

ON SUNDAY, May fifth, on the eve of one of the greatest upheavals in man's history, the world learned about the discovery of a new source of power, millions of times greater than anything known on earth. A newly extracted natural substance, present in relative abundance in many parts of the world, but very difficult to isolate, had been found capable of liberating energy at such an unbelievable rate that one pound of it was the equivalent of 5,000,000 pounds of coal or 8,000,000 pounds of gasoline. In explosive power one pound of the new substance would be equal to 15,000 tons of TNT. Only one chief obstacle remained—to find a method for isolating the substance in large quantities, and scientists were hopeful that such a method would not be long in developing.

The name of the new substance, a veritable Prometheus bringing to man a new form of Olympic fire, is uranium 235, or U-235 for short. It is a rare form of uranium, each 140 pounds of uranium containing one pound of U-235. It differs from uranium in its atomic weight, ordinary uranium being 238 times as heavy as hydrogen (the lightest of the ninety-two elements), whereas U-235 weighs 235 times as much as hydrogen. Hence the name. Even the existence of U-235 was not known until 1935, when it was discovered by means of a highly ingenious "atomic microscope" by Prof. Arthur J. Dempster, at the physics laboratory of the University of Chicago. There was not the slightest reason at the time to expect anything unusual from this newly found relative of the royal uranium family of elements.

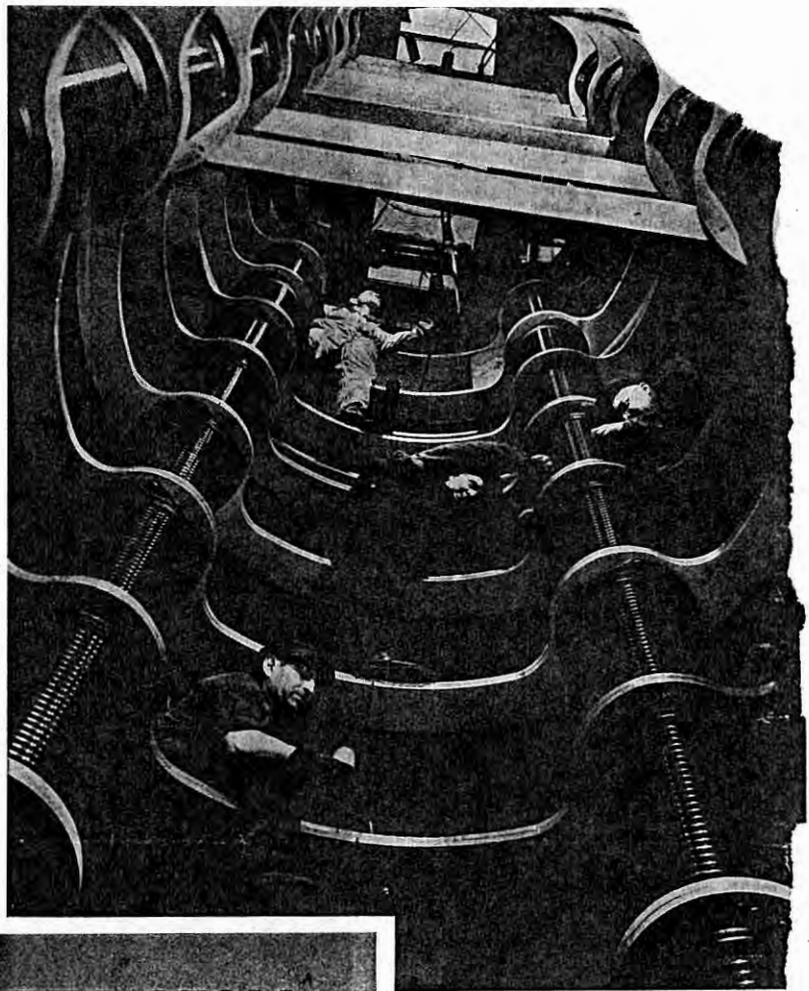
The complete story behind the story of this astonishing development, that may turn out later to be "the greatest story in the world," has until now remained largely untold. The story had its beginning about a year and a half ago, in Berlin, with experiments on uranium conducted by Dr. Lise Meitner and Prof. Otto Hahn, a scientific team that had worked together for twenty years. Like many an explorer before them, among whom Columbus is the best known example, they were seeking a new route between two known points, and came instead upon a miraculous new continent of matter, as rich and wonderful in its way as the Americas proved to be many years after their discovery. And, like Columbus, these modern discoverers of a new continent of vast resources did not themselves realize the nature and extent of their discovery. This was to be determined by later explorers, largely in America.

Meitner and Hahn had set out to repeat a famous experiment carried out by Prof. Enrico Fermi, Nobel Prize winning physicist, who left Fascist Italy to continue his work at Columbia University. Professor Fermi had discovered a strange game of "atomic golf," in which atomic balls, known as neutrons (fundamental, electrically neutral building blocks of the universe), could be made to score "holes in one" with much greater frequency if they were made to travel with slow speed, the "hole" in this case being the nucleus, or core, of the atom.

Through-the-Looking-Glass

THE purpose of this game is to liberate part of the enormous energy locked up in the nucleus of the atom. In playing this game, using uranium as the "atomic golf course," Professor Fermi observed strange Alice-Through-the-Looking-Glass phenomena that did not seem possible. It appeared that in the course of this game new elements had been created heavier than the

Columbia University's Dean Pegram, looking into an operation of his school's giant cyclotron (atom-smashing apparatus), with which it was confirmed that the uranium atom could be split in halves.



A GLOBE PHOTO

One of science's Big Berthas in the war on the atom, the 65-foot atom smasher of the Westinghouse Laboratories. Below, at work—Dr. Alfred O. Nier, 27-year-old Minnesota physicist, who was first to get a pure sample of U-235, a pound of which would have the explosive power of 15,000 tons of TNT.

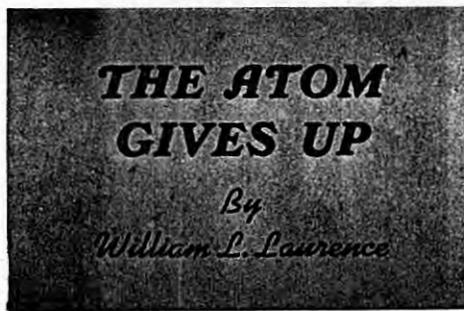
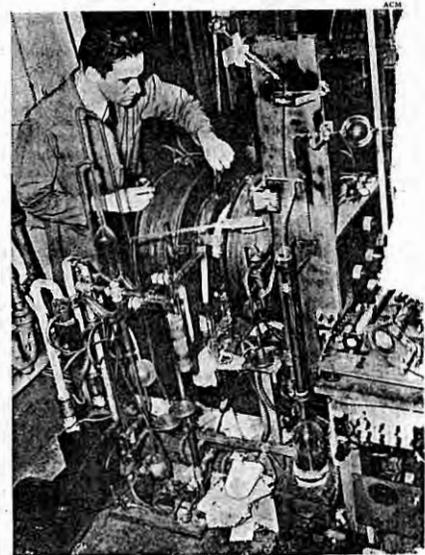


PHOTO BY GORO FROM BLACK STAR



heaviest found in nature, elements beyond uranium, heavyweight of the natural components of the physical universe.

Meitner and Hahn devised a highly delicate "atomic microscope" that enabled them to "see" what was happening chemically on the "atomic golf course" more clearly than could be done before, then proceeded to fire slow-speed neutrons à la Fermi at the uranium nucleus. And the result surprised and startled them so much that they believed some serious error had been made. They repeated the experiment, only to observe once again what they had seen in the first place—an "atomic ghost" that had no business being there. Instead of an element resembling uranium they observed an element totally different, having an atomic weight only little more than half the weight of uranium. The "atomic ghost" was seen to materialize itself, and lo, here, out of nowhere, appeared the element used in the taking of X-ray pictures of internal organs—barium.

