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Introduction to Special Issue on the Early History of Nuclear Fusion

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Abstract — *This introductory paper to the special issue of Fusion Science and Technology commemorates early research on fusion conducted at Los Alamos (the singular entity denoted Los Alamos Laboratory/Los Alamos Scientific Laboratory/Los Alamos National Laboratory at different times is designated "Los Alamos" in this paper) in support of the eventual H-bomb program. We survey the historical origins of the thermonuclear program, what was known of fusion reactions at the outbreak of the war, and the remarkable breakthroughs involving particularly the prospect of deuterium-tritium (DT) reactions conducted during the war, and we summarize the papers in this volume. Much of the nuclear fusion technical history presented herein has not been previously reported. Papers describe aspects of fusion science during these days, on shock hydrodynamics and on electron-radiation coupling, and on nuclear physics including the discoveries of resonances in both the DT cross section and in the lithium tritium-breeding cross section. Three papers follow our colleague Mark Paris's finding Arthur Ruhlig's 1938 paper on the first observation of DT fusion: one on how it influenced subsequent Manhattan Project research, another on a modern calculation of that historic experiment, and a third that has repeated the experiment using modern experimental capabilities. Other papers discuss how the first H-bomb test, Ivy Mike, led to the discovery of the new elements einsteinium and fermium and how the DT fusion processes played a key role in our universe's development after the Big Bang. We also present a paper that analyzes the pioneering Cambridge University 1934 experiment by Marcus Oliphant, Paul Harteck, and Ernest Rutherford where deuterium-deuterium fusion was first observed and that describes how Ernest Lawrence missed identifying fusion in 1933. Finally, we present a summary of early concepts for controlled fusion energy that grew out of wartime discussions at Los Alamos. The papers show how J. Robert Oppenheimer played a leading technical role in the early developments of the H-bomb, before his later opposition—our first paper in this issue addresses the U.S. Department of Energy's 2022 vacating of the earlier 1954 decision to revoke his security clearance.*

Keywords — *Nuclear fusion, thermonuclear, Manhattan Project history, DT reaction.*

Note — *Some figures may be in color only in the electronic version.*

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†M. B. Chadwick served as guest editor for this special issue of *Fusion Science and Technology.*

I. INTRODUCTION

This special issue on the technical history of fusion follows our earlier 2021 Los Alamos[a] fission history project, a series of 24 papers published in the American Nuclear Society's (ANS's) *Nuclear Technology* journal.[1] That collection recognized the 75th anniversary of the Manhattan Project's contribution to the end of World War II. The present fusion history work is a scholarly contribution to the forthcoming 75th anniversary of Los Alamos' first technical demonstrations of the feasibility of the H-bomb in experiments conducted in the Pacific in 1951 and 1952. Our timing also coincides with current enthusiasm for the prospect of controlled nuclear fusion energy that has followed recent remarkable accomplishments: sustained 69-MJ fusion energy at Britain's Joint European Torus (JET) in 2024 (the world record, after the earlier pioneering work at Princeton Plasma Physics Laboratory that produced 10.7 MJ with a 50/50 mixture of deuterium/tritium), and ignition at Lawrence Livermore National Laboratory's (Livermore's) inertial confinement fusion (ICF) National Ignition Facility (NIF) in 2022.

In this introduction, we outline some of the main discoveries made by our team of authors, and it is indeed exciting that there remain interesting technical insights into fusion history yet to be uncovered. We describe how serendipitous findings within our Los Alamos National Security Research Center (NSRC) archives led to unexpected discoveries that include Arthur Ruhlig's first-ever (1938) observation of deuterium-tritium (DT) fusion[2] at the University of Michigan (Michigan); the Manhattan Project discovery of the staggeringly large resonance-enhanced DT cross section through what we call the DT "Bretscher resonance"; and the central and enthusiastic role J. Robert Oppenheimer played from 1942–1945 in foundational fusion technology breakthroughs, which is a revelation that might prove surprising given his opposition to H-bomb development in the late 1940s–1950s.[3]

While the dominant focus during the Manhattan Project was the fission bomb, an innovative research and development (R&D) effort on fusion was established within Enrico Fermi's advanced-concepts F-Division under Edward Teller and Egon Bretscher, both group leaders. Much was accomplished during this 1943–1945 period. At this time, the United States was quite worried that Germany could be on the path to developing a fusion bomb as well as a fission bomb.[4]

Section II of this introduction to the special issue provides historical context for the first 1940s U.S. fusion studies. The prospect of harnessing fusion energy proved be a compelling goal that would provide both an unlimited peaceful source of energy as well as a militarily transformative opportunity for the United States to defend its interests, being even more dramatic and disruptive than fission. Yet, fission had to be the first priority.

Section III describes how some of the new insights in this issue came about, notably the recognition of the first observation of DT in 1938 and the realization of the importance of tritium at Oppenheimer's 1942 University of California, Berkeley (Berkeley) conference comprising his "galaxy of luminaries" (Teller, Emil Konopinski, Hans Bethe, and others). It describes how that conference set subsequent R&D directions on fusion during the Manhattan Project and the subsequent years at Los Alamos. Short summaries of the main findings of each of the papers in this issue are also given.

Section IV spotlights Ruhlig and Bretscher, two scientists who have not received adequate recognition for their early pioneering work. Their findings on the advantageous properties of the DT reaction opened up the potential for fusion technologies.

The origin of the word "fusion" to describe light-nucleus exothermic nuclear reactions is itself interesting. It appears to have been introduced at a relatively late date, by Bethe and Louis Ridenour in 1950,[5–7] when writing about the H-bomb after President Harry Truman's announcement that the United States would pursue the bomb, following the Soviet Union's first A-bomb test in 1949. We are unaware of the motivation for introducing this new term, although the elegance of "fusion" complementing "fission" is apparent.

In the very earliest papers that addressed the source of stellar energy, Arthur Eddington in 1920 used descriptors such as "being combined" and "compounded"[8] for the nuclear reactions that we now call "fusion." Later, in 1929, Robert Atkinson and Friedrich Houtermans talked of "transmutation."[9] Marcus Oliphant, Paul Harteck, and Ernest Rutherford's[10] seminal paper on the first laboratory observation of fusion (deuterons on deuterons) did not actually use the word "fusion" but talked of the "union of two diplons to form a new nucleus."[10] George Gamow introduced the word "thermonuclear" in 1937–1938[11] to mean reactions induced by hot ions with a thermalized distribution of kinetic energies. In the 1940s, the Los Alamos scientists used "thermonuclear," though occasionally one finds variants such as "thermal nuclear" (see Arthur Compton's 1942 letter that we reproduce in the Appendix).

