The Work of Many People

Edward Teller

Department of Physics, University of California, Berkeley

A MODERN technical and scientific development is rightly considered a wonderfully complex and difficult undertaking. The final intricate product has evidently required the greatest refinements of the art of engineering. The engineering phase has to be preceded by an experimental period of trials and adjustments, and even the very conception and theory of the device are rooted in many scientific thoughts and a mass of detailed calculations. Hundreds of ideas and thousands of technical skills are required for success. The hydrogen bomb is an achievement of this kind. It is the work of many excellent people who had to give their best abilities for years and who were all essential for the final outcome.

The story that is often presented to the public is quite different. One hears of a brilliant idea and only too often the name of a single individual is mentioned. This picture is both untrue and unjust. If one emphasizes the interaction of many different minds, one comes closer to the real life and the real excitement of exploration.

Over a number of years I have been closely associated with the development of the hydrogen bomb. I would like to attempt to give a picture of the many-sided efforts that went into this work. I cannot do so with any completeness. I can write only about those aspects of which I happen to be best informed: the conception, the theory, and the calculations. In the nature of things these represent only the beginning of the actual development, and they are not by any means the most important part of the work. I hope that there will be an occasion for others to tell the part of the story where tangible structures started to take the place of fantasies, sketches, and the long rows of formulas and figures.

The story cannot be rightly told without mentioning many of the people whose contributions made the hydrogen bomb possible, but it is even more difficult to attempt any kind of evaluation of the importance of each contribution. I shall mention names and incidents merely as examples of the kind of work that is needed in the close cooperation of which scientific and technologic developments consist. Perhaps this story will recall to some the adventure of trying to do what at one time seemed impossible.

The parts of the story that are most worth remembering are the positive contributions rather than the many mistakes that always necessarily occur in a complex undertaking. However, nature is patient and in the end only those mistakes count which in turn helped to point a way toward the correct ideas. It is the scientific tradition to emphasize what was good in a development, and it is this kind of tradition that makes the history of science so inspiring and accounts for much of the good fellowship among scientific workers.

The Sun and Other Stars

George Gamow escaped from Soviet Russia in 1933 and came to George Washington University in 1934. He had many interesting stories to tell. One of them is the following.

Six years before his arrival in the United States, he reported in the U.S.S.R. Academy of Sciences a paper by the British physicist, Atkinson, and the German physicist, Houtermans. They suggested that the apparently inexhaustible store of energy radiated by the sun and by other stars is due to reactions between atomic nuclei. These particles, tiny even compared with atoms, are known to contain an energy a million times greater than that released in chemical reactions or explosions. Yet they are able to release this energy only when they come in contact with each other. Their electric charges usually prevent contact between them and thus the energy that they have been carrying for billions of years is preserved. In the deep interior of the stars, however, exceedingly high temperatures exist. Owing to the thermal agitation, occasional collisions between the nuclei do occur, and these nuclear reactions ultimately give rise to the brilliance of the stars and to the radiation of our sun.

After Gamow finished his lecture he was approached by a very high Soviet official, Bukharin. By that time Bukharin had lost his real influence and had the job of keeping an eye on scientific developments. A few years later he was to be executed. After the lecture he asked Gamow whether nuclear processes similar to those occurring in the sun could be harnessed to some direct application here on earth. He offered to turn over to Gamow the Electric Works of Leningrad for a few hours at nighttime if that would help in the job. Gamow said that the practical job could not be done, but he remembered this occurrence and he kept his interest in the question of stellar energies.

Of course, we possess no direct knowledge about the interior of stars. Yet astrophysicists, starting with Eddington, had a pretty accurate knowledge of the conditions in those completely inaccessible regions. It may be puzzling to hear that science, which is practically unable to predict properties of matter in its common form encountered on earth, should be able to state with high accuracy how matter behaves inside the stars. The reason is a simple one. At the relatively low temperatures prevailing around us, the properties of materials are determined by a sensitive balance between the attractions of the constituents of atoms and the energy of motion of these same particles. In the stellar interiors the temperatures are extremely high and the balance is destroyed. The atomic constituents —that is, the electrons and the nuclei—rush around at high velocities along straight lines, and the forces between these particles have little effect upon their motion. Thus matter, which in our common experience has many intricate and varied appearances and properties, behaves in a uniform and predictable manner in the inside of the hot gas balls which we call the stars.

The stars lose energy continuously and this energy must be replenished. Atkinson and Houtermans merely pointed out that the most probable source of this energy is the atomic nucleus itself. It was as yet unclear which of the great many possible reactions between nuclei give rise to the solar and stellar energy.

Gamow, with his wonderful sense for interesting problems, and with his contagious curiosity about the structure of the universe, stirred up quite a few of the physicists who had previously considered the interior of stars a too remote question. This soon led to the exploration of thermonuclear reactions, a long word which now has become quite familiar and which means the reaction of nuclei occurring at high temperatures. At that time, the late 1930's, the discussions and work were carried on with no idea of any practical applications. It was done merely to satisfy what to many would appear idle curiosity.