A Deep Mystery of the Laboratory

BARIUM! How the deuce did it get there? Where could it have come from? There definitely was not a trace of barium present when the experiment was started, and yet here it was. It was like placing a duck's egg in an incubator and suddenly seeing it hatch out into a chicken.

Before a solution could be found to this scientific mystery of the first magnitude, Hitler's racial decrees brought Doctor Meitner's career in Germany to an end. It had been discovered that Doctor Meitner, a scion of a family that had lived in Germany for many generations, was not "Aryan." She was forced to leave her native land to seek a haven where she could resume her life's work.

Lise Meitner was on the train bound for Stockholm, sadly looking out of the window at the Berlin where she had spent her life devotedly in the pursuit of knowledge. That was a closed chapter. She was sixty years old, unmarried, and a woman without a country. She was going to a strange land, where she would try to resume her work, her unfinished strange experiment, barium.

She could not get barium out of her mind. Could it have been an impurity? Doctor Hahn was the most careful of chemists. He had been meticulously careful to exclude any possibility of the uranium being contaminated with barium, and yet, in spite of the most careful precautions, the barium appeared, like Hamlet's ghost on the ramparts. Where could the barium have come from? Nothing ever comes from nothing, and there had been no barium there to start with.

Lise Meitner's thoughts wandered far afield and kept coming back to barium. Suddenly, what seemed at first an idle thought, to be dismissed as daydreaming, flashed into her mind. Barium has about half the atomic weight of uranium. Could it be possible that the bombardment of the uranium with the slow-speed neutron bullets split the uranium atoms in two nearly equal halves, one of which was the mysterious ghost of barium that appeared in the experiments?

She attributed the thought as most likely being due to the strain she had been under during the past few days. It was too fantastic to be true. For nothing like it had ever happened before in the hundreds of thousands, if not millions, of atom-smashing experiments in leading scientific institutions all over the world, during the past twenty years. Not even the most powerful atom-smashing machines in America, largest of their kind anywhere in the world, had ever succeeded in chipping off more than a small bit of an atom. Even an elementary student of physics knew that there was not enough power available anywhere on earth to split an atom in halves, particularly the heaviest of all the elements.

She began jotting down figures on paper. Every well-informed layman knows by this time that the material universe is made up of ninety-two fundamental elements, beginning with hydrogen, the lightest, at No. 1, and ending with uranium at ninety-two. What makes the elements differ from one another is the number of positively charged electrical particles, known as protons, in their nucleus, or core. Thus hydrogen has only one positive electrical particle in its nucleus. Helium has two. Carbon has six, nitrogen seven, oxygen eight,

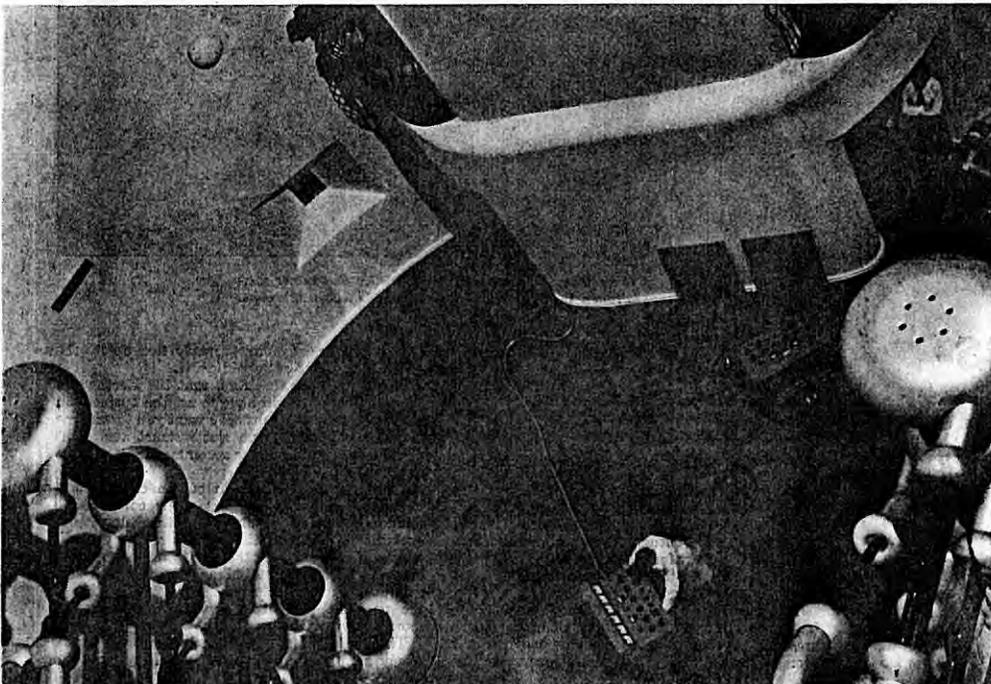
and so forth. If helium were to be split in halves, each half would be not helium but hydrogen. If oxygen were to lose one positive particle (proton) it would no longer be oxygen but nitrogen. Mercury contains eighty positive particles in its nucleus and gold has seventy-nine; hence if one of these could be knocked out of the mercury nucleus it would be transmuted into gold. Similarly, uranium contains ninety-two, barium fifty-six, and krypton thirty-six positive particles respectively, in their central core. Hence, if uranium could be split by some process into two uneven pieces, of fifty-six and thirty-six units each, the broken parts would be, respectively, barium and krypton.

56 and 36 and Energy Undreamed Of

HAVING scribbled the figures 56 and 36 on her notebook, Lise Meitner began doing a little more involved calculation. It takes tremendous energy to hold the unit particles in the central core of the atoms together. This is known as the "binding energy" of the atom. If an atom were to be broken in halves a certain portion of this binding energy would be released, and, in the case of a heavy atom, the amount of such binding energy that would be released should be of tremendous proportions. How much? she wondered. With expert mathematics she quickly arrived at the result and then went over her figures to make sure. . . . Yes, she was right. If a uranium atom of ninety-two positive particles were to be split into two parts, one of which consisted of 56 (barium) and the other of 36 (krypton) particles, the amount of atomic binding energy released would be the hitherto-undreamed-of figure of the order of 200,000,000 electron volts per atom, an energy 5,000,000 times greater than that released in the burning of coal.

The figures before her overwhelmed her. She was experiencing sensations that must have been akin to those of Columbus when he first sighted land, without knowing exactly what the land was. Was it the East Indies? A mirage? A new continent of untold wealth? If her figures were right, and they could well be checked, she and Doctor Hahn had accidentally stumbled upon

(Continued on Page 60)



Dr. Winfield W. Salisbury, busy on the ions of the University of California's 225-ton atom smasher, world's largest.

Today Germany is neck-and-neck with this country in the race to develop the full powers of U-235. At left, Berlin's atom-smashing plant, which German press agents claim is the largest in the world.



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THE ATOM GIVES UP

(Continued from Page 13)

one of the greatest discoveries of the age. They had come upon the trail of what might lead to the shores of the Promised Land of Atomic Energy.

When Lise Meitner arrived in Stockholm she did two things that started off a set of events as dramatic as any in the history of man's endless quest for new means of mastery over his material environment. First, she prepared a report of the results of her strange experiment for a scientific journal, so that scientists in other parts of the world, both inside and outside Germany, might take up the quest for an answer to the puzzle. Second, she telegraphed the gist of her findings to a scientist friend in Copenhagen, Dr. R. Frisch.