The earliest peaceful fusion energy researchwas described, for many decades, as controlled

[a] The singular entity denoted Los Alamos Laboratory/Los Alamos Scientific Laboratory/Los Alamos National Laboratory at different times is designated "Los Alamos" in this paper.

thermonuclear research. In Russia, combining both names together—"thermonuclear fusion" ("termoyadernyy sintez")—remains the preferred nomenclature.[12] Today in the United States, "thermonuclear" is often reserved for a subset of fusion processes: those where the hot fusing ions are within an equilibrated plasma with a thermal distribution of energies characterized by a temperature (as opposed to the fusing of accelerated ions).

II. CONTEXT AND CHALLENGES

The genesis of the American fusion bomb program is usually traced to a conversation between Fermi (Fig. 1) and Teller (Fig. 2) in September or October 1941 at Columbia University, with Fermi proposing that a fission bomb could create conditions of temperature and pressure extreme enough to initiate fusion (Ref. [14], p. 157). We note, however, that a 1942 letter from

Fig. 1. Fermi with Lawrence and Isidor Rabi, Los Alamos, October 1946.

Fig. 2. Teller with Norris Bradbury, at a party in Fuller Lodge, Los Alamos, September 1946. Teller was the "father of the H-bomb," while Bradbury was the Los Alamos director who brought it to fruition in the Ivy Mike test in 1952.[13]

Compton to James Conant that we reproduce in the Appendix discusses an earlier date, 1939, for Fermi's first discussions on this. However this originated (we will stick here to the 1941 date related in Teller's memoirs), Teller was apparently initially skeptical of the idea but of course later came to be consumed by it. While the papers in this issue are testament to the vigorous level of research on fusion carried out during and after the Manhattan Project, the subsequent history of events makes it easy to forget just how speculative the idea of a fission bomb was in the fall of 1941, let alone a fusion device.

In a sense, fusion had a head start on fission. As described by Mark Chadwick et al.[4] and John Lestone[15] in this issue, laboratory-based discovery of deuterium-deuterium (DD) fusion went back to 1934 with Rutherford and his collaborators,[10] and research on this reaction continued into the war years. Also, in the laboratory, Ruhlig's 1938 speculation[2] that the DT reaction could be exceedingly probable would have profound consequences. We record these reactions here with modern Q-values for the sake of completeness. There are two versions of the DD reaction and one DT channel:

$$ {}_1^2\mathrm{H} + {}_1^2\mathrm{H} \rightarrow \mathrm{n} + {}_2^3\mathrm{He} + 3.269\ \mathrm{MeV} \quad (\mathrm{DDn}) $$

$$ {}_1^2\mathrm{H} + {}_1^2\mathrm{H} \rightarrow \mathrm{p} + {}_1^3\mathrm{H} + 4.033\ \mathrm{MeV} \quad (\mathrm{DDp}) $$

$$ {}_1^3\mathrm{H} + {}_1^2\mathrm{H} \rightarrow \mathrm{n} + {}_2^4\mathrm{He} + 17.589\ \mathrm{MeV} \quad (\mathrm{DT\ or\ TD}) \quad . $$

Before these discoveries, understanding of fusion as the energy source of stars had been put on firm foundations by Atkinson, Houtermans, and particularly Bethe (Fig. 3). All of this work predated the discovery of fission, but developing a fusion bomb would require achieving fission bombs first, a matter that, despite Teller's optimism, was a far from certain prospect and would ultimately prove incredibly difficult and expensive. In this section, we review the status of the fission program at the time of Fermi and Teller's fateful conversation and survey the enormous amount of work that yet lay ahead.

The fissile nature of ^{235}U and ^{239}Pu had been verified by the spring of 1941, but only microscopic amounts of either had been isolated. The pivotal third report of Compton's National Academy of Sciences Committee on Atomic Fission, which had been motivated by the British MAUD report and which detailed the prospects (in theory, at least) for fission bombs of "superlatively destructive power," would not be ready until early November, and Vannevar Bush would not forward it to President Franklin

Fig. 3. Bethe, Los Alamos, August 1947.

Roosevelt until November 27, about 10 days before Pearl Harbor. With Roosevelt's OK of January 1942, Bush began reorganizing the project and moving it into high gear. The formal establishment of the Manhattan Engineer District would not take place until August 1942, 3 years after the famous Einstein-Szilard letter to Roosevelt was drafted and nearly a year after Fermi and Teller spoke.

The most pressing immediate requirement for the fission program was to devise ways of sourcing kilograms of fissile material, orders of magnitude more than the micrograms then available. The then most advanced technique for doing so, Ernest Lawrence's (Fig. 1) electromagnetic separators (later, calutrons) for enriching uranium were just being developed but were plagued with teething problems; by February 1942, he could boast of having secured three 75-μg samples enriched to 30% ^{235}U.[16] Experimental calutrons at Oak Ridge would not go into operation until the spring of 1943 and production operations not until November of that year, 2 years after the Fermi-Teller conversation. Problems with developing suitable diffusion membranes for the K-25 gaseous enrichment plant would persist into 1944; that facility would not begin even partial operation until early 1945, and General Leslie Groves would not let a contract for the complementary S-50 liquid diffusion plant until just after D-Day.

As to synthesizing plutonium, pile theory was well advanced thanks to the efforts of Fermi and Leo Szilard, but on the experimental side, Fermi had as yet (1941) constructed only a few trial piles for measuring the neutron-capture properties of graphite. Demonstrating a chain reaction with CP-1 lay over a year in the future, synthesizing gram quantities with the X-10 pile at Oak Ridge a year beyond that, and the startup of the first Hanford pile the better part of a year beyond X-10. This history could have been utterly different: The entire pile program had almost been abandoned but was saved by the intervention of Compton at a crucial meeting of project principals held the day before Pearl Harbor. Ultimately, plutonium would prove to be the more efficient nuclear explosive.

No mention of the Manhattan Project's plutonium production reactors, particularly those at Hanford, would be complete without addressing the seminal role played by Eugene Wigner[17] (Fig. 4). Wigner's renown as a theoretical physicist has tended to overshadow his accomplishments as a nuclear engineer, which were extensive. Formally trained as a chemical engineer, his

