydride mixtures
- hydrides
- hydrogen
- hydrogen-to-uranium ratio
- hydrogenous
- hydrogenous binding agent
- hydrogenous material
- hydrogenous moderator
- imploding cylinders
- implosion
- Parsons, Captain Williams S.
- Penney, William G.

self-assembling or autocatalytic method
Reynolds, USNR, Ensign George T.
Shapiro, Maurice M.
Spedding, Frank H.
Teller, Edward
UH_3
UH_4
UH_{10}
UH_{10} plastic
UH_{30}
UH_{80}
uranium hydride
uranium hydride bomb
uranium hydride gun
uranium hydride mixtures
uranium hydride program
uranium-hydrogen compositions
Urey, Harold C.
Workman, E. J.

Introduction.

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

1.54 . . . Calculations had to be made for three materials :U^{235}, Pu^{239}, and also a new compound, a **hydride of uranium**, which seemed to have certain advantages over metallic uranium as a bomb material. . . .

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

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

1.71 At the beginning of the Los Alamos Project . . . it was not known whether U^{235}, Pu^{239}, or both would be used, or whether the bomb material would be metal or compound. . . .

1.74 The metallurgy program included research and development on the metal reduction of uranium and plutonium, the casting and shaping of these metals and compounds such as **uranium hydride**, as well as various possible tamper materials. . . .

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

1.78 The method of implosion, on the other hand, was much farther removed from existing practice . . . At a meeting on ordnance problems late in April [1943], Neddermeyer presented the first serious theoretical analysis of the **implosion**. His arguments showed that the compression of a solid sphere by detonation of a surrounding high-explosive layer was feasible, and that it would be superior to the gun method both in its higher velocity and shorter path of assembly. Investigation of the method was begun almost immediately. It subsequently received two increases of priority, until at the end of the project it had become the dominant program throughout the Laboratory.

1.83 . . . [The report of the reviewing committee, dated May 10, 1943] took note of the newly discovered possibility for use of **uranium hydride**. Pointing out that the existence of the hydride had been learned of at Los Alamos somewhat by accident, the committee recommended a more systematic technical liaison between this and other branches of the larger project. It also recommended that the study of U^{233} as a possible explosive material be continued.

[**Note** on U^{233}. Rarely mentioned in the general literature as an active bomb material. "Special nuclear material" (SNM) is defined by Title I of the Atomic Energy Act of 1954 as plutonium, U^{233}, or uranium enriched in the isotopes U^{233} or U^{235}. The definition includes any other material that the Commission determines to be special nuclear material, but does not include source material. The NRC has not declared any other material as SNM. U^{233} does not occur naturally but can be formed in nuclear reactors and extracted from the highly radioactive spent fuel by chemical separation. U^{233} can be produced in special 'breeder" reactors that use thorium as fuel. Only small quantities of U^{233} are reported to have been made in the United States.]

The Period April 1943 to August 1944.

3.1 The first period of the Los Alamos Laboratory's existence [April 1943 to August 1944] presented the problems common to organizational beginning . . . In a position of responsibility parallel to that of the Director [J. Robert Oppenheimer] was established the Governing Board. This consisted of the Director, Division Leaders, general administrative officers, and individuals in important technical liaison positions.

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

4.9 . . . A number of quite basic weapon specifications, to go to the next stage, remained undermined 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.

4.12 . . . 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 [neutron] 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 reactions. But if decreasing neutron energy was compensated for by increasing the 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.

4.21 . . . At a Governing Board Meeting of October 28, 1943, the [**implosion**] program was reviewed and the decision made to strengthen and push it . . . 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.

4.25 The quantitative investigation of the hydrodynamics of the **implosion** proved a very difficult job . . . 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.

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 . . . Apart from early work with the **hydride**, effort was first concentrated on the metallurgy of uranium. . . .

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 $\mathbf{B^{10}}$, which constitutes about 20 percent of the normal **boron**. A method for the separation of $\mathbf{B^{10}}$ 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 $\mathbf{B^{10}}$ 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.

5.3 During June 1944, **R. Peierls** took charge of the **Implosion Group** [of the Theoretical Division] 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).

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 isotopically. 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. . . .

5.57 The detailed investigation of damage and other effects of [a] nuclear explosion was not pursued very far in the period under review [April 1943 to August 1944]. 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.

5.60 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 of the hydrodynamical characteristics of the implosion. . . .

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 $\mathbf{B^{10}}$ 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.