It so happens that Doctor Frisch is the son-in-law of Prof. Niels Bohr, of Copenhagen, Denmark, one of the world's most famous pioneers in the investigation of the atom. Professor Bohr was at that time—it was January, 1939—in America, carrying on investigations with his colleague, Einstein, at the Institute for Advanced Study, Princeton, New Jersey, and also with his other Nobel Prize winning colleague, Fermi, at Columbia. Doctor Frisch did two things. He at once cabled the news from Doctor Meitner to Doctor Bohr in America, and he set to work repeating the Hahn-Meitner experiment in Doctor Bohr's physics laboratories at the University of Copenhagen.

The news reached Doctor Bohr on or about Tuesday, January 24, 1939. He lost no time in communicating the startling developments to Doctor Fermi. These two master minds in modern science began making some calculations of their own. Without knowing the full details of Doctor Meitner's figures, they soon arrived independently at the same conclusions. Sure enough, if the uranium atom could be split into two pieces, the parts would fly apart like gigantic atomic cannon balls, the greatest ever produced in any laboratory, each fragment traveling with an energy close to 100,000,000 electron volts, or a total of 200,000,000 electron volts of energy, by far the greatest ever liberated anywhere.

A Surprise for the Physicists

If their calculations were right then the "atomic thermometer" of Columbia's giant atom-smasher should register the fact. They called together a conference of the Columbia atom-smashers, headed by Prof. J. R. Dunning, under the general supervision of Dean George B. Pegram. For a day and a night they labored, preparing, testing, checking, observing. Then, on Wednesday, January twenty-fifth, their labors were finished—a tired group of scientists were anxiously standing around the "atomic thermometer." One of them pressed a button. Yes, the uranium atom was definitely being split. Little David was cracking nature's Goliath in two and forcing him to give up an enormous amount of his strength.

It so happened that on Friday following the experiments there was to be held at George Washington University, Washington, D. C., a conference on theoretical physics in which Doctor Bohr, Doctor Fermi, and a select group of leading American physicists were scheduled for informal discussions on

the latest developments in their probings inside the atom.

There was nothing to indicate that anything out of the ordinary was about to take place when Doctor Bohr rose to speak that afternoon of January 27, 1939, in one of the lecture rooms at George Washington University. It took some minutes before the import of what he was saying, in low, even tones, had impressed itself on their critical minds. Had anyone other than the great Bohr, or another of his stature, uttered the words they were hearing it is doubtful if they would have taken them seriously.

The Atom-Smashers Get Busy

Suddenly there was a commotion and the room became nearly empty. Calm young scientists, leaders in their field, never observed to show undue excitement about anything, were seen rushing to the nearest telephones. One or two science reporters present sensed there was something momentous in the air, but the young physicists were too busy to talk to them. Excitedly they got their colleagues in their laboratories on the telephone. Bohr has just reported something tremendous. Sounds fantastic, unbelievable, but they must get hold at once of a sample of uranium and repeat the experiment Doctor Bohr had just told them about. Columbia had already done it, but they must not lose time to do it on their own.

In almost no time the giant atom-smashers at the Carnegie Institution of Washington, Johns Hopkins University, and a number of other leading scientific institutions, were engaged in a blitzkrieg against the uranium atom, hurling against it billions upon billions of atomic projectiles in the form of slowed-up neutrons. There was no sleep that night in January for any of these scientists in the laboratories of various parts of America, and they kept working on through the morning and into the afternoon.

Finally, late Saturday afternoon, the news came through to the group of physicists at the Washington conference. It was true. The barium came as a result of the uranium atom having been split in two unequal pieces, releasing in the process a quantity of atomic binding energy 5,000,000 times the energy of burning coal.

Then came word from Doctor Frisch by cable to Doctor Bohr that he had achieved the same results a few days ahead of the Americans.

No sooner was the great barium mystery solved than another, equally baffling, presented itself. When the uranium is split in two parts a number of high-speed atomic bullets, in the form of neutrons, should be released in the process from the atom's core. If these neutrons were to be slowed down (slow neutrons are the most accurate) they should start a cyclic action in the manner of a string of firecrackers, one split atom automatically setting off another, which, in turn, would set off a third, and so on, in rapid succession, resulting in a terrific explosion.

When no such explosion was observed, and no chain reaction in the manner of "cosmic firecrackers," the scientists set to wondering. There must be something that extinguishes the cosmic fire. What could that something be?

Doctor Bohr, in collaboration with Dr. J. A. Wheeler, of Princeton, was the first with a theoretical explanation for the problem. Ordinary uranium, it had been found by Doctor Dempster in 1935, consisted of a mixture of three types of the substance differing in their atomic weight, the largest part consisting of atomic weight 238, while the two other types had atomic weights of 235 and 234, respectively. It had also been determined that the ratio of the uranium 238 to uranium 235 was 1 to 139—that is, in every 140 pounds of ordinary uranium there is one pound of pure uranium 235 (U-235), scattered so finely that the job of separation had up till then been regarded as impossible. Uranium 234 is much the rarest of the three, existing in a ratio of 1 to 17,000 of ordinary uranium.

It was the U-235, Doctor Bohr and Doctor Wheeler concluded on the basis of theoretical reasoning, that was starting the atomic fires going. The U-238 was the element that was quenching the fires. If only a sample of the U-235 could be obtained in pure form! But no such sample was available, and until that could be done the world could not know for certain.

Quietly, and it may be imagined feverishly, another scientific race was set going in our leading scientific laboratories. The industrial research laboratories of the General Electric Company, fully realizing what was at stake, joined in the race with improved apparatus. And the race gained impetus by reports that kept trickling out of Germany, through a grapevine in which exiles from German laboratories played a significant part.

Shortly after Lise Meitner was exiled from Germany, Doctor Hahn published a preliminary report on the experiment in a German scientific journal in which he confined himself to the facts, without interpreting them. Since the spectacular corroboration of the experiment, and its full significance, has been published in America not a word has come out officially from German laboratories. But in spite of the strict censorship, and the thick veil of secrecy, reports began trickling through, all fitting together the scat-

tered parts of a jigsaw puzzle. By direct order of Hitler, according to the reports, some 200 of Germany's greatest scientists were concentrating all their joint energies on the solution of the one problem—U-235.

The problem of separating twins of the same element so close to each other in weight was a formidable one and required a considerable amount of experimental ingenuity for its solution. Credit for being the first in the field with a tiny sample of the precious substance goes to Dr. Alfred O. Nier, twenty-seven-year-old physicist of the University of Minnesota. Shortly thereafter another, slightly larger, sample was isolated at the General Electric research laboratories at Schenectady, New York, by Dr. K. H. Kingdon and Dr. H. C. Pollock. Both samples were rushed to Columbia University and submitted to tests, and both provided experimental proof that Doctor Bohr and Doctor Wheeler were right in their theoretical predictions that it was the U-235 that had been split in two and released the greatest amounts of atomic energy ever to be observed.

These first microscopic bits of U-235 may, therefore, well be regarded in the not-too-distant future as the very cornerstone of a new civilization. Fifty years from now, when the present war may be but a memory, the generation then living may look upon this discovery as one of the turning points in human history. Certain it is that it will be regarded as one of the great discoveries in modern science.

But nature has a way of tantalizing man by placing before him a luscious morsel and then interposing seemingly insuperable obstacles between him and the desired object. No sooner was the discovery made of the tremendous power-potentialities of U-235 than it was realized that nature had locked it up so tightly with ordinary uranium that it was, to all intents and purposes, impossible to separate it in pure form in large quantities. The methods used for separating the first tiny samples at the University of Minnesota and the General Electric Company yielded the substance at the rate of 1036 millionths of a gram every ten days, working

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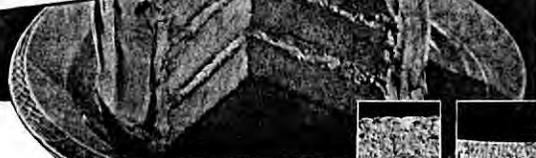
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twenty-four hours a day. At this rate it would take 26,445 years to produce one gram, and 11,995,074 years to extract one pound. It was, therefore, at once realized that the principal problem to be solved before atomic power could become a reality was to devise a method, or methods, that would make possible the extraction of U-235 in practical quantities.

