Hopkins Papers

Atomic Bomb

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"The Smyth Report"

by H.D. Smyth

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The "Smyth Report"

BY H. D. SMYTH '18

On August 14, 1945, President Truman announced the end of the war between the United States and Japan. The city of Hiroshima had been destroyed on August 6th (Tokyo time)[1] by the first atomic bomb ever used as a military weapon. Three days later a similar bomb was dropped on Nagasaki, the last use of an atomic bomb for military purposes as far as is now known. By order of President Truman a full account of the secret development of these weapons by the United States Government was released for the use of radio and press on the weekend of August 11th and 12th. This account had been ready sometime before the bombing of Hiroshima but the decision to publish it was a radical one that had been vigorously debated by Mr. Stimson, then Secretary of War, and his advisers in the early days of August, particularly at a meeting on August 2nd. He decided in favor of publication, but since his advisers were by no means unanimously in favor of such a course he concluded that President Truman should make the final decision. At the time of the Hiroshima attack President Truman was still on the Atlantic Ocean, aboard the cruiser *Augusta*, returning from the Potsdam summit meeting.

By August 9, the President was back in the White House available for consultation. Accompanied by Vannevar Bush, James Bryant Conant, Major General Leslie R. Groves, George L. Harrison, and Secretary of State James F. Byrnes, Secretary of War Henry Stimson presented the case for publication to Mr. Truman. By that time the British had formally agreed although they had

[1] The news reached Washington during the evening of Sunday, August 5th, Washington time.

been somewhat reluctant.[2] After hearing the views of Secretary Stimson and his other advisers, President Truman approved immediate publication. His decision was carried out by releasing the approved document for radio use after 9:00 p.m. on Saturday, August 11, and for newspaper use on the next day, Sunday, August 12.

As everyone now knows, the project described had been one of unprecedented size and secrecy. Starting from a fascinating major scientific discovery it had grown into an industrial enterprise of enormous complexity. So it was not surprising that the packets given to the representatives of radio and the press that August Saturday differed considerably from the usual public relations handout. There was, to be sure, a one-page War Department announcement but there was also a 10½" x 7¾" book of some 170 lithoprinted pages bound in heavy cream-colored paper. Hardly adequate as a report of five years' work by thousands of people, this book was still unusual as a press release.

Its preparation had begun in the spring of 1944 when General Groves had asked me if I would be willing to undertake such a task. Of course, I had accepted without hesitation although it was not clear whether any part of what I wrote would ever be used at all, much less whether I or anyone else would appear as author. It is ironic that the book should have become generally known as the "Smyth Report," a fact noted on even the catalogue card in the Library of Congress.

In the last two or three years my attention has been called to items in various booksellers' lists under my name offering for sale at startling prices copies of the Smyth Report. It is not only the prices that are startling. Consider the following statement: "This advance issue was produced in circumstances of extreme security at the nuclear station at Oak Ridge, Tennessee. To ensure secrecy several mimeograph machines were used, the operator of each being given a series of totally unconnected leaves of the original typescript. The final collation of each copy was personally supervised by General Groves. . . ." The picture of General Groves

[2] Collaboration with the British on developing the atomic bomb has a complicated and not entirely happy history. Since early in 1944 Sir James Chadwick, a leading British physicist, had been in the United States as head of the British team, as the result of an agreement made between Roosevelt and Churchill at Quebec in August 1943. Chadwick and Roger Makins from the British Embassy had been at the August 2 meeting in Stimson's office.

spending the first few days of August 1945 hovering over the mimeograph machines at Oak Ridge has its charm but the statement smacks more of fantasy than of fact. Other descriptions are less fanciful but of the five such items I have before me, four contain errors of date or description so confusing as to make it difficult to identify exactly what book or pamphlet is being offered to the public. As a result of this confusion I have been urged to write a piece about the Smyth Report that may help clear up the situation.

Actually in 1947, two years after the release of the report, I was prompted to write such a piece by an entirely different set of circumstances. David Lilienthal was testifying before a Congressional committee which was considering whether he was or was not qualified to be Chairman of the newly established Atomic Energy Commission, a post for which President Truman had nominated him. Goaded by irrelevant or offensive questions from various senators and anticipating a confrontation with Senator McKellar, an old T.V.A. enemy of his, Mr. Lilienthal referred to the "Smyth Report" as "the principal breach of security since the beginning of the atomic energy project." Also about that time it was alleged that Bernard Baruch had said that General Groves had approved release of the report only after being "lambasted" by the scientists. I felt these statements should not go unanswered but that any comment would better come from General Groves or Dr. Conant than from me. Accordingly I wrote to General Groves and to Dr. Conant on February 4, 1947, suggesting that some statement should be made and enclosed a memorandum on the history of the "Smyth Report" dated January 10, 1947. I asked for comments on the general idea and on the accuracy of my memorandum. In my file there is an answer from General Groves from Florida, dated February 19, 1947, expressing interest and the intention to check his recollection against the files on his return to Washington. According to Mr. Lilienthal's diary, Dr. Conant did speak to him rather vigorously shortly after learning about his testimony. I, however, have found no letter to me from Dr. Conant. In any case what I wrote has rested quietly in my files since 1947. It seems to me to give the picture satisfactorily. Rather than trying to rewrite it after a lapse of thirty years I shall quote most of it, occasionally adding some corrections and some paragraphs having to do with bibliographic details.

MEMORANDUM ON THE HISTORY OF THE PREPARATION OF MY REPORT ON ATOMIC ENERGY FOR MILITARY PURPOSES

Prefatory Note: A great many of the most important decisions with reference to this report were made in conferences of which no record was kept because of security problems. Also, the question of final clearance and release for publication was decided at the highest level and my knowledge of how the decisions were reached and by whom is fragmentary and based on hearsay. Therefore the following memorandum is incomplete and only partially documented. It is based on my own recollection and what few documents and letters I have in my file.

Origin of the idea of the report: I began my association with the uranium project in January or February 1941. In the summer and fall of 1943 and the winter of 1944 I was acting first as associate director of the Metallurgical Laboratory at Chicago and later as consultant. During the summer of 1943 I spent most of my time at Chicago, but in the fall of 1943 President Dodds of Princeton felt that it was impossible to release me for more than half-time work at Chicago.[3] It was therefore arranged that I should spend alternate weeks at Princeton and Chicago. This made it impossible for me to discharge the duties of an associate director at Chicago in the real meaning of the title, so that I functioned largely as a consultant with a somewhat detached point of view. This detachment, coupled with the fact that I had been already closely associated with the two major phases of the work, isotope separation and the chain reaction, put me in a good position to write a general account of the work should it be wanted.

I do not remember exactly when the idea arose that a general report on the atomic bomb project should be prepared for eventual release to the public. As I recall it, I suggested the idea in a discussion with Dr. Arthur H. Compton,[4] who thought well of it. I then arranged to talk with Dr. Conant on one of his visits to Chicago. This talk occurred either in February or March of 1944.

[3] This was because all the other regular members of the Department of Physics at Princeton were engaged in war work, most of them away from Princeton, yet the University was committed to a very heavy load of teaching Army and Navy personnel, all taking physics.

[4] Dr. Arthur H. Compton, Professor of Physics at the University of Chicago and Director of the Metallurgical Laboratory there. The Metallurgical Laboratory was one of the major scientific centers for the Manhattan Project.

I believe that the idea I presented to Dr. Conant was essentially the one which was eventually carried out. I felt that the possibilities of atomic energy, and particularly of the bomb, were so important that the political decisions which would have to be made ought to be based on the widest possible dissemination of information. I felt that it would be extremely dangerous to leave these decisions in the hands of a small number of men without informing the people of the country what the significance of the discoveries was. This idea appealed to Dr. Conant and he told me that he would discuss the matter with General Groves and others in Washington. (I am not absolutely sure whether the original idea of the report came from me or from Dr. Conant, or whether it merely emerged from our conversation, but I believe that at least the rough idea was the occasion of my asking for an interview with Dr. Conant.) I heard nothing more about the proposal until sometime early in April when I was asked to come to Washington to talk with General Groves and Dr. Conant. They told me that they felt it was very desirable that a general overall report of the project should be prepared. It was, however, clearly understood that this involved no decision as to the ultimate use of the report. In other words, it was to be prepared with public release in mind, but the question of how much material, if any, should be eventually released was reserved for later decision.

I received a formal letter from General Groves dated April 17, 1944, asking me to undertake preparation of the report, and I replied on April 21, agreeing to do so. For security reasons, both of these letters are in such general terms that they make no reference to the actual nature of the job under discussion.

In preparing the report, I felt that it was necessary to make it as complete as possible, with the idea that later review could cut out material considered inappropriate for release. From this time on I was specifically working for General Groves in the preparation of this report. General Groves made all the arrangements necessary to give me access to the various laboratories and plants, and I had frequent conferences with him and Dr. Conant. It should be understood that I was at the same time continuing as chairman of the Department of Physics at Princeton and acting as a consultant at Chicago so that the preparation of the report was a part-time job.

I sent the outline of the whole report and a rough draft of about

the first half to General Groves on August 5, 1944. I do not have in my file any comment from him or anyone else on that draft. My recollection is that he and Dr. Conant felt the outline was essentially right and made relatively few suggestions at this stage. On February 23, 1945, I sent General Groves twelve out of thirteen chapters of what I still considered a preliminary draft. I discussed this draft with General Groves and with Dr. Conant in March and wrote a letter to General Groves on March 23 discussing their comments. There was still no decision as to whether this report would be used. On May 12, 1945, in anticipation of another conference with Dr. Conant and General Groves, I wrote General Groves a letter reporting that I had rewritten the first eight chapters along the lines of our previous conference, but was finding some difficulty in working without any idea of when and how the report might be used.

At the May 16-17 conference that followed, General Groves and Dr. Conant said that they did want to use the report and asked me whether I could have it in final form early in June. I think the date mentioned was June 10. I said that this would be impossible, so we settled on June 30 as the date which we would try to meet. By this time the report had been read, in whole or in part, by a number of the project leaders, but it was felt nevertheless that it should be officially circulated to them in final form before it was approved for release. We also realized that it was necessary to eliminate a good deal of the material that I had written on grounds of security and that I could not be expected to take the whole responsibility for judgment as to what should and should not be included. Either at this conference or at one shortly thereafter, it was agreed that Dr. Richard C. Tolman[5] should cooperate with me in censoring the report and that it was necessary to have a directive from General Groves as to the criteria to be used for including or excluding material. Dr. Tolman and I prepared a list of such criteria which we submitted to General Groves. After discussion he modified them somewhat and issued them as a directive to Dr. Tolman and me.

It was evident that it was necessary to edit my manuscript as well as censor it, but security made it impossible to use anyone with professional editorial experience. It was decided that Drs.

[5] Professor of Physical Chemistry and Mathematical Physics, California Institute of Technology. By a curious coincidence, I had worked for Dr. Tolman, then a major in the Chemical Warfare Service, in the summer of 1918.

Paul C. Fine and William A. Shurcliff,[6] both of whom were physicists working as technical aides to Dr. Tolman, should work with me on this job. The process of editing and censoring went on more or less simultaneously in the latter part of June and the first week or so of July. In fact, it was the necessity of making a final revision of the report that prevented me from going to the Alamogordo, New Mexico, test on July 16, 1945. About the middle of July the censored and edited text was mimeographed in Dr. Tolman's office in Washington under the supervision of Fine and Shurcliff. Couriers from General Groves' office then delivered and returned chapters of this mimeographed version to project leaders and a few others at Berkeley, Chicago, Columbia, Stanford, Los Alamos and elsewhere. Those consulted were asked to read the parts submitted to them and to sign a release testifying to the general accuracy of the work and to its conformity with the general instructions for security. I believe that in all cases releases were signed but often they were accompanied by suggestions for minor improvement or deletion. Tentative approval of publication was also obtained from Sir James Chadwick representing the British and Canadians.[7]

* * *

In the last part of July, I considered and in most cases incorporated the suggested modifications in a master copy, chapter by chapter. In my file I have a complete series of such chapters each labelled "Master Copy" including the final version of the preface and a typed copy of Chapter XIII. There is a note on each chapter except Chapter I giving the date of designation as master copy. These dates range from July 14, 1945, for the beginning chapters to July 30 for Chapter XIII. Obviously much retyping had still to be done and it is to this task that General Groves refers in his book when he says: "the report was completed on July 28 but not before we had had to fly some fully cleared MED [Manhattan Engineering District] stenographers up to Washington from Oak Ridge." It was presumably this retyped version which General Groves says was "ready for submission to the printer" on August 2nd.[8]

[6] Technical Aides, Office of Scientific Research and Development, National Defense Research Committee and Manhattan Project, Washington, D.C.
[7] This ends the 1947 memorandum.
[8] *Now It Can Be Told* (New York: Harper, 1962), pp. 349-50.

The retyping and lithoprinting were under the general supervision of Fine and Shurcliff and appropriate officers from General Groves' staff. During this period Dr. Tolman read over the final version with extreme care, marking all passages which might conceivably be questioned on grounds of security and citing justification for publishing them in terms of relevant parts of our guidance orders. He and I then went over those passages together and prepared a letter to General Groves discussing them. This letter is dated July 31, 1945, and marks my last action in the preparation of the report in its first published format. The date is consistent with General Groves' "ready for the printer" date of August 2nd.

The "printer" was in fact the facility for reproducing secret documents in the Adjutant General's Office in the Pentagon. I do not know when the first lithoprinted copies were produced. I believe one thousand copies were made. Whenever they were finished they were immediately slapped into the safe in General Groves' office in the Pentagon because their content was still classified TOP SECRET and remained so until August 11, when the whole report was made public by President Truman's order as I described in the introductory paragraphs of this article.

So much for the narrative of the preparation of the first format in which the Smyth Report was released to the public. In a subsequent article, Datus Smith, formerly Director of the Princeton University Press, will give an account of the publication in book form by that press. Before going on to his account there are several specific aspects of the report that may be of interest.

As is clearly stated in the report itself, the principal reason for its preparation was to inform the public. Let me quote the last sentence of the report: "The people of the country must be informed if they are to discharge their responsibilities wisely." This view was strongly held by most of the civilians in the project and more or less shared, certainly not opposed, by General Groves and his military associates.

On the other hand, General Groves and his colleagues, both military and civilian, recognized that there were many technical developments that should be kept secret. How was this to be done? Were the thousands of people who had worked on the project supposed to go back to ordinary civilian life and say absolutely nothing about what they had been doing in the great war? This would be asking the impossible. The best resolution of this dilemma appeared to be to say as much as possible in an official statement

carefully prepared and reviewed and then to instruct people on the project to say nothing more even after they had left the project.

Achieving this objective was the principal reason for General Groves' support of the report and its validity was certainly accepted by such civilians as Bush, Conant, Tolman and many others. I have always found it curious that two lines of reasoning quite opposite in the abstract led in practice to the same conclusion.

A bonus that came from the release of the report gratified General Groves particularly. He had been a tough taskmaster and knew it, so he was especially glad to see as much recognition as possible given to those who had worked so hard and long on the project. Only a few men could actually be named, but all could point to the published record and say this is what I did in the great war. Welcome as this consequence of publication may have been it was always a secondary consideration. I was dismayed when I read in a recent book, that whatever other reasons lay behind the issuance of the report, scientists were obviously anxious to have their various accomplishments acknowledged.

At the time of writing the report there was no pressure on me from the scientists for personal recognition. Such complaints as came in after publication were surprisingly few and were more concerned about questions of attribution to groups or laboratories than with individual reputations. Even the complaint from Los Alamos which I describe in the next section was clearly asking that elegant solutions of difficult problems should be reported so that they could be appreciated. This is very different from the desire for enhancement of personal reputation implied by the statement to which I have referred.

There were two quite different kinds of questions that had to be answered in deciding what should be in the report and what should be left out. In the first category were the normal questions that arise in writing an account of any large and complex enterprise: how technical to be, how much detail to include, how to be fair to the various groups working on different phases of the enterprise, what names to mention and so on.

Answering these questions was complicated by the compartmentalization imposed by the overall secrecy requirement which prevented one group from knowing what another group was doing, even when their fields of work overlapped. This also complicated

the basic question: How to ensure factual accuracy? However, these complications were trivial compared to some of the other consequences of secrecy.

Many of these were largely mechanical. My office in Palmer Laboratory, Princeton University, not only had its door to the hall tightly locked, but the doorway was blocked on the inside by a large safe whose combination was known only to me, my secretary, and the Manhattan Engineering District security officers. The windows of my office and the adjacent office were barred. Access to my office was possible only through the adjacent office occupied by my secretary and an armed guard. In fact there were three shifts of guards, so there was one present day and night. If I needed papers with me on a trip to see General Groves in Washington, I could not take them with me. They travelled separately by military courier.

It is easy to make fun of these arrangements now—for example, during a period of about a year every time I talked to myself I was breaking the secrecy rules by allowing the head of one group to speak to the head of another group—but rules were taken seriously at the time and I believe rightly so. Secrecy precautions certainly were taken seriously when one of the guards on the four to midnight shift shot himself, inflicting what turned out to be a fatal wound. He had been oddly considerate, waiting until after all the secretaries had left the building so that they would not be distressed. Naturally there were immediate security questions. Was the poor man a German spy or Japanese or Russian? Did he have confederates? Was there a conspiracy? Had he and the other guards been fully investigated? As far as I know his suicide turned out to be purely for personal reasons having nothing to do with security, but naturally it took a good deal of investigation to establish that fact.

Returning to the substance of the report: what should be revealed and what should continue to be kept secret. Until May of 1945, I had almost no formal guidance in answering this question. As I have mentioned, Dr. Conant and General Groves approved the outline I submitted to them in the summer of 1944; and I submitted drafts of various sections to appropriate people from time to time. Many of the typed chapters in my file have pencil notes "read by so and so on such and such a date." But there was no formal or informal board of reviewers or editors before June

1945. This was partly because everyone was so busy, but the chief reason was secrecy. I do remember raising with Dr. Compton the question of whether to leave out everything about plutonium. We decided that to do so would be to eviscerate the report to such an extent as to render it useless. I do not remember when this discussion took place.