The first candidate for any thermonuclear reaction was the lightest of elements, hydrogen. In the sun and the stars this element appears to be by far the most abundant. Nuclei of hydrogen, by fusing into bigger nuclei, could release energies that are rather big even when compared with energies of most nuclear reactions. What is most important, hydrogen nuclei carry less charge than any other nucleus and therefore can approach each other more easily. Under the influence of Gamow's prodding, a small group of physicists and astronomers met at George Washington University and the Carnegie Institution in Washington in the spring of 1938. We had one of those disorganized discussions that we call a conference, which seem to lead nowhere but which often in the past had a great influence upon the development of science.

The conference did little more than pose the problems with some clarity, but the solution followed within the next few months. Hans Bethe, Charles Critchfield, and Gamow succeeded, not only in determining what reactions keep the stars going, but also in reconstructing how stars develop, change their appearance, and finally exhaust their sources of energy. The most remarkable part of this job was done by Bethe, who made a systematic study of all conceivable thermonuclear reactions, cataloging all the relatively meager experimental data of that day and supplementing them by wonderfully enlightened guesses about all the relevant nuclear reactions not yet experimentally explored. He found that, in addition to the possibility of hydrogen nuclei reacting with each other, one has to consider the reactions between hydrogen and carbon nuclei. His treatment of these reactions was so complete that in the next decade nothing useful could be added to his enumeration. Gamow had invented a new kind of game for the physicists, and Bethe proved to be the champion at it.

Conception of the Super

No one expected to be able to approach the conditions of the solar interior in any of our experiments. No container would have withstood the temperatures; no familiar source could deliver the energy in the necessary concentrated form. Then, in December 1938, Otto Hahn and Lise Meitner discovered fission.

It is now well known how fission releases energy. The neutron, a nuclear particle that carries no charge, can approach any nucleus even when no high temperatures are present. Some of the heaviest nuclei split under the impact of a neutron and produce more neutrons in the process. Thus a chain of reactions results, and the immense temperature and pressure of an atomic bomb can be produced.

Several years before Hiroshima, scientists started to wonder whether the high temperatures that were expected to occur in an atomic bomb could be utilized to start reactions similar to those that are proceeding within the sun. To me, this idea was first mentioned with some emphasis by Enrico Fermi. He proposed, in particular, to consider the reactions of heavy hydrogen rather than the reaction of the normal abundant light hydrogen. This heavy hydrogen, or deuterium, is present in ordinary hydrogen in the amount of 1 part in 5000, but it can be separated from the light hydrogen by some processes that are not altogether too costly. Furthermore it was known that the heavy hydrogen nuclei react with each other much more easily than those of light hydrogen. Therefore, the substitution of deuterium for hydrogen would be a long step toward realizing thermonuclear reactions under experimental conditions.

At that time, in the spring of 1942, both Fermi and I were at Columbia University. Physics had moved closer to the grim realities of war. Many of us had started to work on the fission bombs. It had become clear that these atomic bombs would be powerful but expensive. If deuterium could be ignited, it would give a much less expensive fuel.

After a few weeks of hard thought, I decided that deuterium could not be ignited by atomic bombs. I reported my results to Fermi and proceeded to forget about it.

In the early summer I found myself at the Metallurgical Laboratory of Chicago and in the company of Emil Konopinski, another physicist who had started to work on atomic energy. In the bustling laboratory of Chicago we were newcomers and at least for a few days we had no concrete job. I suggested that we go over my arguments about the thermonuclear reactions and that we make a conclusive written report that heavy hydrogen would be of no use in bombs. The more we tried, the harder it seemed few days we found some loopholes that seemed to into arrive at a definite conclusion. In fact, within a dicate that deuterium could be ignited, after all.

In the meantime, Robert Oppenheimer gathered around himself in Berkeley a small group of theoretical physicists for the purpose of investigating the properties and behavior of atomic bombs. This group included Van Vleck, Felix Bloch, Stanley Frankel, Bethe, and Robert Serber. Konopinski and I joined the group when it was just being formed, and all of us were soon engaged in the distant but absorbing question of whether deuterium could be exploded.

It is hard to describe the intensity and the fascination of the discussion that followed. We were again dealing with conditions of high temperature completely unknown to experiment but open to theoretical predictions because of the very simplicity of the types of motion occurring under those conditions. The experience proved perhaps even more challenging than the previous discussion about the interior of the sun. Here we were not bound by the known conditions in a given star but we were free within considerable limits to choose our own conditions. We were embarking on astrophysical engineering.

As fact after fact emerged and was clarified, the prospects changed. One day the job looked hopeless, the next day it seemed easy, only to turn out again to be practically impossible on account of some considerations that had not been previously included. All of us contributed to the discussion, but without Konopinski and Bethe no real progress would have been made. I remember particularly the suggestion of Konopinski that the reactions of tritium should be investigated. At that time it was a mere guess. It turned out to be an inspired one. Bethe subjected all the relevant factors to the same kind of exhaustive scrutiny by which he had clarified the thermonuclear reactions occurring in the stars. By the middle of the summer of 1942, we were all convinced that the job could be done and that it would be relatively easy.

