Mark II:
February 5, 1939 – August 24, 1943

As shown in Chapter 13, the Mark II was an implosion weapon susceptible to use with either slightly enriched uranium or plutonium as the active material. In his memorandum to General Leslie Groves, "Findings of Trip to L. A. [Los Alamos] July 4, 1944," James Conant forecast that the Mark II with a 9 kg U^{235} active would produce a nuclear fission explosion equivalent to the detonation of 1,000 tons of TNT and, if detonated in an optimal air burst, would result in structural damage beyond repair (Class B damage) within an area of 2-5 square miles. James Conant's 4 July 1944 memorandum to Gen. Groves advised the General that the Mark II was certain enough to be used by the Joint Chiefs of Staff for the purposes of operational planning, but Conant stipulated the Mark II would necessarily be once proof fired before the design could be available for use against the enemy. Conant's 4 July 1944 memorandum informed the General that "present indications" permitted the forecast that 3-6 of the Mark II would be available before 1 July 1945 and for the six months following 1 July 1945 either 4 of the Mark I Hiroshima-type bomb or 20 of the Mark II would be available.

On 17 August 1944 James Conant reported to Gen. Groves that, in consequence of the Port Chicago Naval Magazine explosion of 17 July 1944, Los Alamos had agreed the Mark II should be put on the shelf unless other implosion methods of bomb assembly should fail of development, that the upper limit of effectiveness of the Mark II was known and could be somewhat improved, and that the Mark II could be developed for combat use in 3 or 4 months time.

Chapter 13 showed that J. Robert Oppenheimer first proposed a uranium deuterium nuclear fission bomb on 5 February 1939, and Chapter 13 showed that the active fissionable material of the Mark II was a compound of slightly U^{235}-enriched uranium metal and deuterium. Chapter 13 also disclosed that in summer 1993 Vice Admiral Frederick L. Ashworth, USN, Ret., in conversation with this author identified the Mark II to have been the autocatalytic uranium hydride (deuterium) lateral implosion experimental device.

Uranium hydride

Uranium metal and the isotopes of hydrogen, including the deuterium isotope (the deuteride), can be compounded in several different proportions; all compounds of uranium and hydrogen are uranium hydrides. In the language of chemistry, metal hydrogen compounds, in which the metal forms covalent bonds with hydrogen, are not properly called metals. Lithium deuteride and uranium deuteride, as examples of uranium hydrides, are nonmetals because the deuterium (H_2) in the compound exists in the -1 oxidation state while the metal exists in a positive oxidation state. A uranium metal deuteride compound is most efficiently accomplished at a temperature of 225° C in a refractory-lined, sealed container, usually steel, known in the metallurgical sciences as a "bomb"; the result is a fine, black, pyrophoric powder, UH_3. Because, being pyrophoric, UH_3 will spontaneously ignite in air a half dozen small fires each week in the uranium metallurgy work areas at Los Alamos were usual during spring and summer 1943 until methods had been developed to constrain the material's pyrophoric nature.

Two men were responsible for development of the Manhattan Project uranium hydride program, Frank (F.H.) Spedding working at the Ames campus of the University of Iowa and Cyril Stanley Smith, who joined the Project at Los Alamos in April 1943 from the American Brass Company. Spedding's group at Ames first produced a uranium hydride compound, originally thought to be UH_4; Spedding was also responsible for development of an industrial scale method of producing uranium metal, by which was accomplished World War II production of all the uranium metal necessary to Manhattan Project scientific and

weapon purposes. Los Alamos learned of the possibility of large scale, controlled uranium hydride production in April 1943, apparently also a process developed by Frank Spedding.

When Cyril Smith arrived at Los Alamos his first undertaking was development of facilities and the technology there to produce uranium hydride in quantity which, because of its high concentration of neutron energy moderating deuterium, was to be the active material for the uranium deuteride weapon that Oppenheimer had proposed 5 February 1939 and which on 4 July 1944 Conant first identified as the Mark II. The first work in uranium metallurgy at Los Alamos, directed by Cyril Smith, was the preparation and powder metallurgy of uranium hydride. We are fortunate to have available Cyril Smith's previously inaccessible "Semimonthly Reports of the Metallurgy Group," to J. W. Kennedy, Nos. 1-14 for the periods ending July 15, 1943, to February 29 1944. These reports provide an extensively detailed account of the early Los Alamos work in uranium hydride and plutonium metallurgy. Smith's reports are reproduced in Edward F. Hammel's recollections, *Plutonium Metallurgy at Los Alamos, 1943-1945.* Los Alamos: Los Alamos Historical Society, 1998. Hammel served with scientific distinction, perceptive good humor and wit on the Board of Directors of the New Mexico Energy Research and Development Institution during the years of my affiliation with the Institute board.