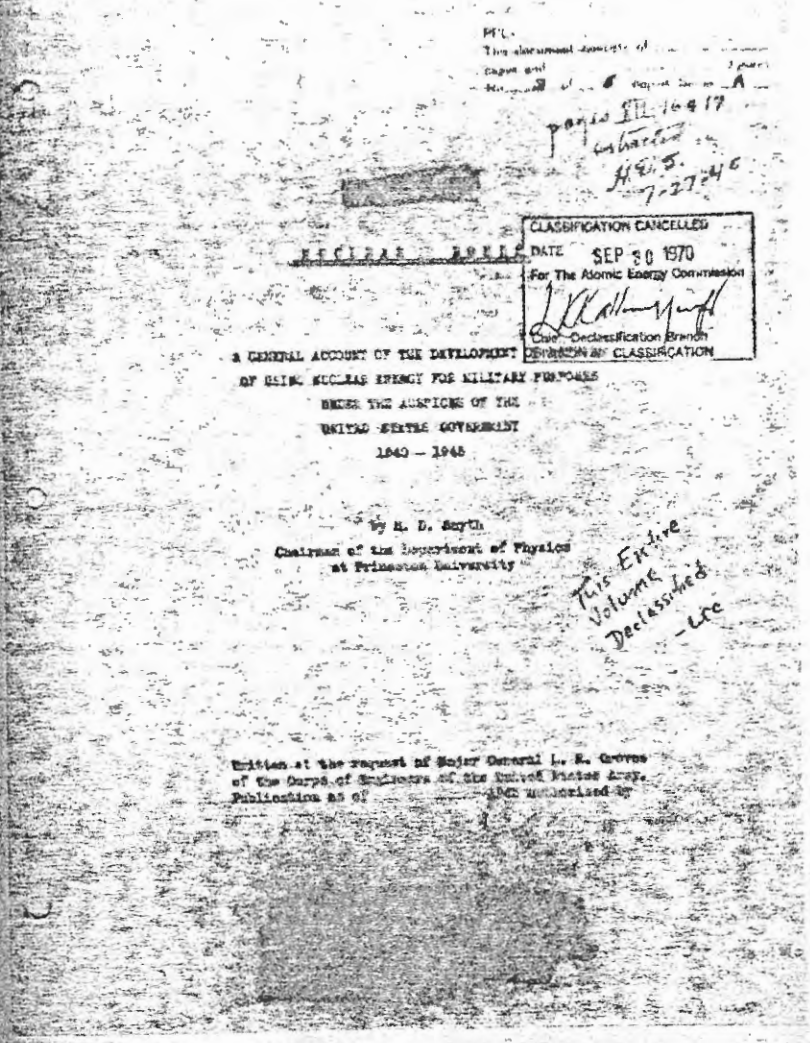
My policy, with one exception, was to include in the drafts as much technical detail as I thought desirable for intelligibility even if some of it had to be later deleted for security reasons. In the case of the bomb laboratory at Los Alamos, however, even my draft chapter left out much of the most interesting material since it was obviously too sensitive for publication. When this draft was submitted to Los Alamos they were so outraged that they sent to Groves a version of their own. It was far more interesting than my version, but unfortunately it violated the security rules that had been set up. This was one of the many occasions when I was grateful for Dr. Tolman's wisdom and good judgment. He read over the Los Alamos version, went through it with the red pencil of censorship, and concluded that what was left was no better than my version which was therefore accepted.

There are two causes for embarrassment in the title of the report or the lack of it. First, to a professional physicist the subject of the report was nuclear energy and nuclear bombs, not atomic energy and atomic bombs. Second, no author is likely to choose a 24-word title for something he has written. Obviously, the only title that appeared on the lithoprinted edition was intended as an explanatory subtitle. This cumbersome subtitle was carried over to the Government Printing Office version as the only title and similarly to the title page of the edition printed in London by His Majesty's Stationery Office.

What was the title supposed to be, and what happened to it? In the various drafts a title page appears for the first time in the typed version from which the mimeographed copies were made for circulation to laboratory directors for correction or approval. This title reads:

Nuclear Bombs

A General Account of the Development of Methods of Using Nuclear Energy for Military Purposes Under the Auspices of the United States Government, 1940-1945.



CLASSIFICATION CANCELLED
DATE SEP 30 1970
For The Atomic Energy Commission
Oak Declassification Branch

A GENERAL ACCOUNT OF THE DEVELOPMENT
OF METHODS OF USING ATOMIC ENERGY FOR MILITARY PURPOSES
UNDER THE AUSPICES OF THE
UNITED STATES GOVERNMENT
1940 – 1945

by H. D. Smyth
Chairman of the Department of Physics
at Princeton University

This Entire Volume Declassified - LTC

Written at the request of Major General L. R. Groves
of the Corps of Engineers of the United States Army.
Publication as of ... not authorized by

Title page of typed version from which mimeographed copies were made for circulation.
Courtesy of Henry DeWolf Smyth.

At the next stage, in the master copy prepared for lithoprinting, the main title was changed to "Atomic Bombs" and "nuclear energy" was changed to "atomic energy" in the subtitle. This is also the way the title appears on the certificate of copyright. But that simple two-word major title did not appear on the lithoprinted edition. As I heard the story, General Groves was very worried about secrecy in spite of the security precautions surrounding the preparation of the lithoprinted copies which I have described. So he would not allow the tell-tale words "Atomic Bombs" to appear on the title page. He had a rubber stamp "Atomic Bombs" prepared and planned to have one of his officers stamp each copy before it was handed out. Apparently the stamp was made and used on the copyright deposit copies but not on any of the copies distributed to the press or public. One result of this omission was that the clumsiness of the title that did appear caused people to refer in self-defense to the lithoprinted version as the Smyth Report. They still do so, even though the Princeton University Press version has a reasonable title, *Atomic Energy for Military Purposes.*

As to the change from "nuclear" to "atomic," it should be remembered that in 1945 the word "nuclear" was either totally unfamiliar to the public or primarily had a biological flavor, whereas "atomic" had a definite association with chemistry and physics. Since in May and June, 1945, it became clear that the report was aimed at a wider audience than nuclear physicists, we decided that atomic was less likely to frighten off readers than nuclear. I believe General Groves suggested the change but I know I accepted it after a somewhat painful suppression of my purist principles. Looking back after thirty years I think the decision was probably right pragmatically. I still find it distasteful, and I welcome the gradual change that has occurred over the years so that the popular press now usually speaks of nuclear energy, nuclear power plants, and nuclear bombs.

While I am writing about titles, let me explain about the copyright. The whole purpose of the report was to spread information. We were glad to have all or part of the text copied and reproduced by anyone who wished to do so. But if we did not take out a copyright we feared someone else might. For this reason the lithoprinted version, the Princeton University Press edition, and those of the Government Printing Office and His Majesty's Stationery

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Princeton University,
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Printed or produced by The Classified Reproduction Center of the Adjutant General's Office, War Department, Washington, D.C.

Date of publication in the United States Aug. 10, 1945
Affidavit received Aug. 13, 1945 Copies received Aug. 13, 1945

Sam B. Warner
Register of Copyrights

Copyright certificate for the Report.
Courtesy of Henry DeWolf Smyth.

fice all have in the front of the book the apparently self-contradictory statement: Copyright 1945 by H. D. Smyth (Reproduction in whole or in part is authorized and permitted).

This procedure and the fact that there seemed to be no simple way for the War Department to pay for the copyright has given me certain amount of mild pleasure. When acquaintances imply that I must be wallowing in wealth as the author of a best seller I am able to state with painful accuracy that my financial balance from the Smyth Report is minus two dollars, the copyright fee.

A more serious bibliographic result of simultaneously copyrighting and giving blanket permission to ignore the copyright was that no one who wished to reprint or translate the whole report or any part of it was under any obligation to ask permission from anyone or even to notify the publisher or me. Consequently, the bibliography is very incomplete, but our objective of wide circulation has certainly been achieved.

As I have said repeatedly, my chief interest in this whole writing enterprise was to get as much information as possible to as many American citizens as possible as soon as possible. A thousand copies distributed to the press were only a beginning. Although the lithoprinted edition would presumably be reprinted in some form by the Government Printing Office, it was not clear at the time how soon this would happen and it was unlikely that distribution and sales would be pushed. So I thought the best route to wide circulation in a minimum of time would be publication by a big established publisher of technical or semi-technical books. McGraw-Hill seemed to fit the bill as well as any. Since I knew some of the people there, I went up to New York to see them. They said "Yes, yes, a fine idea, but in its present form the report is a bit dull in places and a bit difficult in places so why don't you recast it here and there and come back to us." This did not make much sense. The best thing to publish was an account approved by the U.S. Government. Furthermore it could be done quickly. And for obvious reasons I was in no mood to rewrite the report and again ask for the approvals that would be necessary.

Fortunately, Datus Smith, Director of the Princeton University Press, had already asked for the job but, not supposing that a university press could compete with a commercial house in speed and coverage, however well it might otherwise do, I had told him I wanted to try McGraw-Hill. But Datus was enthusiastic and per-

sistent. The Press had the great advantage of being small enough to catch the enthusiasm of its director for an unusual project and to be able to concentrate on it. What is more, its headquarters were in Princeton. So I gave the job to Datus, later getting the blessing of General Groves. That this was one of the wisest decisions I could have made will be evident from the story Datus Smith has to tell in the article following this one.

Thirty-one years have passed since those hectic days when the cooperative efforts of thousands of people were to culminate in the use of two nuclear bombs and the end of the war with Japan. These weapons were not only spectacular in their military effect but were only one of the possible uses of the energy released by nuclear fission. In the larger sense the whole great effort had been to develop the technology of using nuclear energy.

That development added a new category of questions to those arising inevitably in adjusting from a world of war to a world of peace. To quote again from the report: "These questions are not technical questions; they are political and social questions, and the answers given to them may affect all mankind for generations."

The Smyth Report was supposed to furnish material on which the discussion of such questions could be based. Did it do so? I believe it did. How well, it is impossible for me to judge. Of course there were errors of omission and of emphasis; and obscurities of presentation. I regret such faults but granted the subject, the enforced absence of competition and the limitation of time even a bad report would have been a success.

Quantity is easier to judge than quality. The report was reprinted in whole or in part in the newspapers beginning a week after the first bomb was dropped. Within three months thereafter about one hundred thousand copies had been sold in book or pamphlet form. In this early period copies were passed from hand to hand so that each copy had many readers or in some laboratories were duplicated for groups of readers. After November 1945 the Princeton University Press version of the Smyth Report carried official statements from the British and Canadians making clear their full participation in the development of the bomb. Therefore by the end of 1945 there was beginning to be formed a considerable body of citizens within the government and outside who could discuss the problems concerning nuclear fission now con-

fronting the United States and other countries on the basis of fairly extensive information.

In the three years after the publication of the report the discussions that led to the establishment of the U.S. Atomic Energy Commission and those concerned with international control generated hundreds of pages of exposition and argumentation, crystallized to some extent in various committee reports, notably that of Acheson and Lilienthal. Yet, the Smyth Report had continued to function as a basic reference document. By the summer of 1948 much more information was available either as the result of further releases or of publication of new scientific data acquired in three years of work in the world's laboratories. New books began to be written simplifying or revising the treatment of atomic energy as a whole or various phases of it. The Smyth Report was no longer an essential reference book though it remained a convenient one. It had served its purpose and served it well.

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CHAPTER I

INTRODUCTION

1.1. The purpose of this report is to describe the scientific and technical developments in this country since 1940 directed toward the military use of energy from atomic nuclei. Although not written as a "popular" account of the subject, this report is intended to be intelligible to scientists and engineers generally and to other college graduates with a good grounding in physics and chemistry. The equivalence of mass and energy is chosen as the guiding principle in the presentation of the background material of the "Introduction".

The Conservation of Mass and of Energy

1.2. There are two principles that have been cornerstones of the structure of modern science. The first — that matter can be neither created nor destroyed but only altered in form — was enunciated in the eighteenth century and is familiar to every student of chemistry; it has led to the principle known as the law of conservation of mass. The second — that energy can be neither created nor destroyed but only altered in form — emerged in the nineteenth century and has ever since been the plague of inventors of perpetual-motion machines; it is known as the law of conservation of energy.

1.3. These two principles have constantly guided and disciplined the development and application of science. For all practical purposes they were unaltered and separate until some five years ago. For most practical purposes they are still so, but it is now known that they are, in fact, two phases of a single principle for we have discovered that energy may sometimes be converted into matter and matter into energy. Specifically, such a conversion is observed in the phenomenon of nuclear fission of uranium, a process in which atomic nuclei split into fragments with the release of an enormous amount of energy. The military use of this energy has been the object of the research and production projects described in this report.

The Equivalence of Mass and Energy

1.4. One conclusion that appeared rather early in the development of the theory of relativity was that the inertial mass of a moving body increased as its speed increased. This implied an equivalence between an increase in energy of motion of a body, that is, its kinetic energy, and an increase in its mass. To most practical physicists and engineers this appeared a mathematical fiction of no practical importance. Even Einstein

could hardly have foreseen the present applications, but as early as 1905 he did clearly state that mass and energy were equivalent and suggested that proof of this equivalence might be found by the study of radioactive substances. He concluded that the amount of energy, E, equivalent to a mass, m, was given by the equation

$$E = mc^2$$

where c is the velocity of light. If this is stated in actual numbers, its startling character is apparent. It shows that one kilogram (2.2 pounds) of matter, if converted entirely into energy, would give 25 billion kilowatt hours of energy. This is equal to the energy that would be generated by the total electric power industry in the United States (as of 1939) running for approximately two months. Compare this fantastic figure with the 8.5 kilowatt hours of heat energy which may be produced by burning an equal amount of coal.

1.5. The extreme size of this converstion figure was interesting in several respects. In the first place, it explained why the equivalence of mass and energy was never observed in ordinary chemical combustion. We now believe that the heat given off in such a combustion has mass associated with it, but this mass is so small that it cannot be detected by the most sensitive balances available. (It is of the order of a few billionths of a gram per mole.) In the second place, it was made clear that no appreciable quantities of matter were being converted into energy in any familiar terrestrial processes, since no such large sources of energy were known. Further, the possibility of initiating or controlling such a conversion in any practical way seemed very remote. Finally, the very size of the conversion factor opened a magnificent field of speculation to philosophers, physicists, engineers, and comic-strip artists. For twenty-five years such speculation was unsupported by direct experimental evidence, but beginning about 1930 such evidence began to appear in rapidly increasing quantity. Before discussing such evidence and the practical partial conversion of matter into energy that is our main theme, we shall review the foundations of atomic and nuclear physics. General familiarity with the atomic nature of matter and with the existence of electrons is assumed. Our treatment will be little more than an outline which may be elaborated by reference to books such as Pollard and Davidson's Applied Nuclear Physics and Stranathan's The Particles of Nuclear Physics.

Radioactivity and Atomic Structure

1.6. First discovered by H. Becquerel in 1896 and subsequently studied by Pierre and Marie Curie, E. Rutherford, and many others, the phenomena of radioactivity have played leading roles in the discovery of the general laws of atomic structure and in the verification of the equivalence of mass and energy.

Ionization by Radioactive Substances

1.7. The first observed phenomenon of radioactivity was the blackening of photographic plates by uranium minerals. Although this effect is still used to some extent in research on radioactivity, the property of radioactive substances that is of greatest scientific value is their ability to ionize gases. Under normal conditions air and other gases do not conduct electricity — otherwise power lines and electrical machines would not operate in the open as they do. But under some circumstances the molecules of air are broken apart into positively and negatively charged fragments, called ions. Air thus ionized does conduct electricity. Within a few months after the first discovery of radioactivity Becquerel found that uranium had the power to ionize air. Specifically he found that the charge on an electroscope would leak away rapidly through the air if some uranium salts were placed near it. (The same thing would happen to a storage battery if sufficient radioactive material were placed near by.) Ever since that time the rate of discharge of an electroscope has served as a measure of intensity of radioactivity. Furthermore, nearly all present-day instruments for studying radioactive phenomena depend on this ionization effect directly or indirectly. An elementary account of such instruments, notably electroscopes, Geiger-Müller counters, ionization chambers, and Wilson cloud chambers is given in Appendix 1.

The Different Radiations or Particles

1.8. Evidence that different radioactive substances differ in their ionizing power both in kind and in intensity indicates that there are differences in the "radiations" emitted. Some of the radiations are much more penetrating than others; consequently, two radioactive samples having the same effect on an "unshielded" electroscope may have very different effects if the electroscope is "shielded," i.e., if screens are interposed between the sample and the electroscope. These screens are said to absorb the radiation.

1.9. Studies of absorption and other phenomena have shown that in fact there are three types of "radiation" given off by radioactive substances. There are alpha particles, which are high-speed ionized helium atoms (actually the nuclei of helium atoms), beta particles, which are high-speed electrons, and gamma rays, which are electromagnetic radiation similar to X-rays. Of these only the gamma rays are properly called radiations, and even these act very much like particles because of their short wave-length. Such a "particle" or quantum of gamma radiation is called a photon. In general, the gamma rays are very penetrating, the alpha and beta rays less so. Even though the alpha and beta rays are not very penetrating, they have enormous kinetic energies for particles of atomic size, energies thousands of times greater than the kinetic energies which the molecules of a gas have by reason of their thermal motion, and thousands of times greater than the energy changes per atom in chemical reactions. It was for this reason that Einstein suggested that studies of radioactivity might show the equivalence of mass and energy.

The Atom

1.10. Before considering what types of atoms emit alpha, beta, and gamma rays, and before discussing the laws that govern such emission, we shall describe the current ideas on how atoms are constructed, ideas based partly on the study of radioactivity.

1.11. According to our present view every atom consists of a small heavy nucleus approximately 10^{-12} cm in diameter surrounded by a largely empty region 10^{-8} cm in diameter in which electrons move somewhat like planets about the sun. The nucleus carries an integral number of positive charges, each 1.6×10^{-19} coulombs in size. (See Appendix 2 for a discussion of units.) Each electron carries one negative charge of this same size, and the number of electrons circulating around the nucleus is equal to the number of positive charges on the nucleus so that the atom as a whole has a net charge of zero.

1.12. Atomic Number and Electronic Structure. The number of positive charges in the nucleus is called the atomic number, Z. It determines the number of electrons in the extranuclear structure, and this in turn determines the chemical properties of the atom. Thus all the atoms of a given chemical element have the same atomic number, and conversely all atoms having the same atomic number are atoms of the same element regardless of possible differences in their nuclear structure. The extranuclear electrons in an atom arrange themselves in successive shells according to well-established laws. Optical spectra arise from disturbances in the outer parts of this electron structure; X-rays arise from disturbances of the electrons close to the nucleus. The chemical properties of an atom depend on the outermost electrons, and the formation of chemical compounds is accompanied by minor rearrangements of these electronic structures. Consequently, when energy is obtained by oxidation, combustion, explosion, or other chemical processes, it is obtained at the expense of these structures so that the arrangement of the electrons in the products of the process must be one of lowered energy content. (Presumably the total mass of these products is correspondingly lower but not detectably so.) The atomic nuclei are not affected by any chemical process.

1.13. Mass Number. Not only is the positive charge on a nucleus always an integral number of electronic charges, but the mass of the nucleus is always approximately a whole number times a fundamental unit of mass which is almost the mass of a proton, the nucleus of a hydrogen atom. (See Appendix 2.) This whole number is called the mass number, A, and is always at least twice as great as the atomic number except in the cases of hydrogen and a rare isotope of helium. Since the mass of a proton is about 1800 times that of an electron, the mass of the nucleus is very nearly the whole mass of the atom.

1.14. Isotopes and Isobars. Two species of atoms having the same atomic number but different mass numbers are called isotopes. They are chemically identical, being merely two species of the same chemical element. If two species of atoms have the same mass number but different atomic numbers, they are called isobars and represent two different chemical elements.

Radioactivity and Nuclear Change

1.15. If an atom emits an alpha particle (which has an atomic number of two and a mass of four), it becomes an atom of a different element with an atomic number lower by two and a mass number lower by four. The emission by a nucleus of a beta particle increases the atomic number by one and leaves the mass number unaltered. In some cases, these changes are accompanied by the emission of gamma rays. Elements which spontaneously change or "disintegrate" in these ways are unstable and are described as being "radioactive." The only natural elements which exhibit this property of emitting alpha or beta particles are (with a few minor exceptions) those of very high atomic numbers and mass numbers, such as uranium, thorium, radium, and actinium, i.e., those known to have the most complicated nuclear structures.

Half-Lives; The Radioactive Series

1.16. All the atoms of a particular radioactive species have the same probability of disintegrating in a given time, so that an appreciable sample of radioactive material, containing many millions of atoms, always changes or "disintegrates" at the same rate. This rate at which the material changes is expressed in terms of the "half-life," the time required for one half the atoms initially present to disintegrate, which evidently is constant for any particular atomic species. Half-lives of radioactive materials range from fractions of a second for the most unstable to billions of years for those which are only slightly unstable. Often, the "daughter" nucleus like its radioactive "parent" is itself radioactive and so on down the line for several successive generations of nuclei until a stable one is finally reached. There are three such families or series comprising all together about forty different radioactive species. The radium series starts from one isotope of uranium, the actinium series from another isotope of uranium, and the thorium series from thorium. The final product of each series, after ten or twelve successive alpha and beta particle emissions, is a stable isotope of lead.