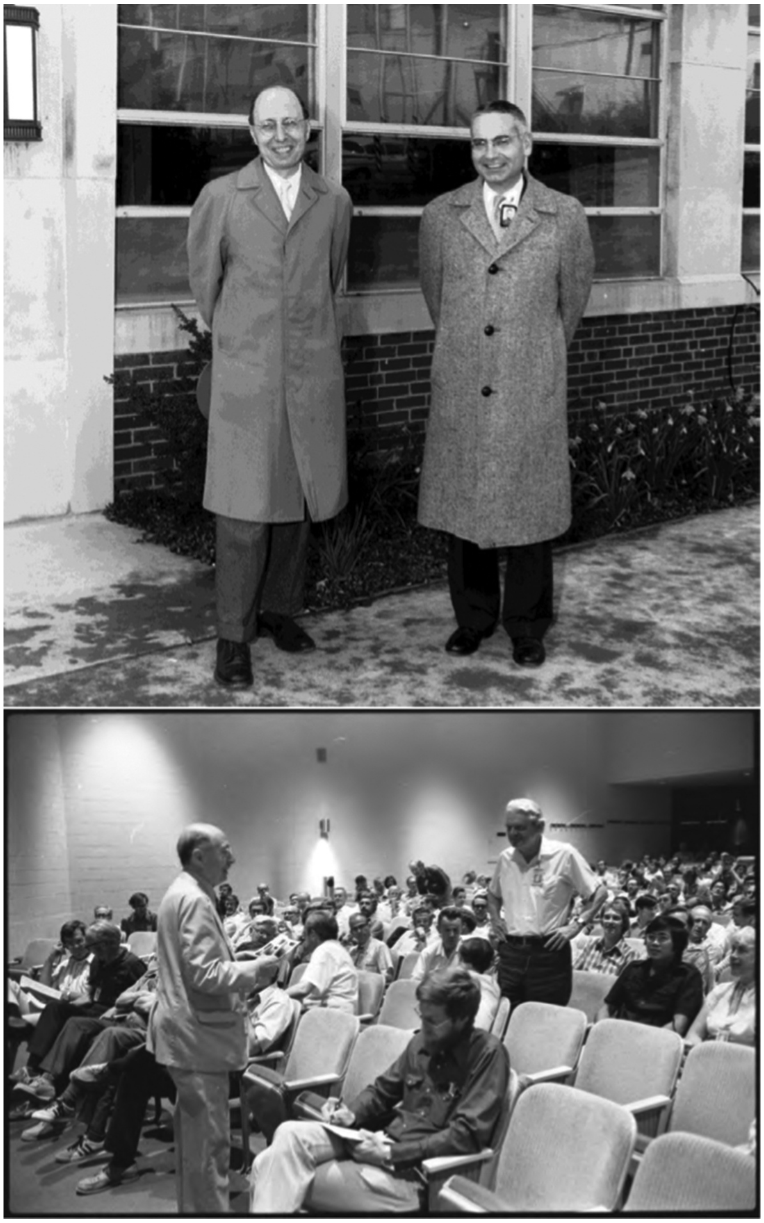


Fig. 4. Upper figure: Wigner and Alvin Weinberg at Oak Ridge, celebrating the 20th anniversary of first criticality of the graphite reactor, 1963. Lower figure: Wigner at a colloquium in Los Alamos, June 1976. The other standing man is Charles Critchfield, also seen in Fig. 6 when he was 30 years younger.

abilities in that area in combination with his strength as a mathematician made him uniquely qualified to pioneer this new discipline. At the time of Lyman Briggs' Uranium Committee in late 1939, Wigner had already begun working on the theory of chain reactions. Present at the startup of Fermi's CP-1 pile in late 1942, he was assigned by Compton to lead the design of a 500-MW pile that would produce 500 g of plutonium per day. This design would evolve into DuPont's three 250-MW Hanford piles, for which he reviewed every blueprint. Ultimately, Wigner would personally accrue 37 patents for reactors of various designs. Toward the end of the war, he drew up plans for what would become the Oak Ridge National Laboratory, where he would become closely involved in the design of the enriched-uranium Materials Testing Reactor and also develop plans for a molten-sodium–cooled fast-neutron breeder reactor. Much of what today's nuclear practitioners do descends from Wigner's groundbreaking work. Regarding the physics of light-nucleus fusion reactions, Wigner and Leonard Eisenbud's R-matrix theory[18] is still the basis for modern analyses, as described in our paper in this issue.[4]

Turning fissile material into bombs was the job of Los Alamos, which would not open its doors for some 18 months after Fermi and Teller speculated on fusion weapons. The collection of papers in the 2021 *Nuclear Technology* volume[1,19] speaks to the enormous amount of work done there in nuclear experimentation, theory, chemistry, metallurgy, electronics, criticality experiments, computation, and ordnance engineering. The ^{240}Pu spontaneous fission and Hanford xenon-poisoning crises were not entirely unanticipated, but their severities came as rude surprises that nearly scuttled the plutonium program. The entire enterprise also depended on less well-known but equally crucial contributions such as the Ames project to perfect processes for producing pure uranium metal by the ton and Monsanto's Dayton laboratory, where the polonium used in the bombs' neutron initiators was isolated from slugs of bismuth that had been irradiated in Hanford piles.

Finally, designing, making, and testing bombs is one thing; there is then the practical issue of getting the multi-ton devices to their targets in combat conditions. This would end up requiring its own dedicated Army Air Force unit equipped with long-range B-29 bombers, manned by men who had undergone months of specialized, highly secret training even as designs for the bomb casings evolved; securing reproducible ballistics and reliable fusing mechanisms would prove to be their own challenges.

When Fermi and Teller spoke, all of the above lay in the future, but a period of intense optimism would reign for awhile through the summer of 1942. On May 19 of that year, Oppenheimer wrote to Lawrence that "with three experienced men and perhaps an equal number of younger ones" it should be possible to solve the theoretical problems involved with a fission bomb (Ref. [20], p. 42)! As to fusion, in a July 25, 1942, letter to John Manley at the University of Chicago (Chicago) (soon after Konopinski raised the importance of DT with him; see Sec. III), Oppenheimer wrote the following: (Ref. [20], p. 47[4]):

> There is one other experiment which we have just got to get done. It is a lulu!
>
> We want to know the cross section for the reaction of T with D and the two lithium isotopes. We have about a microgram of this material in 10g of heavy water and Lawrence suggested that we could get rid of the DH beam using the longer range of the T energy of the cyclotron.
>
> Now I have a complicated proposal which comes from Bethe, although he would not like Gibbs at Cornell to know that. It is this. There is a good cyclotron at Harvard where the beam can be brought out. At Cornell there are two men, Baker and Holloway, who have had a lot experience with the tricky detection work. Our recommendation is that these men and others to assist them, perhaps Livingood and Livingston, be sent to Harvard to operate the cyclotron there, which can carry out, among other things, these experiments with T. I want to add a word of caution about this experiment. If our ordinary conversations are secret, this should be secret squared; that is, we should like to have knowledge of our interest in this subject restricted to the absolute minimum and if possible not to have it appear to having connection with our tubealloy project.…also, we should like to know the cross section for the reaction 6Li(n,T) for neutrons in the range of 100 or so kilovolts.

So, already by 1942, Oppenheimer was commissioning measurements for the key DT and lithium reactions. The DT reaction was measured during the Manhattan Project, first in 1943 at Purdue University (Purdue) (not Harvard University) and then in 1945–1946 at Los Alamos,[4] and the lithium reactions were also measured at Los Alamos, from 1944 on.[21]

In the following section, we discuss a September 1942 letter from Compton to Conant that indicates that the Project's principals clearly understood how a fusion bomb could be vastly more powerful than a fission bomb (see Fig. A.1 in the Appendix).