Manhattan Project historian David Hawkins: the UH bomb

David Hawkins' *Manhattan District History: Project Y, the Los Alamos Project* (Volume I) provides the most succinctly accumulated details of the history of the developments that produced the Mark II. A concatenated abstract of all references in the Hawkins history to those developments is provided as Appendix B, but will be here summarized in abstract; paragraph numbers of the Hawkins history are omitted here but are provided in Appendix B.

Hawkins' text makes frequent mention of the "hydride gun." Until February or August 1944 consideration was given to the use of uranium hydride as the active material for use with the gun assembly bomb

design (Mark I). Very little declassified information is available about the hydride gun program. Hawkins reports that development of the hydride gun continued, "until February 1944, by which time the hydride gun had been abandoned"; the U.S. Department of Energy Los Alamos history, *Critical Assembly. A Technical History of Los Alamos during the Oppenheimer Years, 1943-1945,* reports, "At Teller's suggestion T-Division investigated uranium hydride . . . by August 1944, interest in the hydride gun had disappeared."

There are, however, several evidences made available to me since 1982 which raise the possibility that a nuclear fission detonation of energy yield in the range of 50-100 tons TNT equivalent was achieved by a hydride gun assembly, probably using a modified 3"/50 Navy anti-aircraft gun equipped with an unrifled tube, at 21:00 hours on 26 December 1943 at the Alamogordo Bombing Range in New Mexico, on a playa in the vicinity of Oscuro Peak. Physical evidence of that test, if it occurred, is recognized in aerial photographs, seismic records, and Landsat thermal images of that area in which a circular scar of high thermal index may represent the area of thermally-fused sand which would have been the consequence of that detonation. Two Army veterans told the National Association of Atomic Veterans in 1982, or earlier, that 100 U.S. Army volunteers had been proximate to that detonation in slit trenches and in the open.

Short-wave radio transmissions intercepted by U.S. Army Intelligence in May 1944, broadcast from the U.S., and which detail the activities of a group of 12 Spanish Fascist espionage agents operating in the U.S., report that "a chemical explosion which reached a temperature of 1,000,000 degrees occurred at Alamogordo. We will all die." The Spanish agent who made that report was, several weeks later, reported by his compatriots to have been shot and killed in Las Vegas—whether Las Vegas, New Mexico, or Las Vegas, Nevada, cannot be determined. Probably U.S. Army Intelligence caught him and he was summarily executed. This particular information was discovered by NHK News researchers working in Washington, D.C. and was made available to this author by NHK News executives in meeting at Thanksgiving 1982 in Santa Fe, New Mexico.

In 1984, New Mexico Institute of Mining and Technology geophysicists affiliated with the programs of the New Mexico Energy Research and Development Institute identified for this author the relevant Landsat images.

Several of David Hawkins' mentions of the uranium hydride bomb development are transcribed here:

> "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.

> "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.

> "In this connection the Laboratory undertook to procure large amounts of boron enriched in B^{10}, which constitutes about 20 percent of the normal boron. A method for the separation of B^{10} had been developed by [Harold] 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.

> "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.

"The attack on the many-velocity [neutron] 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. 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 . . . By July 1944, however, a method had been developed which was applicable to a spherical core and tamper.

"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.

"After the formation of the Uranium and Plutonium Metallurgy Group in April 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 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. 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.

"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.

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

Mark II: Autocatalytic fission bomb assembly

In chemistry, a **catalyst** is a substance that is usually present in small amounts relative to chemical reactants which, in a chemical reaction, modifies the rate of the chemical reaction. Especially a catalyst modifies the rate of a chemical reaction by an increase of the rate of a chemical reaction. **Catalysis** is the action of a catalyst which modifies the rate of a chemical reaction, especially as the action of the catalyst increases the rate of a chemical reaction. **Autocatalysis** is catalysis of a chemical reaction by one of the chemical products of the reaction. An autocatalytic chemical reaction does itself produce a catalyst which increases the rate of the chemical reaction in which it is present, which

chemical reaction does produce more of the catalyst and, therefore, does create a condition favorable to continuation and increase of the chemical reaction.

Analogously, an angry argument among persons produces more anger, which is a catalyst that increases an already angry argument and promotes a condition favorable to continuation and augmentation of an angry argument. An angry argument can be said to be autocatalytic because it does, itself, create the catalyst that continues and increases the intensity of an angry argument.

An autocatalytic method of nuclear fission bomb assembly is one which, as it progresses, does itself create a chemical reaction that will increase a condition favorable to effectuate a nuclear fission chain reaction. However, in the case of the Mark II the generally compre-hended definition of autocatalysis was reversed: Rather than an increase of a chemical reaction to augment a condition favorable to effectuate a nuclear fission chain reaction in the Mark II, a decrease of a chemical reaction was necessary to augment a condition favorable to effectuate a nuclear fission reaction.