First Demonstration of Artificial Nuclear Disintegration

1.17. Before 1919 no one had succeeded in disturbing the stability of ordinary nuclei or affecting the disintegration rates of those that were naturally radioactive. In 1919 Rutherford showed that high-energy alpha particles could cause an alteration in the nucleus of an ordinary element. Specifically he succeeded in changing a few atoms of nitrogen into atoms of oxygen by bombarding them with alpha particles. The process involved may be written as

$$He^4 + N^{14} \longrightarrow O^{17} + H^1$$

meaning that a helium nucleus of mass number 4 (an alpha particle) striking a nitrogen nucleus of mass number 14 produces an oxygen nucleus of mass number 17 and a hydrogen nucleus of mass number 1. The hydrogen nucleus, known as the "proton," is of special importance since it has the smallest mass of any nucleus. Although protons do not appear in natural radioactive processes,

there is much direct evidence that they can be knocked out of nuclei.

The Neutron

1.18. In the decade following Rutherford's work many similar experiments were performed with similar results. One series of experiments of this type led to the discovery of the neutron, which will be discussed in some detail since the neutron is practically the theme song of this whole project.

1.19. In 1930 W. Bothe and H. Becker in Germany found that if the very energetic natural alpha particles from polonium fell on certain of the light elements, specifically beryllium, boron or lithium, an unusually penetrating radiation was produced. At first this radiation was thought to be gamma radiation although it was more penetrating than any gamma rays known, and the details of experimental results were very difficult to interpret on this basis. The next important contribution was reported in 1932 by Irene Curie and F. Joliot in Paris. They showed that if this unknown radiation fell on paraffin or any other hydrogen-containing compound it ejected protons of very high energy. This was not in itself inconsistent with the assumed gamma-ray nature of the new radiation, but detailed quantitative analysis of the data became increasingly difficult to reconcile with such an hypothesis. Finally (later in 1932) J. Chadwick in England performed a series of experiments showing that the gamma ray hypothesis was untenable. He suggested that in fact the new radiation consisted of uncharged particles of approximately the mass of the proton, and he performed a series of experiments verifying his suggestion. Such uncharged particles are now called neutrons.

1.20. The one characteristic of neutrons which differentiates them from other subatomic particles is the fact that they are uncharged. This property of neutrons delayed their discovery, makes them very penetrating, makes it impossible to observe them directly, and makes them very important as agents in nuclear change. To be sure, an atom in its normal state is also uncharged, but it is ten thousand times larger than a neutron and consists of a complex system of negatively charged electrons widely spaced around a positively charged nucleus. Charged particles (such as protons, electrons, or alpha particles) and electromagnetic radiations (such as gamma rays) lose energy in passing through matter. They exert electric forces which ionize atoms of the material through which they pass. (It is such ionization processes that make the air electrically conducting in the path of electric sparks and lightning flashes.) The energy taken up in ionization equals the energy lost by the charged particle, which slows down, or by the gamma ray, which is absorbed. The neutron, however, is unaffected by such forces; it is affected only by a very short-range force, i.e., a force that comes into play when the neutron comes very close indeed to an atomic nucleus. This is the kind of force that holds a nucleus together in spite of the mutual repulsion of the positive charges in it. Consequently a free neutron goes on its way unchecked until it makes a "head-on" collision with an atomic nucleus. Since nuclei are very small, such collisions occur but rarely and the neutron travels a long way before colliding. In the case of a collision of the "elastic" type, the ordinary laws of momentum apply as they do in the elastic collision of billiard balls. If the nucleus that is struck is heavy, it ac-

quires relatively little speed, but if it is a proton, which is approximately equal in mass to the neutron, it is projected forward with a large fraction of the original speed of the neutron, which is itself correspondingly slowed. Secondary projectiles resulting from these collisions may be detected, for they are charged and produce ionization. The uncharged nature of the neutron makes it not only difficult to detect but difficult to control. Charged particles can be accelerated, decelerated, or deflected by electric or magnetic fields which have no effect on neutrons. Furthermore, free neutrons can be obtained only from nuclear disintegrations; there is no natural supply. The only means we have of controlling free neutrons is to put nuclei in their way so that they will be slowed and deflected or absorbed by collisions. As we shall see, these effects are of the greatest practical importance.

The Positron and the Deuteron

1.21. The year 1932 brought the discovery not only of the neutron but also of the positron. The positron was first observed by C. D. Anderson at the California Institute of Technology. It has the same mass and the same magnitude of charge as the electron, but the charge is positive instead of negative. Except as a particle emitted by artificially radioactive nuclei, it is of little interest to us.

1.22. One other major discovery marked the year 1932. H. C. Urey, F. G. Brickwedde, and G. M. Murphy found that hydrogen had an isotope of mass number 2, present in natural hydrogen to one part in 5000. Because of its special importance this heavy species of hydrogen is given a name of its own, deuterium, and the corresponding nucleus is called the deuteron. Like the alpha particle it is not one of the fundamental particles but does play an important role in certain processes for producing nuclear disintegration.

Nuclear Structure

1.23. The idea that all elements are made out of a few fundamental particles is an old one. It is now firmly established. We believe that there are three fundamental particles -- the neutron, the proton, and the electron. A complete treatise would also discuss the positron, which we have mentioned, the neutrino and the mesotron. The deuteron and alpha particle, which have already been mentioned, are important complex particles.

1.24. According to our present views the nuclei of all atomic species are made up of neutrons and protons. The number of protons is equal to the atomic number, Z. The number of neutrons, N, is equal to the difference between the mass number and the atomic number, or A - Z. There are two sets of forces acting on these particles, ordinary electric coulomb forces of repulsion between the positive charges and very short-range forces of attraction between all the particles. These last forces are only partly understood, and we shall not attempt to discuss them. Suffice it to say that combined effects of these attractive and repulsive forces are such that only certain combinations of neutrons and protons are stable. If the neutrons and protons are few in number, stability occurs when their numbers are about equal. For larger nuclei, the proportion of neutrons required for stability

is greater. Finally, at the end of the periodic table, where the number of protons is over 90 and the number of neutrons nearly 150, there are no completely stable nuclei. (Some of the heavy nuclei are almost stable as evidenced by very long half-lives.) If an unstable nucleus is formed artificially by adding an extra neutron or proton, eventually a change to a stable form occurs. Strangely enough, this is not accomplished by ejecting a proton or a neutron but by ejecting a positron or an electron; apparently within the nucleus a proton converts itself to a neutron and positron (or a neutron converts itself into a proton and electron), and the light charged particle is ejected. In other words, the mass number remains the same but the atomic number changes. The stability conditions are not very critical so that for a given mass number, i.e., given total number of protons and neutrons, there may be several stable arrangements of protons and neutrons (at most three or five) giving several isobars. For a given atomic number, i.e., given number of protons, conditions can vary still more widely so that some of the heavy elements have as many as ten or twelve stable isotopes. Some two hundred and fifty different stable nuclei have been identified, ranging in mass number from one to two hundred and thirty-eight and in atomic number from one to ninety-two.

1.25. All the statements we have been making are based on experimental evidence. The theory of nuclear forces is still incomplete, but it has been developed on quantum-mechanical principles sufficiently to explain not only the above observations but more detailed empirical data on artificial radioactivity and on differences between nuclei with odd and even mass numbers.

Artificial Radioactivity

1.26. We mentioned above the emission of positrons or electrons by nuclei seeking stability. Electron emission (beta rays) was already familiar in the study of naturally radioactive substances, but positron emission was not found in the case of such substances. In fact, the general discussion presented above obviously was based in part on information that cannot be presented in this report. We shall, however, give a brief account of the discovery of "artificial" radioactivity and what is now known about it.

1.27. In 1934, Curie and Joliot reported that certain light elements (boron, magnesium, aluminum) which had been bombarded with alpha particles continued to emit positrons for some time after the bombardment was stopped. In other words, alpha-particle bombardment produced radioactive forms of boron, magnesium, and aluminum. Curie and Joliot actually measured half-lives of 14 minutes, 2.5 minutes, and 3.25 minutes, respectively, for the radioactive substances formed by the alpha-particle bombardment.

1.28. This result stimulated similar experiments all over the world. In particular, E. Fermi reasoned that neutrons, because of their lack of charge, should be effective in penetrating nuclei, especially those of high atomic number which repel protons and alpha particles strongly. He was able to verify his prediction almost immediately, finding that the nucleus of the bombarded atom captured the neutron and that there was thus produced an unstable nucleus which then achieved stability by emitting an electron. Thus,

the final, stable nucleus was one unit higher in mass number and one unit higher in atomic number than the initial target nucleus.

1.29. As a result of innumerable experiments carried out since 1934, radioactive isotopes of nearly every element in the periodic table can now be produced. Some of them revert to stability by the emission of positrons, some by the emission of electrons, some by a process known as K-electron capture which we shall not discuss, and a small number (probably three) by alpha particle emission. Although some five hundred unstable nuclear species have been observed, and in most cases their atomic numbers and mass numbers have been identified.

1.30. Not only do these artificially radioactive elements play an important role throughout the project with which we are concerned, but their future value in medicine, in "tracer" chemistry, and in many other fields of research can hardly be overestimated.

Energy Considerations

Nuclear Binding Energies

1.31. In describing radioactivity and atomic structure we have deliberately avoided quantitative data and have not mentioned any applications of the equivalence of mass and energy which we announced as the guiding principle of this report. The time has now come when we must speak of quantitative details, not merely of general principles.

1.32. We have spoken of stable and unstable nuclei made up of assemblages of protons and neutrons held together by nuclear forces. It is a general principle of physics that work must be done on a stable system to break it up. Thus, if an assemblage of neutrons and protons is stable, energy must be supplied to separate its constituent particles. If energy and mass are really equivalent, then the total mass of a stable nucleus should be less than the total mass of the separate protons and neutrons that go to make it up. This mass difference, then, should be equivalent to the energy required to disrupt the nucleus completely, which is called the binding energy. Remember that we said that the masses of all nuclei were "approximately" whole numbers. It is the small differences from whole numbers that are significant.

1.33. Consider the alpha particle as an example. It is stable; since its mass number is four and its atomic number two it consists of two protons and two neutrons. The mass of a proton is 1.00758 and that of a neutron is 1.00893 (see Appendix 2), so that the total mass of the separate components of the helium nucleus is

$$2 \times 1.00758 + 2 \times 1.00893 = 4.03302$$

whereas the mass of the helium nucleus itself is 4.00280. Neglecting the last two decimal places we have 4.033 and 4.003, a difference of 0.030 mass units.

This, then, represents the "binding energy" of the protons and neutrons in the helium nucleus. It looks small, but recalling Einstein's equation, $E = mc^2$, we remember that a small amount of mass is equivalent to a large amount of energy. Actually 0.030 mass units is equal to 4.5×10^{-5} ergs per nucleus or 2.7×10^{19} ergs per gram molecule of helium. In units more familiar to the engineer or chemist, this means that to break up the nuclei of all the helium atoms in a gram of helium would require 1.62×10^{11} gram calories or 190,000 kilowatt hours of energy. Conversely, if free protons and neutrons could be assembled into helium nuclei, this energy would be released.

1.34. Evidently it is worth exploring the possibility of getting energy by combining protons and neutrons or by transmuting one kind of nucleus into another. Let us begin by reviewing present-day knowledge of the binding energies of various nuclei.

Mass Spectra and Binding Energies

1.35. Chemical atomic-weight determinations give the average weight of a large number of atoms of a given element. Unless the element has only one isotope, the chemical atomic weight is not proportional to the mass of individual atoms. The mass spectrograph developed by F. W. Aston and others from the earlier apparatus of J. J. Thomson measures the masses of individual isotopes. Indeed, it was just such measurements that proved the existence of isotopes and showed that on the atomic-weight scale the masses of all atomic species were very nearly whole numbers. These whole numbers, discovered experimentally, are the mass numbers which we have already defined and which represent the sums of the numbers of the protons and neutrons; their discovery contributed largely to our present views that all nuclei are combinations of neutrons and protons.

1.36. Improved mass spectrograph data supplemented in a few cases by nuclear reaction data have given accurate figures for binding energies for many atomic species over the whole range of atomic masses. This binding energy, B, is the difference between the true nuclear mass, M, and the sum of the masses of all the protons and neutrons in the nucleus. That is,

$$B = (ZM_p + NM_n) - M$$

where M_p and M_n are the masses of the proton and neutron respectively, Z is the number of protons, $N = A - Z$ is the number of neutrons, and M is the true mass of the nucleus. It is more interesting to study the binding energy per particle, B/A, than B itself. Such a study shows that, apart from fluctuations in the light nuclei, the general trend of the binding energy per particle is to increase rapidly to a flat maximum around A = 60 (nickel) and then decrease again gradually. Evidently the nuclei in the middle of the periodic table -- nuclei of mass numbers 40 to 100 -- are the most strongly bound. Any nuclear reaction where the particles in the resultant nuclei are more strongly bound than the particles in the initial nuclei will release energy. Speaking in thermochemical terms, such reactions are exothermic. Thus, in general, energy may be gained by combining light nuclei to form heavier ones or by breaking very heavy ones into two or three smaller frag-

ments. Also, there are a number of special cases of exothermic nuclear disintegrations among the first ten or twelve elements of the periodic table, where the binding energy per particle varies irregularly from one element to another.

1.37. So far we seem to be piling one supposition on another. First we assumed that mass and energy were equivalent; now we are assuming that atomic nuclei can be rearranged with a consequent reduction in their total mass, thereby releasing energy which can then be put to use. It is time to talk about some experiments that convinced physicists of the truth of these statements.

Experimental Proof of the Equivalence of Mass and Energy

1.38. As we have already said, Rutherford's work in 1919 on artificial nuclear disintegration was followed by many similar experiments. Gradual improvement in high voltage technique made it possible to substitute artificially produced high-speed ions of hydrogen or helium for natural alpha particles. J. D. Cockcroft and E. T. S. Walton in Rutherford's laboratory were the first to succeed in producing nuclear changes by such methods. In 1932 they bombarded a target of lithium with protons of 700 kilovolts energy and found that alpha particles were ejected from the target as a result of the bombardment. The nuclear reaction which occurred can be written symbolically as

$$_3Li^7 + {}_1H^1 \longrightarrow {}_2He^4 + {}_2He^4$$

where the subscript represents the positive charge on the nucleus (atomic number) and the superscript is the number of massive particles in the nucleus (mass number). As in a chemical equation, quantities on the left must add up to those on the right; thus the subscripts total four and the superscripts eight on each side.

1.39. Neither mass nor energy has been included in this equation. In general, the incident proton and the resultant alpha particles will each have kinetic energy. Also, the mass of two alpha particles will not be precisely the same as the sum of the masses of a proton and a lithium atom. According to our theory, the totals of mass and energy taken together should be the same before and after the reaction. The masses were known from mass spectra. On the left ($Li^7 + H^1$) they totalled 8.0241, on the right (2 He^4) 8.0056, so that 0.0185 units of mass had disappeared in the reaction. The experimentally determined energies of the alpha particles were approximately 8.5 million electron volts each, a figure compared to which the kinetic energy of the incident proton could be neglected. Thus 0.0185 units of mass had disappeared and 17 Mev of kinetic energy had appeared. Now 0.0185 units of mass is 3.07×10^{-26} grams, 17 Mev is 27.2×10^{-6} ergs and c is 3×10^{10} cm/sec. (See Appendix 2.) If we substitute these figures into Einstein's equation, $E = mc^2$, on the left side we have 27.2×10^{-6} ergs and on the right side we have 27.6×10^{-6} ergs, so that the equation is found to be satisfied to a good approximation. In other words, these experimental results prove that the equivalence of mass and energy was correctly stated by Einstein.

Nuclear Reactions

Methods of Nuclear Bombardment

1.40. Cockcroft and Walton produced protons of fairly high energy by ionizing gaseous hydrogen and then accelerating the ions in a transformer-rectifier high-voltage apparatus. A similar procedure can be used to produce high-energy deuterons from deuterium or high-energy alpha particles from helium. Higher energies can be attained by accelerating the ions in cyclotrons or Van de Graaff machines. However, to obtain high-energy gamma radiation or -- most important of all -- high-energy neutrons, nuclear reactions themselves must be used as sources. Radiations of sufficiently high energy come from certain naturally radioactive materials or from certain bombardments. Neutrons are commonly produced by the bombardment of certain elements, notably beryllium or boron, by natural alpha particles, or by bombarding suitable targets with protons or deuterons. The most common source of neutrons is a mixture of radium and beryllium where the alpha particles from radium and its decay products penetrate the Be^9 nuclei, which then give off neutrons and become stable C^{12} nuclei (ordinary carbon). A frequently used "beam" source of neutrons results from accelerated deuterons impinging on "heavy water" ice. Here the high-speed deuterons strike the target deuterons to produce neutrons and He^3 nuclei. Half a dozen other reactions are also used involving deuterium, lithium, beryllium, or boron as targets. Note that in all these reactions the total mass number and total charge number are unchanged.

1.41. To summarize, the agents that are found to initiate nuclear reactions are — in approximate order of importance -- neutrons, deuterons, protons, alpha particles, gamma rays and, rarely, heavier particles.

Results of Nuclear Bombardment

1.42. Most atomic nuclei can be penetrated by at least one type of atomic projectile (or by gamma radiation). Any such penetration may result in a nuclear rearrangement in the course of which a fundamental particle is ejected or radiation is emitted or both. The resulting nucleus may be one of the naturally available stable species, or -- more likely -- it may be an atom of a different type which is radioactive, eventually changing to still a different nucleus. This may in turn be radioactive and, if so, will again decay. The process continues until all nuclei have changed to a stable type. There are two respects in which these artificially radioactive substances differ from the natural ones: many of them change by emitting positrons (unknown in natural radioactivity) and very few of them emit alpha particles. In every one of the cases where accurate measurements have been made, the equivalence of mass and energy has been demonstrated and the mass-energy total has remained constant. (Sometimes it is necessary to invoke neutrinos to preserve mass-energy conservation.)

Notation

1.43. A complete description of a nuclear reaction should include the nature, mass and energy of the incident particle, also the nature (mass

number and atomic number), mass and energy (usually zero) of the target particle, also the nature, mass and energy of the ejected particles (or radiation), and finally the nature, mass and energy of the remainder. But all of these are rarely known and for many purposes their complete specification is unnecessary. A nuclear reaction is frequently described by a notation that designates first the target by chemical symbol and mass number if known, then the projectile, then the emitted particle, and then the remainder. In this scheme the neutron is represented by the latter n, the proton by p, the deuteron by d, the alpha particle by α, and the gamma ray by γ. Thus the radium-beryllium neutron reaction can be written $Be^9(\alpha,n)C^{12}$ and the deuteron-deuteron reaction $H^2(d,n)He^3$.

Types of Reaction

1.44. Considering the five different particles (n, p, d, α, γ) both as projectiles and emitted products, we might expect to find twenty-five combinations possible. Actually the deuteron very rarely occurs as a product particle, and the photon initiates only two types of reaction. There are, however, a few other types of reaction, such as (n,2n), (d,H^3), and fission, which bring the total known types to about twenty-five. Perhaps the (n,γ) reaction should be specifically mentioned as it is very important in one process which will concern us. It is often called "radiative capture" since the neutron remains in the nucleus and only a gamma ray comes out.