The prize at the end of the rainbow was in itself great enough to start a friendly scientific race among America's leading university and industrial laboratories. But this friendly race, usual among scientists as among the rest of mankind, assumed an ominous aspect as the tentacles of the swastika cast a shadow on the tranquil walls of our laboratories. For here again it was realized that, with all their superior equipment and ingenuity, the American scientists, because of the very limited funds available for research, were at a considerable disadvantage in working against scientists of totalitarian Germany, who had practically unlimited resources at their disposal. What if the Germans succeeded in attaining their goal? A few hundred pounds of U-235, even in a concentration of only 10 to 50 per cent purity, according to calculations, would place in German hands potentially the most powerful fuel ever dreamed of.

It is the prevailing opinion among American scientists that, in spite of the enormously greater resources at the disposal of the German laboratories, they could not possibly solve the problem in less than ten years, and probably much longer. Yet developments in science move so fast these days that no one is willing to make definite predictions as to what might, or might not, be done in the near future.

The Problem of Isolating U-235

Even now there are signs on the horizon promising considerably improved methods for the separation of U-235 in larger quantities. A number of new methods are being quietly developed in American laboratories, and one of them in particular, known as the "thermal-diffusion method," taking advantage of differences in temperature to separate lighter particles from their heavier components, is being thoroughly investigated as the most promising for the present.

The development of this method furnishes another fine example of the fact that progress in modern science is the result of contributions by many scientists in many lands. The method was originally developed in Germany for other purposes a few years ago. Later it was improved upon in America. More recently, Prof. W. H. Furry, of Harvard, Prof. Lars Onsager, of Yale, and others, worked out by mathematics a theory for employing the method with greater efficiency. Taking advantage of all these contributions, Prof. Wilhelm Krasny-Ergen, of the University of Stockholm, Sweden, designed an apparatus last summer which, he believed, would increase the yield of U-235 more than 12,500 times over present methods, provided certain chemical compounds of uranium could be produced.

Unfortunately, the invasion of Norway brought Doctor Krasny-Ergen's work to a stop before he had even completed his apparatus, so that for the present it still remains a purely theoretical calculation, and with no one willing to swear that the theory behind the calculations is watertight.

All that scientists are willing to say now is that "it appears probable that it will work," but that "there may be several years of concentrated work needed before success is reached." Even then, when U-235 is obtained, they add, "there is the very serious problem of shielding the operators from the U-235's radiation." The screens may have to be so bulky as to prohibit the use of the material as a lightweight power source.

Moreover, practical scientists point out, even if the Krasny-Ergen method did work, a method that increases the rate of yield by 12,500 times would still be very slow, requiring some 350,000 days (960 years) for the isolation of one pound.

Future Power Possibilities

However, still speaking theoretically, this would be true only for one unit of the apparatus. If the apparatus should be found to work, and scientists believe that it probably would, the problem would become largely an economic one. If it would take 350,000 days for one unit to produce one pound, then 1000 units would produce a pound in 350 days, and 100,000 such units, easy and cheap to make, would yield one pound of U-235 every three and a half days.

In a country like Germany, with its totalitarian economy, the cost of any undertaking is a very minor consideration when the government decrees that it is vital for the national economy, and, if the reports are correct, the Nazi government has so decreed.

One pound of pure U-235 would have the explosive power of 15,000 tons of TNT, or 300 carloads of fifty tons each. But such a substance would not likely be wasted on explosives. A five-pound lump of only 10 to 50 per cent purity would be sufficient to drive ocean liners and submarines back and forth across the seven seas without refueling for months. And the technique that would be required for its utilization would be even more simple than the burning of coal or oil, according to present theories based on small-scale experiments.

Just as coal needs a fire to release its energy, the U-235 would need only water. All that would be needed to start it would be to place it in water. The water would first be turned into steam and the steam would run powerful turbines.

When all the water had been used up the process would automatically stop, until more water was supplied to start it again. A constant supply of cold water, well regulated, would keep the process going on for months, or even years, depending on the quantity of the U-235 present.

The basis for these theoretical considerations rests on the discovery by Professor Fermi that neutrons when slowed down, by being made to go through water, become thousands of times more accurate in hitting bull's-eyes square into the hearts of atoms. Fast neutrons have tremendous speed, but no control. They pass right through, or by, atoms without hurting them. Neutrons slowed down to low speeds, the lower the better, gain in control what they lose in speed. They go straight for the heart of the atom, and once they enter it they have not enough energy to get out. In the case of the U-235 atom, because of its bulk and inherent instability, the slow neutron, on entering, splits it in half. The splitting, it is believed, automatically releases other neutrons, which, slowed

down in turn, will split more U-235 atoms, starting a firecracker action in a process that would be both automatic and self-regulating.

The neutrons have a weight very close to that of hydrogen. Since two thirds of the atoms of water consist of hydrogen, the neutrons, on being made to pass through water, strike the equal weights of the hydrogen atoms, and in doing so yield up most of their energy, so that they are slowed down to speeds corresponding to energies of one fortieth of an electron volt (an electron volt is a very small fraction of an erg, or unit of work).

On being slowed down the neutron is said to become "tuned" to the central core of the atom, so that it heads straight for it. To use a golf analogy, the slow neutron behaves as though a golf ball were magnetized and aimed at a hole containing a powerful magnet. Even the poorest of golfers could, under such circumstances, make holes in one.

To start the fires of atomic energy burning in U-235 it would not be necessary, according to theory, to provide neutrons from an outside source. What are known as "free" neutrons are present everywhere in the universe. Cosmic rays that keep entering the atmosphere from the outside at all times during day and night, and minute amounts of radium present in the air, continually collide with the oxygen and nitrogen atoms in the atmosphere with such force that fast neutrons are liberated. When a piece of U-235 will be placed in water, these fast neutrons would therefore be slowed down and start the automatic release of atomic energy, as long as there was water at the proper cool room temperature. Hot water, or steam, would not slow the neutrons down sufficiently to be effective.

Energy Still Untamed

Tremendous as the release of atomic energy from U-235 is, it must be realized that it constitutes only a very small fraction, less than one tenth of 1 per cent, of the total power contained in the U-235 atom if its mass could be completely utilized as energy. Each unit of atomic weight has an equivalent in energy of a billion electron volts, so that U-235, having 235 such units, contains the enormous energy of 235,000,000,000 electron volts, or 1175 times greater than the 200,000,000 electron-volt energy yielded by the splitting of the U-235 atom. In other words, if all the mass of one pound of U-235 could be converted into energy it would yield the equivalent in power of 5,875,000,000 pounds of coal. Stated in other terms, one pound of U-235 contains a total energy of 10,000,000,000 kilowatt hours of electricity, of which only less than one tenth of 1 per cent, or 10,000,000 kilowatt hours, could be utilized by the splitting of the U-235 atoms with slow neutrons.

Not even in the stars and sun is the entire mass of atoms converted into energy. It has been calculated that one thirtieth of a gram of water (there are 453.59 grams per pound), converted into pure energy, would yield enough heat to turn 1000 tons of water into steam. In one whole gram of water there is a sufficient store of energy to raise a load of 1,000,000 tons to the top of a mountain six miles high. A breath of air would operate a powerful airplane continuously for a year; a handful of snow would heat a large apartment house for a year; the

pasteboard in a small railroad ticket would run a heavy passenger train several times around the globe; a cup of water would supply the power of a great generating station of 100,000-kilowatt capacity for a year.