As work at Los Alamos got underway, the initial optimism gave way to the realization that a fission bomb would demand an immense program of experimental and theoretical work, with success not at all guaranteed. Fusion would have to take lower priority although remain an area of active research. On October 20, 1944, Conant wrote to Bush that a "real super bomb is probably at least as distant now as was the fission bomb when you and I first heard of the enterprise…."[22]

To close, we can do no better than to quote Bethe:

> After the Los Alamos Laboratory was started in the Spring of 1943, it became clear that the development of a fission bomb was far more difficult than had been anticipated. If our work was to make any contribution to victory in World War II, it was essential that the whole Laboratory agree on one or a very few major lines of development and that all else be considered of low priority.[23]

III. HIGHLIGHTS FROM PAPERS IN SPECIAL ISSUE

The DT fusion reaction is central to enabling successful fusion technologies; its cross section is roughly 100 times that for DD fusion.[4] Teller attributed the realization of the potential value of DT to his younger colleague Konopinski (Fig. 5), who raised the usefulness of tritium at a 1942 "luminaries" meeting in Berkeley convened by Oppenheimer (see Ref. [4], in this issue). What prompted Konopinski's key insight? Later, in 1955, Teller wrote[24] that it was a "mere guess" but an "inspired one," which is an observation that we now know to be wrong. What we discovered[4] was that Konopinski knew of earlier *experimental* work from 1938[2] that has since been forgotten. We show[4] that in 1942 there was really no theoretical basis to predict a large DT cross section, for one would expect DT to be similar in magnitude to that for DD; the discovery of a resonance in DT fusion came later, during the Manhattan Project.[4]

Our NSRC archives contain numerous oral history taped interviews in addition to very many historical technical documents. I (MBC) listened through a 1986 interview with Konopinski[25] about his work at Los Alamos in the laboratory's early days. Toward the end of that interview, he was asked about his clever Berkeley 1942 DT insight. He answered that he was aware of "pre-war work" that pointed to the value of DT; Konopinski's own words can be heard in a YouTube recording we have made available,[25] transcribed in Ref. [4]. At first, I thought this must surely be a mistake, a memory slip, because we know that the DT cross section was first measured *later*, in 1943, during the Manhattan Project, after a heroic effort at Berkeley's cyclotron to make enough tritium for this pioneering measurement. How could Konopinski have known of a favorable DT cross section before it was measured?

Enter Mark Paris, Los Alamos Theoretical Division fusion theory expert and coauthor of many papers in this issue. Mark took Konopinski's words seriously and

Fig. 5. Konopinski (second from right), with William Penney (far left, later head of the British Atomic Weapon Establishment laboratory), and Beatrice and Lawrence Langer, in Los Alamos. By today's standards they are well dressed for their road trip.

initiated a literature search to find Konopinski's source. On various occasions he would be providing me with an update on his efforts, and I would tell him that he was wasting his time; Konopinski must have misremembered! Mark is persistent, though, and kept looking. By following the trail of various late-1930s papers in *Physical Review*, Mark came across a short but quite remarkable 1938 paper by Ruhlig[2] describing DD accelerator measurements (that is, deuteron projectiles on a deuterated target) at Michigan in which Ruhlig also observed DT secondary fusion events that must be "exceedingly probable." Reference [4] in this issue describes the evidence that Konopinski and Bethe knew of Ruhlig's experiment, including Bethe being thanked by Ruhlig in the paper and Konopinski and Ruhlig being contemporaries at Michigan. Ruhlig's paper has been hiding in plain sight for 85 years, its first-ever DT observation unappreciated and uncited until we noted it in articles in ANS's *Nuclear News*[26,27] in 2023 and in the *Physics Today* magazine.[28]

Thus, our understanding of the first human observation of the DT fusion process has been pushed back 5 years from the 1943 Manhattan Project measurements at Purdue by Charles Baker, Marshall Holloway, L. D. Percival King, and Raemer Schreiber[29], [b] to 1938 at Michigan. Although tritium was not available in 1938 for a careful cross-section measurement, DD experiments produce secondary tritons, and some in-flight secondary TD 14-MeV neutron signatures that Ruhlig correctly identified. (The 14 MeV quoted here differs from the Q-value in the DT reaction listed in Sec. II in that the latter is the total energy release; 14 MeV goes into the kinetic energy of the neutron.)

Often, one discovery leads to other exciting lines of inquiry. It will not be surprising for readers to hear that Los Alamos has a depth of expertise in modeling fusion reactions in our radiation transport simulation codes. The secondary in-flight reactions observed by Ruhlig have been studied for decades in other contexts, including in ICF applications. Therefore, I (MBC) asked my colleague Lestone whether he could simulate Ruhlig's 1938 experiment with our codes to see if we could quantitatively understand his results. That study is reported in Ref. [30] in this issue, and there we note that an independent calculation by our Livermore colleague George Zimmerman agrees fairly well with our LANL calculation; differences are attributed to ambiguities that still exist in the ion stopping powers. We also thought it important to repeat Ruhlig's experiment, and another paper in this issue[31] describes using the Triangle University Nuclear Laboratory (Duke University) accelerator to create deuteron reactions on deuterated targets, measure the number of in-flight secondary DT fusion reactions, and compare the results to Ruhlig's 1938 findings and to our calculated predictions.

III.A. Oppenheimer's Role, 1942–1945

Oppenheimer (Fig. 6) played an important role in the development of the early H-bomb concepts. This is discussed in Ref. [4], in this issue. He sent a summary of the aforementioned 1942 Berkeley meeting, "Memorandum on Nuclear Reactions,"[32], [c] to presumably Conant, Bush, and others. That memorandum (which remains classified) provides a surprisingly optimistic assessment of the feasibility of developing an H-bomb. It caused waves in Washington, and in Chicago, where Compton was coordinating work on the bomb project. Figure A.1 in the Appendix reproduces a letter from Compton to Conant that was written just a few weeks after these Berkeley breakthroughs.

This letter is interesting in a number of ways. Compton refers to the H-bomb ideas as "Oppenheimer's new result," which surely does not give adequate credit to Teller, Konopinski, Bethe, and Fermi, though it does reflect the fact that the memorandum was authored solely by Oppenheimer. The letter reflects with awe on the potential power of the bomb, likely a result of Oppenheimer's consultation with Compton on such an explosion possibly igniting a fusion chain reaction in the atmosphere.[3] This idea turned out to be a red herring.[33], [d] Also, Compton communicates his desire to be able to consult leading scientists that he trusted—Fermi, Wigner, and Samuel Allison—whom he thought more "practical" than the theorists who gathered in Berkeley! His letter makes the case that foreign-born scientists should be allowed to see the new results and provide advice. The request was initially denied, but

[b] Reference 29: Los Alamos Technical Report LAMS-11, Contract W-7405-ENG-146 between Manhattan District and the Purdue Research Foundation.

[c] To whom this memorandum was sent is not known, but we think it would have been sent to leaders such as Compton, Bush, Conant, and Groves.