Language is often a casualty of war but, as often, war is a catalyst to new idiomatic language constructs which may in time become catholic in use as, for example, a "Pyrrhic victory" for 2,000 years has denoted a military victory achieved with staggering loses, as occurred when Pyrrhus, the Greek King of Epirus, defeated the Romans in the Battle of Asculum (279 B.C.) but lost 3,500 men, including many of his ablest officers. When he was congratulated on the victory, Pyrrhus is reported to have said, "If we defeat the Romans in one more such battle, we shall be completely ruined."

Specifically, in the static condition, inclusion of the boron-10 isotope (B^{10}) in the slightly U^{235}-enriched active material of the Mark II acted as a very efficient absorber of the occasional high energy neutrons produced in the Mark II active material as the result of the spontaneous fission of U^{235} nuclei. In the static condition, the inclusion of B^{10} in the Mark II active permitted the accumulation of a fissionable active greater than the critical mass. In the static condition, absent the ef-fective B^{10} absorption of those spontaneously produced high energy

neutrons in the Mark II U^{235} active, a spontaneous fission chain reaction (predetonation) in the Mark II active was certain to occur.

However, when deliberate detonation of the bomb would be initiated the action of B^{10} in the fissionable active as a high energy neutron absorber would inhibit the condition favorable to effectuate a nuclear fission chain reaction by absorption of high energy neutrons produced by and essential to continuation of the chain reaction. Therefore for operation of the Mark II, rather than an increase of a chemical reaction to augment a condition favorable to a fission chain reaction a decrease of a chemical reaction was required. Either a means could be devised to remove the neutron-absorbing B^{10} from the Mark II active as the detonation progressed or, alternatively, a means could be devised to suppress the efficiency of the B^{10} as a high energy neutron absorber as the detonation progressed.

The M.A.U.D. Committee report, July 1941

The first substantially detailed analysis of possible methods to develop a nuclear fission chain reaction as a source of power and as a weapon for military purposes was the British M.A.U.D. Committee report of July 1941. Within 30 days a copy of the M.A.U.D. report had been delivered by the British to the U.S. and was received by Director of the Office of Scientific Research and Development Vannevar Bush. At that time Adm. William S. Parsons (then Commander Parsons) was the Navy Bureau of Ordnance liaison officer with the Naval Research Laboratory; Parsons' office at the NRL adjoined that of Ross Gunn. (Ross Gunn, see Chapter 12.) Commander Parsons was then working directly with Vannevar Bush to coordinate Naval Research Laboratory, OSRD and NDRC programs in development of the proximity fuze.

The M.A.U.D. report that Vannevar Bush had received from the British in August 1941 proposed a gun assembly atomic bomb design. Parsons, then working directly with Vannevar Bush, was the Navy's most knowledgeable and experienced experimental ordnance officer; particularly he was the Navy's most knowledgeable and experienced officer in the design and manufacture of naval guns. At the end of the next four years Capt. Parsons at Los Alamos had successfully devel-

oped the most powerful gun that, to that date, had ever been made, the Mark I gun assembly atomic bomb detonated in combat at Hiroshima, 6 August 1945.

I have discovered no documentary evidence to conclusively establish that in August 1941 Vannevar Bush provided the M.A.U.D. report to Commander Parsons for his evaluation of the gun assembly atomic bomb proposed by that report, but whom else would Vannevar Bush turn to for evaluation of that British proposal for a gun assembly atomic bomb than the Navy's most accomplished ordnance officer, with whom Bush was well acquainted and whose office at the Naval Research Laboratory was just several miles down the Potomac River from the Government center of the District of Columbia where Vannevar Bush worked?

I expect when more complete research inquiry is made into Adm. Parsons naval career by future scholars the history will show that his involvement with the development of the Mark I gun assembly bomb began in August 1941 and at that time Vannevar Bush asked him to appraise the British proposal for a gun assembly atomic bomb.

Historian Al Christman has written in his biography of Parsons, *Target Hiroshima,* "In March 1943 Parsons knew nothing of the chain of nuclear events in Vannevar Bush's life that were about to encircle him as well."

We do know that Vannevar Bush, as well as Adm. Purnell of the Atomic Bomb Military Policy Committee, had recommended Parsons to Gen. Groves when the Manhattan Project was established. At Los Alamos when he arrived in May 1943 Capt. Parsons was named head of the Ordnance Division and in the weeks following the Port Chicago explosion he was named Los Alamos Laboratories Associate Director.