For the Research Laboratories

Writing in the General Electric Review for June, 1940, Doctor Kingdon sums up the general attitude of the research worker in the field as follows:

While it seems unlikely that this energy source will displace our present means of getting power, it cannot be denied that such a source should have important applications, as it is estimated that several million times as much power could be obtained from U-235 as from an equal weight of coal. These applications will involve problems of proper control of the power, and protection against the tremendous neutron and X-ray radiations which will accompany it. It may be that the use of these radiations in therapy will be one of the most important applications. But detailed discussion of these questions is premature until further progress has been made in the separation of large quantities of U-235.

Indeed, it would be just as premature to discuss in detail the possible applications and potentialities for the future of U-235 as a new source of power as it would have been to discuss the potentialities of the electromagnetic (radio) wave when it was first produced by Hertz, or of the steam engine, dynamo, internal-combustion engine or airplane, when they were first invented. For the next few years, at least, operators of coal mines and oil wells, and distributors of power need not lose sleep over U-235.

Nevertheless, it would be lacking in farsightedness for our industrialists not to watch with keen interest the developments in this field, and it would be downright shortsighted not to aid the pioneer scientists in this highly important research so that America may be in the lead when the time comes for the practical application of this tremendous new potential source of power. It would be tragic indeed, if America were to lose the lead it is now believed to have in this field because its scientists, as the result of lack of funds, could not keep up in the race with their totalitarian rivals. A few thousands of dollars invested for research now may be worth hundreds of millions in the future.

Fortunately, the indications are that some of our leading industrialists and public-utility leaders are already taking a keen interest in the matter. This is shown by the fact that at a round-table discussion in April to "explore the public utilities outlook" for the immediate future, under the auspices of the Savings Bank Journal, attended by more than thirty industrial and political authorities in the field, U-235 was one of the topics discussed, and, according to an editorial comment in the Savings Bank Journal for May, 1940, "aroused great interest and speculation."

Correction

AN ARTICLE, Good-by, Boys, I'm Through, by Joseph F. Dinneen, in the issue of August third, stated: "For months the Guild had supported a strike against the Newark News." This was incorrect. The American Newspaper Guild strike to which the article referred was against the Newark Ledger. The Post regrets the error and is glad to correct it.



Sue Read, famous as the "Most Televised Girl in America," is shown . . .



. . . at the wheel of a fast speed boat. It's a thrilling sport, but mighty tough on engine lubricant. That's why, for safe lubrication, so many . . .



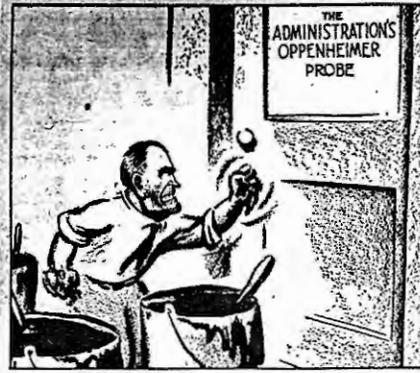
. . . motor boat and yacht owners depend on Sinclair Motor Oils. These are the same quality oils sold by . . .



. . . your nearby Sinclair Dealer for your car. Ask him for Sinclair Opaline or Sinclair Pennsylvania Motor Oil. You'll find they last so long they save you money.

The Atomic File

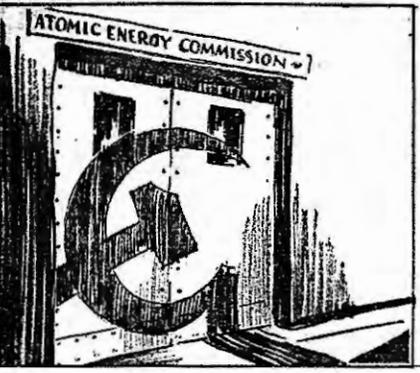
HOW EIGHT CARTOONISTS REACTED TO THE DEVELOPMENTS IN THE OPPENHEIMER CASE



Little in The Nashville Tennessean "Who slammed that door in my face?"



Kerlock in The Washington Post and Times-Herald "Who's being walled off from what?"



Keesler in The Cleveland Plain Dealer "The shadow."



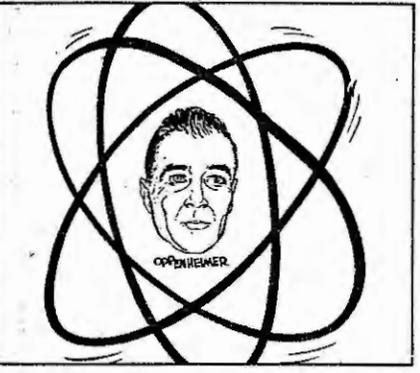
Rosen in The Albany Times-Union "Now this!"



Morris, A. P. Newsfeatures "The other end of the horn."



Nuttin in The Philadelphia Inquirer "Time out for thinking."



Dobbins in The Boston Post "'O' Bomb?"



Burck in The Chicago Sun-Times "Don't tell anyone—all you do is split a scientist."

THE DRAMA OF THE HYDROGEN BOMB—AND DR. OPPENHEIMER'S KEY ROLE

By E. W. KENWORTHY
Fifteen years ago this month a paper in the Physical Review set the United States on the course that led to Hiroshima.

1. THE BEGINNINGS
The atomic age began theoretically in 1905 when Albert Einstein advanced the proposition that matter could be converted into energy.

went forward, the Government began planning production. In the fall of 1942, Maj. Gen. Leslie R. Groves was made head of the over-all Manhattan Engineer District.

Security Case Focuses Attention on Disputes That Preceded First Successful Test of H-Bomb at Pacific Proving Ground
was the draftsman, David Greenglass, who worked on a lens mold.

evidence of Soviet hostility and, the growing evidences of Soviet power.
This massive evidence did not bring a reversal of the post-war cutbacks in American armed strength.

war against cities—might encourage local aggression, it was argued.
Therefore, the scientists recommended concentration on large A-bombs; a family of atom weapons (already under way at Los Alamos) that could be used in tactical support of ground troops, and an enlarged air defense network.

than in warning all mankind to avert it."
Again dealing with the dangers of using the Super as a deterrent, he said that this may be "a fine thing," but he asked, "What happens if the fighting starts?"

article appeared, Admiral Strauss became the new chairman of the A. E. C. Four days after he took over, he ordered the removal of classified documents from Dr. Oppenheimer's custody, pending a review of his security file.



om was split into lighter elements; in the fission, some matter is converted into energy with explosive force. That small flash was the precursor of the A-bomb and the H-bomb.

Soon after, Lise Meitner, who worked with Hahn, fled Germany. She passed the news on to Niels Bohr in Copenhagen. At a conference later in Washington, Bohr and Enrico Fermi, a refugee Italian physicist working at Columbia, put their heads together. In April, 1939, Fermi and Leo Szilard published a paper on their own researches on a bombardment of uranium. Einstein read it. He, Szilard and Eugene O. Wigner of Princeton met with Alexander Sachs of the Lehman Corporation to discuss the possibility of an atomic bomb.

On Oct. 11, 1939, Sachs read to President Roosevelt a letter from Einstein and a memo from Szilard. A President ordered an Advisory Committee on Uranium to be set up. In February, 1940, \$6,000 was allotted for the work at Columbia. June the Uranium Committee was created under the newly created National Defense Research Committee (later the Office of Scientific Research and Development) headed by Vannevar Bush. After Pearl Harbor, the decision was made to all-out.

Through 1942, the laboratory work went forward at tremendous speed. At Columbia, the Subatomic Matter Laboratory (SAML) Laboratory under Harold Urey was developing a method of testing the gaseous diffusion process of separating out the uranium isotope U-235. At the University of California in Berkeley, scientists in the Radiation Laboratory under the direction of Ernest Lawrence worked on the electro-magnetic process of separating U-235. At the Argonne Laboratory the University of Chicago, physicists under Fermi were constructing a pilot atomic pile (sketch below).

trained scientists and technicians. By the spring of 1945, Los Alamos had a staff of nearly 4,000.