[d] This consultation is depicted in the movie *Oppenheimer*, written, directed, and produced by Christopher Nolan; however, Nolan's depiction is a case of poetic license, using Einstein instead of Compton, which Nolan stated was done for dramatic effect.

Fig. 6. Oppenheimer at parties in Los Alamos. Upper figure: with Eric Jette and Critchfield, 1946. Lower figure: with Jette and others, August 1946.

Bush and Conant subsequently acquiesced, to the lasting benefit of the United States.

Oppenheimer was also a coauthor (with Teller, Konopinski, and Bethe) of the first patent on the H-bomb, written between 1944 and 1946.[34] That patent specifically cites Oppenheimer's 1942 "Memorandum on Nuclear Reactions"[32] as the earliest document on thermonuclear bomb concepts. While Oppenheimer did oppose the crash research effort on the H-bomb that started in Los Alamos in the late 1940s,[35] it should be understood that he had previously played a leading role in advancing the earliest concepts.

To close this section, we give brief summaries of the many results of the contributed papers in this special issue. Any reader interested in this history will surely find many of them to be of value.

III.A.1. T. E. Mason, "Thoughts on the H-Bomb Decision, Oppenheimer's Loyalty/Security Hearing, and Vacating of the AEC Decision"[35]

Thomas Mason, the current director of Los Alamos, provides some context for Oppenheimer's 1949 recommendation against the development of the

H-bomb and the subsequent 1954 decision not to renew his clearance. An April 2022 letter from nine Los Alamos directors to U.S. Secretary of Energy Jennifer Granholm is reproduced, which contributed to her December 2022 directive to vacate the original clearance decision.

III.A.2. M. B. Chadwick, M. W. Paris, G. M. Hale, J. P. Lestone, S. Alhumaidi, J. B. Wilhelmy, and N. A. Gibson, "Early Nuclear Fusion Cross-Section Advances 1934–1952 and Comparison to Today's ENDF Data"[4]

This comprehensive paper reviews the advancing understanding of nuclear fusion cross sections from the 1930s through the first DT measurements during the Manhattan Project war years, to the precise measurements made at Los Alamos in 1952. The evolving understanding of the $3/2^+$ Bretscher-state resonant enhancement of the DT cross section is discussed, from its first 1945 identification and characterization to modern theoretical insights. The paper also provides context around the U.S. race against Germany and then the USSR to develop thermonuclear devices. It lays out evidence that Ruhlig's strangely neglected 1938 first observation of DT at Michigan inspired Konopinski in 1942 to inform Oppenheimer, Teller, and Bethe about the potential importance of tritium and thus helped launch the first fusion and H-bomb R&D at Los Alamos. The next two papers further study Ruhlig's 1938 pioneering work.

III.A.3. J. P. Lestone, C. R. Bates, M. B. Chadwick, and M. W. Paris, "Ruhlig's 1938 First-Ever Observation of the Fusion of A = 3 Ions with Deuterium: An Analysis of Secondary Reactions Following dd Fusion in a Heavy Phosphoric Target"[30]

A theoretical analysis of Ruhlig's 1938 experiment is provided in an effort to understand his reported measurement of in-flight secondary DT fusion. In one phase of the 1938 experiment, it is concluded that Ruhlig did indeed detect DT fusion neutrons. However, regarding a separate phase where Ruhlig reported a quantitative result, Lestone et al. conclude that the the true number of DT fusions occurring should be smaller than was reported, or alternatively that Ruhlig was unwittingly observing in-flight secondary D-^{3}He fusion reactions, or tritium had built up in his target during the experiment or in previous experiments (assessed, though, to be very unlikely). A lack of reported details regarding the original experimental setup hampers a definitive conclusion.

III.A.4. J. P. Lestone, S. W. Finch, F. Friesen, E. Mancil, W. Tornow, J. Wilhelmy, and M. B. Chadwick, "Observation of d(t,n)α Neutrons Following d(d,p)t Reactions in a Deuterium Gas Cell: An Attempt to Repeat Ruhlig's 1938 Observation of Secondary Reactions"[31]

We commissioned a series of experiments to repeat Ruhlig's seminal experiment, at the Triangle University Nuclear Laboratory (TUNL) Van de Graaff accelerator at Duke University. The original cloud chamber detection method was replaced by more modern methods: neutron time-of-flight detection with an organic scintillator, and use of activation foils. Different deuterated target compounds were used, including deuterated phosphoric acid (like Ruhlig), heavy water, and deuterium gas. These DD experiments did see secondary in-flight DT fusion neutrons, and theoretical predictions are in fair agreement with the TUNL measured data. Also, these experiments provided useful data to validate Lestone's calculational methods that were also used to analyze Ruhlig's original 1938 experiment, described in the previous item.

III.A.5. J. P. Lestone, "Some of the History Surrounding the Oliphant et al. Discovery of dd Fusion and an Inference of the d(d,p)t Cross Section from the 1934 Paper"[15]

The Oliphant, Harteck, and Rutherford 1934 paper[10] was famous for first using the newly invented Cockcroft-Walton accelerator in Cambridge University (Cambridge) to observe DD fusion and to discover the tritium isotope.[36] That paper did not, however, report a DD cross-section value. Lestone analyzes the (thick-target) data that were reported and infers a DDp cross section for deuterons with energies from ~20 to 200 keV, finding values that agree within a factor of 2 compared to our best values today—not bad for a first-ever 1934 study that did not even aim to quantify the cross section!

Lestone's paper also reminds us that Lawrence observed DD fusion 1 year earlier, in 1933,[37,38] but misinterpreted his data, missing an important discovery. We describe some aspects of this episode here because of its importance in the earliest history of nuclear fusion reactions research and because it has largely been forgotten by today's nuclear science community.

Lawrence's contemporaries such as Merle Tuve, Chadwick, Rutherford, and Werner Heisenberg and, later, historians John Heilbron and Robert Seidel were quite critical of Lawrence on this.[39] Lawrence found that when deuterons from his cyclotron impinged on a variety of targets, he always saw neutrons and protons in equal quantities. He jumped to the conclusion that this was from deuteron breakup, whereas in 1934, Oliphant (Cambridge) told him the correct explanation,[39] i.e., that deuterons from the beam had built up as contamination in his target and that he was observing the two branches (DDn and DDp) of deuteron-deuteron reactions, the very fusion reactions that the Cambridge group discovered in 1934.

To account for the high proton and neutron energies that he observed, and assuming the mechanism to be a deuteron breakup reaction, Lawrence proposed that the deuteron "exploded" in the Coulomb field of the target nucleus, a concept dismissed by Rutherford for being an "exothermal nucleus." Instead of having the negative Q-value of −2.2 MeV that we know today, Lawrence had to hypothesize a neutron mass that was very different from Chadwick's recently measured value, so as to produce a *positive* Q-value of 4.8 MeV; see Table I. Lawrence wrote the following[38]: "All of these observations corroborate the view that the deuton disintegrates with the release of about 4.8 MV of energy and consequently that neutrons produced in the process have a mass of about 1.0006 mass units."