5.64 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}.

6.29 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) [McKibben] which showed approximately 1/v dependence from 0.4 Mev down to 100 Mev [sic; should be "down to 100 kev"]. 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.

6.36 . . . 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 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 considerably 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**.

6.49 . . . 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. . . .

6.53 By the end of October 1943, back-scattering measurements had been completed for a large list of substances, and a number of [tamper] substances, and a number of instrumental improvements had been made . . . 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 the 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.

6.56 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. . . .

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.

6.59 . . . For economy of material it was important to find the optimum concentration of the solution [for the Water Boiler]. 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.

6.66 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. . . .

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.

6.75 Toward the end of the first period of the Laboratory [April 1943 to August 1944], plans were underway 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).

7.3 In May [1943] 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 arranged at the request of General Groves, on the recommendation of [James B.] **Conant** and [Vannevar] **Bush** and with the approval of the Governing Board. Capt. **Parsons** returned in June as Division Leader of the Ordnance Division.

7.5 After **Parsons'** first visit in May he investigated the possibilities of obtaining a competent chief engineer to head group E-6 [Ordnance Division–Engineering]. 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).

7.7 In the fall of 1943 Groups E-7 [Delivery] under [Norman] Ramsey and E-8 [Interior Ballistics] under [Joseph O.] **Hirschfelder** were added to this [Ordnance] division.

7.10 When **Parsons** returned to Washington after his first Los Alamos trip [May 1943], 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 [Bureau of Ordnance]. 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 [Office of Scientific Research and Development], Project at the University of New Mexico, Albuquerque.

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.

[**Note.** Neutron-producing impurities (specifically, Pu^{240}) in the plutonium produced at Hanford, Washington, posed the likelihood of predetonation in the gun assembly Mark I weapon using a plutonium active. The rate of critical assembly accomplished by a 3,000 feet per second plutonium projectile was initially considered sufficiently rapid to preclude predetonation, if the presence of impurities in the Hanford plutonium could be significantly reduced. By 11 July 1944 Los Alamos had determined that impurities in the Hanford plutonium could not be significantly reduced. James Conant recorded in his "Historical Note" of 27 July 1944, "It was concluded that the evidence was so clear that ‘49’ [Pu^{239}] prepared at Hanford could not be used in the gun method of assembly that all work on the purification of ‘49’ and on the ‘49’ gun should be dropped.” The Mark I gun assembly weapon was then available for use only with slightly a U^{235}-enriched **uranium hydride** active or highly enriched uranium metal active. With either of those active materials the required Mark I projectile velocity and muzzle pressure fell within the range of conventional Navy gun design and operation.]

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 [National Defense Research Committee] through R. C. Tolman. These were computed by the ballistics group at Section 1 of the Geophysical Laboratory under the supervision of [Joseph 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 [autofrettage], 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 mechanism 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 [1943]. 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. [For a bibliography on autofrettage, see:

http://users.rcn.com/harwood.ma.ultranet/t19.html]

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 of 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 [E-1, Lt. Comdr. Albert Francis Birch, USN, group leader], 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 the plan for a proving ground became impractical for the work on high explosives when the latter work became more elaborate [i.e., Mark IV, spherical implosion design], the gun work was adequately implemented at the original proving ground at Anchor Ranch. The buildings of the 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.

[**Note.** The gun of emplacement No. 1 was a 3"/50 Naval anti-aircraft gun equipped with unrifled tubes. If a 50-100 tons TNT equivalent uranium hydride gun fission explosion was made 26 December 1943 at the Alamogordo Bombing Range, that successful demonstration of the prototype Mark I gun assembly design was made with the 3"/50 caliber Navy anti-aircraft gun.]

7.25 The proof firing between September and March [1943-1944] was done chiefly with the 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. . . .

7.26 . . . 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 Explosive Research Laboratory at Bruceton, Pa., thus adding to the preparation for the special gun.

7.27 In February [1944], the direction of Anchor Ranch was assumed by Comdr. F. Birch, with [Edwin] 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 would 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 [1944], 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.

7.31 Before any work was started on these developments, the plan was complicated by the further uncertainty in the amount of active materials that could be safely disposed in the [gun] projectile alone, or in the [gun] 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 [U^{235}] 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.

7.39 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. . . .