Probability and Cross Section

1.45. So far nothing has been said about the probability of nuclear reactions. Actually it varies widely. There is no guarantee that a neutron or proton headed straight for a nucleus will penetrate it at all. It depends on the nucleus and on the incident particle. In nuclear physics, it is found convenient to express probability of a particular event by a "cross section." Statistically, the centers of the atoms in a thin foil can be considered as points evenly distributed over a plane. The center of an atomic projectile striking this plane has geometrically a definite probability of passing within a certain distance (r) of one of these points. In fact, if there are n atomic centers in an area A of the plane, this probability is $n\pi r^2/A$, which is simply the ratio of the aggregate area of circles of radius r drawn around the points to the whole area. If we think of the atoms as impenetrable steel discs and the impinging particle as a bullet of negligible diameter, this ratio is the probability that the bullet will strike a steel disc, i.e., that the atomic projectile will be stopped by the foil. If it is the fraction of impinging atoms getting <u>through</u> the foil which is measured, the result can still be expressed in terms of the equivalent stopping cross section of the atoms. This notion can be extended to any interaction between the impinging particle and the atoms in the target. For example, the probability that an alpha particle striking a beryllium target will produce a neutron can be expressed as the equivalent cross section of beryllium for this type of reaction.

1.46. In nuclear physics it is conventional to consider that the

impinging particles have negligible diameter. The technical definition of cross section for any nuclear process is therefore:

$$\frac{\text{number of processes occurring}}{\text{number of incident particles}} = (\text{number of target nuclei per cm}^2) \times (\text{nuclear cross section in cm}^2).$$

It should be noted that this definition is for the cross section per nucleus. Cross sections can be computed for any sort of process, such as capture, scattering, production of neutrons, etc. In many cases, the number of particles emitted or scattered in nuclear processes is not measured directly; one merely measures the attenuation produced in a parallel beam of incident particles by the interposition of a known thickness of a particular material. The cross section obtained in this way is called the total cross section and is usually denoted by σ.

1.47. As indicated in paragraph 1.11, the typical nuclear diameter is of the order of 10^{-12} cm. We might therefore expect the cross sections for nuclear reactions to be of the order of $\pi d^2/4$ or roughly 10^{-24} cm^2 and this is the unit in which they are usually expressed. Actually the observed cross sections vary enormously. Thus for slow neutrons absorbed by the (n, γ) reaction the cross section in some cases is as much as 1000×10^{-24} cm^2, while the cross sections for transmutations by gamma-ray absorption are in the neighborhood of $(1/1000) \times 10^{-24}$ cm^2.

Practicability of Atomic Power in 1939

Small Scale of Experiments

1.48. We have talked glibly about the equivalence of mass and energy and about nuclear reactions, such as that of protons on lithium, where energy was released in relatively large amounts. Now let us ask why atomic power plants did not spring up all over the world in the thirties. After all, if we can get 2.76×10^{-5} ergs from an atom of lithium struck by a proton, we might expect to obtain approximately half a million kilowatt hours by combining a gram of hydrogen with seven grams of lithium. It looks better than burning coal. The difficulties are in producing the high-speed protons and in controlling the energy produced. All the experiments we have been talking about have been done with very small quantities of material, large enough in numbers of atoms, to be sure, but in terms of ordinary masses infinitesimal -- not tons or pounds or grams, but fractions of micrograms. The amount of energy used up in the experiment was always far greater than the amount generated by the nuclear reaction.

1.49. Neutrons are particularly effective in producing nuclear disintegration. Why weren't they used? If their initial source was an ion beam striking a target, the limitations discussed in the last paragraph applied. If a radium and beryllium source was to be used, the scarcity of radium was a difficulty.

The Need of a Chain Reaction

1.50. Our common sources of power, other than sunlight and water power, are chemical reactions -- usually the combustion of coal or oil. They release energy as the result of rearrangements of the outer electronic structures of the atoms, the same kind of process that supplies energy to our bodies. Combustion is always self-propagating; thus lighting a fire with a match releases enough heat to ignite the neighboring fuel, which releases more heat which ignites more fuel, and so on. In the nuclear reactions we have described this is not generally true; neither the energy released nor the new particles formed are sufficient to maintain the reaction. But we can imagine nuclear reactions emitting particles of the same sort that initiate them and in sufficient numbers to propagate the reaction in neighboring nuclei. Such a self-propagating reaction is called a "chain reaction" and such conditions must be achieved if the energy of the nuclear reactions with which we are concerned is to be put to large-scale use.

Period of Speculation

1.51. Although there were no atomic power plants built in the thirties, there were plenty of discoveries in nuclear physics and plenty of speculation. A theory was advanced by H. Bethe to explain the heat of the sun by a cycle of nuclear changes involving carbon, hydrogen, nitrogen, and oxygen, and leading eventually to the formation of helium.* This theory is now generally accepted. The discovery of a few (n,2n) nuclear reactions (i.e., neutron-produced and neutron-producing reactions) suggested that a self-multiplying chain reaction might be initiated under the right conditions. There was much talk of atomic power and some talk of atomic bombs. But the last great step in this preliminary period came after four years of stumbling. The effects of neutron bombardment of uranium, the most complex element known, had been studied by some of the ablest physicists. The results were striking but confusing. The story of their gradual interpretation

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*The series of reactions postulated was

$$(1)\quad {}_6C^{12} + {}_1H^{1} \longrightarrow {}_7N^{13}$$

$$(2)\quad {}_7N^{13} \longrightarrow {}_6C^{13} + {}_1e^{0}$$

$$(3)\quad {}_6C^{13} + {}_1H^{1} \longrightarrow {}_7N^{14}$$

$$(4)\quad {}_7N^{14} + {}_1H^{1} \longrightarrow {}_8O^{15}$$

$$(5)\quad {}_8O^{15} \longrightarrow {}_7N^{15} + {}_1e^{0}$$

$$(6)\quad {}_7N^{15} + {}_1H^{1} \longrightarrow {}_6C^{12} + {}_2He^{4}$$

The net effect is the transformation of hydrogen into helium and positrons (designated as ${}_1e^{0}$) and the release of about thirty million electron volts energy.

is intricate and highly technical, a fascinating tale of theory and experiment. Passing by the earlier inadequate explanations, we shall go directly to the final explanation, which, as so often happens, is relatively simple.

Discovery of Uranium Fission

1.52. As has already been mentioned, the neutron proved to be the most effective particle for inducing nuclear changes. This was particularly true for the elements of highest atomic number and weight where the large nuclear charge exerts strong repulsive forces on deuteron or proton projectiles but not on uncharged neutrons. The results of the bombardment of uranium by neutrons had proved interesting and puzzling. First studied by Fermi and his colleagues in 1934, they were not properly interpreted until several years later.

1.53. On January 16, 1939 Niels Bohr of Copenhagen, Denmark, arrived in this country to spend several months in Princeton, N. J., and was particularly anxious to discuss some abstract problems with A. Einstein. (Four years later Bohr was to escape from Nazi-occupied Denmark in a small boat.) Just before Bohr left Denmark two of his colleagues, O. R. Frisch and L. Meitner (both refugees from Germany), had told him their guess that the absorption of a neutron by a uranium nucleus sometimes caused that nucleus to split into approximately equal parts with the release of enormous quantities of energy, a process that soon began to be called nuclear "fission." The occasion for this hypothesis was the important discovery of O. Hahn and F. Strassmann in Germany (published in Naturwissenschaften in early January 1939) which proved that an isotope of barium was produced by neutron bombardment of uranium. Immediately on arrival in the United States Bohr communicated this idea to his former student J. A. Wheeler and others at Princeton, and from them the news spread by word of mouth to neighboring physicists including E. Fermi at Columbia University. As a result of conversations between Fermi, J. R. Dunning, and G. B. Pegram, a search was undertaken at Columbia for the heavy pulses of ionization that would be expected from the flying fragments of the uranium nucleus. On January 26, 1939 there was a Conference on Theoretical Physics at Washington, D. C., sponsored jointly by the George Washington University and the Carnegie Institution of Washington. Fermi left New York to attend this meeting before the Columbia fission experiments had been tried. At the meeting Bohr and Fermi discussed the problem of fission, and in particular Fermi mentioned the possibility that neutrons might be emitted during the process. Although this was only a guess, its implication of the possibility of a chain reaction was obvious. A number of sensational articles were published in the press on this subject. Before the meeting in Washington was over, several other experiments to confirm fission had been initiated, and positive experimental confirmation was reported from four laboratories (Columbia University, Carnegie Institution of Washington, Johns Hopkins University, University of California) in the February 15, 1939 issue of the Physical Review. By this time Bohr had heard that similar experiments had been made in his laboratory in Copenhagen about January 15th. (Letter by Frisch to Nature dated January 16, 1939 and appearing in the February 18th issue.) F. Joliot in Paris had also published his first results in the

Comptes Rendus of January 30, 1939. From this time on there was a steady flow of papers on the subject of fission, so that by the time (December 6, 1939) Turner wrote a review article on the subject in the Reviews of Modern Physics nearly one hundred papers had appeared. Complete analysis and discussion of these papers have appeared in Turner's article and elsewhere.

General Discussion of Fission

1.54. Consider the suggestion of Frisch and Meitner in the light of the two general trends that had been discovered in nuclear structure: — first, that the proportion of neutrons goes up with atomic number; second, that the binding energy per particle is a maximum for the nuclei of intermediate atomic number. Suppose the U-238 nucleus is broken exactly in half; then, neglecting the mass of the incident neutron, we have two nuclei of atomic number 46 and mass number 119. But the heaviest stable isotope of palladium (Z = 46) has a mass number of only 110. Therefore to reach stability each of these imaginary new nuclei must eject nine neutrons, becoming $_{46}Pd^{110}$ nuclei; or four neutrons in each nucleus must convert themselves to protons by emitting electrons, thereby forming stable tin nuclei of mass number 119 and atomic number 50; or a combination of such ejections and conversions must occur to give some other pair of stable nuclei. Actually, as was suggested by Hahn and Strassmann's identification of barium (Z = 56, A = 135 to 140) as a product of fission, the split occurs in such a way as to produce two unequal parts of mass numbers about 140 and 90 with the emission of a few neutrons and subsequent radioactive decay by electron emission until stable nuclei are formed. Calculations from binding-energy data show that any such rearrangement gives an aggregate resulting mass considerably less than the initial mass of the uranium nucleus, and thus that a great deal of energy must be released.

1.55. Evidently, there were three major implications of the phenomenon of fission: the release of energy, the production of radioactive atomic species and the possibility of a neutron chain reaction. The energy release might reveal itself in kinetic energy of the fission fragments and in the subsequent radioactive disintegration of the products. The possibility of a neutron chain reaction depended on whether neutrons were in fact emitted — a possibility which required investigation.

1.56. These were the problems suggested by the discovery of fission, the kind of problem reported in the journals in 1939 and 1940 and since then investigated largely in secret. The study of the fission process itself, including production of neutrons and fast fragments, has been largely carried out by physicists using counters, cloud chambers, etc. The study and identification of the fission products has been carried out largely by chemists, who have had to perform chemical separations rapidly even with sub-microscopic quantities of material and to make repeated determinations of the half-lives of unstable isotopes. We shall summarize the state of knowledge as of June 1940. By that time the principal facts about fission had been discovered and revealed to the scientific world. A chain reaction had not been obtained, but its possibility — at least in principle — was clear and several paths that might lead to it had been suggested.

State of Knowledge in June 1940

Definite and Generally-Known Information on Fission

1.57. All the following information was generally known in June 1940, both here and abroad:

(1) That three elements -- uranium, thorium, and protoactinium -- when bombarded by neutrons sometimes split into approximately equal fragments, and that these fragments were isotopes of elements in the middle of the periodic table, ranging from selenium (Z = 34) to lanthanum (Z = 57).

(2) That most of these fission fragments were unstable, decaying radioactively by successive emission of beta particles through a series of elements to various stable forms.

(3) That these fission fragments had very great kinetic energy.

(4) That fission of thorium and protoactinium was caused only by fast neutrons (velocities of the order of thousands of miles per second).

(5) That fission in uranium could be produced by fast or slow (so-called thermal-velocity) neutrons; specifically, that thermal neutrons caused fission in one isotope, U-235, but not in the other, U-238, and that fast neutrons had a lower probability of causing fission in U-235 than thermal neutrons.

(6) That at certain neutron speeds there was a large capture cross section in U-238 producing U-239 but not fission.

(7) That the energy released per fission of a uranium nucleus was approximately 200 million electron volts.

(8) That high speed neutrons were emitted in the process of fission.

(9) That the average number of neutrons released per fission was somewhere between one and three.

(10) That high-speed neutrons could lose energy by inelastic collision with uranium nuclei without any nuclear reaction taking place.

(11) That most of this information was consistent with the semi-empirical theory of nuclear structure worked out by Bohr and Wheeler and others; this suggested that predictions based on this theory had a fair chance of success.

Suggestion of Plutonium Fission

1.58. It was realized that radiative capture of neutrons by U-238 would probably lead by two successive beta-ray emissions to the formation of a nucleus for which Z = 94 and A = 239. Consideration of the Bohr-Wheeler theory of fission and of certain empirical relations among the nuclei by L. A. Turner and others suggested that this nucleus would be a fairly stable alpha emitter and would probably undergo fission when bombarded by thermal neutrons. Later the importance of such thermal fission to the maintenance of

The chain reaction was foreshadowed in private correspondence and discussion. In terms of our present knowledge and notation the particular reaction suggested is as follows:

$$_{92}U^{238} + {}_{0}n^{1} \longrightarrow {}_{92}U^{239} \longrightarrow {}_{93}Np^{239} + {}_{-1}e^{0}$$

$$_{93}Np^{239} \longrightarrow {}_{94}Pu^{239} + {}_{-1}e^{0}$$

where Np and Pu are the chemical symbols now used for the two new elements, neptunium and plutonium; ${}_{0}n^{1}$ represents the neutron, and ${}_{-1}e^{0}$ represents an ordinary (negative) electron. Plutonium 239 is the nucleus rightly guessed to be fissionable by thermal neutrons. It will be discussed fully in later chapters.

General State of Nuclear Physics

1.59. By 1940 nuclear reactions had been intensively studied for over ten years. Several books and review articles on nuclear physics had been published. New techniques had been developed for producing and controlling nuclear projectiles, for studying artificial radioactivity, and for separating sub-microscopic quantities of chemical elements produced by nuclear reactions. Isotope masses had been measured accurately. Neutron-capture cross sections had been measured. Methods of slowing down neutrons had been developed. Physiological effects of neutrons had been observed; they had even been tried in the treatment of cancer. All such information was generally available; but it was very incomplete. There were many gaps and many inaccuracies. The techniques were difficult and the quantities of materials available were often sub-microscopic. Although the fundamental principles were clear, the theory was full of unverified assumptions and calculations were hard to make. Predictions made in 1940 by different physicists of equally high ability were often at variance. The subject was in all-too-many respects in art, rather than a science.

Summary

1.60. Looking back on the year 1940, we see that all the prerequisites to a serious attack on the problem of producing atomic bombs and controlling atomic power were at hand. It had been proved that mass and energy were equivalent. It had been proved that the neutrons initiating fission of uranium reproduced themselves in the process and that therefore a multiplying chain reaction might occur with explosive force. To be sure, no one knew whether the required conditions could be achieved, but many scientists had clear ideas as to the problems involved and the directions in which solutions might be sought. The next chapter of this report gives a statement of the problems and serves as a guide to the developments of the past five years.

CHAPTER II

STATEMENT OF THE PROBLEM

Introduction

2.1. From the time of the first discovery of the large amounts of energy released in nuclear reactions to the time of the discovery of uranium fission, the idea of atomic power or even atomic bombs was discussed off and on in scientific circles. The discovery of fission made this talk seem much less speculative, but realization of atomic power still seemed in the distant future and there was an instinctive feeling among many scientists that it might not, in fact, ever be realized. During 1939 and 1940 many public statements, some of them by responsible scientists, called attention to the enormous energy available in uranium for explosives and for controlled power, so that U-235 became a familiar by-word indicating great things to come. The possible military importance of uranium fission was called to the attention of the government (see Chapter III), and in a conference with representatives of the Navy Department in March 1939 Fermi suggested the possibility of achieving a controllable reaction using slow neutrons or a reaction of an explosive character using fast neutrons. He pointed out, however, that the data then available might be insufficient for accurate predictions.

2.2. By the summer of 1940 it was possible to formulate the problem fairly clearly, although it was still far from possible to answer the various questions involved or even to decide whether a chain reaction ever could be obtained. In this chapter we shall give a statement of the problem in its entirety. For purposes of clarification we may make use of some knowledge which actually was not acquired until a later date.

The Chain-Reaction Problem

2.3. The principle of operation of an atomic bomb or power plant utilizing uranium fission is simple enough. If one neutron causes a fission that produces more than one new neutron, the number of fissions may increase tremendously with the release of enormous amounts of energy. It is a question of probabilities. Neutrons produced in the fission process may escape entirely from the uranium, may be captured by uranium in a process not resulting in fission, or may be captured by an impurity. Thus the question of whether a chain reaction does or does not go depends on the result of a competition among four processes:

(1) escape,
(2) non-fission apture by uranium,
(3) non-fission pture by impurities,
(4) fission captures.

If the loss of neutrons by the first three processes is less than the surplus

produced by the fourth, the chain reaction ceases; otherwise it does not. Evidently any one of the first three processes may have such a high probability in a given arrangement that the extra neutrons created by fission will be insufficient to keep the reaction going. For example, should it turn out that process (2) -- non-fission capture by uranium -- has a much higher probability than fission capture, there would presumably be no possibility of achieving a chain reaction.

2.4. An additional complication is that natural uranium contains three isotopes: U-234, U-235, and U-238, present to the extent of approximately 0.006, 0.7, and 99.3 percent, respectively. We have already seen that the probabilities of processes (2) and (4) are different for different isotopes. We have also seen that the probabilities are different for neutrons of different energies.

2.5. We shall now consider the limitations imposed by the first three processes and how their effects can be minimized.

Neutron Escape; Critical Size

2.6. The relative number of neutrons which escape from a quantity of uranium can be minimized by changing the size and shape. In a sphere any surface effect is proportional to the square of the radius, and any volume effect is proportional to the cube of the radius. Now the escape of neutrons from a quantity of uranium is a surface effect depending on the area of the surface, but fission capture occurs throughout the material and is therefore a volume effect. Consequently the greater the amount of uranium, the less probable it is that neutron escape will predominate over fission capture and prevent a chain reaction. Loss of neutrons by non-fission capture is a volume effect like neutron production by fission capture, so that increase in size makes no change in its relative importance.

2.7. The critical size of a device containing uranium is defined as the size for which the production of free neutrons by fission is just equal to their loss by escape and by non-fission capture. In other words, if the size is smaller than critical, then -- by definition -- no chain reaction will sustain itself. In principle it was possible in 1940 to calculate the critical size, but in practice the uncertainty of the constants involved was so great that the various estimates differed widely. It seemed not improbable that the critical size might be too large for practical purposes. Even now estimates for untried arrangements vary somewhat from time to time as new information becomes available.

Use of a Moderator to Reduce Non-fission Capture

2.8. In Chapter I we said that thermal neutrons have the highest probability of producing fission of U-235 but we also said that the neutrons emitted in the process of fission had high speeds. Evidently it was an oversimplification to say that the chain reaction might maintain itself if more neutrons were created by fission than were absorbed. For the probability both of fission capture and of non-fission capture depends on the speed of the neutrons. Unfortunately, the speed at which non-fission capture is most

probable is intermediate between the average speed of neutrons emitted in the fission process and the speed at which fission capture is most probable.

2.9. For some years before the discovery of fission, the customary way of slowing down neutrons was to cause them to pass through material of low atomic weight, such as hydrogenous material. It was E. Fermi and L. Szilard who proposed the use of graphite as a moderator for a chain reaction. The process of slowing down or moderation is simply one of elastic collisions between high-speed particles and particles practically at rest. The more nearly identical the masses of neutron and struck particle the greater the loss of kinetic energy by the neutron. Therefore the light elements are most effective as "moderators", i.e., slowing down agents, for neutrons.