While such an exothermic mechanism in retrospect seems fanciful, its radical novelty is not so very different from that of the remarkable phenomenon of nuclear fission discovered 5 years later, with its large energy production following neutron bombardment on uranium.

Five years later, when Ruhlig performed deuteron-on-target experiments and observed some unexpected high-energy ejectile particles (>14-MeV neutrons), he correctly hypothesized that these were from a secondary TD fusion reaction [after a primary D(d,p)T reaction]; Lawrence could have made a somewhat analogous inference in 1933: that his primary deuterons first built up in the target and then underwent subsequent D(d,p) and D(d,n) fusion reactions making the high-energy proton ejectiles that he observed, but he did not.

Lawrence's 1933 papers and conference presentations led to criticisms from his peers and competitors: for proposing the exploding deuteron, for his haste in proposing a very different neutron mass, and for his lack of care in avoiding beam deuteron contamination. Interestingly, though, no papers seem to have been written in peer-reviewed journals that pointed out Lawrence's miss of the discovery of nuclear fusion. This was a close and competitive community of researchers who maintained certain professional courtesies; Oliphant just used a private letter to tell Lawrence of his mistake.[39] Indeed, when our colleague Lestone read Lawrence's 1933 papers, he realized that Lawrence must have been unwittingly observing DD fusion. (Because of the now well-known nuclear masses, it was clear to Lestone that breakup could not produce the highly energetic protons that Lawrence observed and that fusion was the only explanation). At first he thought that he, Lestone, was the first to realize this as it was not reported elsewhere in the peer-reviewed journal literature. But, on further study, we realized that this whole episode was nicely documented in Heilbron and Seidel's book.[39]

III.A.6. S. A. Becker, "The Serendipitous Discovery of the New Elements Einsteinium and Fermium from the Debris of the Mike Thermonuclear Test"[40]

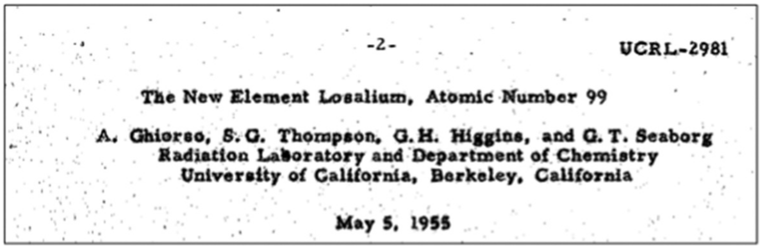
Stephen Becker describes how the first 1952 H-bomb test by Los Alamos unexpectedly discovered two new elements (Z = 99, 100) and 15 new heavy isotopes and makes comparisons to synthesis mechanisms in the astrophysical r-process. In the report UCRL-2981, "The New Element Losalium, Atomic Number 99," May 5, 1955 (see Fig. 7), Berkeley proposed that Z = 99 be named losalium, but because the radiochemical research involved was

TABLE I

Masses for the Neutrons, Protons, and Deuterons, Assumed by Lawrence, Compared to Values "Known" in the 1933 Time Frame When He Did His Experiment and Compared to Modern Values Today*

Particle	1933 Era	Lawrence (1933)	Today
D	2.01363	2.01363	2.01355
n	1.0067	1.0006[a]	1.0087
p	1.0078	1.0078	1.0073
Q(D-n-p)	−0.8 MeV	4.8 MeV	−2.2 MeV

*Neutron, proton, and deuteron masses are in atomic mass units.
[a]Lawrence's inference of the neutron mass was at odds with Chadwick's value from the time and with today's best value.



-2- UCRL-2981

The New Element Losalium, Atomic Number 99

A. Ghiorso, S. G. Thompson, G. H. Higgins, and G. T. Seaborg
Radiation Laboratory and Department of Chemistry
University of California, Berkeley, California

May 5, 1955

Fig. 7. A 1955 Berkeley report UCRL-2981 proposing that the newly discovered Z = 99 element be called losalium.

a collaboration among Berkeley, Argonne National Laboratory (Argonne), and Los Alamos, the name "einsteinium" was chosen instead; for Z = 100, "fermium" was chosen.[41] Classification concerns had held back this Berkeley–Argonne–Los Alamos report until 1955,[41] but in a Letter to the Editor in *Physical Review*,[41] Glenn Seaborg's team described "the results of experiments performed in December, 1952 and the following months … which represent the discovery of the elements with the atomic numbers 99 and 100." Earlier 1954 Berkeley *Physical Review* publications that identified various einsteinium isotopes through laboratory experiments had made it clear that there existed unpublished (nuclear test debris) information that had predated those laboratory experiments.[36]

III.A.7. M. W. Paris and M. B. Chadwick, "Anthropic Importance of the 'Bretscher State' in DT Fusion"[42]

This paper describes how DT fusion in the first 3 min of the life of the Universe created 99% of the primordial ^{4}He produced in the Big Bang, a discovery by the California Institute of Technology (Caltech) group.[43] It emphasizes how the aforementioned Bretscher-state resonant enhancement of the DT cross section causes DT to be the dominant pathway for helium production. When heavier elements like carbon, nitrogen, and oxygen were later synthesized in stars, over one-quarter of them (by mass) were synthesized from these helium products of DT fusion. The anthropic importance of DT fusion to our human existence is discussed.

III.A.8. J. Katz, "The First Calculation of Comptonization"[44]

This is the first of two papers reporting on research during the Manhattan Project that Los Alamos is now making openly available. Jonathan Katz's paper reproduces two of Henry Hurwitz's reports, LA-301 (1945) and LA-553 (1946), on energy exchange between electrons and radiation and provides a commentary. Until now, the broader community had assumed that the first author to publish on this topic was Alexander Kompaneyets in Russia, in 1950. Today, this physics is relevant in ICF applications.

III.A.9. L. G. Margolin and K. L. van Buren, "Richtmyer on Shocks: 'Proposed Numerical Method for Calculation of Shocks,' an Annotation of LA-671"[45]

This paper reproduces an important but previously unavailable 1948 work by Robert Richtmyer and provides a commentary on it. Richtmyer's paper informed his later publication with John von Neumann and includes insights into hydrodynamic flow with shocks, artificial viscosity, and turbulence, all factors needed in hydrodynamics simulation codes. Len Margolin and Kendra Van Buren's work is a follow-on to Nathaniel Morgan and Bill Archer's paper[46] in our Manhattan Project volume[19] that noted the earliest hydrodynamics and artificial viscosity ideas by Rudolf Peierls and von Neumann, which were used in their 1944–1945 calculations. Again, this physics is relevant in ICF applications.