The M.A.U.D. report does not propose an autocatalytic method of bomb assembly, but does emphasize that an optimal fission chain reaction in U^{235} would require that fast fission-produced neutrons be energy moderated by elastic collisions with deuterium:

"Since slow neutrons are so much more effective in causing fission of 235 it is clear that the conditions for a chain reaction will be more favorable if the fast neutrons present in the system can be slowed down. This can readily be achieved by adding to the uranium a suitable compound of a light element such as hydrogen, deuterium, or carbon. The reduction in velocity of the neutrons takes place by the collision of the neutron with the light atomic nucleus . . .The loss of neutrons due to capture by deuterium is much less than that due to hydrogen."

The report also suggested that an improvement in the conditions favorable to a fission chain reaction "can be achieved by arranging the slowing-down material in alternate layers or blocks instead of a uniform mixture." The first nuclear fission power reactor, constructed at the University of Chicago by Enrico Fermi and first operated 2 December 1942, utilized natural uranium oxide and metal distributed in pockets throughout 350 tons of graphite blocks, but for the Mark II bomb the "slowing-down" material, deuterium, was compounded with slightly U^{235}-enriched uranium metal to form uranium hydride (uranium deuteride) and was thus a uniform mixture.

Autocatalysis, the Briggs report, October 1941

On 28 October 1941 National Bureau of Standards Director Lyman J. Briggs, via National Defense Research Committee Chairman James Conant, transmitted to Arthur Holly Compton at the University of Chicago Metallurgical Laboratory "a special report on chain reactions from the group dealing with the theoretical aspects of the uranium work." Section 4 of Briggs' report responds to Compton's written question, "Can the system be controlled by the 'expulsion' method?":

> "This method consists in removing neutron absorbing materials from the uranium by means of an ordinary explosion. In one of its modifications it is intended to operate in such a way as to make use of a small fraction of the released nuclear energy to eject the absorber at a rate which increases as the reaction progresses.
>
> "It is felt that this method has possibilities which should be investigated. It might lead to elimination of effects of spontaneous fission, to a simplification of the arrangement and to an increase in the

energy evolved. It is felt that a mathematical investigation of the possibilities would be helpful."

Autocatalysis, Edward Teller, Spring 1942

In spring 1942 meetings with J. Robert Oppenheimer and others at the University of California, Berkeley, to consider possible ways to design an atomic bomb, Edward Teller proposed the autocatalytic bomb assembly concept that would be developed to be the Mark II bomb. Teller suggested that B^{10} might be admixed with the fissionable active of a uranium hydride bomb, and he proposed that when the B^{10} was highly compressed in a nuclear explosion the absorption of neutrons by B^{10} would diminish as a result of compression, promoting an increase in the criticality of the bomb's active and boosting the energy release from the bomb. This was, in fact, the first perception of the ionization implosion pressure principle that would be the basis of the hydrogen fusion bomb. In 1944 John von Neumann proposed that the B^{10} in Teller's autocatalytic system for the Mark II bomb be replaced by a deuterium-tritium (D-T) mixture, in which von Neumann theorized that thermonuclear ignition of the D-T mixture would occur as a result of heating and ionization compression in the conditions of an nuclear fission explosion.

Von Neumann's proposal was an important step toward the creation of a thermonuclear-boosted atomic bomb. The Mark II which was proof fired at the Port Chicago Naval Magazine 17 July 1944 was the world's first nuclear fission bomb, but the Mark II in several ways that are not presently disclosed in the declassified literature was the first important precursor of the hydrogen fusion bomb, the H-bomb. I consider likely that an insignificant component of deuterium fusion energy was produced by the detonation of the Mark II at the Port Chicago Naval Magazine.

Autocatalysis, Manley, Oppenheimer, Serber, and Teller, November 1942

On November 26, 1942 University of Chicago Metallurgical Laboratory Director Arthur Holly Compton, in his "Report on the Feasibility of the '49' Project," considers the autocatalytic method for use with

plutonium; this report is exclusively concerned with the application of plutonium to fission bombs and therefore does not consider uranium hydride bomb autocatalysis.

In discussion of an autocatalytic plutonium bomb Compton wrote in this report, "If, due to impurities or otherwise, too many natural neutrons are emitted from the '49', an efficient explosion can be produced by an 'autocatalytic' method. This procedure requires perhaps 6 times more material than does the direct method, and is more hazardous in use, but is, nevertheless, considered practicable if the requirements make it necessary."

Section F of Compton's 26 November 1942 report includes "The Use of Materials in a Fission Bomb," by John H. Manley, J. Robert Oppenheimer, Robert Serber, and Edward Teller. Page 3 of the Manley, Oppenheimer, Serber and Teller report describes the autocatalytic method for plutonium bomb assembly under the heading, "Detonation Autocatalysis":

"Since the autocatalytic method circumvents the difficulty of predetonation it will be illustrated by an example:

"In the mass of '49' small spheres (approx. 3 cm diameter) of B^{10} (or B^{10} hydride) are imbedded. The explosion is brought about by an external change such as bringing a neutron reflector closer to the bomb. In the course of the explosion the small boron bubbles will be compressed and their power of neutron absorption thereby decreased. This increases the neutron reproduction ability of the bomb and the explosion proceeds.