2.10. It occurred to a number of physicists that it might be possible to mix uranium with a moderator in such a way that the high-speed fission neutrons, after being ejected from uranium and before re-encountering uranium nuclei, would have their speeds reduced below the speeds for which non-fission capture is highly probable. Evidently the characteristics of a good moderator are that it should be of low atomic weight and that it should have little or no tendency to absorb neutrons. Lithium and boron are excluded on the latter count. Helium is difficult to use because it is a gas and forms no compounds. The choice of moderator therefore lay between hydrogen, deuterium, beryllium, and carbon. Even now no one of these substances can be excluded from the list of practical possibilities.

Use of a Lattice to Reduce Non-fission Capture

2.11. The general scheme of using a moderator mixed with the uranium was pretty obvious. A specific manner of using a moderator was first suggested in this country, so far as we can discover, by Fermi and Szilard. The idea was to use lumps of uranium of considerable size imbedded in a matrix of moderator material. Such a lattice can be shown to have real advantages over a homogeneous mixture. As the constants were more accurately determined, it became possible to calculate theoretically the type of lattice that would be most effective.

Reduction of Non-fission Capture by Isotope Separation

2.12. In Chapter I it was stated that for neutrons of certain intermediate speeds (corresponding to energies of a few electron volts) U-238 has a large capture cross section for the production of U-239 but not for fission. There is also a considerable probability of inelastic (i.e., non-capture-producing) collisions between high-speed neutrons and U-238 nuclei. Thus the presence of the U-238 tends both to reduce the speed of the fast neutrons and to effect the capture of those of moderate speed. Although there may be some non-fission capture by U-235, it is evident that if we can separate the U-235 from the U-238 and discard the U-238, we can reduce non-fission capture and can thus promote the chain reaction. In fact, the probability of fission of U-235 by high-speed neutrons may be great enough to make the use of a moderator unnecessary once the U-238 has been removed. Unfortunately, U-235 is present in natural uranium only to the extent of about one part in 140. Also, the relatively small difference in mass between the two isotopes

make separation difficult. In fact, in 1940 no large-scale separation of isotopes had ever been achieved except for hydrogen, whose two isotopes differ in mass by a factor of two. Nevertheless, the possibility of separating U-235 was recognized early as being of the greatest importance, and such separation has, in fact, been one of the two major lines of Project effort during the past five years.

Production and Purification of Materials

2.13. It has been stated above that the cross section for capture of neutrons varies greatly among different materials. In some it is very high compared to the maximum fission cross section of uranium. If, then, we are to hope to achieve a chain reaction, we must reduce effect (3) -- non-fission capture by impurities -- to the point where it is not serious. This means very careful purification of the uranium metal and very careful purification of the moderator. Calculations show that the maximum permissible concentrations of many impurity elements are a few parts per million -- in either the uranium or the moderator. When it is recalled that up to 1940 the total amount of uranium metal produced in this country was not more than a few grams (and even this was of doubtful purity), that the total amount of metallic beryllium produced in this country was not more than a few pounds, that the total amount of concentrated deuterium produced was not more than a few pounds, and that carbon had never been produced in quantity with anything like the purity required of a moderator, it is clear that the problem of producing and purifying materials was a major one.

Control of the Chain Reaction

2.14. The problems that have been discussed so far have to do merely with the realization of the chain reaction. If such a reaction is going to be of use, we must be able to control it. The problem of control is different depending on whether we are interested in steady production of power or in an explosion. In general, the steady production of atomic power required a slow-neutron-induced fission chain reaction occurring in a mixture or lattice of uranium and moderator, while an atomic bomb requires a fast-neutron-induced fission chain reaction in U-235 or Pu-239, although both slow- and fast-neutron fission may contribute in each case. It seemed likely, even in 1940, that by using neutron absorbers a power chain reaction could be controlled. It was also considered likely, though not certain, that such a chain reaction would be self-limiting by virtue of the lower probability of fission-producing capture when a higher temperature was reached. Nevertheless, there was a possibility that a chain-reacting system might get out of control, and it therefore seemed necessary to perform the chain-reaction experiment in an uninhabited location.

Practical Application of the Chain Reaction

2.15. Up to this point we have been discussing how to produce and control a nuclear chain reaction but not how to make use of it. The technological gap between producing a controlled chain reaction and using it as a large-scale power source or an explosive is comparable to the gap between the discovery of fire and the manufacture of a steam locomotive.

2.16. Although production of power has never been the principal object of this project, enough attention has been given to the matter to reveal the major difficulty: the attainment of high-temperature operation. An effective heat engine must not only develop heat but must develop heat at a high temperature. To run a chain-reacting system at a high temperature and to convert the heat generated to useful work is very much more difficult than to run a chain-reacting system at a low temperature.

2.17. Of course, the proof that a chain reaction is possible does not itself insure that nuclear energy can be effective in a bomb. To have an effective explosion it is necessary that the chain reaction build up extremely rapidly; otherwise only a small amount of the nuclear energy will be utilized before the bomb flies apart and the reaction stops. It is also necessary that no premature explosion occur. This entire "detonation" problem was and still remains one of the most difficult problems in designing a high-efficiency atomic bomb.

Possibility of Using Plutonium

2.18. So far, all our discussion has been primarily concerned with the use of uranium itself. We have already mentioned the suggestion that the element of atomic number 94 and mass 239, commonly referred to as plutonium, might be very effective. Actually, we now believe it to be of value comparable to pure U-235. We have mentioned the difficulty of separating U-235 from the more abundant isotope U-238. These two isotopes are, of course, chemically identical. But plutonium, although produced from U-238, is a different chemical element. Therefore, if a process could be worked out for converting some of the U-238 to plutonium, a chemical separation of the plutonium from uranium might prove more practicable than the isotopic separation of U-235 from U-238.

2.19. Suppose that we have set up a controllable chain reaction in a lattice of natural uranium and a moderator -- say carbon, in the form of graphite. Then as the chain reaction proceeds, neutrons are emitted in the process of fission of the U-235 and many of these neutrons are absorbed by U-238. This produces U-239, each atom of which then emits a beta particle, becoming neptunium ($_{93}Np^{239}$). Neptunium, in turn, emits another beta particle, becoming plutonium ($_{94}Pu^{239}$), which emits an alpha particle, decaying again to U-235, but so slowly that in effect it is a stable element. If, after the reaction has been allowed to proceed for a considerable time, the mixture of metals is removed, it may be possible to extract the plutonium by chemical methods and purify it for use in a subsequent fission chain reaction of an explosive nature.

Combined Effects and Enriched Piles

2.20. Three ways of increasing the likelihood of a chain reaction have been mentioned: use of a moderator; attainment of high purity of materials; use of special material, either U-235 or Pu. The three procedures are not mutually exclusive, and many schemes have been proposed for using small amounts of separated U-235 or Pu-239 in a lattice composed primarily of ordinary uranium or uranium oxide and of a moderator or two different

moderators. Such proposed arrangements are usually called "enriched piles."

Use of Thorium or Protoactinium or Other Material

2.21. All our previous discussion has centered on the direct or indirect use of uranium, but it was known that both thorium and protoactinium also underwent fission when bombarded by high-speed neutrons. The great advantage of uranium, at least for preliminary work, was its susceptibility to slow neutrons. There was not very much consideration given to the other two substances. Protoactinium can be eliminated because of its scarcity in nature. Thorium is relatively plentiful but has no apparent advantage over uranium.

2.22. It is not to be forgotten that theoretically many nuclear reactions might be used to release energy. At present we see no way of initiating or controlling reactions other than those involving fission, but some such synthesis as has already been mentioned as a source of solar energy may eventually be produced in the laboratory.

Amounts of Materials Needed

2.23. Obviously it was impossible in the summer of 1940 to make more than guesses as to what amounts of materials would be needed to produce:

(1) a chain reaction with use of a moderator;
(2) a chain-reaction bomb in pure, or at least enriched, U-235 or plutonium.

A figure of one to one hundred kilograms of U-235 was commonly given at this time for the critical size of a bomb. This would, of course, have to be separated from at least 140 times as much natural uranium. For a slow-neutron chain reaction using a moderator and unseparated uranium it was almost certain that tons of metal and of moderator would be required.

Availability of Materials

2.24. Estimates of the composition of the earth's crust show uranium and thorium both present in considerable quantities (about 4 parts per million of uranium and 12 parts per million of thorium in the earth's crust). Deposits of uranium ore are known to exist in Colorado, in the Great Bear Lake region of northern Canada, in Joachimstal in Czechoslovakia, and in the Belgian Congo. Many other deposits of uranium ore are known, but their extent is in many cases unexplored. Uranium is always found with radium although in much larger quantity. Both are often found with vanadium ores. Small quantities of uranium oxide have been used for many years in the ceramics industry.

2.25. Thorium is also rather widely distributed, occurring as thorium oxide in fairly high concentration in monazite sands found to some extent in this country but particularly in Brazil and in British India.

2.26. Early rough estimates, which are probably optimistic, were that the nuclear energy available in known deposits of uranium was adequate to supply the total power needs of this country for 200 years (assuming utilization of U-238 as well as U-235).

2.27. As has already been mentioned, little or no uranium metal had been produced up to 1940 and information was so scant that even the melting point was not known. (For example, the Handbook of Physics and Chemistry for 1943-1944 says only that the melting point is below 1850°C whereas we now know it to be in the neighborhood of 1150°.) Evidently, as far as uranium was concerned, there was no insurmountable difficulty as regards obtaining raw materials or producing the metal, but there were very grave questions as to how long it would take and how much it would cost to produce the necessary quantities of pure metal.

2.28. Of the materials mentioned above as being suitable for moderators, deuterium had the most obvious advantages. It is present in ordinary hydrogen to the extent of about one part in 5000. By 1940 a number of different methods for separating it from hydrogen had been developed, and a few liters had been produced in this country for experimental purposes. The only large-scale production had been in a Norwegian plant, from which several hundred liters of heavy water (D_2O, deuterium oxide) had come. As in the case of uranium, the problem was one of cost and time.

2.29. Beryllium in the form of beryllium silicates is widely found but only in small quantities of ore. Its use as an alloying agent has become general in the last few years; for such use, however, it is not necessary to produce the beryllium in metallic form. In 1940 only 700 pounds of the metal were produced in this country.

2.30. As far as carbon was concerned, the situation was obviously quite different. There were many hundreds of tons of graphite produced every year in this country. This was one of the reasons why graphite looked very desirable as a moderator. The difficulties lay in obtaining sufficient quantities of graphite of the required purity, particularly in view of the expanding needs of war industry.

Time and Cost Estimates

2.31. Requirements of time and money depended not only on many unknown scientific and technological factors but also on policy decisions. Evidently years of time and millions of dollars might be required to achieve the ultimate objective. About all that was attempted at this time was making estimates as to how long it would take and how much it would cost to clarify the scientific and technological prospects. It looked as if it would not be a very great undertaking to carry along the development of the thermal-neutron

chain reaction in a graphite-uranium lattice to the point of finding out whether this reaction would in fact go. Estimates made at the time were that approximately a year and $100,000 would be required to get an answer. These estimates applied to a chain-reacting system of very low power without a cooling system or any means for using the energy released.

Health Hazards

2.32. It had been known for a long time that radioactive materials were dangerous. They give off very penetrating radiation -- gamma rays -- which are much like X-rays in their physiological effects. They also give off beta and alpha rays which, although less penetrating, can still be dangerous. The amounts of radium used in hospitals and in ordinary physical measurements usually comprise but a few milligrams. The amounts of radioactive material produced by the fission of uranium in a relatively small chain-reacting system may be equivalent to hundreds or thousands of grams of radium. A chain-reacting system also gives off intense neutron radiation known to be comparable to gamma rays as regards health hazards. Quite apart from its radioactive properties, uranium is poisonous chemically. Thus nearly all work in this field is hazardous -- particularly work on chain reactions and the resulting radioactive products.

Method of Approach to the Problem

2.33. There were two ways of attacking the problem. One was to conduct elaborate series of accurate physical measurement on absorption cross sections of various materials for various neutron-induced processes and various neutron energies. Once such data were available, calculations as to what might be done in the way of a chain reaction could be made with fair accuracy. The other approach was the purely empirical one of mixing uranium or uranium compounds in various ways with various moderators and observing what happened. Similar extremes of method were possible in the case of the isotope-separation problem. Actually an intermediate or compromise approach was adopted in both cases.

Power vs. Bomb

2.34. The expected military advantages of uranium bombs were far more spectacular than those of a uranium power plant. It was conceivable that a few uranium bombs might be decisive in winning the war for the side first putting them into use. Such thoughts were very much in the minds of those working in this field, but the attainment of a slow-neutron chain reaction seemed a necessary preliminary step in the development of our knowledge and became the first objective of the group interested in the problem. This also seemed an important step in convincing military authorities and the more skeptical scientists that the whole notion was not a pipe dream. Partly

for these reasons and partly because of the extreme secrecy imposed about this time, the idea of an atomic bomb does not appear much in the records between the summer of 1940 and the fall of 1941.

Military Usefulness

2.35. If all the atoms in a kilogram of U-235 undergo fission, the energy released is equivalent to the energy released in the explosion of about 20,000 short tons of TNT. If the critical size of a bomb turns out to be practical -- say, in the range of one to one hundred kilograms -- and all the other problems can be solved, there remain two questions. First, how large a percentage of the fissionable nuclei can be made to undergo fission before the reaction stops; i.e., what is the efficiency of the explosion? Second, what is the effect of so concentrated a release of energy? Even if only 1 percent of the theoretically available energy is released, the explosion will still be of a totally different order of magnitude from that produced by any previously known type of bomb. The value of such a bomb was thus a question for military experts to consider very carefully.

Summary

2.36. It had been established (1) that uranium fission did occur with release of great amounts of energy; and (2) that in the process extra neutrons were set free which might start a chain reaction. It was not contrary to any known principle that such a reaction should take place and that it should have very important military application as a bomb. However, the idea was revolutionary and therefore suspect; it was certain that many technical operations of great difficulty would have to be worked out before such a bomb could be produced. Probably the only materials satisfactory for a bomb were either U-235, which would have to be separated from the 140-times more abundant isotope U-238, or Pu-239, an isotope of the hitherto unknown element plutonium, which would have to be generated by a controlled chain-reacting process itself hitherto unknown. To achieve such a controlled chain reaction it was clear that uranium metal and heavy water or beryllium or carbon might have to be produced in great quantity with high purity. Once bomb material was produced a process would have to be developed for using it safely and effectively. In some of the processes, health hazards of a new kind would be encountered.

Policy Problem

2.37. By the summer of 1940 the National Defense Research Committee had been formed and was asking many of the scientists in the country to work on various urgent military problems. Scientific personnel was limited (although this was not fully realized at the time). It was, therefore, really difficult to decide at what rate work should be carried forward on an atomic

bomb. The decision had to be reviewed at frequent intervals during the subsequent four years. An account of how these policy decisions were made is given in Chapters III and V.

CHAPTER III

ADMINISTRATIVE HISTORY UP TO DECEMBER 1941

Interest in Military Possibilities

3.1. The announcement of the hypothesis of fission and its experimental confirmation took place in January 1939, as has already been recounted in Chapter I. There was immediate interest in the possible military use of the large amounts of energy released in fission. At that time American-born nuclear physicists were so unaccustomed to the idea of using their science for military purposes that they hardly realized what needed to be done. Consequently the early efforts both at restricting publication and at getting government support were stimulated largely by a small group of foreign-born physicists centering on L. Szilard and including E. Wigner, E. Teller, V. F. Weisskopf, and E. Fermi.

Restriction of Publication

3.2. In the spring of 1939 the group mentioned above enlisted Niels Bohr's cooperation in an attempt to stop publication of further data by voluntary agreement. Leading American and British physicists agreed, but F. Joliot, France's foremost nuclear physicists, refused, apparently because of the publication of one letter in the Physical Review sent in before all Americans had been brought into the agreement. Consequently publication continued freely for about another year although a few papers were withheld voluntarily by their authors.

3.3. At the April 1940 meeting of the Division of Physical Sciences of the National Research Council, G. Breit proposed formation of a censorship committee to control publication in all American scientific journals. Although the reason for this suggestion was primarily the desire to control publication of papers on uranium fission, the "Reference Committee" as finally set up a little later that spring (in the National Research Council) was a general one, and was organized to control publication policy in all fields of possible military interest. The chairman of the committee was L. P. Eisenhart; other members were G. Breit, W. M. Clark, H. Fletcher, E. B. Fred, G. B. Pegram, H. C. Urey, L. H. Weed, and E. G. Wever. Various subcommittees were appointed, the first one of which had to do with uranium fission. G. Breit served as chairman of this subcommittee; its other members were J. W. Beams, L. J. Briggs, G. B. Pegram, H. C. Urey, and E. Wigner. In general, the procedure followed was to have the editors of various journals send copies of papers in this field, in cases where the advisability of publication was in doubt, either directly to Breit or indirectly to him through Eisenhart. Breit then usually circulated them to all members of the subcommittee for consideration as to whether or not they should be published, and informed the editors as to the outcome. This arrangement was very successful in preventing publication and was still nominally in effect in June 1945, in modified form. Actually the absorption of most physicists in this country into war work of one sort or another soon reduced the number

of papers referred to the committee practically to the vanishing point. It is of interest to note that this whole arrangement was a purely voluntary one; the scientists of the country are to be congratulated on their complete cooperation. It is to be hoped that it will be possible after the war to publish these papers at least in part so that their authors may receive proper professional credit for their contributions.

Initial Approaches to the Government. The First Committee

3.4. On the positive side --- government interest and support of research in nuclear physics --- the history is a much more complicated one. The first contact with the government was made by Pegram of Columbia in March 1939. Pegram telephoned to the Navy Department and arranged for a conference between representatives of the Navy Department and Fermi. The only outcome of this conference was that the Navy expressed interest and asked to be kept informed. The next attempt to interest the government was stimulated by Szilard and Wigner. In July 1939 they conferred with A. Einstein, and a little later Einstein, Wigner, and Szilard discussed the problem with Alexander Sachs of New York. In the fall Sachs, supported by a letter from Einstein, explained to President Roosevelt the desirability of encouraging work in this field. The President appointed a committee, known as the "Advisory Committee on Uranium" and consisting of Briggs as chairman, Colonel K. F. Adamson of the Army Ordnance Department, and Commander G. C. Hoover of the Navy Bureau of Ordnance, and requested this committee to look into the problem. This was the only committee on uranium that had official status up to the time of organization of the National Defense Research Committee in June 1940. The committee met very informally and included various additional scientific representatives in its meetings.

3.5. The first meeting of the Uranium Committee was on October 21, 1939 and included, besides the committee members, F. L. Mohler, Alexander Sachs, L. Szilard, E. Wigner, E. Teller, and R. B. Roberts. The result of this meeting was a report dated November 1, 1939 and transmitted to President Roosevelt by Briggs, Adamson, and Hoover. This report made eight recommendations, which need not be enumerated in detail. It is interesting, however, that it specifically mentions both atomic power and an atomic bomb as possibilities. It specifically recommended procurement of 4 tons of graphite and 50 tons of uranium oxide for measurements of the absorption cross section of carbon. Others of the recommendations either were of a general nature or were never carried out. Apparently a memorandum prepared by Szilard was more or less the basis of the discussion at this meeting.