The spirit of spontaneity, adventure, and surprise of those weeks in Berkeley was never recaptured for me in the many years of hard work in which atomic bombs were developed. As the problems inevitably grew, as they came closer to the realities of engineering and hardware, exploration had to be replaced by schedule and spontaneous exchange of ideas by organization. I am sure that all the participants in those discussions still remember vividly the days when we thought that the atomic bomb could be easily used for a stepping-stone toward a thermonuclear explosion, which we called a "Super" bomb.

Certainly Difficult, Perhaps Possible

When Los Alamos was established in the spring of 1943, the exploration of the Super was among its objectives. Within a year, however, the picture changed completely. This was due to the fact that both the atomic bomb and the Super bomb proved to be more difficult than had been expected. Our discussion of the thermonuclear reactions proved to be incomplete, and it became clear that to make a Super would be difficult, if not impossible. At the same time, it became clear that the construction of the A-bomb was a much bigger job than we anticipated, and yet this had to be done before our enemies could do it. That it was done in time to have an influence upon the war was to a great extent due to the leadership of our director, Oppenheimer. He knew what was going on in every corner of the big laboratory and was prompt both in his understanding and in his encouragement. In his office there was a poster with Lincoln's picture, carrying the modified quotation, "This world cannot exist half slave and half free." It was hardly necessary, and yet helpful to remind everybody in the laboratory of this fact. We had to win the war and there was no time for the Super.

In spite of the urgency of the situation, Oppenheimer did not lose sight of the more distant possibilities. He continued to urge me with detailed and helpful advice to keep exploring what lay beyond the immediate aims of the laboratory. This was not easy advice to give, nor was it easy to take. It is easier to participate in the work of the scientific community, particularly when a goal of the highest interest and urgency has been clearly defined. Every one of us considered the present war and the completion of the A-bomb as the problems to which we wanted to contribute most. Nevertheless, Oppenheimer, Fermi, and many of the most prominent men in the laboratory continued to say that the job at Los Alamos would not be complete if we should remain in doubt whether or not a thermonuclear bomb was feasible. Furthermore, the purely scientific aspects of the Super were so fascinating that the problem continued to attract attention even in the hectic days in which our efforts on the atomic bomb approached completion and success. Thus in early 1945 a small but very able group started to concentrate its efforts on the thermonuclear bombs.

Most of this work continued to be pure theory, but there was less discovery and more quantitative evaluation. A number of talented young people joined our group. One of Bethe's students, Henry Hurwitz, proved that he had learned from his professor how to be systematic and ingenious. Two students from George Washington University, Geoffrey Chew and Harold Argo, interrupted their studies and came to Los Alamos to help us. Anthony Turkevich from Chicago contributed his knowledge of the theory of chemical reactions. Rolf Landshoff, a refugee from Germany, was the only one of the group who was going to stay at Los Alamos uninterruptedly from those days up to the present time. Two mathematicians, Stan Ulam and Jack Calkin, started to make calculations which even to a theorist seemed abstract. Nicholas Metropolis became interested in the use of computing machines, which in the later development turned out to be of great importance.

The experimental approach was not completely neglected. Early measurements by John Manley, Elizabeth Graves, Marshall Holloway, and Charles Baker were continued by Egon Bretscher and other members of the British contingent. They, as well as our other British friends, participated without any restrictions in our great common work at Los Alamos.

Some of the most famous men in the laboratory kept in very close touch with our work and helped with frequent suggestions and criticism. One was Fermi, a physicist equally eminent in theory and experiment, the other was John von Neumann, one of the rare mathematicians who can descend to the level of a physicist.

In spite of all these contributions, no definite answer was reached. As the months went by, we still did not know whether the job could be done. But, paradoxical as this may seem, our very lack of certainty was based on a broader and more secure foundation. At the same time, all these people became acquainted with the increasingly complex arguments, and thus many of them could prepare themselves for further contributions in the future.

The most important part of all this work, however, was focused on one man, Konopinski. It was he who brought newcomers up to date, who made sure that none of the questions of which we were aware should go unexplored, and who finally made sure that our accumulating knowledge was preserved in clear and usable documents. Together with a young physicist, Cloyd Marvin, Jr., he also completed a strange and important task. He proved that a thermonuclear reaction, even if initiated on the earth, could not spread under any circumstances. It was necessary to prove, and he did prove, that the Super bomb could not ignite the atmosphere or the ocean. Later, his work was reviewed by one of the most conscientious, meticulous and painstaking physicists, Gregory Breit. It was clearly necessary to prove this point beyond the shadow of any doubt and it was so proved.

Then, in the summer of 1945, the work of the laboratory culminated in complete and terrifying success. The war was ended and the temper of the country and of the physicists seemed to preclude any further great efforts on the thermonuclear bomb. Some members of the wartime group, however, stayed on to prepare a summary review of the possibilities. It was Frankel and Metropolis who worked hardest and longest in preparing this report on the feasibility of the Super. The verdict was: Difficult, but with hard work, hopeful.