"This method has the advantage that there is no danger of predetonation from stray neutrons caused by spontaneous fission, or by (α,n) reactions, or cosmic rays, or enemy defense measures.

"The disadvantages are: 1.) The boron absorbers would make it necessary to increase the amount of '49' by about a factor of 3 or more to obtain the same efficiency and by a further factor of 2 to make the experimental control safe. 2.) A good fast-neutron absorber such as separated B^{10} would be required. 3.) The calculations involved for the construction are considerably more complicated and this increases the difficulty of experimental control and testing."

Autocatalysis, Robert Serber, April 1943

Robert Serber's Los Alamos "Indoctrination Course" was a series of lectures given by Serber at Los Alamos during the first two weeks of April 1943. The earliest document catalogued at Los Alamos is "LA-1. The Los Alamos Primer," which consists of manuscript notes taken during those lectures by Edward (E. U.) Condon.

"LA-1, The Los Alamos Primer" is available online at the link:

http://upload.wikimedia.org/wikipedia/commons/9/9c/Los_Alamos_Primer.pdf

and is commercially published: Serber, Robert. *The Los Alamos Primer. First lectures on how to build an atomic bomb.* University of California Press, 1992.

During April 1943 newly arriving members of the scientific staff at Los Alamos who attended these lectures were informed by Serber, "All autocatalytic schemes that have been thought of so far require large amounts of active material, are low in efficiency unless very large amounts are used, and are dangerous to handle. Some bright ideas area needed."

"LA-1" does not disclose why Serber described autocatalytic "schemes" as dangerous to handle, but in early autocatalytic assembly experiments at Los Alamos the safe proportions of active material and catalyst were not known. The speed of the autocatalytic reaction as the proportions of active material and catalyst were experimentally augmented or diminished was not known; and the effect on the reaction rate consequent to tamper materials variations and even minimal compression of the active and catalyst were not known. Furthermore, because those autocatalytic assembly experiments were performed with uranium hydride, the hazard of fire was ever present—uranium hydride is pyrophoric—until means had been devised to constrain spontaneous combustion of the uranium hydride.

"LA-1" proposes two methods of autocatalytic uranium fission bomb assembly. The second of the two, the "boron bubble scheme," is the

method developed for the Mark II. The text of "LA-1" describes the generalized conception of that method.
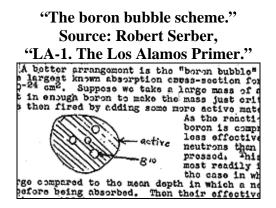
The "boron bubble scheme"

Serber instructed attendees at those lectures that, with reference to the autocatalytic methods of assembly, "A better arrangement is the 'boron bubble' scheme. B^{10} has the largest known absorption cross-section for fast neutrons . . . Suppose we take a large mass of active material and put in enough boron to make the mass just critical. The device is then fired by adding some more active material or tamper. As the reaction proceeds the boron is compressed and is less effective at absorbing neutrons than when not compressed. This can be seen most readily if one considers the case in which the bubbles are large compared to the mean depth in which a neutron goes in boron before being absorbed. Then their effectiveness in removing neutrons will be proportional to their total area and so will drop on compression. Hence ν [the neutron number] will increase as the bubbles are compressed. If the bomb is sufficiently large this tendency is bound to overweigh the opposing one due to the general expansion of the bomb material, since the distance the edge of the bomb must move to produce a given decrease in ν increases with the radius of the bomb, whereas for a larger bomb the distance the edge of a bubble must move is unchanged, since it is not necessary to increase the radius of the bubbles but only to use more of them."

**"The boron bubble scheme."
Source: Robert Serber,
"LA-1. The Los Alamos Primer."**

Basically this means that in an uncompressed condition, at normal atmospheric pressure, bubbles of B^{10} mixed into the U^{235} active material of a fission bomb will be very effective high energy neutron absorbers. The fission efficiency potential of the bomb required the safe accumulation of active material substantially in excess of the critical mass. But as the minimum critical mass is exceeded the hazard of a spontaneous fission chain reaction occurring in the active material rapidly increases, for the reason that spontaneous nuclear fissions within the active material each produce an average of 2.5 high energy

neutrons, and each of those spontaneously fission-produced neutrons is theoretically capable of inducing a nuclear fission chain reaction in the active material.