3.6. The first transfer of funds ($6,000) from the Army and Navy to purchase materials in accordance with the recommendation of November 1st is reported in a memorandum from Briggs to General E. M. Watson (President Roosevelt's aide) on February 20, 1940. The next meeting of the "Advisory Committee on Uranium" was on April 28, 1940 and was attended by Sachs, Wigner, Pegram, Fermi, Szilard, Briggs, Admiral H. G. Bowen, Colonel Adamson, and Commander Hoover. By the time of this meeting two important new factors had come into the picture. First, it had been discovered that the uranium fission caused by neutrons of thermal velocities occurred in the U-235 isotope only.

Second, it had been reported that a large section of the Kaiser Wilhelm Institute in Berlin had been set aside for research on uranium. Although the general tenor of this meeting seems to have been that the work should be pushed more vigorously, no definite recommendations were made. It was pointed out that the critical measurements on carbon already under way at Columbia should soon give a result, and the implication was that definite recommendations should wait for such a result.

3.7. Within the next few weeks a number of people concerned, particularly Sachs, urged the importance of greater support and of better organization. Their hand was strengthened by the Columbia results (as reported, for example, in a letter from Sachs to General Watson on May 15, 1940) showing that the carbon absorption was appreciably lower than had been previously thought and that the probability of carbon being satisfactory as a moderator was therefore considerable. Sachs was also active in looking into the question of ore supply. On June 1, 1940, Sachs, Briggs, and Urey met with Admiral Bowen to discuss approaching officials of the Union Minière of the Belgian Congo. Such an approach was made shortly afterwards by Sachs.

3.8. The general status of the problem was discussed by a special advisory group called together by Briggs at the National Bureau of Standards on June 15, 1940. This meeting was attended by Briggs, Urey, M. A. Tuve, Wigner, Breit, Fermi, Szilard, and Pegram. "After full discussion, the recommendation of the group to the Uranium Committee was that funds should be sought to support research on the uranium-carbon experiment along two lines:

(A) further measurements of the nuclear constants involved in the proposed type of reaction;

(B) experiments with amounts of uranium and carbon equal to about one fifth to one quarter of the amount that could be estimated as the minimum in which a chain reaction would sustain itself.

"It was estimated that about $40,000 would be necessary for further measurements of the fundamental constants and that approximately $100,000 worth of metallic uranium and pure graphite would be needed for the intermediate experiment." (Quotations from memorandum of Pegram to Briggs, dated August 14, 1940)

The Committee Reconstituted under NDRC

3.9. Before any decisions made at this meeting could be put into effect, the organization of the National Defense Research Committee was announced in June 1940, and President Roosevelt gave instructions that the Uranium Committee should be reconstituted as a subcommittee of the NDRC, reporting to Vannevar Bush (chairman, NDRC). The membership of this reconstituted Uranium Committee was as follows: Briggs, Chairman; Pegram, Urey, Beams, Tuve, R. Gunn and Breit. On authorization from Briggs, Breit consulted Wigner and Teller frequently although they were not members of the committee.

From that time until the summer of 1941 this committee continued in control with approximately the same membership. Its recommendations were transmitted by Briggs to the NDRC, and suitable contracts were made between the NDRC and various research institutions. The funds, however, were first supplied by the Army and Navy, not from regular NDRC appropriations.

Support of Research

3.10. The first contract let under this new set-up was to Columbia University for the two lines of work recommended at the June 15th meeting as described above. The project was approved by the NDRC and the first NDRC contract (NDCrc-32) was signed November 8, 1940, being effective from November 1, 1940 to November 1, 1941. The amount of this contract was $40,000.

3.11. Only very small expenditures had been made before the contract went into effect. For example, about $3,200 had been spent on graphite and cadmium, this having been taken from the $6,000 allotted by the Army and Navy in February, 1940.

3.12. We shall not attempt to review in detail the other contracts that were arranged prior to December 1941. Their number and total amount grew gradually. Urey began to work on isotope separation by the centrifuge method under a Navy contract in the fall of 1940. Other contracts were granted to Columbia University, Princeton University, Standard Oil Development Company, Cornell University, Carnegie Institution of Washington, University of Minnesota, Iowa State College, John Hopkins University, National Bureau of Standards, University of Virginia, University of Chicago, and University of California in the course of the winter and spring of 1940-1941 until by November 1941 the total number of projects approved was sixteen, totalling about $300,000.

3.13. Scale of expenditure is at least a rough index of activity. It is therefore interesting to compare this figure with those in other branches of war research. By November 1941 the total budget approved by NDRC for the Radiation Laboratory at the Massachusetts Institute of Technology was several million dollars. Even a relatively small project like that of Section S of Division A of the NDRC had spent or been authorized to spend $136,000 on work that proved valuable but was obviously not potentially of comparable importance to the uranium work.

Committee Reorganized in Summer of 1941

3.14. The Uranium Committee as formed in the summer of 1940 continued substantially unchanged until the summer of 1941. At that time the main committee was somewhat enlarged and subcommittees formed on isotope

separation, theoretical aspects, power production and heavy water.* It was thereafter called the Uranium Section or the S-1 Section of NDRC. Though not formally disbanded until the summer of 1942, this revised committee was largely superseded in December 1941 (see Chapter V).

The National Academy Reviewing Committee

3.15. In the spring of 1941, Briggs, feeling that an impartial review of the problem was desirable, requested Bush to appoint a reviewing committee. Bush then formally requested F. B. Jewett, President of the National Academy of Sciences, to appoint such a committee. Jewett complied, appointing A. H. Compton, chairman; W. D. Coolidge, E. O. Lawrence, J. C. Slater, J. H. Van Vleck, and B. Gherardi. (Because of illness, Gherardi was unable to serve.) This committee was instructed to evaluate the military importance of the uranium problem and to recommend the level of expenditure at which the problem should be investigated.

3.16. This committee met in May and submitted a report. (This report and the subsequent ones will be summarized in the next chapter.) On the basis of this report and the oral exposition by Briggs before a meeting of the NDRC, an appropriation of $267,000 was approved by the NDRC at its meeting of July 18, 1941, and the probability that much larger expenditures would be necessary was indicated. Bush asked for a second report with emphasis on engineering aspects, and in order to meet this request O. E. Buckley of the Bell Telephone Laboratories and L. W. Chubb of the Westinghouse Electrical and Manufacturing Company were added to the committee. (Compton was in South America during the summer and therefore did not participate in the summer meetings of the committee.) The second report was submitted by Coolidge. As a result of new measurements of the fission cross section of U-235 and of increasing conviction that isotope separation was possible, Compton and Lawrence suggested to J. B. Conant of NDRC, who was working closely with Bush, in September 1941, that a third report was desirable. Since Bush and Conant had learned during the summer of 1941 that the British also felt increasingly optimistic, the committee was asked to make another study of the whole subject. For this purpose the committee was enlarged by the addition of W. K. Lewis, R. S. Mulliken, and G. B. Kistiakowsky. This third report was submitted by Compton on November 6, 1941.

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*Uranium Section: Briggs, chairman; Pegram, vice-chairman; S. K. Allison, Beams, Breit, E. U. Condon, H. D. Smyth, Urey.

Separation Subsection: Urey, chairman; Beams.

Power Production Subsection: Pegram, chairman; Allison, Fermi, Smyth, Szilard.

Heavy Water Subsection: Urey, chairman; T. H. Chilton.

Theoretical Aspects Subsection: Fermi, chairman; Breit, C. H. Eckart, Smyth, Szilard, J. A. Wheeler.

Information received from the British

3.17. Beginning in 1940 there was some interchange of information with the British and during the summer of 1941 Bush learned that they had been reviewing the whole subject in the period from April to July. They too had been interested in the possibility of using plutonium; in fact, a suggestion as to the advisability of investigating plutonium was contained in a letter from J. D. Cockcroft to R. H. Fowler dated December 28, 1940. Fowler, who was at that time acting as British scientific liaison officer in Washington, passed Cockcroft's letter on to Lawrence. The British never pursued the plutonium possibility, since they felt their limited manpower should concentrate on U-235. Chadwick, at least, was convinced that a U-235 bomb of great destructive power could be made, and the whole British group felt that the separation of U-235 by diffusion was probably feasible.

3.18. Accounts of British opinion, including the first draft of the British report reviewing the subject, were made available to Bush and Conant informally during the summer of 1941, although the official British report of July 15th was first transmitted to Conant by G. P. Thomson on October 3rd. Since, however, the British review was not made available to the committee of the National Academy of Sciences, the reports by the Academy committee and the British reports constituted independent evaluations of the prospects of producing atomic bombs.

3.19. Besides the official and semi-official conferences, there were many less formal discussions held, one of these being stimulated by M. L. E. Oliphant of England during his visit to this country in the summer of 1941. As an example of such informal discussion we might mention talks between Conant, Compton, and Lawrence at the University of Chicago semi-centennial celebration in September 1941. The general conclusion was that the program should be pushed; and this conclusion in various forms was communicated to Bush by a number of persons.

3.20. In the fall of 1941 Urey and Pegram were sent to England to get first-hand information on what was being done there. This was the first time that any Americans had been to England specifically in connection with the uranium problem. The report prepared by Urey and Pegram confirmed and extended the information that had been received previously.

Decision to Enlarge and Reorganize

3.21. As a result of the reports prepared by the National Academy committee, by the British, and by Urey and Pegram, and of the general urging by a number of physicists, Bush, as Director of the Office of Scientific Research and Development (of which NDRC is a part), decided that the uranium work should be pushed more aggressively.

3.22. Before the National Academy issued its third report and before Pegram and Urey visited England, Bush had taken up the whole uranium question with President Roosevelt and Vice-President Wallace. He summarized for them the British views, which were on the whole optimistic, and pointed

out the uncertainties of the predictions. The President agreed that it was desirable to broaden the program, to provide a different organization, to provide funds from a special source, and to effect complete interchange of information with the British. It was agreed to confine discussions of general policy to the following group: The President, Vice-President, Secretary of War, Chief of Staff, Bush, and Conant. This group was often referred to as the Top Policy Group.

3.23. By the time of submission of the National Academy's third report and the return of Urey and Pegram from England, the general plan of the reorganization was beginning to emerge. The Academy's report was more conservative than the British report, as Bush pointed out in his letter of November 27, 1941, to President Roosevelt. It was, however, sufficiently optimistic to give additional support to the plan of enlarging the work. The proposed reorganization was announced at a meeting of the Uranium Section just before the Pearl Harbor attack and will be described in Chapter V.

Summary

3.24. In March 1939, only a few weeks after the discovery of uranium fission, the possible military importance of fission was called to the attention of the government. In the autumn of 1939 the first government committee on uranium was created. In the spring of 1940 a mechanism was set up for restricting publication of significant articles in this field. When the NDRC was set up in June 1940, the Uranium Committee was reconstituted under the NDRC. However, up to the autumn of 1941 total expenditures were relatively small. In December 1941, after receipt of the National Academy report and information from the British, the decision was made to enlarge and reorganize the program.

CHAPTER IV

PROGRESS UP TO DECEMBER 1941

The Immediate Questions

4.1. In Chapter II the general problems involved in producing a chain reaction for military purposes were described. Early in the summer of 1940 the questions of most immediate importance were:

(1) Could any circumstances be found under which the chain reaction would go?

(2) Could the isotope U-235 be separated on a large scale?

(3) Could moderator and other materials be obtained in sufficient purity and quantity?

Although there were many subsidiary problems, as will appear in the account of the progress made in the succeeding eighteen months, these three questions determined the course of the work.

The Chain Reaction

Program Proposed June 15, 1940

4.2. In June 1940, nearly all work on the chain reaction was concentrated at Columbia under the general leadership of Pegram, with Fermi and Szilard in immediate charge. It had been concluded that the most-easily produced chain reaction was probably that depending on thermal neutron fission in a heterogeneous mixture of graphite and uranium. In the spring of 1940 Fermi, Szilard and H. L. Anderson had improved the accuracy of measurements of the capture cross section of carbon for neutrons, of the resonance (intermediate-speed) absorption of neutrons by U-238, and of the slowing down of neutrons in carbon.

4.3 Pegram, in a memorandum to Briggs on August 14, 1940, wrote, "It is not very easy to measure these quantities with accuracy without the use of large quantities of material. The net results of these experiments in the spring of 1940 were that the possibility of the chain reaction was not definitely proven, while it was still further from being definitely disproven. On the whole, the indications were more favorable than any conclusions that could fairly have been claimed from previous results."

4.4. At a meeting on June 15th (see Chapter III) these results were discussed and it was recommended that (A) further measurements be made

on nuclear constants, and (B) experiments be made on lattices of uranium and carbon containing amounts of uranium from one fifth to one quarter the estimated critical amounts.

Progress up to February 15, 1941

4.5. Pegram's report of February 15, 1941 shows that most of the work done up to that time was done on (A), while (B), the so-called intermediate experiment, was delayed by lack of materials.

4.6. Paraphrasing Pegram's report, the main progress was as follows:

(a) The slowing down of neutrons in graphite was investigated by studying the intensity of activation of various detectors (rhodium, indium, iodine) placed at various positions inside a rectangular graphite column of dimensions 3 x 3 x 8 feet when a source of neutrons was placed therein. By suitable choice of cadmium screens the effects of resonance and thermal neutrons were investigated separately.* A mathematical analysis, based on diffusion theory, of the experimental data made it possible to predict the results to be expected in various other arrangements. These results, coupled with theoretical studies of the diffusion of thermal neutrons, laid a basis for future calculations of the number of thermal and resonance neutrons to be found at any point in a graphite mass of given shape when a given neutron source is placed at a specified position within or near the graphite.

(b) The number of neutrons emitted in fission. The experiments on slowing down neutrons showed that high-energy (high-speed) neutrons such as those from fission were practically all reduced to thermal energies (low speeds) after passing through 40 cm or more of graphite. A piece of uranium placed in a region where thermal neutrons are present absorbs the thermal neutrons and -- as fission occurs -- re-emits fast neutrons, which are easily distinguished from the thermal neutrons. By a series of measurements with and without uranium present and with various detectors and absorbers, it is possible to get a value for the constants, the number of neutrons emitted per thermal

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*The presence of neutrons can be detected by ionization chambers or counters or by the artificial radioactivity induced in various metal foils. (See Appendix 1.) The response of each of these detectors depends on the particular characteristics of the detector and on the speed of the neutrons (e.g., neutrons of about 1.5 volts energy are particularly effective in activating indium). Furthermore, certain materials have very large absorption cross sections for neutrons of particular ranges of speed (e.g., cadmium for thermal neutrons). Thus measurements with different detectors with or without various absorbers give some indication of both the number of neutrons present and their energy distribution. However, the state of the art of such measurements is rather crude.

neutron absorbed by uranium. This is not the number of neutrons emitted per fission, but is somewhat smaller than that number since not every absorption causes fission.

(c) Lattice theory. Extensive calculations were made on the probable number of neutrons escaping from lattices of various designs and sizes. This was fundamental for the so-called intermediate experiment, mentioned above as item (B).

Initiation of New Programs

4.7. Early in 1941 interest in the general chain-reaction problem by individuals at Princeton, Chicago, and California led to the approval of certain projects at those institutions. Thereafter the work of these groups was coordinated with the work at Columbia, forming parts of a single large program.

Work on Resonance Absorption*

4.8. In Chapter II it is stated that there were advantages in a lattice structure of "pile" with uranium concentrated in lumps regularly distributed in a matrix of moderator. This was the system on which the Columbia group was working. As is so often the case, the fundamental idea is a simple one. If the uranium and the moderator are mixed homogeneously, the neutrons on the average will lose energy in small steps between passages through the uranium so that in the course of their reduction to thermal velocity the chance of their passing through uranium at any given velocity, e.g., at a velocity corresponding to resonance absorption, is great. But, if the uranium is in large lumps spaced at large intervals in the moderator, the amounts of energy lost by neutrons between passages from one lump of uranium to another will be large and the chance of their reaching a uranium lump with energy just equal to the energy of resonance absorption is relatively small. Thus the chance of absorption by U-238 to produce U-239, compared to the chance of absorption as thermal neutrons to cause fission, may be reduced sufficiently to allow a chain reaction to take place. If one knew the exact values of the cross sections of each uranium isotope for each type of absorption and every range of neutron speed, and had similar knowledge for the moderator, one could calculate the "optimum lattice," i.e., the best size, shape and spacing for the lumps of uranium in the matrix of moderator. Since such data were only partially known, a direct experimental approach appeared

*The term "resonance absorption" is used to describe the very strong absorption of neutrons by U-238 when the neutron energies are in certain definite portions of the energy region from 0 to 1000 electron volts. Such resonance absorption demonstrates the existence of nuclear energy levels at corresponding energies. On some occasions the term resonance absorption is used to refer to the whole energy region in the neighborhood of such levels.

to be in order. Consequently it was proposed that the absorption of neutrons by uranium should be measured under conditions similar to those expected in a chain-reacting pile employing graphite as moderator.

4.9. Experiments of this type were initiated at Columbia, and were continued at Princeton in February 1941. Essentially the experiment consisted of studying the absorption of neutrons in the energy range extending from a few thousand electron volts down to a fraction of an electron volt (thermal energies), the absorption taking place in different layers of uranium or uranium oxide spheres embedded in a pile of graphite.

4.10. In these experiments, a source of neutrons was provided by a beam of protons (accelerated by a cyclotron) impinging in a beryllium target. (The resulting yield of neutrons was equivalent to the yield from a radium-beryllium source of about 3500 curies strength.) The neutrons thus produced had a wide, continuous, velocity distribution. They proceeded from this source into a large block of graphite. By placing the various uranium or uranium-oxide spheres inside the graphite block at various positions representing increasing distances from the source, absorption of neutrons of decreasing average speeds down to thermal speeds was studied. It was found that the total absorption of neutrons by such spheres could be expressed in terms of a "surface" effect and a "mass" effect.

4.11. These experiments, involving a variety of sphere sizes, densities, and positions were continued until the spring of 1942, when most of the group was moved to Chicago. Similar experiments performed at a later date at the University of Indiana by A. C. G. Mitchell and his co-workers have verified and in some cases corrected the Princeton data, but the Princeton data were sufficiently accurate by the summer of 1941 to be used in planning the intermediate-pile experiments and the subsequent experiments on operating piles.

4.12. The experimental work on resonance absorption at Princeton was done by R. R. Wilson, E. C. Creutz, and their collaborators, under the general leadership of H. D. Smyth; they benefitted from the constant help of Wigner and Wheeler and frequent conferences with the Columbia group.

The First Intermediate Experiments

4.13. About July 1941 the first lattice structure of graphite and uranium was set up at Columbia. It was a graphite cube about 8 feet on an edge, and contained about 7 tons of uranium oxide in iron containers distributed at equal intervals throughout the graphite. A preliminary set of measurements was made on this structure in August 1941. Similar structures of somewhat larger size were set up and investigated during September and October, and the so-called exponential method (described below) of determining the multiplication factor was developed and first applied. This work was done by Fermi and his assistants, H. L. Anderson, B. Feld, G. Weil, and W. H. Zinn.

4.14. The multiplication-factor experiment is rather similar to that already outlined for the determination of η, the number of neutrons produced

per thermal neutraon absorbed. A radium-beryllium neutron source is placed near the bottom of the lattice structure and the number of neutrons is measured at various points throughout the lattice. These numbers are then compared with the corresponding numbers determined when no uranium is present in the graphite mass. Evidently the absorption of neutrons by U-238 to produce U-239 tends to reduce the number of neutrons, while the fissions tend to increase the number. The question is: Which predominates? or, more precisely, Does the fission production of neutrons predominate over all neutron-removal processes other than escape? Interpretation of the experimental data on this crucial question involves many corrections, calculations, and approximations, but all reduce in the end to a single number, the multiplication factor k.