Recruitment was a terrific problem. The nation was at war. Most scientists were already engaged on other essential defense work. Life at Los Alamos was not an inviting prospect. It was a military post. The scientists were asked to sign up for the duration.

The burden of recruitment fell largely on Dr. Oppenheimer. For months he traveled about the country, persuading scientists of the urgency of the work they would be doing. His fervor was infectious. Few scientists refused him.

Security considerations had been uppermost in the selection of Los Alamos. But the Army did not rely on remoteness. The whole area was fenced and constantly patrolled by armed guards like the one shown below. Mail was censored, all telephone



calls monitored. The scientists were permitted to leave the post only on business, or for the most urgent personal reasons. When they left, they were kept under surveillance.

There was good reason for the precautions. The Army Counter Intelligence Corps and the F. B. I. had ample evidence that the Communists knew much and were after more. Under the direction of Steve Nelson, openly the party organizer of the San Francisco Bay Area and covertly an NKVD agent in charge of atomic espionage, the Communists had managed to plant a small cell in the Radiation Laboratory at Berkeley.

A few months before Dr. Oppenheimer had left for Los Alamos, the Communists had made approaches to him through an old friend. According to testimony in postwar Congressional hearings, Dr. Oppenheimer had replied that the giving of information would be "reasonable." But he failed to report the incident until several months after it occurred.

Despite all the precautions, Los Alamos was not spy-proof. For almost two years—from August, 1944 to June, 1946—Klaus Fuchs worked at Los Alamos, sitting in on the most secret sessions. At Los Alamos, also,

words:

"Time and again we had in the technical work almost paralyzing crises. Time and again the laboratory drew itself together and faced the new problems and got on with the work. We worked by night and by day; and in the end the many jobs were done."

On July 16, 1945, this mushroom cloud rose out of the desert at Alamogordo.



On the day of Hiroshima, Secretary Stimson said:

"The development of the bomb itself has been largely due to his [Dr. Oppenheimer's] genius and the inspiration and leadership he has given to his associates."

Even as the bombs dropped on Japan, the scientists at Los Alamos were discussing the future of atomic energy. The fissionable atom, in a world at peace, could multiply the wealth of mankind. It could also, in a world not at peace, become the trigger for a vastly more powerful thermonuclear bomb.

3. THE HIATUS

In the fall of 1945, the nation knew little and cared less about H-bombs. The A-bomb was felt to be plenty big enough. It had stunned the world with its power. The problem was how to control it.

This was the Indian summer of large hopes—in the unity of the victors, in the United Nations, in permanent peace. United States forces were quickly brought home and demobilized. The Congress set to work on plans for civilian control of atomic energy. The Government set to work on plans for international control of atomic armaments.

The Indian summer became a cold winter and a false spring. In June, 1946, Russia flatly turned down the Baruch plan for international control of atomic energy. As Dr. Oppenheimer, who had been a consultant to Mr. Baruch, wrote later: "Openness, friendliness and cooperation did not seem to be what the Soviet Government most prized on this earth. . . . Instead we came to grips . . . with the massive

H-bomb. Among them was Admiral Lewis L. Strauss, a member of the Atomic Energy Commission. A tremendous controversy began in the most secret councils of the nation.

The Atomic Energy Commission in October called for a special meeting of the General Advisory Committee of scientists, of which Dr. Oppenheimer was chairman. The A. E. C. asked for an opinion on the "crash" program. The G. A. C. reported back before the month was out. Unanimously it opposed the crash program. Behind the committee's opposition were these considerations:

There was the question of feasibility. The committee estimated that with "an imaginative and concerted attack" there was a "better than even chance" of producing the H-bomb within five years. But there were tremendous technical difficulties to solve. Some scientists doubted whether the intense heat of the A-bomb could be concentrated long enough to set off the H-bomb.

There was the question of atomic "drain." At that time, plans called for using tritium as the key component in the H-bomb charge. The production of tritium would utilize facilities otherwise capable of producing plutonium for A-bombs. The scientists doubted whether this drain was justified when the nation already had A-bombs more powerful than that which had knocked out Hiroshima and Nagasaki.

There was also the question of defense. Dr. Oppenheimer (shown with Dr. Einstein below) felt strongly



that continental defenses could be strengthened.

Finally there was the big question of basing the nation's security chiefly on strategic atom-bombing. Many scientists agreed with top Army and Navy officials that the atomic bomb was not an "ultimate" weapon, and that there were many local situations in which it could not be used. The enemy's knowledge of the bomb's limitations—together with his confidence that the U. S. would not initiate a massive atom-



ner T. Pike, Dr. Henry D. Smyth aligned themselves with the Advisory Committee. Admiral Strauss and Gordon Dean dissented. The President turned the controversy over to Secretary of State Dean Acheson, Secretary of Defense Louis Johnson and Mr. Lillenthal. The three men met on Jan. 31, 1950. Mr. Acheson and Mr. Johnson favored the crash program. The committee walked across the street to the White House. The President listened to the arguments. That afternoon he gave the go-ahead on the H-bomb.

4. BUILDING THE BOMB

Work on the H-bomb got under way immediately at Los Alamos. In charge of the program was Dr. Edward Teller, the Hungarian-born physicist who had long been at work on the theoretical problems.

At the outset, planning was based on the assumption that the H-bomb would use tritium, and in January, 1951, ground was broken on a billion-dollar plant at Savannah River, South Carolina, to produce the tritium.

Meanwhile, however, Teller was working on a revolutionary scheme that might obviate the need of tritium. At Eniwetok in the spring of 1951, it was reported that a device was tested which established the soundness of his theory. From then on things moved with tremendous speed. Teller was installed at the A. E. C. laboratory at Livermore, Calif., which became the Los Alamos of the H-bomb.

As the work on the H-bomb continued, so did the controversy. The scientists who had warned against excessive reliance on strategic atomic bombs found point for their warnings in the Korean war.

In a speech to the New York Bar Association in January, 1951, Dr. Oppenheimer raised again the question of the military uses of the atom as against the political uses as a deterrent:

"[They [atomic bombs] are not primarily weapons of totality or terror, but weapons used to give combat forces help that they would otherwise lack. Only when the atomic bomb is recognized . . . as an integral part of military operations will it really be of much help in the fighting of a war, rather

when Defense Minister Arthur A. Fleming said that Soviet Russia had the capacity to deliver "the most destructive weapon ever devised . . . on chosen targets in the United States"; Secretary of Defense Wilson said Russia was "three or four years back of where we are"; and W. Sterling Cole, chairman of the Joint Congressional Committee on Atomic Energy, asked for expenditure of "10 billion a year on continental defense.

The President stepped in, saying that the Russians had the capacity to make "an atomic attack on us." He put an end to widespread reports that the Administration would launch "Operation Candor," a series of speeches on the whole atomic situation. He said, "We do not intend to disclose the details of our strength . . ." In mid-December he made his proposal for an atomic pool for peaceful purposes. Two weeks later, Dr. Oppenheimer (shown with physicist Hans Bethe)



5. THE HYDROGEN AGE

The United States now had a Super-monopoly. The nation found some comfort in it, but not nearly so much as it had found in the A-bomb monopoly. The man in the street knew instinctively what the atomic physicist knew positively—that if the Russians could master the A-bomb, they could master the H-bomb, and that it would be only a matter of time before instruments in the free world would pick up radiation waves let loose in the fastnesses of Siberia.

The knowledge intensified the old controversy. But now there was intense public interest in the debate.