However, the introduction of bubbles of B^{10} within the active material to absorb spontaneously fission-generated high energy neutrons permitted the accumulation of a safe supercritical active. Thereby the difficulty of accumulating a safe supercritical U^{235} active that would, when fully assembled, enable at least a moderately efficient nuclear fission explosion was achieved by the introduction of B^{10} bubbles into the active material. The next problem was to devise a method to eliminate the B^{10} from the active material at the moment the assembly was so complete that the explosive fission reaction could proceed, and proceed without the hindrance of high energy fission-generated neutron absorption by the B^{10} bubbles. Elimination of the B^{10} from the active was known as the "expulsion" method of autocatalytic assembly.

Alternatively, and more advantageously, radical compression of the boron bubbles would have the result that "their effectiveness in removing neutrons will be proportional to their total area and so will drop on compression. Hence ν [the neutron number] will increase as the bubbles are compressed." This method was known as the "compression" method of autocatalytic assembly, and was the method of assembly employed by the autocatalytic uranium hydride lateral implosion experimental device, named by James Conant the Mark II.

May 1943, refinement of autocatalysis

In a report dated 10 May 1943 the Los Alamos Reviewing Committee noted the "recently discovered" possibility for use of uranium hydride as the active material of a nuclear fission bomb; the report noted that Los Alamos had learned of the existence of the hydride "somewhat by accident." In source materials available to me I have found no information that amplifies the statement that Los Alamos learned of the existence of uranium hydride somewhat by accident. Frank Spedding at the University of Iowa, Ames, first produced the uranium hydride, apparently early in 1943; perhaps Spedding didn't consider his accom-

plishment of sufficient importance to merit communication to any of his Manhattan Project colleagues.

Implosion and the first Los Alamos implosion experiment, July 4, 1943

In a meeting at Los Alamos on ordnance problems late in April 1943, Seth Neddermeyer presented the first persuasive theoretical analysis of the implosion method of assembling a supercritical fissionable active. Neddermeyer showed, mathematically, that the compression of a solid sphere of plutonium or uranium by the detonation of an encasing layer of a high explosive was feasible and that spherical implosion assembly would be superior to the gun assembly method of the Mark I design because of the higher velocity and shorter path of assembly achieved by implosion.

On May 17, 1943 James Conant wrote to George Kistiakowsky, head of the Bruceton, Pennsylvania, Explosive Research Laboratory (ERL), operated on the grounds of the U.S. Bureau of Mines Experiment Station by the National Defense Research Committee Division 8 (Ralph Connor, chief): "This is to authorize the visit of S. Neddermeyer and Edward McMillan to Bruceton, and authorize you to show them whatever they may desire to see."

During that visit the first experimental implosion of a cylinder was conducted. During a 17 April 1985 interview with McMillan conducted by Robert Seidel, McMillan recalled that visit had produced "some experiments with cylindrical implosions . . . (using an iron) pipe and making some explosives in a shell around it." Page 88 of the DOE Los Alamos history *Critical Assembly* further reports of that visit: "Ignition of the explosives wrapped around the pipe 'at a few points' set up a convergent wave and one could see clearly that 'the pipe had closed in.' These experiments demonstrated that one could actually 'drive matter in.' "

The Mark II autocatalytic uranium hydride lateral implosion experimental device was in fact an imploded cylindrical design, rather than the imploded spherical design of the Mark III and Mark IV. "Lateral" means "of, relating to, or situated at or on the side or sides." (*The*

American Heritage Dictionary of the English Language. New York: American Heritage Publishing Co., Inc., 1971.) Lateral implosion of the Mark II defines the implosion of a cylinder by means of an explosive charge wrapped around the length of the cylindrical bomb, and with the ends of the cylinder capped so that an implosive force would be exerted with some uniformity laterally upon the cylinder wall. The uranium hydride active filled the interior of the thick-walled mild steel cylinder, which was in my estimation 3 inches of interior diameter and 2 feet long.

Multi-point detonation of the explosive which encased the cylinder crushed (imploded) the steel cylinder upon the active material along the length of the cylinder and thereby was assembled the active material of the Mark II into a highly compressed supercritical mass. The dense mass of the highly compressed cylinder walls and cylinder end caps confined the active material for the brief fraction of a second necessary for the initiation of a nuclear fission chain reaction by means of a neutron source placed within the active, and thenceforth propagation of an explosive fission chain reaction.

The high explosive which encased the Mark II cylinder was itself confined by a casing of depleted uranium or lead beneath an outer cylinder of tensile steel, which collectively acted as a tamper. In total the Mark II weighed approximately 1,120 pounds (510 kg). Navy Capt. William S. Parsons said the process of imploding a cylinder capable of momentarily containing an evolving fission chain reaction would be "like trying to squash a full can of beer without ejecting any of the beer." The Mark II was that theoretical can of beer. No illustration of the actual construction of the Mark II is available in the declassified literature. The Mark II was essentially a nuclear fission pipe bomb.