Hibernation

For several months after the end of the war it seemed likely that the Los Alamos Laboratory would be discontinued. Such an event would have been most dangerous for the security of the United States. That it did not happen was due to the effort of a few determined people who considered it their duty to try to keep Los Alamos alive, whatever the odds might be. The man whose leadership was crucial in those days was the new director, Norris Bradbury.

To keep Los Alamos alive was an uphill fight which remains clear in the memory of both those who stayed and those who left. One nontechnical event of great importance which all of us remember was the water shortage. In the fall of 1945 the snowfall came late, but frost came early. The water pipes froze and soon water had to be brought up the hill in trucks. We lacked ample water, one of the vital elements of civilization; this great discomfort continued into Christmas of 1945 and beyond. Los Alamos was a town of young people and there were many babies. Mothers started to wonder about dangers of epidemics, which fortunately never materialized. Many who had hesitated decided to leave Los Alamos. I recall one detail that may seem insignificant. During the war I developed, somewhat to my own surprise, two affections: a liking for strangers and the love of green grass. Both were in short supply. In the water shortage of 1945 the grass was dying.

Throughout all these difficulties Bradbury stayed on, kept smiling and encouraged others to stay with the job. Without his dedicated work the Russians would now be ahead of us in the atomic race.

I was one of the majority who returned at that time to academic work. The very success of the atomic bomb had raised some obstacles to the continuation of work on the thermonuclear weapon. There were those who felt that it would take a lifetime before the brilliant achievements of the war work could be improved. Of our thermonuclear group, only Landshoff remained, and most of his work was required for more immediate problems. Ulam left the laboratory for a short period and then returned to develop the Monte Carlo method, a highly successful procedure to deal with statistical problems by investigating individual happenings rather than the mass of the data. This fine work, however, was unrelated to the work on the Super. Ulam's contributions in that field came later. Thus, of the small group of experts whose skill was developed during the war, not one continued to devote his full time and energy to the next big problem in atomic weapons.

However, the idea of the Super bomb survived as a challenge and as a future task for Los Alamos. An exceedingly small group, headed by Robert Richtmyer, started to take over where others left off. They kept the spark alive, and their work should therefore be particularly remembered. In the following years I made many visits to Los Alamos and kept in close touch with the work of these people. From the very beginning this work had assumed a new direction and acquired a new style.

In the development of the atomic bomb, use of automatic computing machines had played an important role. It was essential that atomic bombs should be available without a lengthy preliminary period of experimentation. Furthermore, small-scale experimentation, similar in function to that of a pilot plant in industry, was out of the question in connection with atomic bombs: If you try to make a small atomic explosion you are likely to get no atomic explosion. Therefore, theoretical predictions had to be particularly well considered and carried out in meticulous detail. This would have been most difficult without the extensive use of big computing devices. In the postwar period the theorists of the Super bomb turned their interest toward the most advanced computing machines.

In the months following the explosion of the first atomic weapons, Frankel and Metropolis started work on the new fast electronic computer in Philadelphia, the ENIAC. Their work was soon taken over by Richtmyer, Foster and Cerda Evans, and a few others. At the same time von Neumann, together with many excellent people throughout the country, was working hard at further plans and improvements of the fast electronic computers. After an absence of more than a year, Metropolis rejoined Los Alamos and started to build the MANIAC (which is supposed to mean Mathematical Analyzer Numerical Integrator And Computer). Richtmyer became interested in these computational methods and became one of the real masters in handling the machines.

A fast computer, while very efficient, needs detailed instructions, and it is quite an art to transform a mathematical problem into a set of symbols that will make the machine operate properly. Furthermore, this set of symbols is hardly ever free from errors, so that after a problem is fed into the machine the first set of answers usually makes no sense. There must follow a period of "debugging" in which the very answers obtained lead to the discovery of mistakes in the original instructions. To make matters worse, the machine itself makes occasional mistakes and these may get confused with errors in the instructions.

In the normal course of operating a computing machine, several people cooperate: the physicist who sets up the problem, the mathematician who provides the rigorous formulation, the coder who "explains" the problem to the machine, the machine operator who straightens out the errors, and then the physicist again who uses the answers to set up the next problem. Richtmyer argued that all these operations can be performed by a single man more efficiently, and he proceeded to demonstrate that this could be done. This style of computation is practiced at present by many able people.

Work on a difficult subject such as a Super bomb depends on the mutual help and encouragement of at least a few people. When the group is small there is danger that the effort will cease altogether; but in the case of the thermonuclear effort the small group of people kept on increasing, even though the increase was slow. The evident importance and scientific interest of the problem caught and held the imagination of additional workers. Frank Hoyt, a professor at the University of Chicago, visited from time to time to help out. Later he joined Los Alamos on a permanent basis. His quiet and devoted work contributed much to the sustained effort. Some of my students at Chicago also became interested and joined the laboratory after completing their graduate work. Harris Mayer and John Reitz were to make lasting contributions, and Marshall Rosenbluth eventually became one of the key men who carried the calculations to their successful conclusion.