III.A.10. C. R. Bates and M. B. Chadwick, "Lithium Neutron Cross Sections During the Manhattan Project and the Quest for the H-Bomb"[21]

Cross sections for neutrons on lithium in the fast region were first measured during the Manhattan Project, where the important ^{6}Li 240-keV resonance was also discovered, in 1944. Lithium was used in nuclear tests after 1953 and will be used to breed tritium in planned peaceful fusion energy technologies.

III.A.11. K. Schoenberg, "A Historical Perspective of Controlled Thermonuclear Research at Los Alamos (1946–1990)"[47]

This paper describes the earliest ideas in magnetically confined controlled fusion concepts that came out of

Fig. 8. A view of "Tuck's Table" from the Overlook on the front hill road, Los Alamos. Old-timers called the Mesa (left-of middle in image) with three humps Triple Bump Mesa. The flat one on the end is Tuck's Table. Tuck used to lead Newcomers Club hikes there, providing a very scenic view of Black Mesa and the Sangre de Cristo mountains. (Courtesy Len Margolin)

discussions during the Manhattan Project. James Tuck's (Fig. 8) role in the 1950s controlled nuclear fusion Project Sherwood is discussed; his Scylla theta pinch machine was the first machine worldwide to produce a controlled fusion plasma (here, making DD fusion neutrons from a plasma at ~1.5 keV, in 1958[48]).

In the race to harness thermonuclear fusion for peaceful energy applications in the late 1950s, there were some false alerts. Product neutrons were sometimes observed that had come from accelerated fusing ions and not from a true equilibrated thermalized plasma. Early claims from Britain's Zeta pinch machine in 1957 had to be withdrawn; in a 1957 description of contemporaneous Soviet work, Igor Kurchatov stated that "Acceleration of ions and electrons in the longitudinal electrical field near the discharge axis is possibly the explanation of neutrons and penetrating X-rays."[49] It was Tuck's Los Alamos 1958 Scylla I pinch experiments in which "thermonuclear fusion was achieved in any laboratory," based on the measured energy distributions of the neutrons, protons, and tritons.[50] Tuck described how "the machine was exhibited in operation at the Atoms for Peace conference in Geneva, Switzerland with the clear understanding that this was probably thermonuclear," stating that "we are now quite certain beyond a shadow of doubt"…and that "we succeeded at the second attempt in 1958," the first attempt being with his Perhapsatron.[51] Tuck's *Nature* article summarizing the results[52] stated "we have achieved the first controlled thermonuclear reaction" and "the strongest thermonuclear reaction."

By 1964, Tuck was making 1 billion neutrons per shot[51] at Los Alamos (millijoules of energy—and he later quoted one hundred times this amount[52]), which can be compared to ~5 MJ from the NIF and the latest

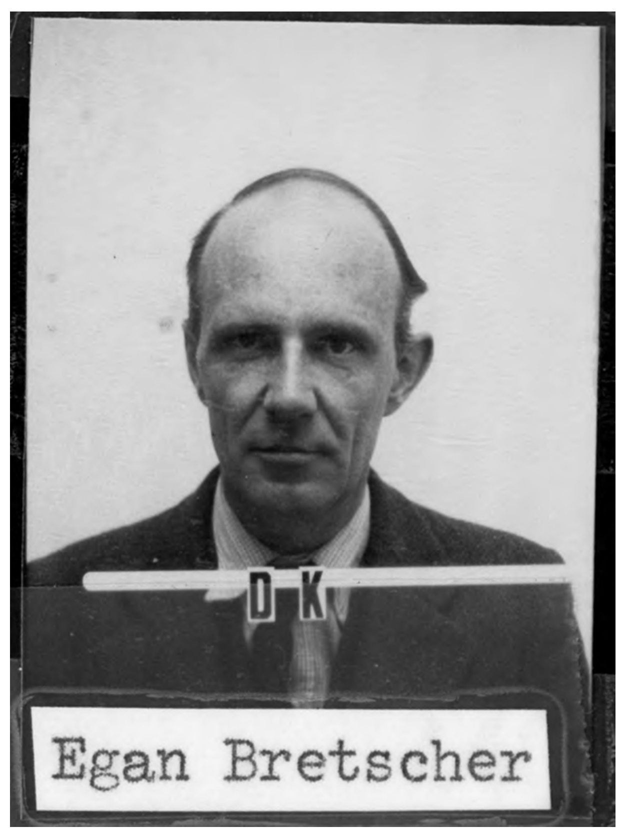


Fig. 9. Egon Bretscher's Los Alamos badge photograph, 1944 (his first name is misspelled).

world record of 69 MJ from Britain's Joint European Torus (JET) at Culham.

IV. EPILOGUE

Ruhlig is something of an unsung hero for us, given that his first observation of DT fusion was forgotten for decades. We have asked ANS and American Physical Society readers for any information they might have regarding Ruhlig's subsequent career as it pertains to fusion.[28,53] In the brief biographical section on Ruhlig at the end of our paper in this issue,[4] we describe how our NSRC archives show that he led a Naval Research Laboratory group that assisted Los Alamos staff in the 1951 Greenhouse George test's diagnostic measurements of fusion. Therefore, not only did Ruhlig first observe DT fusion events in 1938, he also witnessed in 1951 the first ignited burning DT fusion plasma since the Big Bang.

The other unsung hero described in our papers[4,26,27] is Bretscher (Fig. 9), the Swiss-British scientist who joined the British Mission of about 25 scientists who came to Los Alamos during the Manhattan Project. A brief biographical sketch of Bretscher is provided toward the end of our paper in this issue.[4] His first 1945–1946 identification and characterization of the $3/2^+$ resonance[4,54] that we dub the "Bretscher state" since it enhances the DT fusion cross section a hundredfold is of utmost importance to astrophysics and nuclear technologies. Its importance is as significant as that of the "Hoyle State" in ^{12}C that is responsible for the nucleosynthesis of carbon. We have pointed out (following the Caltech group's work[43]) that almost all the ^4He created in Big Bang nucleosynthesis was made via resonant DT fusion and that we humans can trace at least a quarter of our bodies' mass (beyond hydrogen) to nuclei synthesized from these products of DT fusion in the Big Bang. This same $3/2^+$ Bretscher-state enhancement of DT is also what gives some hope for controlled fusion energy. For example, we posed the following question to our colleagues who simulate the ignition process at the NIF: "How much more powerful would the NIF lasers have to be if there were no $3/2^+$ Bretscher state?" Their answer was "approximately 70 times!"[55]—an unattainable goal. But, the true, physical, resonant-enhanced DT cross section puts controlled fusion energy within reach.

Numerous books and papers have described the history of the development of the H-bomb, Refs. [56], [57], and [58] being examples. Our colleagues are presently writing a book on this Los Alamos invention,[13] to be published in a few years' time (coinciding with its 75th anniversary). Their definitive account—aimed at a broad readership—benefits from access to our unique archival records at the Los Alamos NSRC. The present special issue of papers complements these works in describing fusion's scientific origins at a greater level of technical depth.