The text of *Critical Assembly* on pages 88-90 gives a good summary of the development of cylindrical implosion technology at Los Alamos:

> "The early Los Alamos implosion research was remarkably crude. It was carried out in an arroyo on South Mesa. The first test, using tamped TNT surrounding hollow steel cylinders, was made on the Fourth of July (!) 1943, with [Capt. William S.] Parsons attending. The team centered a piece of steel pipe in a larger piece of stove

pipe, and after packing granular TNT into the annular space between the pipes, detonated the implosion using Primacord. Other versions of the experiment used powdered TNT and plastic explosive to squash mild steel pipes into solid bars. Using the 'Edison approach' [the "trial and error" method of experimentation], Neddermeyer's group repeated this basic experiment many times, varying all the parameters—the explosive arrangement, size of the pipes, and nature of the explosives. The experimental data to be analyzed consisted of a motley collection of bashed-in pipes. These data were subjected to a primitive version of the analysis, which in the program would later be referred to as 'terminal observations.' The method centered on studying the remains of imploded material after the test shots.

"Summarizing the implosion experiments done in July and August [1943], Neddermeyer wrote in one of the earliest technical Los Alamos reports:

"[In tests] 'which were of necessity done with meager equipment, the aim has been first to observe the main features of the phenomena when metal shells undergo extreme and rapid plastic flow under external pressure, and to make an empirical determination of the relation between collapse ratio and mass ratio. These experiments are being followed by observations of the velocities and times of collapse, for which several direct methods have been devised.'

"To cast the needed high explosive for these experiments, E-Division erected a small casting plant at Anchor Ranch."

Data from early implosion tests. Source: Seth Neddermeyer, "LA-18, Collapse of Hollow Steel Cylinders," August 9, 1943.

David Hawkins' *Manhattan District History: Project Y, the Los Alamos Project* (Volume I) reports in paragraph 7.53 that "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."

A meeting of Los Alamos Laboratories Governing Board on 28 October 1943 made the decision to emphasize the implosion assembly program. As reported by David Hawkins, at the end of October 1943 ordnance and engineering work at Los Alamos "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 . . . 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 . . . 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. [Percy] 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 . . . 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 . . . 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."

Mark II, the first nuclear fission bomb

The Military Policy Committee report of 21 August 1943

Four months after Cyril Smith began his April 1943 work at Los Alamos on uranium hydride metallurgy, and at which time Los Alamos learned of the possibility of large scale controlled uranium hydride production, and six weeks after the first cylinders were imploded at Los Alamos, the Atomic Bomb Military Policy Committee—formally the Military Policy Committee on Atomic Fission Bombs (appointed 23 September 1942)—in their "Report of August 21, 1943 on Present Status and Future Program on Atomic Fission Bombs" informed Vice President Henry Wallace, Secretary of War Henry Stimson and Chief

of Staff Gen. Marshall, "There is a chance, and a fair one if a process involving the use of a hydride form of material proves feasible, that the first bomb can be produced in the fall of 1944." Members of the Military Policy Committee were Vannevar Bush, James Conant as Bush's alternate on the committee, Rear Admiral William R. Purnell, USN and General Wilhelm D. Styer, USA.

On 21 August 1943 the Military Policy Committee forecast a fair chance the first atomic bomb, which would employ a "hydride form of material," could be produced in the fall of 1944. Necessarily that hydride material was uranium hydride. An undated manuscript note from James Conant to National Defense Research Committee Vice Chairman Richard C. Tolman, which contextually can be dated to the same period or earlier than the Military Policy Committee's report of 21 August 1943 reads: "For your information and return. I guess I am satisfied that the '23' [U^{233}, see note] project should be of second order presently though if they get stuck on the decontamination of '49' [plutonium], '23' might well prove a better bet. Furthermore if the 25 [U^{235}] hydride looks iffy '23' will prove more attractive than at present since it seems pretty certain '49' doesn't form a hydride."

[Note: "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}. U^{233} and plutonium do not occur naturally but can be formed in nuclear reactors and extracted from the highly radioactive spent fuel by chemical separation. U^{233} also can be produced in special reactors that use thorium as fuel. Only small quantities of U^{233} are reported to have ever been produced in the United States.]

The Military Policy Committee report of 21 August 1943 to Vice President Wallace, Secretary of War Stimson and Chief of Staff General Marshal forecast the "fair chance" that the first atomic bomb, a uranium hydride bomb, could be produced "in the fall of 1944." That forecast was pin-point accurate. The fall of 1944 is understood in the Northern Hemisphere to be the autumn of the year, from the autumnal equinox on about 22 September to the winter solstice on about 22 December. James Conant informed Gen. Groves in the memorandum "Report to Gen. Groves on Visit to Los Alamos on August 17, 1944"

that the Mark II could be developed for combat use in three or four months time, which places that development of the first atomic bomb, as a combat weapon, between 17 November and 17 December 1944 and exactly in the fall of 1944.