7.52 After the April [1943, Los Alamos] 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 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 of 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 came 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 [sic] 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 [high explosive] production plant and research program as well as for general assistance to the research in implosion dynamics, the consulting services of

[George B.] **Kistiakowsky** were required by the Laboratory in the fall of 1943. In February 1944, **Kistiakowsky** joined the staff as Capt. **Parsons'** deputy for implosion. In April he assumed full direction of the rapidly increasing administrative problems of the work.

[**Note.** The documented administrative and evident interpersonal conflicts that developed between Captain Parsons and George **Kistiakowsky** prior to February 1944, as well as before and after **Kistiakowsky**'s April 1944 assumption of full direction of the implosion program, have not yet been satisfactorily detailed nor well appraised in the published Manhattan Project historical literature. Many important documents that would permit the necessary detail to evaluate the difficult interactions of the two men are presently CLASSIFIED. Among those academic-based scientists at Los Alamos, whom General Groves characterized as "prima donnas," **Kistiakowsky** was outstanding. Captain **Parsons**' character, contrarily, is best distinguished by his own aphorism, "There is no limit to the amount of good a man can do if he does not insist that he be acclaimed for his work."]

7.70 On the occasion of [Norman] 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 flaring and stabilizing fins for such a bomb.

7.71 . . . 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 [George Chadwick], and Ramsey and Capt. **Parsons** visited the Muroc Airbase [Muroc Lake, California; now Edwards Air Force Base] to make the necessary test station plans.

HYDRIDES

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

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

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

The period August 1944 to August 1945

9.4 Shortly before the general reorganization of the Laboratory [1 August 1944], Oppenheimer outlined a plan to replace the Governing Board by two separate boards. The Governing Board had served as a policy making body attempting to handle general administrative problems and technical policies and serving as a medium for communicating technical developments. By the middle of 1944 it was seriously overburdened. The new plan divided the functions of the Governing Board between an Administrative and a Technical Board. Both of those bodies were advisory to the Director. The members of the Administrative Board appointed in July 1944 included Lt. Col. Ashbridge (Commanding Officer), Bacher, Bethe, Dow, Kennedy, **Kistiakowsky**, Mitchell, **Parsons**, and Shane; those of the Technical Board, Alvarez, Bacher, Bainbridge, Bethe, [James] Chadwick, Fermi, Kennedy, **Kistiakowsky**, McMillan, Neddermeyer, Captain **Parsons**, Rabi, Ramsey, Smith, Teller, and Wilson. . . .

[**Note.** As epitomized by paragraph 9.4, Captain **William S. Parsons'** administrative and scientific eminence at Los Alamos and his confederation with those most universally acclaimed civilian members of the Project Y scientific staff were so prominent that the reader must wonder what pervasive ignorance or prejudice of scholarship has excluded due notice and acclaim of that prominence from, essentially, the entire body of the published Manhattan Project historical literature. The record of Captain **Parsons'** fundamental and essential contributions to the Project and the record of the United States Navy's fundamental and essential contributions to the Project are amply registered by the most basic of all Manhattan Project historians, David Hawkins, but that record has been slighted by, essentially, every subsequent Manhattan Project historian.]

9.6 The Intermediate Scheduling Conference was an interdivisional committee which began meeting in August 1944 to coordinate the activities, plans and schedules of groups more or less directly concerned with the design and testing of the **implosion** bomb. The committee was formalized in November [1944] with Capt. **Parsons** as chairman, [Comdr. Frederick L.] **Ashworth** (19.3), Bacher, Bainbridge, Brode, Galloway, Henderson, **Kistiakowsky**, Lockridge, and Ramsey as permanent members and Alvarez, Bradbury, Doll, and Warner as alternates . . . Eventually the conference was concerned with both the gun assembly and implosion bombs. The agenda of its meetings included chiefly procurement arrangements for items needed for the final weapons, the test program carried out in cooperation with the Air Forces, and details of the packaging and assembly of the bomb parts for overseas shipment. . . .

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

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

[**Note.** For the history of Project A, see: Harlow W. Russ, *Project Alberta. The Preparation of Atomic Bombs for use in World War II.* Los Alamos Historical Society, Los Alamos, 1984; Exceptional Books, Ltd., Los Alamos, 1990.]

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

Damage

11.20 Much more extensive investigation of the behavior and effects of a nuclear explosion were made during this period than had been possible before, tracing the history of the process from the initial expansion of the active material and tamper through the final stages. These investigations included the formation of the shock wave in air, the radiation history of the early stages of the explosion, the formation of the "**ball of fire**," the attenuation of the blast wave in air at greater distances, and the effects of blast and radiations of [sic] human beings and structures.