I particularly remember a short visit from Lothar Nordheim from Duke University. He is a man who is likely to sit through a dinner among friends without participating in the conversation and who is likely to come up toward the end with just one remark. Later it turns out that one can recall, of the whole evening, only Nordheim's comment. His work proved to be of the same quality. He started it in the lean years. Later he joined Los Alamos for an extended period, always working inconspicuously either alone or with very few people. In the beginning his effort did not seem to amount to much. In the end it turned out to be one of the really significant contributions.

In the middle of 1949 I went back to Los Alamos to spend a considerable length of time there. In the preceding 3 years the laboratory had recovered to a remarkable extent. We had lost somewhat in competence as compared with the high-powered crew that was available at the end of the war, but we had progressed in some ideas, and the technique of big-scale computations had been developed. Then the Soviet bomb brought the realization that the arms race was no longer a possibility but a frightening reality.

The Crisis

It is clear that the hydrogen bomb would not have been built except for the efforts of a considerable number of people whose contributions ranged from political decisions to organization and on into the scientific work. My knowledge and appreciation are, of course, greatest in connection with this last phase, which is closest to my own interests. I shall talk only about the work that concerned equations and atoms and will stop short of blueprints and bombs. About the latter I do not know enough to give a just description. About the political decisions that had to be made after the explosion of the Soviet bomb, I know even less. My direct experience is limited to the few occasions when I was asked to give my opinion on technical possibilities and probabilities; but I feel that great gratitude is due to the men who in those difficult weeks arrived at the correct conclusions.

The decision concerning how to respond to the threat of a Soviet bomb was not an easy one. At Los Alamos there was a widespread feeling that the laboratory should turn to the development of the hydrogen bomb. During the war it had been understood that this possibility must be explored. Many people felt that the time for this had come. In the administration of the laboratory the first to make a concrete and determined effort toward planning a big-scale approach was Darol Froman. A 6-day work week was adopted upon the urging of Holloway and others.

However, the center of the hydrogen bomb activity remained for some time in the theoretical group on which plans had to depend. This group, although small, was of high quality and expanded rapidly.

Much of the credit for its build-up and successful activity must go to its able organizer and leader, Carson Mark.

Frederic de Hoffmann had joined the laboratory in the early part of 1949. Even before the Soviet explosion he felt that the hydrogen bomb must be our main task. Now he acted like a man who has been freed from a terrible restriction. He was the kind of associate who would never let me forget the importance of the job that we were doing, and I am sure that my own effectiveness depended greatly on his skill, devotion, and example. Nordheim joined the laboratory to explore further the consequence of his earlier work. John Wheeler from Princeton interrupted a well-deserved sabbatical leave in Europe and, together with some of his students, plunged into furious and effective activity. Roy Goranson helped to maintain contact between the theory and the practical execution. James Tuck shelved his greatest interest, peacetime applications of atomic energy, and devoted himself to the urgent phase of the program. Together with a group of able experimental physicists, he made some measurements of vital interest to the thermonuclear program. Von Neumann and Fermi helped, if less frequently, no less effectively than during the war.

Our most urgent task was to reconsider with the greatest possible rigor the favored design of the hydrogen bomb. We intended to do this with the help of the high-speed computing machines. The best of these, however, were not yet operating at the time, and the calculation was set up on the ENIAC, which in the intervening years had been moved to the Aberdeen Proving Grounds. Ulam, with the able help of another mathematician, Cornelius Everett, undertook to execute the same job by straightforward hand computation. The next few months saw an amazing competition between the tortoise and the (electronic) hare.

The big modern computing machines open up possibilities of complex calculations which seemed to be beyond our reach only a few years ago, but real mathematical ingenuity, coupled with hard work, can on some occasions overcome computational difficulties with even greater success than the best apparatus so far invented. This is precisely what happened in the case of Ulam's calculation. It proceeded with a speed that surpassed all expectations. Results were available even before the lengthy instructions to the machines had been completed. Those who like to contrast ingenuity and endurance of the human brain with the lightning speed of standard operations on a machine will be able to conclude: In a real emergency the mathematician still wins—if he is really good.

Ulam's first partial results were disquieting; the more complete answers, most discouraging. I felt at the time that these calculations, which seemed to be in conflict with earlier results obtained on machines, were hard to believe. In actual fact they were correct, and they served a most important purpose in alerting us at an early date to the difficulties that we were facing. A few weeks later, when the more detailed and accurate results from the machine were in, it became completely clear that the plans which we had considered most hopeful had to be revised.

The probable success of a radically new device such as the hydrogen bomb is not likely to depend on one particular line of approach. Real progress depends on the complete understanding of the field and on the efficiency of methods that apply this understanding to detailed designs. It furthermore depends on experiments and tests to compare the theoretical knowledge with reality. The work of the years that had passed since 1942 had left us with a great store of knowledge of the principles and methods, but the calculations of Ulam and Everett deprived us of the best example of a device to which we could point and say: This is how we actually want to do it.