In public speeches the debate was earnest and temperate. But behind the scenes there were rumors, allegations, suspicions and charges, and some of these found their way into print. In May, 1953, Fortune Magazine ran a piece on "The Hidden Struggle for the H-bomb," which said that Dr. Teller "had reason to believe" that the Atomic Energy Commission "under Oppenheimer's influence" had tried "to postpone, if not stifle," the building of the H-bomb, and that Dr. Oppenheimer had "tried to stop the test" at Eniwetok.

Two months later an article by Dr. Oppenheimer on "Atomic Weapons and American Policy" got wide attention. He laid great stress on the need for defensive measures, and the need for "candor" with the American people and our Allies. He criticized "the great rigidity of policy."

In the week the Oppenheimer

when Defense Minister Arthur A. Fleming said that Soviet Russia had the capacity to deliver "the most destructive weapon ever devised . . . on chosen targets in the United States"; Secretary of Defense Wilson said Russia was "three or four years back of where we are"; and W. Sterling Cole, chairman of the Joint Congressional Committee on Atomic Energy, asked for expenditure of "10 billion a year on continental defense.

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was called in by Admiral Strauss and given the alternative of resigning as consultant to the A. E. C. or facing a security hearing.

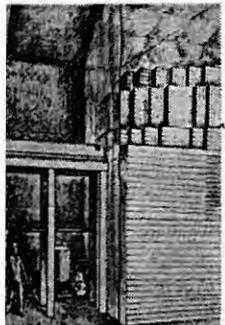
The March tests at Bikini raised a new storm, as the nation and world were shown pictures of the 1952 explosion that obliterated a small island and were informed by Admiral Strauss that the March 1 blast would have destroyed Manhattan.

The President said the U. S. saw no need for building a bigger bomb. This did not dispel the fears, for the nation was also told that if the Bikini bomb were enclosed in a cobalt sheath, the explosion would send a deadly radioactive cobalt dust cloud over thousands of square miles.

"Knowledge comes," said Tennyson, "but wisdom lingers." The world now had the knowledge to destroy itself. The question was whether it could command the wisdom to save itself.

PICTURE CREDITS

Photos in the above article were taken by U. S. Army, U. S. Air Force, Associated Press, The New York Times (Gertrude Bromberg).



At the end of the year the Metallurgical Laboratory under Arthur Compton began working on the production of plutonium. While the laboratory experiments

May 15, 1952

Dear Mr. Goldent:

It is in cognisance of and salute to the exceptional experience of encountering a kinship of spirit and attunement that I am departing from a schedule of priorities to transmit herewith the documentary material alluded to in a conversation that I wish had not been so much of a monologue.

The collect consists of the following octet: (1) a memorandum to the late President Roosevelt in the autumn of 1944 entitled "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War"; (2) summary of the memorandum to him immediately after his return from Yalta; (3) copy of a letter to Secretary Patterson in the summer of 1946 which disclosed the original plan for the use of the bomb that was transformed into a less ethical mode of use by the new "powers that be"; (4) an aide-memoire for a conference with Secretary Patterson in the autumn of that year regarding the proposed "Institute for Advanced Study in National Security and International Affairs Related to the New War Technology and Unfolding International Problems"; (5) copy of a letter to General Eisenhower three years later, or September 1949, regarding the establishment of such an Institute by and at Columbia; (6) the Look article of March 1950 by Mr. N. S. Finney which was based on some of the early material as designated herein and supplementary material examined by him from my files that has not been listed herein; (7) a reprint in early 1951 of a memorandum of mine and a sequential letter in The Times, best described by the title of the memorandum, "State Department's Misconceived Approval UN 'Cease-Fire' Plan and Accumulated Misorientation of Our Far Eastern Policies"; and (8) a reprint of an address in 1949 before the American Academy of Political Science on "Restoring the Economic-Cultural Bases of American Foreign Investment," - significant for the summary descriptions of the psychological malaise of our time and of the bemusement of this generation with philo-Sovietism. This octet has thus an internal rhythm and a development of contrapuntal themes. To the octet I have added material on two cognate themes, which for the sake of reciprocal time-economy I am confining to one illustrative case out of a rather crowded series and one elaborated case out of a series that ought to have been crowded with predecessors and successors, but just wasn't on account of time pressures. So the ninth member is copy of a telegram addressed to Secretary Acheson in July 1950, harking back to Casandra warnings that had been conveyed to him prior to the aggression and looking forward to an alternative diplomatic strategy

that it is now clear would have been far sounder than the one that was followed. There is attached to that copy of a letter from the Secretary to me in the mid-Thirties, whose only value for the instant purpose consists in showing that the alternative policy recommendation did not come from a stranger to him. The tenth and final member is the discussion of psychological warfare with Russia and the Iron Curtain as embodied in a long letter to Mr. Gordon Wasson of Morgan's in the summer of 1951.

While all of this material has had some circulation of different extent, I would like to be advised in advance if you contemplate passing on any part of it to persons who might be interested in this or that member of the collect. Having tried hard to live up to the role of "service" ranging from anonymity to special linkage, it is necessary, in keeping with that role, to be so advised, whether in writing or by telephone, before further enlargement of the circle.

With kind regards,

Sincerely yours,

Mr. William Golden
44 Wall Street
New York City

April 5, 1954

ALEXANDER SACHS, originator of Atomic Project in conferences with President Roosevelt in October 1939. As confidential advisor to the President since the mid-Thirties on the growing international dangers and as friend and associate of refugee scientists from Nazi Germany who continued their atomic researches in this country, learned of the German achievement, at the beginning of 1939, in atomic fission and of the prospect opened up in the spring by two refugee scientists at work here that the fission could lead to a chain reaction and the construction of bombs of hitherto unenviaged potency and scope. In the ensuing half year through the first month of the outbreak of World War II there devolved upon me, in consultation with the scientists, the mission of preparing an appeal to President Roosevelt to aid and promote the then embryonic research on this side so that we could overtake the lead of Nazi Germany and the threats, in the event of war, to the parallel researches in England and France. Accordingly, on October 11th, in the wake of the revision of the Neutrality Act, a personal presentation was made by the writer, with the documentary support of letters and memoranda prepared and arranged by me during the summer, including a letter by Dr. Albert Einstein. At the immediate behest of the President and through the Presidential Secretary, General Edwin M. Watson, there was established a Uranium Committee to explore the feasibility of the Project, - which Committee was composed of selected scientists active in the research, the Director of the Bureau of Standards in the role of Chairman, single representatives of the Army and Navy, and myself as the Presidential representative. Then after success with an initial experiment and the removal of doubts entertained by the representatives of the Services, the President, who was kept au courant, accepted, on May 15, 1940, my further proposal for the establishment of a Scientific Council on National Defense that, under the modified name of Office of Scientific Research and Development and under the direction of Drs. James B. Conant of Harvard and Vannevar Bush of the Carnegie Institution, had charge of the implementation of the Atomic Project and the development of the atomic bomb.*

* References in public sources; opening testimony by Alexander Sachs on "Background and Early History Atomic Bomb Project" in hearings before the Senate Committee on Atomic Energy, November 27, 1945; H.D.W. Smyth, Atomic Energy for Military Purposes, official report, 1945, sections 3:4-7; article by N.S. Finney, Look, March 14, 1950, "How FDR Planned to Use the A-Bomb"; John Gunther, Roosevelt in Retrospect, Harper's, 1950, pp. 303-4.

April 5, 1954

Dear Dr. Pool:

At this long last I am enclosing the response to your request and desideratum. The delay has been due in part to an initial indisposition when your first inquiry reached me and to continuing pressures and difficulties. But independently of these obstacles has been the challenge of the task itself. That involved recapturing and condensing the cumulative crises of history and the human tragedies of displaced, yet striving physical scientists who, together with this social scientist, sensed a portentous danger from an as yet uncreated but inevitable new weapon.