[**Note.** Compare the text above with the text of the document, **"History of 10,000 ton gadget"** in *The Last Wave from Port Chicago,* Chapters 5 and 6.]

Much of this information was of importance in making plans for the Trinity text. It was essential to know also the probable fate of Pluto-nium and fission products in the **ball of fire** and the smoke cloud ascending out of it. These calculations, plus calculations of blast and radiation, were essential in planning experiments and observations at Trinity, and in planning for the protection of personnel. Theoretical studies of damage to structures and to personnel were, of course, made in anticipation of combat use. Extensive use in this connection was made of British data on damage to various kinds of structure caused by high explosive bombs. General responsibility for this work was given to Group T-7, with the advice and assistance of W. J. [sic] **Penney**.

[**Note.** William **G**eorge Penney. This particular typographical error in the Hawkins' *History* was carried over to the text of the 1993 DOE Los Alamos history, *Critical Assembly,* on page 344: "By January 1945, **Hirschfelder** and British physicist William J. **Penney** had gathered a great deal of data from Britain on the structural damage caused by German high-explosive bombs. These data proved extremely useful in the group's further calcul-ations, and by the next month it had developed a hypothetical "history" of the explosion of a nuclear weapon with the explosive power of 10,000 tons of TNT."

[That "hypothetical history," composed by Joseph O. **Hirschfelder** and William George **Penney,** is the document reproduced and discussed in

Chapters 5 and 6, the “History of 10,000 ton gadget.” Logically and etymologically, a “hypothetical history” is a contradictory conjunction of terms. A history by definition is a record and analysis of past events; most of the information provided by the “History of 10,000 ton gadget” is predictive of the Trinity test and is not, therefore, history. But Step 10 of the “History of 10,000 ton gadget” does, in one instance, report history, and specifically the history of the **Port Chicago explosion** in precise description of the Port Chicago ball of fire: “. . . **ball of fire** reached 2,000 ft. . . .” The column of flame from the Port Chicago explosion ascended 8,000 to 10,000 feet, but the discrete and typical nuclear explosion **ball of fire** from the **Port Chicago explosion** ascended to 2,000 feet before it disintegrated into a rising column of turbulent convection currents.]

14.1 As a result of the August 1944 reorganization of the Laboratory . . . by the end of September the organization of the Ordnance Division was . . . [7 groups, including] O-6, Water Delivery, Exterior Ballistics, M. M. **Shapiro** [group leader]. . . .

14.20 From the experimental data it was discovered, contrary to expectation, that a surface explosion produced larger gravity wave [in water] than a subsurface explosion of the same size. From a theoretical analysis, scaling laws were derived which made it possible to predict with some assurance the effects of the surface or near-surface detonation of atomic bombs. This program was the work of the Water Delivery and Exterior Ballistic Group [led by Maurice M. **Shapiro**], with the assistance of **Penney** and von Neumann. It had been begun at the end of the previous period [to August 1944] by McMillan.

15.4 The work of the Critical Assemblies Group was carried out at Omega Site, (6.64 ff) where it shared space with the Water Boiler Group. Its main work was to carry out experiments with critical amounts of active materials, including both **hydrides** and metals. It was given the further responsibility of investigating the necessary precautions to be observed in the handing and fabrication of active materials at Los Alamos, to be certain that in these operations no uncontrolled nuclear reactions could occur. When G Division acquired the definite responsibility of designing and preparing the core and tamper—the “pit assembly”—of the Trinity and subsequent implosion bombs, members of the Critical Assemblies Group were given this responsibility.

15.5 During the early period of this group's existence, a large number of critical assemblies were made with various **uranium hydride mixtures**. A relatively large amount of effort was spent in investigating these assemblies for two reasons. The first was that there was not yet enough material for a metal critical assembly without **hydrogen**. The second was that by successively lowering the **hydrogen** content of the material as more U^{235} became available, experience was gained with faster and faster reactions. It was also still not ruled out, at this time, that **hydride bombs** using small amounts of material might be built.

15.6 By November 1944 enough **hydride-plastic cubes of composition UH_{10}** had been accumulated to make a cubical reacting assembly in the beryllia tamper, if the effective composition was reduced to **UH_{80}** by stacking seven polythene cubes for each cube of **UH_{10}** plastic. Further experiments were made with less **hydrogen** and other tampers. In February 1944 [sic; should be 1945] this **hydride** was sent back to the chemists and metallurgists for recovery and conversion to metal, and the program of **hydride critical assemblies** was ended.