It is clear that there had to be discouragement. The remarkable thing is that the majority of the people engaged in the work at Los Alamos kept on working hard and with a good spirit. This included almost all the theorists who had been working on the project.

The plan for a complex apparatus like a hydrogen bomb is not tied to one single design. There are many possibilities and each possibility can be handled in many different ways. In early 1950 we had 8 years of fantasies, theories, and calculations behind us. We also had some significant measurements performed in the laboratories on the basic process, but we had no experience whatsoever that would tell us whether or not our assumptions and general ideas had anything to do with the behavior of real objects. It had become most urgent to come back to solid ground by establishing a connection between theory and practice. In other words, we needed a significant test. Without such a test no one of us could have had the confidence to proceed further along speculations, inventions, and the difficult choice of the most promising possibility. This test was to play the role of a pilot plant in our development.

The first immediate job was, therefore, to make detailed calculations concerning the test. Because of the shortage of high-speed computing equipment, much of this arduous work still had to be carried out by hand. Under the supervision of Wheeler, Landshoff, Richtmyer, and some new recruits among whom were Conrad Longmire, Rosenbluth, and Burton Freeman, an incredible amount of numerical data was turned out by the untiring work of the people in the Los Alamos computing division. Thus, the comparison between the results of the test and the theory of thermonuclear burning could be anticipated with some measure of confidence.

In the second half of 1950 and in early 1951, the most complex kind of apparatus was being built in order to observe the results of the test. It is impossible for me to describe the excellent effort that went into this work. The device we were building was going to function for a minute fraction of a second. The observing equipment was going to be destroyed by the test explosion, yet delicate effects had to be recorded before the test apparatus was vaporized. We had to find out not only what actually happened in this test but also which were the best observational tools to be used in future tests.

Under the direction of Alvin Graves, Frederick Reines, Jack Clark, William Ogle, and others, an intricate laboratory was built on Eniwetok. The Los Alamos effort was augmented by excellent crews from the Naval Research Laboratory and from the Radiation Laboratory of the University of California. Never before have so many experimental scientists believed in such a mass of complex calculations not as yet compared with any process in the real world.

During most of these preparations plans for an eventually successful device had to take a low priority. The most important thing was the test, which required such a great effort and which was to confirm or disprove our ideas. We had to establish beyond a doubt that thermonuclear burning was possible. The question whether it could be used in an economically designed weapon had to wait for many months. Yet, it would be a mistake to believe that in this period plans were not maturing. The most fruitful suggestions often occur when one is occupied with a different and urgent project, and many of the hard-working physicists contributed in offhand discussions their ideas, sometimes fantastic, sometimes practical, to what might in the end become a usable device.

However, the immediate thing ahead of us in the spring of 1951 was the test in the Pacific: Greenhouse. I do not know how many scientific experiments have been made under conditions as exotic or in a place as beautiful as was the setting for the first thermonuclear experiment. There must have been other events as strange, exciting, and unforgettable. What remains most clear in my mind is the contrast between the spectacular explosion, which in itself meant nothing, and the small piece of paper handed to me by my good friend, Louis Rosen, which showed that the experiment was a success. The test gave us the assurance that we needed. Our detailed calculations agreed remarkably well with the results of the test.

Success

All of us knew that after Greenhouse we faced the real decision: can a usable device be constructed or not? All of us worried about this question. Some made very specific plans. Wheeler set up a group in Princeton which was preparing for the long hard pull in the calculations that were to decide the issue. But the essential parts of the decision started to come faster than had been expected by anybody.

A few months before the Greenhouse test all calculations had to be completed, and at that time it became possible for many of us to devote our full attention to the problems of the construction of an actual bomb. This time the challenge found our group in Los Alamos fully prepared. Calculational techniques were developed to a high pitch. Half-examined ideas were lying around by the score. They had to be shoved aside for the sake of more immediate calculations. Now we had the opportunity to look at them in detail. A year had passed since the decision to go ahead at the fastest possible rate with the hydrogen program, and everyone was eager and anxious to come to grips with the real problem. Two signs of hope came within a few weeks: one sign was an imaginative suggestion by Ulam; the other sign was a fine calculation by de Hoffmann.

I cannot refrain from mentioning one particularly human detail in de Hoffmann's work. Since I had made the suggestion that led to his calculation, I expected that we would jointly sign the report containing the results. Freddie, however, had other plans. He signed the report with my name only and argued that the suggestion counted for everything and the execution for nothing. I still feel ashamed that I consented.

Even before the Greenhouse test it became evident to a small group of people in Los Alamos that a thermonuclear bomb might be constructed in a comparatively easy manner. To many who were not closely connected with our work this has appeared as a particularly unexpected and ingenious development. In actual fact this too was the result of hard work and hard thought by many people. The thoughts were incomplete, but all the fruitful elements were present, and it was clearly a question of only a short period until the ideas and suggestions were to crystallize into something concrete and provable. Both Los Alamos and the newly formed group in Princeton immediately started calculations on this new approach.