The Manhattan Project historical literature universally reports that a dire uncertainty of the Project's success ceaselessly harried the endeavor, but on 21 August 1943 the Military Policy Committee exactly predicted that the first atomic bomb "can be produced in the fall of 1944." The Mark II, however, was a tactical nuclear fission weapon of 1,000 tons TNT equivalent energy yield and, therefore, the Mark II was not the militarily-decisive strategic weapon of energy yield equal to or greater than 10,000 tons TNT equivalent that the Project was mandated to produce for use during the war. Consequently, on 17 August 1944 following the successful test of the Mark II on 17 July 1944, which had demonstrated the feasibility of large scale nuclear fission weapons, the Mark II was "put on the shelf" and work at Los Alamos on the more powerful militarily-decisive weapons that would be detonated in combat one year later at Hiroshima and Nagasaki proceeded with greater confidence.

The Quebec Agreement, August 14-24, 1943

Of very great historical interest is the fact that the 21 August 1943 report of the Military Policy Committee, which forecast that the first atomic bomb could be available in the fall of 1944, is dated two days after President Roosevelt and Prime Minister Churchill signed the Quebec Agreement at Quebec City, Canada, on 19 August 1943—that Military Policy Committee report is, in fact, dated during the proceedings of the 14-24 August 1943 Quebec Conference.

The Quebec Agreement of 19 August 1943 ("Articles of Agreement Governing Collaboration Between the Authorities of the U.S.A. and the U.K. in the Matter of Tube Alloys") established that cooperation between Britain and the U.S.A. in the development of atomic bombs was imperative: "Whereas it is vital to our common safety in the present war to bring the TUBE ALLOYS project [i.e. the atomic bomb project] to fruition at the earliest moment; and whereas this may be

more speedily achieved if all available British and American brains and resources are pooled. . . .”

The fifth provision of the Quebec Agreement established the British and U.S. Combined Policy Committee. Among the functions delegated to the committee by Prime Minister Churchill and President Roosevelt were the activities necessary “to keep all sections of the project under constant review” and to maintain “complete interchange of information and ideas on all sections of the project between members of the Policy Committee and their immediate technical advisers.” The Quebec Agreement named the following persons to the Combined Policy Committee, and each of those men named to the committee was present at the conference:

- The Secretary of War, (Henry Stimson, United States)
- Dr. Vannevar Bush. (United States)
- Dr. James B. Conant. (United States)
- Field-Marshal Sir John Dill, G.C.B., C.M.G., D.S.O. (UK)
- Colonel the Right Hon. J. J. Llewellin, C.B.E.1 M-0., M.P. (UK)
- The Honorable C. D. Howe. (Canada)

The Military Policy Committee’s 21 August 1943 complex and fulsome SECRET report of more than 20 pages, which forecast that the first atomic bomb would be produced in the fall of 1944, is dated two days after 19 August, the date the Quebec Agreement was signed.

Certainly that report had been finalized and approved by the full Military Policy Committee before the Quebec Conference convened on 14 August. Military Policy Committee Chairman Vannevar Bush, even with James Conant’s assistance, could not possibly have finalized that comprehensive report while the Quebec Conference was in progress. Moreover, that 21 August report had been approved by the full Military Policy Committee, but committee members Adm. Purnell and Gen. Styer were not present at the Quebec Conference. Admiral Purnell and Gen. Styer had approved that finalized 21 August Military Policy Committee report before the Quebec Conference convened.

The report had been finalized before the Quebec Conference and therefore necessarily postdated to 21 August in anticipation that the Quebec Conference by that date would approve the proposed terms of the Quebec Agreement, that the proposed Combined Policy Committee would be established by the terms of that Agreement, and that during the Quebec Conference, on or after 21 August, the British and U.S. members of the Combined Policy Committee present at Quebec would meet, and the information provided by the Military Policy Committee report dated 21 August would be then disclosed to the British. Probably before the Quebec Conference ended on 24 August 1943 the British members of the Combined Policy Committee learned there was a fair chance, if a process involving uranium hydride proved feasible, that the first atomic bomb could be produced in the fall of 1944. That bomb would be the Mark II, and the British physicist James Chadwick, at the beginning of August 1944, would be among the first persons to review the reports and analyses of the 17 July 1944 Port Chicago explosion that were prepared by Los Alamos.

Photographs and illustrations credits.

"The boron bubble scheme." Source: Serber, Robert. "LA-1, The Los Alamos Primer," April 1943; Section 21, "Autocatalytic Methods," page 23.

"Data from one of Seth Neddermeyer's earliest implosion tests. The center ring is an untested cross section of the carbon steel tubing used in the first implosion experiments at Los Alamos." Source: Neddermeyer, Seth Neddermeyer. "LA-18, Collapse of Hollow Steel Cylinders," August 9, 1943.