The Multiplication Factor k

4.15. The whole success or failure of the uranium project depended on the multiplication factor k, sometimes called the reproduction factor. If k could be made greater than one in a practical system, the project would succeed; if not, the chain reaction would never be more than a dream. This is clear from the following discussion, which applies to any system containing fissionable material. Suppose that there is a certain number of free neutrons present in the system at a given time. Some of these neutrons will themselves initiate fissions and will thus directly produce new neutrons. The multiplication factor k is the ratio of the number of these new neutrons to the number of free neutrons originally present. Thus, if in a given pile comprising uranium, carbon, impurities, containers, etc., 100 neutrons are produced by fission, some will escape, some will be absorbed in the uranium without causing fission, some will be absorbed in the carbon, in the containers or in impurities, and some will cause fission, thereby producing more neutrons. If the fissions are sufficiently numerous and sufficiently effective individually, more than 100 new neutrons will be produced and the system is chain reacting. If the number of new neutrons is 105, k = 1.05. But if the number of new neutrons per 100 initial ones is 99, k = .99 and no chain reaction can maintain itself.

4.16. Recognizing that the intermediate or "exponential" experiment described above was too small to be chain reacting, we see that it was a matter of great interest whether any larger pile of the same lattice structure would be chain reacting. This could be determined by calculating what the value of k would be for an infinitely large lattice of this same type. In other words, the problem was to calculate what the value of k would be if no neutrons leaked away through the sides of the pile. Actually it is found that, once a chain-reacting system is well above the critical size -- say two or three times as great -- and is surrounded by what is called a reflector, the effective value of k differs very little from that for infinite size provided that k is near 1.00. Consequently, it has become customary to characterize the chain-reaction potentialities of different mixtures of metal and moderator by the value of k_∞ the multiplication constant obtained by assuming infinite size of pile.

4.17. The value of k_∞ as reported by Fermi to the Uranium Section in the fall of 1941 was about 0.87. This was bassed on results from the second

Columbia intermediate experiment. All agreed that the multiplication factor could be increased by greater purity of materials, different lattice arrangements, etc. None could say with certainty that it could be made greater than one.

Experiments on Beryllium

4.18. At about the same time that the work on resonance absorption was started at Princeton, S. K. Allison, at the suggestion of A. H. Compton, began work at Chicago under a contract running from January 1, 1941 to August 1, 1941. The stated objectives of the work were to investigate (a) the increase in neutron production when the pile is enclosed in a beryllium envelope or "reflector", and (b) the cross sections of beryllium. A new contract was authorized on July 18, 1941 to run to June 30, 1942. This stated the somewhat broader objective of investigating uranium-beryllium-carbon systems generally. The appropriations involved were modest: $9,500 for the first contract, and $30,000 for the second contract.

4.19. As has already been pointed out in Chapter II, beryllium has desirable qualities as a moderator because of its low atomic weight and low neutron-absorption cross section; there was also the possibility that a contribution to the number of neutrons would be realized from the (n, 2n) reaction in beryllium. The value of the cross section was not precisely known; furthermore it was far from certain that any large amount of pure beryllium could be obtained. Allison's problem was essentially similar to the Columbia problem, except for the use of beryllium in place of graphite. Because of the scarcity of beryllium it was suggested that it might be used in conjunction with graphite or some other moderator, possibly as a reflector.

4.20. In the Chicago experiments, neutrons produced with the aid of a cyclotron were caused to enter a pile of graphite and beryllium. Allison made a number of measurements on the slowing down and absorption by graphite which were valuable checks on similar experiments at Columbia. He finally was able to obtain enough beryllium to make significant measurements which showed that beryllium was a possible moderator comparable to graphite. However, beryllium was not in fact used at all extensively in view of the great difficulty of producing it in quantity in the required structural form.

4.21. This Chicago project as described above became part of the Metallurgical Laboratory project established at the University of Chicago early in 1942.

Theoretical Work

4.22. Both the intermediate experiments at Columbia and the continued resonance-absorption work at Princeton required skilful theoretical interpretation. Fermi worked out the theory of the "exponential" pile and Wigner the theory of resonance absorption; both these men were constantly conferring and contributing to many problems. Wheeler of Princeton, Breit of Wisconsin, and Eckart of Chicago -- to mention only a few -- also made contributions to general pile theory and related topics. Altogether one can say that by the end of 1941 the general theory of the chain reaction for slow neutrons was

almost completely understood. It was the numerical constants and technological possibilities that were still uncertain.

4.23. On the theory of a fast neutron reaction in U-235 a good deal of progress had also been made. In particular, new estimates of the critical size were made, and it was predicted that possibly 10 percent of the total energy might be released explosively. On this basis one kilogram of U-235 would be equivalent to 2000 tons of TNT. The conclusions are reviewed below in connection with the National Academy Report. It is to be remembered that there are two factors involved: (1) how large a fraction of the available fission energy will be released before the reaction stops; (2) how destructive such a highly concentrated explosion will be.

Work on Plutonium

4.24. In Chapter I mention is made of the suggestion that the element 94, later christened plutonium, would be formed by beta-ray disintegrations of U-239 resulting from neutron absorption by U-238 and that plutonium would probably be an alpha-particle emitter of long half-life and would undergo fission when bombarded by neutrons. In the summer of 1940 the nuclear physics group at the University of California in Berkeley was urged to use neutrons from its powerful cyclotron for the production of plutonium, and to separate it from uranium and investigate its fission properties. Various pertinent experiments were performed by E. Segrè, G. T. Seaborg, J. W. Kennedy, and M. H. Wahl at Berkeley prior to 1941 and were reported by E. O. Lawrence to the National Academy Committee (see below) in May 1941 and also in a memorandum that was incorporated in the Committee's second report dated July 11, 1941. It will be seen that this memorandum includes one important idea not specifically emphasized by others (paragraph 1.58), namely, the production of large quantities of plutonium for use in a bomb.

4.25. We quote from Lawrence's memorandum as follows:

> Since the first report of the National Academy of Sciences Committee on Atomic Fission, an extremely important new possibility has been opened for the exploitation of the chain reaction with unseparated isotopes of uranium. Experiments in the Radiation Laboratory of the University of California have indicated (a) that element 94 is formed as a result of capture of a neutron by uranium 238 followed by two successive beta-transformations, and furthermore (b) that this transuranic element undergoes slow neutron fission and therefore presumably behaves like uranium 235.
>
> It appears accordingly that, if a chain reaction with unseparated isotopes is achieved, it may be allowed to proceed violently for a period of time for the express purpose of manufacturing element 94 in substantial amounts. This material could be extracted by ordinary chemistry and would presumably be the equivalent of uranium 235 for chain reaction purposes.
>
> If this is so, the following three outstanding important possibilities are opened:

1. Uranium 238 would be available for energy production, thus increasing about one hundred fold the total atomic energy obtainable from a given quantity of uranium.

2. Using element 94 one may envisage preparation of small chain reaction units for power purposes weighing perhaps a hundred pounds instead of a hundred tons as probably would be necessary for units using natural uranium.

3. If large amounts of element 94 were available it is likely that a chain reaction with fast neutrons could be produced. In such a reaction the energy would be released at an explosive rate which might be described as a "super bomb".

Radioactive Poisons

4.26. As previously stated, the fragments resulting from fission are in most cases unstable nuclei, that is, artifically radioactive materials. It is common knowledge that the radiations from radioactive materials have deadly effects akin to the effects of X-rays.

4.27. In a chain-reacting pile these radioactive fission products build up as the reaction proceeds. (They have, in practice, turned out to be the most troublesome feature of a reacting pile.) Since they differ chemically from the uranium, it should be possible to extract them and use them like a particularly vicious form of poison gas. This idea was mentioned in the National Academy report (see paragraph 4.48) and was developed in a report written December 10, 1941, by E. Wigner and H. D. Smyth, who concluded that the fission products produced in one day's run of a 100,000 kw chain-reacting pile might be sufficient to make a large area uninhabitable.

4.28. Wigner and Smyth did not recommend the use of radioactive poisons nor has such use been seriously proposed since by the responsible authorities, but serious consideration was given to the possibility that the Germans might make surprise use of radioactive poisons and defensive measures were planned.

Isotope Separation

Small-Scale Separation by the Mass Spectrograph

4.29. In Chapter I the attribution of thermal-neutron fission of uranium to the U-235 isotope was mentioned as being experimentally established. This was done by partly separating minute quantities of the uranium isotopes in A. O. Nier's mass spectograph and then studying the nuclear properties of the samples. Additional small samples were furnished by Nier in the summer of 1941 and studied by N. P. Heydenburg and others at M. A. Tuve's laboratory at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. But results of such experiments were still preliminary, and it was evident that further study of larger and more completely separated

samples was desirable.

4.30. The need of larger samples of U-235 stimulated E. O. Lawrence at Berkeley to work on electromagnetic separation. He was remarkably successful and by December 6, 1941 reported that he could deposit in one hour one microgram of U-235 from which a large proportion of the U-238 had been removed.

4.31. Previously, at a meeting of the Uranium Committee, Smyth of Princeton had raised the question of possible large-scale separation of isotopes by electromagnetic means, but had been told that it had been investigated and was considered impossible. Nevertheless, Smyth and Lawrence at a chance meeting in October 1941 discussed the problem and agreed that it might yet be possible. Smyth again raised the question, at a meeting of the Uranium Committee on December 6th and at the next meeting (December 18, 1941) there was a general discussion of large-scale electromagnetic methods in connection with Lawrence's report of his results already mentioned. The consequences of this discussion are reported in Chapter XI.

The Centrifuge and Gaseous Diffusion Methods

4.32. Though we have made it clear that the separation of U-235 from U-238 might be fundamental to the whole success of the project, little has been said about work in this field. Such work had been going on since the summer of 1940 under the general direction of H. C. Urey at Columbia. Since this part of the uranium work was not very much affected by the reorganization in December 1941, a detailed account of the work is reserved for Chapters IX and X. Only a summary is presented here.

4.33. After careful review and a considerable amount of experimenting on other methods, it had been concluded that the two most promising methods of separating large quantities of U-235 from U-238 were by the use of centrifuges and by the use of diffusion through porous barriers. In the centrifuge, the forces acting on the two isotopes are slightly different because of their differences in mass. In the diffusion through barriers, the rates of diffusion are slightly different for the two isotopes, again because of their differences in mass. Each method required the uranium to be in gaseous form, which was an immediate and serious limitation since the only suitable gaseous compound of uranium then known was uranium hexafluoride. In each method the amount of enrichment to be expected in a single production unit or "stage" was very small; this indicated that many successive stages would be necessary if a high degree of enrichment was to be attained.

4.34. By the end of 1941 each method had been experimentally demonstrated in principle; that is, single-stage separators had effected the enrichment of the U-235 on a laboratory scale to about the degree predicted theoretically. K. Cohen of Columbia and others had developed the theory for the single units and for the series or "cascade" of units that would be needed. Thus it was possible to estimate that about 5000 stages would be necessary for one type of diffusion system and that a total area of many acres of diffusion barrier would be required in a plant separating a

kilogram of U-235 each day. Corresponding cost estimates were tens of millions of dollars. For the centrifuge the number of stages would be smaller, but it was predicted that a similar production by centrifuges would require 22,000 separately driven, extremely high-speed centrifuges, each three feet in length -- at a comparable cost.

4.35. Of course, the cost estimates could not be made accurately since the technological problems were almost completely unsolved, but these estimates as to size and cost of plant did serve to emphasize the magnitude of the undertaking.

Thermal Diffusion in Liquids

4.36. In September 1940, P. H. Abelson submitted to Briggs a 17-page memorandum suggesting the possibility of separating the isotopes of uranium by thermal diffusion in liquid uranium hexafluoride. R. Gunn of the Naval Research Laboratory was also much interested in the uranium problem and was appointed a member of the Uranium Committee when it was reorganized under the NDRC in the summer of 1940. As a result of Abelson's suggestion and Gunn's interest, work was started on thermal diffusion at the National Bureau of Standards. This work was financed by funds from the Navy Department and in 1940 was transferred to the Naval Research Laboratory, still under the direction of Abelson, where it has continued.

4.37. We shall discuss the thermal-diffusion work further in a later chapter, but we may mention here that significant results had already been obtained by the end of 1941 and that in January 1942, using a single separation column, a separation factor had been obtained which was comparable or superior to the one obtained up to that time in preliminary tests on the diffusion and centrifuge methods.

The Production of Heavy Water

4.38. It was pointed out in Chapter II that deuterium appeared very promising as a moderator because of its low absorption and good slowing-down property but unpromising because of its scarcity. Interest in a deuterium moderator was stimulated by experimental results obtained in Berkeley demonstrating that the deuterium absorption cross section for neutrons was, in fact, almost zero. Since oxygen has a very low absorption coefficient for neutrons, it was usually assumed that the deuterium would be used combined with oxygen, that is, in the very convenient material: heavy water. Work at Columbia on possible methods of large-scale concentration of heavy water was initiated in February 1941 under the direction of H. C. Urey (under an OSRD contract). Early in 1941, R. H. Fowler of England reported the interest of the British group in a moderator of deuterium in the form of heavy water and their conviction that a chain reaction would go in relatively small units of uranium and heavy water.

4.39. Urey and A. von Grosse had already been considering the concentration of heavy water by means of a catalytic exchange reaction between hydrogen gas and liquid water. This process depends on the fact that, when isotopic equilibrium is established between hydrogen gas and water, the water

contains from three to four times as great a concentration of deuterium as does the hydrogen gas. During 1941, this exchange reaction between water and hydrogen was investigated and extensive work was done toward developing large-scale methods of producing materials suitable for catalyzing the reaction.

4.40. The further development of this work and of other methods of producing heavy water are discussed in Chapter IX. Like the other isotope-separation work at Columbia, this work was relatively unaffected by the re-organization in December 1941. It is mentioned in preliminary fashion here to indicate that all the principal lines of approach were under investigation in 1941.

Production and Analysis of Materials

4.41. By the end of 1941 not very much progress had been made in the production of materials for use in a chain reacting system. The National Bureau of Standards and the Columbia group were in contact with the Metal Hydrides Company of Beverly, Massachusetts. This company was producing some uranium in powdered form, but efforts to increase its production and to melt the powdered metal into solid ingots had not been very successful.

4.42. Similarly, no satisfactory arrangement had been made for obtaining large amounts of highly purified graphite. The graphite in use at Columbia had been obtained from the U. S. Graphite Company of Saginaw, Michigan. It was of high purity for a commercial product, but it did contain about one part in 500,000 of boron, which was undesirable.

4.43. Largely through the interest of Allison the possibility of increasing the production of beryllium had been investigated to the extent of ascertaining that it would be difficult and expensive, but probably possible.

4.44. Though little progress had been made on procurement, much progress had been made on analysis. The development of sufficiently accurate methods of chemical analysis of the materials used has been a problem of the first magnitude throughout the history of the project, although sometimes overshadowed by the more spectacular problems encountered. During this period C. J. Rodden and others at the National Bureau of Standards were principally responsible for analyses; H. T. Beans of Columbia also cooperated. By 1942 several other groups had started analytical sections which have been continuously active ever since.

4.45. To summarize, by the end of 1941 there was no evidence that procurement of materials in sufficient quantity and purity was impossible, but the problems were far from solved.

Exchange of Information with the British

4.46. Prior to the autumn of 1941 there had been some exchange of reports with the British and some discussion with British scientific representatives who were here on other business. In September 1941, it was decided that Pegram and Urey should get first-hand information by a trip to England. They completed their trip in the first week of December 1941.

4.47. In general, work in England had been following much the same lines as in this country. As to the chain-reaction problem, their attention had focussed on heavy water as a moderator rather than graphite; as to isotope separation, they had done extensive work on the diffusion process including the general theory of cascades. Actually the principal importance of this visit and other interchanges during the summer of 1941 lay not in accurate scientific data but in the general scientific impressions. The British, particularly J. Chadwick, were convinced that a U-235 chain reaction could be achieved. They knew that several kilograms of heavy water a day were being produced in Norway, and that Germany had ordered considerable quantities of paraffin to be made using heavy hydrogen; it was difficult to imagine a use for these materials other than in work on the uranium problem. They feared that if the Germans got atomic bombs before the Allies did, the war might be over in a few weeks. The sense of urgency which Pegram and Urey brought back with them was of great importance.

The National Academy Committee Report

4.48. The appointment of a National Academy committee was mentioned in Chapter III. The committee's first report in May 1940 mentioned (a) radioactive poisons, (b) atomic power, and (c) atomic bombs, but the emphasis was on power. The second report stressed the importance of the new results on plutonium, but was not specific about the military uses to which the fission process might be put. Both these reports urged that the project be pushed more vigorously.

4.49. The third report (November 6, 1941) was specifically concerned with the "possibilities of an explosive fission reaction with U-235." Although neither of the first two National Academy reports indicated that uranium would be likely to be of decisive importance in the present war, this possibility was emphasized in the third report. We can do no better than quote portions of this report.

> Since our last report, the progress toward separation of the isotopes of uranium has been such as to make urgent a consideration of (1) the probability of success in the attempt to produce a fission bomb, (2) the destructive effect to be expected from such a bomb, (3) the anticipated time before its development can be completed and production be underway, and (4) a preliminary estimate of the costs involved.

1. Conditions for a fission bomb

A fission bomb of superlatively destructive power will result from bringing quickly together a sufficient mass of element U-235. This seems to be as sure as any untried prediction based upon theory and experiment can be.

Our calculations indicate further that the required masses can be brought together quickly enough for the reaction to become efficient...

2. Destructive effect of fission bombs

a. Mass of the bomb

The mass of U-235 required to produce explosive fission under appropriate conditions can hardly be less than 2 kg nor greater than 100 kg. These wide limits reflect chiefly the experimental uncertainty in the capture cross section of U-235 for fast neutrons...

b. Energy released by explosive fission

Calculations for the case of masses properly located at the initial instant indicate that between 1 and 5 percent of the fission energy of the uranium should be released at a fission explosion. This means from 2 to 10 x 10^8 kilocalories per kg of uranium 235. The available explosive energy per kg of uranium is thus equivalent to about 300 tons of TNT.

3. Time required for development and production of the necessary U-235

a. Amount of uranium needed

Since the destructiveness of present bombs is already an important factor in warfare, it is evident that, if the destructiveness of the bombs is thus increased 10,000-fold, they should become of decisive importance.

The amount of uranium required will, nevertheless, be large. If the estimate is correct that 500,000 tons of TNT bombs would be required to devastate Germany's military and industrial objectives, from 1 to 10 tons of U-235 will be required to do the same job.

b. Separation of U-235

The separation of the isotopes of uranium can be done in the necessary amounts. Several methods are under development, at least two of which seem definitely adequate, and are approaching the stage of practical test. These are the methods of the centrifuge and of diffusion through porous barriers. Other methods are being investigated or

need study which may ultimately prove superior, but are now farther from the engineering stage.

c. Time required for production of fission bombs

An estimate of time required for development, engineering and production of fission bombs can be made only very roughly at this time.

If all possible effort is spent on the program, one might however expect fission bombs to be available in significant quantity within three or four years.

4. Rough estimate of costs

(The figures given in the Academy report under this heading were recognized as only rough estimates since the scientific and engineering data to make them more precise were not available. They showed only that the undertaking would be enormously expensive but still in line with other war expenditures.)

4.50. The report then goes on to consider immediate requirements and desirable reorganization.

Summary

4.51. At the end of Chapter I we summarized the knowledge of nuclear fission as of June 1940, and in Chapter II we stated the outstanding problems as of the same date. In the light of these statements we wish to review the eighteen months' progress that has just been recounted. The tangible progress was not great. No chain reaction had been achieved; no appreciable amount of U-235 had been separated from U-238; only minute amounts of Pu-239 had been produced; the production of large quantities of uranium metal, heavy water, beryllium, and pure graphite was still largely in the discussion stage. But there had been progress. Constants were better known; calculations had been checked and extended; guesses as to the existence and nuclear properties of Pu-239 had been verified. Some study had been made of engineering problems, process effectiveness, costs, and time schedules. Most important of all, the critical size of the bomb had been shown to be almost certainly within practical limits. Altogether the likelihood that the problems might be solved seemed greater in every case than it had in 1940. Perhaps more important than the actual change was the psychological change. Possibly Wigner, Szilard, and Fermi were no more thoroughly convinced that atomic bombs were possible than they had been in 1940, but many other people had become familiar with the idea and its possible consequences.