The calculations on the new plans, though still crude, were presented at a meeting in Princeton to the Atomic Energy Commission and its advisers shortly after the Greenhouse test. Even while this meeting was in session, fresh results from Wheeler's group were being brought in. This group, which was organized in a period of uncertainty, was gaining remarkable momentum and hope and their mood was contagious. In the Princeton meeting everyone clearly recognized that with a little luck, only a great deal of hard work stood between us and final success.

Now at last the high-speed computing machines started to play the significant role that had been foreseen a few years earlier. A somewhat modest but very efficient machine, the SEAC, was in operation at the National Bureau of Standards, and the director, Edward Condon, invited us to make use of it. With the help of this facility, initial details of the plans were ironed out in a few weeks rather than in tedious months. Soon even faster machines, including particularly the Los Alamos MANIAC, helped to speed our work, so that the calculations on the design could be carried through more thoroughly and in shorter time than anyone could have expected. The art of machine calculations was now shared by many of the leading theorists in Los Alamos. In the hands of Rosenbluth, Longmire, Nordheim, Freeman, and many others, speculations hardened into complete specifications. Wheeler's group at Princeton developed similar expert knowledge in an amazingly short time. John Toll, Kenneth Ford, and others not only helped to make sure of the success of the immediate plans but started to contribute toward designs that were to bear fruit only in the more distant future.

In the fall of 1951 I left Los Alamos. I felt sure that everything was going to be done to construct a thermonuclear bomb with the greatest possible care and precision. The theoretical division under Carson Mark had grown into a most able outfit, which was to be joined for the critical months by Bethe. All of us felt that his presence would make sure that nothing would be forgotten in the preparations. Yet people kept worrying about possible difficulties, even dreaming about them, up to the time of the final test explosion. Thus, with Los Alamos furnishing the solid foundation and Princeton much of the drive and optimism, one could look with confidence into the future.

It was a great disappointment to me that I could not participate in the final phases of this magnificent undertaking. The main reason that persuaded me to leave Los Alamos was a conviction that this was an opportune time in which to start plans for a second weapons laboratory. Science, as well as any kind of technical work, thrives on friendly competition, on the fostering of different points of view, and on the exchange of ideas developed in various surroundings. It is only too easy for a single group to become fascinated by some special aspects of a development and to neglect other hopeful approaches. I felt that the safety of our country could not be entrusted to a single laboratory, even though that laboratory were as excellent as Los Alamos.

In the course of time this second laboratory was established at the Livermore site of the Radiation Laboratory of the University of California under the directorship of Herbert York. Its work so far has been mostly that of learning the difficult art of inventing and making nuclear weapons. All the magnificent achievements that have become in the meantime known to the world have been accomplished by Los Alamos. But in the intervening years a group of young experts has grown up in Livermore. The more they see that Los Alamos is a long distance ahead of them, the more eager they are to catch up. Having had the privilege of being associated with this young and vigorous group, I feel sure that the work at Los Alamos and Livermore will be mutually helpful to the two laboratories and will be of the greatest importance to the country as a whole. It is of no interest which of the two laboratories will be able to accomplish the most in the future. The only important thing is that each of them should do what it can and that together they should do what is enough.

The difficulties of the task of a weapons laboratory could be no more clearly illustrated than by describing the work of the last year that preceded the explosion of the first successful hydrogen bomb in Eniwetok. My knowledge of the details of this undertaking is not good enough, however, to justify any description or evaluation of the great work of this year, but I would like to mention the kind of difficulties that had to be faced.

Traditional engineering is thoroughly empirical. The usual sound practice is to make progress in small steps. A big plant is preceded by a small pilot plant. A full-scale device is not started until details have been checked on models. Work on atomic bombs makes it necessary to change this conservative practice. The final device is put together without any significant model experimentation. Tests, such as Greenhouse, can give guidance to the theorists, but they give little concrete support to the engineers. Dimensions, tolerances, strange materials go into the final design which would leave traditional engineers bewildered and helpless. Los Alamos had developed in the experience of many hard years an effective method of dealing with these grotesque difficulties of hardware. I can only guess how great these actual difficulties are, but I suspect that the greatest achievement in the production of the hydrogen bomb was not the conception or the invention but the execution. The man who was in charge of this undertaking was Marshall Holloway. I hope that at some time the story of this phase of the undertaking can be told, but the most important fact is this simple one: It was difficult and it was completely successful.

In October 1952 I was kindly invited to attend the explosion of the first full-scale device called "Mike." I would have liked to go, but it was clear that I would not have been of any concrete use in the Pacific. At the same time Livermore, only a few weeks old, was requiring the fullest attention of all of its members, so I chose the second best solution, which was much less expensive in money, effort, and time. I attended the first hydrogen shot by watching the sensitive seismograph in Berkeley.