APPENDIX

COMPTON'S 1942 LETTER TO CONANT

The University of Chicago

Metallurgical Laboratory

September 18, 1942

Jacob H. Kahn
June 11, 1971

Dr. James B. Conant
1530 P Street, N. W.
Washington, D. C.

Dear Dr. Conant:

1. As I mentioned to you in conversation recently, my request of August 29 to discuss Oppenheimer's new results with Allison, Wigner and Fermi is really essential. In view of your wired reply declining such authorization and the request that we ask those who have already heard of the matter to erase it from their memories, may I explain why such discussion with these additional physicists is needed.

It is obvious that the weapon under discussion may be a very powerful one. We need to know (1) how confident can we be that the weapon will actually operate as the theoretical investigators have predicted and (2) how great is the danger that the weapon will get out of hand and result in vastly greater destruction than is planned.

Advice on these points is of value only from those competent in the field of nuclear physics. We have already the best advice available in the country from the strictly theoretical physicists. What is needed next is an assessment of their conclusions by a group of men who combine theoretical and experimental physics, and are well familiar with this field. It is the three men, Fermi, Wigner and Allison that I have chosen as most competent representatives of this aspect of physics. I might have added the name of Dempster.

In our present organization it becomes my responsibility to make recommendations regarding the development of this weapon. It is not possible for me to make such recommendations without the advice of such physicists as those just mentioned, for I should rely more strongly upon their practical judgment of the problems involved than I should on the judgment of the theoretical physicists who are responsible for its development to the present stage. It would be well to set up a new committee to consider these problems, as I understand you have in mind. It is, however, this group of physicists on which any such committee must rely for guidance of the investigations.

It has become clear that further investigations are necessary if we are to be able to use this new tool by the time the requisite materials are available. I have accordingly called a conference here in Chicago beginning Monday, September 21, at which Messrs. Oppenheimer, Van Vleck, Bethe, Teller and Manley, all of whom are acquainted with the developments thus far, will [illegible] the type of experiments that should be performed. In accord with our conversation on Monday, I have also invited Mr. Ed. MacMillan to attend this conference, having in mind the probability that MacMillan

Fig. A.1. Compton's letter to Conant, September 18, 1942.[22] This was written soon after Compton heard of Oppenheimer's team's breakthroughs on H-bomb ideas at Berkeley in the summer of 1942. Page 1 of 3 is shown; pages 2 and 3 are overleaf.

Dr. James B. Conant Page 2 September 19, 1942

may take a leading part in the future development of this aspect of our program. It would be most helpful if we are able to include Messrs. Fermi, Allison, and Wigner in this discussion since these men should be concerned with guiding the experiments to be performed. I should greatly appreciate receiving authorization by Monday to include them in the conference.

2. At Mr. Bush's request you have asked us to prepare a list of those who are acquainted with what has so far been done. With Mr. Oppenheimer's help I have made up the following list which is as complete as I know how to make it.

(1) The theoretical committee responsible for the detailed development of the subject who know the complete story: Oppenheimer, Chairman, Bethe, Bloch, Frenkel (Ph.D. with Oppenheimer) Konopinski, Nelson (Ph.D. with Oppenheimer) Serber, Teller, Van Vleck.

(2) Our Executive Committee: Conant, Briggs, Compton, Lawrence, Murphree, Urey.

(3) Bush

(4) Klein, to him it would appear that Lawrence and Oppenheimer gave only partial information in response to Klein's request to know how much material was required. Klein has probably told some others in Stone & Webster organization with regard to this possibility. He knows nothing of the details which make the process possible. We recommend omitting further comments on this subject to Klein or Stone & Webster representatives to indicate that our interest in the matter is dropped.

(5) At Chicago, Teller spoke freely to his chief, Wigner, with regard to the developments about August 25, before I had given Teller instructions to the contrary. This was in accord with my earlier instructions to Teller that he was free to discuss all theoretical problems freely with Wigner. Allison and Fermi know we are interested in deuterium but know no more with regard to the reason for this interest than they have guessed from their own information. Manley, in charge of fast neutron experiments, has discussed the subject freely with Oppenheimer, as has also my aide, Hilberry, who himself guessed most of the subject matter before being told. Szilard suspects deuterium and guesses why but knows nothing of the recent theoretical results.

(6) At Cambridge, Bacher and Alverez have been told in a general way by Oppenheimer the reason for our interest in deuterium and understand the underlying principles. They know few details.

(7) Colonel Nichols and Colonel Marshall have inferred from conversations in our executive committee that we have an interest in deuterium in connection with its use with the bomb. How or why it is to be used, I doubt if they know.

As far as I know, this is a complete list of those who have been introduced to the possibilities of thermal nuclear explosion. Every effort is being maintained to avoid further extension of this information without authorization.

I should add that the possibility of a thermal nuclear explosion has been recognized for some years. It was investigated several years ago by Szilard who dropped the subject because he found no practical method of detonating the

Dr. James B. Conant Page 3 September 19, 1942

explosion. Fermi discussed this possibility freely with his colleagues engaged upon this work in 1939. These colleagues would include Teller, Wigner, Wheeler and probably others. Any evidence of our special interest in the neutron characteristics of deuterium might lead to the suspicion that we have found some practical method for thus using the material. This statement, however, does not apply to experiments on heavy water, which has its own use in connection with concentrated power plants. It is reasonable to suppose that the Germans will have considered the possibility of thermal nuclear explosions, but it is very probable that they may have never demonstrated theoretically its feasibility.

It is thus not practicable to keep the scientific public ignorant of the idea of such explosions. What may be hoped is to avoid any indication of our active interest in this direction or any indication of what the conditions are that must be satisfied for detonation.

3. May I point out further that our appreciation of the possibilities in this direction has been made possible only by making the venture of placing this vital problem in the hands of a group of investigators, most of whom on the basis of rigid F.B.I. tests would be considered ineligible to our investigations. There are literally no other persons in the country who would have been able to bring this problem to a successful conclusion. Had we played safe in the sense of secrecy, we would have taken the greater risk of remaining ignorant of a powerful weapon which our enemies may use against us. Our investigations and developments are not yet complete. It is essential that we continue to be free to use our country's best talent on this problem, even at the risk of using men who are questioned on grounds of nationality and political partisanship. It is useless to attempt to develop this program without making use of the only brains our country has that are competent to handle it.

In order that delays in the necessary experiments shall not prevent us from making full use of these newly recognized possibilities, may I accordingly ask you to review this matter once more with Mr. Bush and authorize me as soon as possible to discuss these developments with Messrs. Fermi, Allison and Wigner.*

Sincerely yours,

Arthur H. Compton

Arthur H. Compton

KT
cc: Mr. Bush

Acknowledgments

We warmly acknowledge our colleagues who authored the various papers on fusion history and thank them for many enjoyable discussions. We thank John Moore of the NSRC for providing historical Los Alamos photographs, Tom Kunkle for pointing us to Compton's letter shown in the Appendix, and Craig Carmer for providing much editorial expertise.

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