15.7 The most spectacular experiments performed with the **hydride** were those in which a slug of **UH_{30}** was dropped through the center of an almost critical assembly of **UH_{30}** so that for a short time the assembly was supercritical for prompt neutrons alone. This experiment was called "tickling the dragon's tail," or simply the "dragon." The velocity of the falling slug was measured electrically. Before the experiment was actually performed a number of tests were made to prove that it was safe, for example that the plastic would not expand under strong neutron irradiation, thus causing the slug to stick and cause an explosion. On January 18, 1945, strong neutron bursts were obtained, of the order of 10^{12} neutrons.

15.8 These experiments gave direct evidence of an explosive chain reaction. They gave an energy production up to twenty million watts, with a temperature rise in the **hydride** up to 2°C per millisecond. The strongest burst obtained produced 10^{15} neutrons. The dragon is of historical importance. It was the first controlled nuclear reaction which was supercritical with prompt neutrons alone.

17.4 The flow of beta stage enriched uranium received from the Y-12 plant was generally as follows: the material was received as a purified fluoride and reduced directly to metal. For **hydride** experiments the metal was converted to **hydride** and formed by plastic bonding. When **hydride** or metal experiments were completed, the material was returned for recovery, as in the meantime were crucibles, liners, and other containers that had been used in fabrication. Recovered solutions were converted hexanitrate, extracted with ether, and precipitated as reduced oxalate. The oxalate was ignited to oxide and converted back to the original tetrafluoride.

19.5 In March 1945, Project Alberta or Project A was established to provide a more effective means of integrating the activities of the various Los Alamos groups working on problems of preparation and delivery of a combat bomb than the Delivery Group by itself had been able to offer . . . Captain **Parsons** was the officer in charge of Project Alberta, with Ramsey and later Bradbury as deputies for scientific and technical matters. The organization included three groups—and administrative group known as Headquarters Staff, a technical policy committee called the Weapons Committee (9.10) and a working group of representatives from other divisions. Comdr. **Ashworth** was operations officer and military alternate for Capt. **Parsons** and served as chief of the Headquarters staff . . . Group representatives [on the Weapons Committee] included [among others] . . . Comdr. **Ashworth** [Tests at Wendover], [Hans] Bethe [General Theory], [William G.] **Penney** [Damage], [Maurice M.] **Shapiro** [Ballistics].

19.7 . . . The emphasis during this period was on supplying the many details necessary for successful operation and correcting faults which became apparent in tests . . . Liaison problems in connection with the development of bombs were of great importance during this period and were handled primarily by Capt. **Parsons** and Comdr. **Ashworth**. Among the military and semimilitary organizations and individuals involved in addition to the United States [Army] Engineers were the 20th Air Force, the Bureau of Ordnance, the Assistant Chief of Naval Operations for Material, Commander Western Sea Frontier, **Commandant 12th Naval District** [San Francisco], **Commandant Navy Yard Mare Island**, Bureau of Yards and Docks Navy Department, NOTS Inyokern [Naval Ordnance Test Station, Inyokern, California; now Naval Weapons Center, China Lake], NAD Yorktown [Naval Ammunition Depot, Yorktown, Virginia; now Naval Weapons Station, Yorktown], and NAD McAlester [Naval Ammunition Depot, McAlester, Oklahoma; now McAlester Army Ammunition Plant]. After Parsons and Ashworth went overseas much of this work was handled by Capt. R. R. Larkin, USN, who arrived at Los Alamos in June [1945].

19.9 Perhaps the most important function of Project Alberta was planning and preparing for overseas operations. As early as December 1944 the initial planning and procurement of some kits of tools and materials had begun, and these activities continued at an accelerated rate through July [1945]. In February Comdr. **Ashworth** was sent to Tinian to make a preliminary survey of the location and select a site for project activities. By March the construction needs for the Tinian Base, known as Destination, were frozen, and construction began in April.

19.10 As early as June 1944, the need had been considered for selecting personnel for field crews required in the final delivery of the bomb and in the later stages of experimentation and testing prior to delivery . . . Actually the personnel for the project teams at Tinian were selected early in May 1945, and were organized as follows:

Officer-in-Charge	Captain **Parsons**
Scientific and Technical Deputy	Norman Ramsey
Operations Officer and Military Alternate	Comdr. **Ashworth**
Team members included . . .[among 36]	Ens. **Reynolds**.