In the morning of 1 November 1952 I was shown into the basement where the seismograph was operating. This seismograph is a recording instrument that writes with the help of a fine beam of light on a photographic film. The room was completely dark except for the tiny luminous spot that the pencil of light threw on the photographic paper. After my eyes became accustomed to the darkness, I noticed that the spot seemed quite unsteady. Clearly this was more than what could be due to the continuous trembling of the earth, to the "microseisms" that are caused by the pounding of the ocean waves on the shores of the continent. It was due to the movements of my own eyes, which in the darkness were not steadied by the surrounding picture of solid objects. Soon the luminous point gave me the feeling of being aboard a gently and irregularly moving vessel, so I braced a pencil on a piece of the apparatus and held it close to the luminous point. Now the point seemed steady, and I felt as if I had come back to solid ground again. This was about the time of the actual shot. Nothing happened or could have happened. About a quarter of an hour was required for the shock to travel, deep under the Pacific basin, to the California coast. I waited with little patience, the seismograph making at each minute a clearly visible vibration which served as a time signal. At last the time signal came that had to be followed by the shock from the explosion and there it seemed to be: the luminous point appeared to dance wildly and irregularly. Was it only that the pencil which I held as a marker trembled in my hand? I waited for many more minutes to be sure that the record did not miss any of the shocks that might follow the first. Then finally the film was taken off and developed. By that time I had almost convinced myself that I must have been mistaken and that what I saw was the motion of my own hand rather than the signal from the first hydrogen bomb. Then the trace appeared on the photographic plate. It was clear and big and unmistakable. It had been made by the wave of compression that had traveled for thousands of miles and brought the positive assurance that "Mike" was a success.

What Next?

I believe that everyone who has worked on the hydrogen bomb was appalled by the success and by its possible consequences. I also believe that everyone who was closely or distantly connected with the effort and who made any contribution, great or small, had a clear feeling that the work was necessary in the interest of the safety of our country. To that extent I feel that all of us had an equal sense of satisfaction in the final success on 1 November 1952 at Eniwetok in the Marshall Islands.

In the whole development I want to claim credit in one respect only. I believed and continued to believe in the possibility and the necessity of developing the thermonuclear bomb. I feel that it was a great privilege that I could stay with it until a time at which the successful conclusion was in sight.

At the present time I find myself unhappily in a situation of being given certainly too much credit and perhaps too much blame for what has happened. Yet, I feel that the development of the hydrogen bomb should not divide those who in the past have argued about it but rather should unite all of us who in a close or distant way, by work or by criticism, have contributed toward its completion. Disunity of the scientists is one of the greatest dangers for our country.

The very size of our progress has opened up other dangers. We may be led to think that this accomplishment is something ultimate. I do not believe that this is so. Where the next steps will lead, I do not know. It is not likely that it will be just bigger bombs again. The world is full of surprises, and great developments rarely go along straight lines. But the skills and the knowledge that developed the A-bomb and the H-bomb can undoubtedly be turned to new directions, and we shall fail if we rest upon our accomplishments.

The greatest and most obvious danger of the hydrogen bomb is its destructive power. Some may think that it would have been better never to develop this instrument. I respect their opinion and I understand their feelings. There can be nothing more strong and definite than our desire for peace and I am sure that those who were most closely connected with the development of the new destructive weapons feel this at least as strongly as anyone else. But I also believe that we would be unfaithful to the tradition of Western civilization if we were to shy away from exploring the limits of human achievement. It is our specific duty as scientists to explore and to explain. Beyond that our responsibilities cannot be any greater than those of any other citizen of our democratic society.

It is clear and it is true that atomic bombs and hydrogen bombs are terrible and unprecedented, but so have been many other developments that past generations have faced. The construction of the thermonuclear weapon was a great challenge to the technical people of this country. To be in possession of this instrument is an even greater challenge to the free community in which we live. I am confident that, whatever the scientists are able to discover or invent, the people will be good enough and wise enough to control it for the ultimate benefit of everyone.

A Labile Precursor of Citrovorum Factor

Charles A. Nichol,* Aaron H. Anton,† Sigmund F. Zakrzewski

Department of Pharmacology, School of Medicine, Yale University, New Haven, Connecticut

THE 4-amino antagonists of pteroylglutamic acid (PGA), such as Aminopterin and A-methopterin, apparently exert their effect by blocking the formation of derivatives of folic acid concerned as coenzymes with the transfer of single carbon units, and thus with the synthesis of several components of proteins and nucleic acids (*1*). Aminopterin was the first agent to show striking effectiveness in the treatment of acute leukemia of children (*2*). The availability of an organism, *Leuconostoc citrovorum,* ATCC 8081 (recently reclassified as a typical strain of *Pediococcus cerevisiae,* *3*), which requires a reduced derivative of PGA, has made possible the observations that PGA is reduced metabolically and that the formation of citrovorum factor (CF, N^5-formyl-5,6,7,8-tetrahydro-PGA) derived from PGA by liver preparations and by suspensions of bacterial or leukemic cells, is blocked effectively by Aminopterin (*1, 4*).

Although evidence was available that CF itself is