Apparently, the British and the Germans, both grimly at war, thought the problem worth undertaking. Furthermore, the whole national psychology had changed. Although the attack at Pearl Harbor was yet to come, the impending threat of war was much more keenly felt than before, and expenditures of effort and money that would have seemed enormous in 1940 were considered obviously necessary precautions in December 1941. Thus it was not surprising that Bush and his associates felt it was time to push the uranium project vigorously. For this purpose, there was created an entirely new administrative organization which will be described in the next chapter.

CHAPTER V

ADMINISTRATIVE HISTORY 1942-1945

5.1. In Chapter III the administrative history of the uranium work up to December 1941 was reviewed. Chapter IV reported the progress of the scientific work up to the same date. The present chapter describes the administrative reorganization that took place in December 1941 and various changes that occurred after that time.

Reorganization of NDRC Uranium Section - Transfer to OSRD

5.2. Two major decisions were required in the further planning of the uranium or atomic-bomb program. These decisions were made by Vannevar Bush, Director of the Office of Scientific Research and Development (which included NDRC), after conference with various scientists and administrators concerned. (See Chapter III.) The decisions were: first, that the possibility of obtaining atomic bombs for use in the present war was great enough to justify an "all out" effort for their development; second, that the existing organization, the NDRC Uranium Section (known as the S-1 Section, and consisting of L. J. Briggs, Chairman; G. B. Pegram, Vice-Chairman; H. T. Wensel, Technical Aide; S. K. Allison, J. W. Beams, G. Breit, E. U. Condon, R. Gunn, H. D. Smyth, and H. C. Urey) was not properly organized for such an effort.

5.3. At a meeting of the National Defense Research Committee on November 28, 1941, Dr. Bush explained why he felt that it was desirable to set up the uranium program outside NDRC. The members of NDRC agreed to a transfer. Accordingly, the NDRC as an organization had no further connection with the uranium program, which was administered for some time thereafter by the OSRD directly through an OSRD S-1 Section, and later through an OSRD S-1 Executive Committee.

5.4. At a meeting of the S-1 Section of OSRD on December 6, 1941, J. B. Conant, speaking for Bush, announced the proposed "all out" effort and the reorganization of the group. The S-1 Section itself had not been formally consulted on the proposed reorganization, but there is no doubt that most of its members were strongly in favor of the new proposals. The membership of the reorganized S-1 Section was as follows:

J. B. Conant, Representative of V. Bush
L. J. Briggs, Chairman
G. B. Pegram, Vice-Chairman
A. H. Compton, Program Chief
E. O. Lawrence, Program Chief
H. C. Urey, Program Chief
E. V. Murphree, Chairman of the separately organized Planning Board
H. T. Wensel, Technical Aide
S. K. Allison
J. W. Beams

G. Breit
E. U. Condon
H. D. Smyth

Formation of the Planning Board

5.5. At the time the S-1 Section was reorganized, Bush also set up a Planning Board to be responsible for the technical and engineering aspects of the work, for procurement of materials and for construction of pilot plants and full-size production plants. This Planning Board consisted of E. V. Murphree (Chairman), W. K. Lewis, L. W. Chubb, G. O. Curme, Jr., and P. C. Keith.

Functions of the Planning Board and OSRD S-1 Section

5.6. It was arranged that contracts for the scientific parts of the work would be recommended to Bush not by the full S-1 Section but by Briggs and Conant after conferences with the program chiefs involved and that recommendations on engineering contracts would be made to Bush by the Planning Board. (The contracts which had been made on behalf of the old Uranium Section had been administered through the NDRC.) Contracts for the development of diffusion and centrifuge separation processes were to be recommended by the Planning Board, which would be responsible for the heavy-water production program also. Bush stated that the Planning Board "will be responsible for seeing to it that we have plans on which to proceed with the next step as expeditiously as possible."

5.7. The scientific aspects of the work were separated from the procurement and engineering phases. The Program Chiefs -- H. C. Urey, E. O. Lawrence, and A. H. Compton -- were to have charge of the scientific aspects. Initially it was proposed that Urey should have charge of the separation of isotopes by the diffusion and the centrifuge methods and of the research work on the production of heavy water. Lawrence was to have charge of the initial production of small samples of fissionable elements, of quantity production by electromagnetic separation methods, and of certain experimental work relating to the properties of the plutonium nucleus. Compton was to have charge of fundamental physical studies of the chain reaction and the measurement of nuclear properties with especial reference to the explosive chain reaction. As an afterthought, he was authorized to explore also the possibility that plutonium might be produced in useful amounts by the controlled chain-reaction method. It was understood, however, that this division of responsibility was to be more precisely defined in later conferences. (The written records of that period do not always give adequate accounts of what was in the minds of the men concerned. In deference to security requirements, references to the importance of plutonium and even to the bomb itself were often omitted entirely.)

5.8. The effect of the reorganization was to put the direction of the projects in the hands of a small group consisting of Bush, Conant, Briggs, Compton, Urey, Lawrence, and Murphree. Theoretically, Compton, Lawrence, Urey, and Murphree were responsible only for their respective divisions of the

program. Each met with Conant and Briggs or occasionally with Bush to discuss his specific problems, or even the overall program.

Meeting of Top Policy Group - Approval of Reorganization

5.9. A meeting of the Top Policy Group, consisting of Vice-President Henry A. Wallace, Secretary of War Henry L. Stimson, and Dr. V. Bush, was held on December 16, 1941. General George C. Marshall and Dr. J. B. Conant, also members of the group, were absent; Mr. H. L. Smith of the Budget Bureau attended. Bush described the reorganization that was in progress and his plans were approved. In a memorandum to Conant describing this meeting, Bush wrote, "It was definitely felt by the entire group that OSRD should press as fast as possible on the fundamental physics and on the engineering planning, and particularly on the construction of pilot plants." Bush estimated the cost of this aspect of the work would be four or five million dollars, and stated the Army should take over when full-scale construction was started, presumably when pilot plants were ready. He suggested the assignment of a technically trained Army officer to become familiar with the general nature of the uranium problem. It was made clear at this meeting that the international relations involved were in the hands of the President, with Bush responsible for liaison on technical matters only.

Meeting of OSRD S-1 Section on December 18, 1941

5.10. On December 18, 1941, a meeting of the reorganized S-1 Section was held. Conant was present and discussed the new policy, which called for an all-out effort. He emphasized that such an effort was justified only by the military value of atomic bombs and that all attention must be concentrated in the direction of bomb development. The whole meeting was pervaded by an atmosphere of enthusiasm and urgency. Several methods of electromagnetic separation were proposed and discussed, and a number of new contracts were recommended.

Meeting of OSRD S-1 Section on January 16, 1942

5.11. Another meeting of the OSRD S-1 Section was held on January 16, 1942. Informal discussions of the various production methods took place, and tentative estimates were made as to when each method would produce results. These forecasts actually were no more than guesses since at that time the scientific information available was very incomplete and the problems of applying such data as did exist to the construction and operation of production plants had hardly been approached.

Rearrangement of the Work Early in 1942

5.12. In the middle of January 1942, Compton decided to concentrate the work for which he was responsible at the University of Chicago. The Columbia group under Fermi and its accumulated material and equipment and the Princeton group which had been studying resonance absorption were moved to Chicago in the course of the spring. Certain smaller groups elsewhere remained active under Compton's direction. Under Lawrence the investigation of large-scale electromagnetic separation was accelerated at the University of

California at Berkeley and a related separation project was started at Princeton. Research and development on the diffusion process and on the production of heavy water continued at Columbia under Urey; under the general supervision of Murphree, the centrifuge work continued at the University of Virginia under Beams while the Columbia centrifuge work was transferred to the laboratories of the Standard Oil Development Co. at Bayway, New Jersey.

Report to the President by Bush on March 9, 1942

5.13. In a report dated February 20, 1942, Conant recommended that all phases of the work be pushed at least until July 1, 1942. Similarly, on March 9, 1942, Dr. Bush sent a report to the President reflecting general optimism but placing proper emphasis on the tentative nature of conclusions. His report contemplated completion of the project in 1944. In addition, the report contained the suggestion that the Army be brought in during the summer of 1942 for construction of full-scale plants.

Reviews of the Program by Conant

5.14. The entire heavy-water program was under review in March and April 1942. The reviews followed a visit to the United States in February and March 1942 by F. Simon, H. Halban, and W. A. Akers from England. In a memorandum of April 1, 1942 addressed to Bush, Conant reviewed the situation and reported on conferences with Compton and Briggs. His report pointed out that extremely large quantities of heavy water would be required for a plutonium production plant employing heavy water instead of graphite as a moderator. For this reason, he reported adversely on the suggestion that Halban be invited to bring to this country the 165 liters of heavy water which he then had in England.

5.15. In a memorandum written to Bush on May 14, 1942 (shortly before a proposed meeting of Program Chiefs), Conant estimated that there were five separation or production methods which were about equally likely to succeed: the centrifuge, diffusion, and electromagnetic methods of separating U-235; the uranium-graphite pile and the uranium-heavy-water pile methods of producing plutonium. All were considered about ready for pilot plant construction and perhaps even for preliminary design of production plants. If the methods were to be pushed to the production stage, a commitment of five hundred million dollars would be entailed. Although it was too early to estimate the relative merits of the different methods accurately, it was presumed that some methods would prove to be more rapid and efficient than others. It was feared, however, that elimination of any one method might result in a serious delay. It was thought that the Germans might be some distance ahead of the United States in a similar program.

5.16. Conant emphasized a question that has been crucial throughout the development of the uranium project. The question was whether atomic bombs would be decisive weapons or merely supplementary weapons. If they were decisive, there was virtually no limit to the amount of effort and money that should be put into the work. The question was complicated by the uncertainty as to how effective the atomic bombs would be.

Change from OSRD S-1 Section to OSRD S-1 Executive Committee

5.17. In May 1942, Conant suggested to Bush that instead of encouraging members of the section individually to discuss their own phases of the work with Conant and Briggs, the OSRD S-1 Section should meet for general discussions of the entire program. Bush responded by terminating the OSRD S-1 Section and replacing it with the OSRD S-1 Executive Committee, consisting of the following:

J. B. Conant, Chairman
L. J. Briggs
A. H. Compton
E. O. Lawrence
E. V. Murphree
H. C. Urey

H. T. Wensel and I. Stewart were selected to sit with the Committee as Technical Aide and Secretary respectively.

5.18. The following members of the old OSRD S-1 Section were appointed as consultants to the new Committee:

S. K. Allison
J. W. Beams
G. Breit
E. U. Condon
H. D. Smyth

5.19. The functions of the new OSRD S-1 Executive Committee were:

(a) To report on the program and budget for the next eighteen months, for each method.

(b) To prepare recommendations as to how many programs should be continued.

(c) To prepare recommendations as to what parts of the program should be eliminated.

5.20. Recommendations relative to matters of OSRD S-1 policy and relative to the letting of OSRD S-1 contracts were made on the basis of a majority vote of the Committee. Conant refrained from voting except in case of a tie vote. While Bush alone had the authority to establish OSRD policies and commit OSRD funds, he ordinarily followed the recommendations of the S-1 Executive Committee.

Report to the President by Bush and Conant on June 17, 1942

5.21. On June 13, 1942, Bush and Conant sent to Vice-President Henry A. Wallace, Secretary of War Henry L. Stimson, and Chief of Staff General George C. Marshall a report recommending detailed plans for the expansion and continuation of the atomic-bomb program. All three approved the report. On

June 17, 1942, the report was sent by Bush to the President, who also approved. The report, which is too long to present in full, contained four principal parts, which dealt with: (a) The status of the development as appraised by the senior scientists; (b) Recommendations by the program chiefs and Planning Board; (c) Comments by Bush, Conant, and General W. D. Styer; (d) Recommendations by Bush and Conant. We may paraphrase parts (a) and (c) as follows:

(a) The status of the program.

(1) It was clear that an amount of U-235 or plutonium comprising a number of kilograms would be explosive, that such an explosion would be equivalent to several thousand tons of TNT, and that such an explosion could be caused to occur at the desired instant.

(2) It was clear that there were four methods of preparing the fissionable material and that all of these methods appeared feasible; but it was not possible to state definitely that any given one of these is superior to the others.

(3) It was clear that production plants of considerable size could be designed and built.

(4) It seemed likely that, granted adequate funds and priorities, full-scale plant operation could be started soon enough to be of military significance.

(c) Comments by Bush, Conant, and General Styer.

Certain recommendations had been made by Lawrence, Urey, Compton, and Murphree. These recommendations had been reviewed by Bush, Conant, and General Styer (who was instructed by General Marshall to follow the progress of the program) and their comments concerning the program were as follows:

(1) If four separate methods all appeared to a highly competent scientific group to be capable of successful application, it appeared certain that the desired end result could be attained by the enemy, provided he had sufficient time.

(2) The program as proposed obviously could not be carried out rapidly without interfering with other important matters, as regards both scientific personnel and critical materials.

A choice had to be made between the military result which appeared attainable and the certain interference with other war activities.

(3) It was unsafe at that time, in view of the pioneering nature of the entire effort, to concentrate on only one means of obtaining the result.

(4) It therefore appeared best to proceed at once with those phases of the program which interfered least with other important war activities. Work on other phases of the program could proceed after questions of interference were resolved.

5.22. The June 13, 1942, report to the President and Bush's transmittal letter dated June 17, 1942, were returned to Bush with the initialled approval of the President. A copy of the report was then sent by Bush to General Styer on June 19, 1942.

Selection of Colonel J. C. Marshall

5.23. On June 18, 1942, Colonel J. C. Marshall, Corps of Engineers, was instructed by the Chief of Engineers to form a new district in the Corps of Engineers to carry on special work (atomic bombs) assigned to it. This district was designated the Manhattan District and was officially established on August 13, 1942. The work with which it was concerned was labeled, for security reasons, the "DSM Project" (Development of Substitute Materials).

Selection of General L. R. Groves

5.24. On September 17, 1942, the Secretary of War placed Brigadier General L. R. Groves of the Corps of Engineers in complete charge of all Army activities relating to the DSM Project.

Military Policy Committee; Functioning of the OSRD Committees

5.25. A conference was held on September 23, 1942, among those persons designated by the President to determine the general policies of the project, and certain others. Those present were Secretary of War Henry L. Stimson, Chief of Staff General George C. Marshall, Dr. J. B. Conant, Dr. V. Bush, Major General Brehon Somervell, Major General W. D. Styer, and Brigadier General L. R. Groves. (Vice-President Henry A. Wallace was unable to attend.) A Military Policy Committee was appointed consisting of Dr. V. Bush as Chairman with Dr. J. B. Conant as his alternate, Major General W. D. Styer, and Rear Admiral W. R. Purnell. General Groves was named to sit with the committee and act as Executive Officer to carry out the policies that were determined. The duties of this committee were to plan military policies relating to materials, research and development, production, strategy, and tactics, and to submit progress reports to the policy group designated by the President.

5.26. The appointment of the Military Policy Committee was approved by the Joint New Weapons Committee, established by the U. S. Joint Chiefs of Staff and consisting of Dr. V. Bush, Rear Admiral W. R. Purnell, and Brig. General R. G. Moses.

5.27. The creation of the Military Policy Committee in effect placed all phases of the DSM Project under the control of Dr. Bush, Dr. Conant, General Styer, Admiral Purnell, and General Groves.

5.28. The OSRD S-1 Executive Committee held meetings about once every month from June 1942 to May 1943 and once after that time, in September 1943. These meetings were normally attended by General Groves, after September 1942, and Col. Marshall, and frequently by representatives of the industrial companies concerned with the production plants. Recommendations of the Committee were not binding but were usually followed. Thus it served as an advisory body to Dr. Bush and General Groves, and as an initial liaison group between the scientific, industrial, and military parts of the DSM Project. The S-1 Executive Committee has never been formally dissolved, but it has been inactive since the fall of 1943.

5.29. The procurement and engineering functions of the Planning Board were taken over by the Manhattan District in the summer of 1942 and that board then became inactive.

5.30. By the spring of 1943 it was felt that the Manhattan District was in a position to take over research and development contracts from the OSRD. Such a transfer was effected as of May 1, 1943, and marked the end of the formal connection of OSRD with the uranium project.

5.31. In July 1943 Conant and R. C. Tolman were formally asked by General Groves to serve as his scientific advisers. They had already been doing so informally and have continued to do so. Coordination of the various scientific and technical programs was accomplished by meetings between General Groves and the leaders of the various projects, in particular, Compton, Lawrence, Oppenheimer (see Chapter XII), and Urey.

Subsequent Organization; the Manhattan District

5.32. Since 1943 there have been no important changes in the form of the organization and few of importance in the operating personnel. General Groves has continued to carry the major responsibility for correlating the whole effort and keeping it directed toward its military objectives. It has been his duty to keep the various parts of the project in step, to see that raw materials were available for the various plants, to determine production schedules, to make sure that the development of bomb design kept up with production schedules, to arrange for use of the bombs when the time came, and to maintain an adequate system of security. In discharging these duties General Groves has had the help of his tremendous organization made up of civilian scientists and engineers and Engineer officers and enlisted men. Many of the civilians have been mentioned already or will be mentioned in later chapters dealing with particular projects. Brigadier General T. F. Farrell has acted as General Groves' deputy in the important later phases of the

of the project. Colonel K. D. Nichols, the District Engineer of the Manhattan District with his headquarters at the Clinton Engineer Works, has been connected with the project since 1942. He has been concerned with the research and production problems of both U-235 and plutonium and has always shown exceptional understanding of the technical problems and their relative importance. Two other officers who should be mentioned are Colonel F. T. Matthias and Colonel S. L. Warren. Colonel Matthias has discharged major responsibilities at the Hanford Engineer Works in an extremely able manner; his duties have been concerned with both the construction and operational phases of the project. Colonel Warren is chief of the Medical Section of the Manhattan District and therefore has had ultimate responsibility for health problems in all parts of the project.

Summary

5.33. By the end of 1941 an extensive review of the whole uranium situation had been completed. As a result of this review Bush and his advisors decided to increase the effort on the uranium project and to change the organization. This decision was approved by President Roosevelt. From January 1942 until early summer of 1942 the uranium work was directed by Bush and Conant working with the Program Chiefs and a Planning Board. In the summer of 1942 the Army, through the Corps of Engineers, was assigned an active part in the procurement and engineering phases, organizing the Manhattan District for the purpose. In September 1942, Dr. Bush, Dr. Conant, General Styer, and Admiral Purnell were appointed as a Military Policy Committee to determine the general policies of the whole project. Also in September, General Groves was appointed to take charge of all Army activities of the project. The period of joint OSRD and Army control continued through April 1943 with the Army playing an increasingly important role as the industrial effort got fully under way. In May 1943 the research contracts were transferred to the Corps of Engineers; the period of joint OSRD-Army control ended and the period of complete Army control began.

5.34. Since the earliest days of the project, President Roosevelt had followed it with interest and, until his death, he continued to study and approve the broad programs of the Military Policy Committee. President Truman, who as a United States Senator had been aware of the project and its magnitude, was given the complete up-to-date picture by the Secretary of War and General Groves at a White House conference immediately after his inauguration. Thereafter the President gave the program his complete support, keeping in constant touch with the progress.