19.15 Since the earliest date previously discussed for combat delivery [of the Mark I] was August 5 (at one time the official date was August 15), **Parsons** and Ramsey cabled Gen. Groves for permission to drop the first active unit as early as August 1. [For the 6 August 1945 Hiroshima combat mission with the Mark I] Col. P. W. Tibbets was pilot of the Enola Gay, the B-29 which carried the bomb. Maj. Thomas Ferebee was the bombardier, Capt. **Parsons** was bomb commander, and Lt. Morris Jepson was electronics test officer for the bomb.

19.16 Only a few days before the scheduled drop it was decided by the technical group that it was not safe to take off with the bomb completely assembled, since a crash might mean tremendous destruction to men and materials on Tinian. Full safing could not be secured, but it was finally agree that a partial safeguard would come if the cartridge which contained the propellant charge were inserted through the opening in the breech block during flight rather than on the ground. This scheme had been considered before (14.14) but was not finally adopted until this time. Capt. Parsons, who was already assigned to the crew as weaponeer, was given the job. This decision meant that Capt. **Parsons** had to be trained in a short time to perform the operation, and also that the bomb bay of the B-29 had to be modified to provide him with a convenient place to stand while completing the assembly. These things were done and the bomb was not completely assembled until the plane was safely in flight. [For extensive elaboration see, Harlow W. Russ, *op. cit.*]

19.17 The progress of the mission is described in the log which Capt. **Parsons** kept during the flight:

6 August 1945

0245	Take Off
0300	Started final loading of gun
0315	Finished loading
0605	Headed for Empire from Iwo
0730	Red plugs in (these plugs armed the bomb so it would detonate if released
0741	Started climb
	Weather report received that weather over primary and tertiary targets was good but not over secondary target
0838	Leveled off at 32,700 feet
0847	All Archies (electronic fuses) test to be OK
0904	Course west
0909	Target (Hiroshima) in sight
0915-1/2	Dropped bomb (Originally scheduled time was 0915)
	Flash followed by two slaps on plane. Huge cloud.

1000	Still in sight of cloud which must be over 40,000 feet high
1003	Fighter reported
1041	Lost sight of cloud 363 miles from Hiroshima with the aircraft being 26,000 feet high

The crews of the strike and observation aircraft reported that, five minutes after release, a low 3 miles diameter dark grey cloud hung over the center of Hiroshima, out of the center of this a white column of smoke rose to a height of 35,000 feet with the top of the cloud being considerably enlarged. Four hours after the strike, photo-reconnaissance planes found that most of the city of Hiroshima was still obscured by the cloud created by the explosion, although fires could be seen around the edges. Pictures were obtained the following day and showed 60 per cent of the city destroyed.

19.19 The first Fat Man bomb [Mark IV] was scheduled for dropping on August 11 (at one time the schedule called for August 20, but by August 7 it was apparent that the schedule could be advanced to August 10. When **Parsons** and Ramsey proposed this change to Tibbets he expressed regret that the schedule could not be advanced two days instead of only one, since good weather was forecast for August 9 and bad weather for the five succeeding days. It was finally agreed that Project Alberta would try to be ready for August 9, provided it was understood by all concerned that the advancement of the date by two full days introduced a large measure of uncertainty. All went well with the assembly, however, and the unit was loaded and fully checked late in the evening of August 8. The strike plane and two observing planes took off shortly before dawn on August 9. Maj. C. W. Sweeney was pilot of the strike ship Great Artiste, Capt. K. K. Beahan was bombardier, Comdr. **Ashworth** was bomb commander, and Lt. Philip Barnes was electronics test officer.

19.22 On the day following the Nagasaki mission, the Japanese initiated surrender negotiations and further activity in preparing active [atomic bomb] units was suspended. The entire project was maintained in a state of complete readiness for further assemblies in the event of a failure in the peace negotiations. It was planned to return all Project

Alberta technical personnel to the United States on August 20, except for those assigned to the [General Thomas F.] Farrell mission for investigating the results of the bombing in Japan. Because of the delays in the surrender procedures, Gen. Groves requested all key personnel to remain at Tinian until the success of the occupation of Japan was assured. The scientific and technical personnel finally received authorization and left Tinian on September 7, except for Col. Kirkpatrick and Comdr. **Ashworth** who remained to make final disposition of project property.