The simplifications of the journalists and the penchant for personalities imputed the transformative history to a physicist of genius whose role was minor and incidental. But as my situation did not permit me to devote the time immediately after the war to the finishing of the then desired book on Perspectives on Atomic Energy and World Security, the initial over-simplifications and distortions have become canonical.

Against this background this submission will, I trust, serve your purpose - with such modifications as you deem advisable - of a miniature history that converges the significant and luminous facets of the unique predicaments that were presented and that were mastered into a product that, like others of the fruits of the Tree of Knowledge, has the ambivalence of danger and opportunity.

Sincerely yours,

Dr. David de Sola Pool
99 Central Park West
New York City 23

April 5, 1954

ALEXANDER SACHS, originator of Atomic Project in conferences with President Roosevelt in October 1939. As confidential advisor to the President since the mid-Thirties on the growing international dangers and as friend and associate of refugee scientists from Nazi Germany who continued their atomic researches in this country, learned of the German achievement, at the beginning of 1939, in atomic fission and of the prospect opened up in the spring by two refugee scientists at work here that the fission could lead to a chain reaction and the construction of bombs of hitherto unenvisaged potency and scope. In the ensuing half year through the first month of the outbreak of World War II there devolved upon me, in consultation with the scientists, the mission of preparing an appeal to President Roosevelt to aid and promote the then embryonic research on this side so that we could overtake the lead of Nazi Germany and the threats, in the event of war, to the parallel researches in England and France. Accordingly, on October 11th, in the wake of the revision of the Neutrality Act, a personal presentation was made by the writer, with the documentary support of letters and memoranda prepared and arranged by me during the summer, including a letter by Dr. Albert Einstein. At the immediate behest of the President and through the Presidential Secretary, General Edwin M. Watson, there was established a Uranium Committee to explore the feasibility of the Project, - which Committee was composed of selected scientists active in the research, the Director of the Bureau of Standards in the role of Chairman, single representatives of the Army and Navy, and myself as the Presidential representative. Then after success with an initial experiment and the removal of doubts entertained by the representatives of the Services, the President, who was kept au courant, accepted, on May 15, 1940, my further proposal for the establishment of a Scientific Council on National Defense that, under the modified name of Office of Scientific Research and Development and under the direction of Drs. James B. Conant of Harvard and Vannevar Bush of the Carnegie Institution, had charge of the implementation of the Atomic Project and the development of the atomic bomb.*

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File

B

(see Unfinished work for extra copies)

letter to be dated June 28, 1954

Dear Dr. Bush:

It is out of community of concern over the problems dealt with so incisively in your article entitled "If we Alienate Our Scientists" that I am venturing to bring to your attention a series of representations on my part to a personality close to the White House. The selected members of the series that started in April comprises the letters of May 19th, June 4th, June 10th, and June 17th, - with the last containing a page of excerpts from your article.

The gravamen of the charge ~~that the AEC~~ in that last communication is that "the true parties to the controversy are not just the AEC officialdom and the individual scientist, but our governmental system vis-a-vis the community of science, world opinion, and American traditions as an integral part of the traditions of the Great Society." Furthermore, the considerations ~~are~~ condensed in that communication - as amplified in the alluded-to prior memorandum, in refutation of the evidentiary material in the majority report - point to the need for finding an independent organ of review which, I am convinced, would lead to revision of such adverse verdict. For, however unwittingly, the Board and Commission have not dealt with clarified and contextualized facts, but with turgid and x convoluted charges in the guise of facts. And pervading these charges are modes of emotive thought infected by totalitarian attitudes. So for all the presumptive detachment of the Board members, the very unavailability to them, as to the public generally, of a correct orientation on the evolving history of pre-World War II and of the pre-cold war has led to mis-evaluations and to mis-judgments.

What there should have been is the equivalent of a Royal Commission that would have comprised individuals who on the record have been insightful and

foresighted with regard to the political and cultural crises of this generation. Such an assemblage of political and social scientists who had escaped bemusement by the totalitarianisms both of the Right and of the Left could have fulfilled the function that Royal Commissions perform with regard to the far easier subject matter of ways and means out of dilemmas in political economy. The common functions and purposes of such trans-juridical bodies were aptly defined, as I recall, by the present Justice Frankfurter in his Terry Lectures of 1930, when he was still a Professor at Harvard, namely 'to deflate feeling, ascertain facts, pose right problems, define issues, and formulate alternative remedies.' In the instant situation it is ~~xxx~~ in the sense that the bureaucratic process did not effect a 'sifting of evidence and ascertainment of facts' that the psychological-moral identification threatens to produce a permanent state of alienation. So ~~x~~ apart from the vulnerability of our technological economy to the consequences from such alienation, it is necessary to devise ways for overcoming the schism and effecting a re-integration.

In the actual confluence of forces the vulnerability can be shown to be both highly probable and of projectible gravity: Indeed, with respect to higher technology as well as fundamental science it is demonstrable that Britain and France are, particularly with adjustment for relative population, more than on a parity with us, and over against the blatant preference of the majority report for conformized and subordinate science the calls of our interest as well as of our traditions are all for free questing spirit. In this connection it may not be amiss to recapture the view that was voiced by me in the opening testimony before the Senate Committee on the Atomic Project at the end of 1945:

"The issue was too important to wait, because if there was something to it there was danger of our being blown up. We had to take time by the forelock, and we had to be ahead of the Germans.

"One great advantage that we had was that these refugees,

these scientists themselves, responded to that very spirit of freedom that brought the Pilgrim Fathers over here, the search for freedom of speech and religion and, if you will, free science and free thought. They were saturated by ideas and motives which the regimented scientists could not have, and so the transplanted and the American scientists, if given the means, would make advances much faster." (p. 560 of Testimony)

With high regard,

Sincerely yours,

Dr. Vannevar Bush

[Conroy - Walker]

File
W

October 20, 1954

Dear Mr. Walker:

As the introduction and visit effected by our common friend, Mr. Robert LeBaron, were under such delightful and leisurely auspices, the subjects touched on in the course of a three-part conversation lasting for several hours are too numerous for effective follow-up. Besides, after such intense and prolonged interchange an interlude rather than ~~an immediate~~^{immediate} continuation might well be in order. But as Robert and I believe that in this whole domain of nuclear development we need to accelerate greatly the tempo that has been customary not only for prior research but the now vital entrepreneurship in its application, it behooves me to engage upon such follow-up forthwith rather than awaiting and responding to your inquiry.

Accordingly, I am assembling a brief collect that you might look through at your leisure between now and our next meeting, dealing with my past associations with the Atomic Project and my orientation on certain (international) opportunities as voiced around the beginning of this year in an exchange of correspondence. (Enc. 1a-b) In this notable instance there is a presence, or continuing operativeness in the unfinished present, of the past, even though its beginnings go back more than a decade and a half. Hence the deemed relevance and pertinence for the future of my testimony of late 1945 called "Background and Early History Atomic Bomb Project in Relation to President Roosevelt." (Enc. 2) Thus a sentence towards the end of that testimony has such a contemporary ring and significance:

" ... the majority, accustomed to the small scale of physical laboratories at the universities and the correspondingly reduced scales of the budgets of governmental scientific laboratories, did not appear ready to design a large-scale and comprehensive program, and instead insisted on 'bit-a-bit' procedures with ranked preferences and time deferments." (p. 570, reprint, end top paragraph)

My own position - which after much persuasion was accepted by the scientists and, through such consensus, also by the President - was that, in contrast with such small- and slowly augmenting scale of activities, it was urgent to coordinate and synchronize all phases and fields of the great work and to facilitate organic interaction and growth. Hence my labor at the outset at providing an historically applicable conjecture of the magnitude of the financial undertaking, - which happened to have proved correct - namely of the order

of \$2 billion. These and many other labors in the direction of placing that project on the right foundations and paths from the very inception contributed, I believe, to the historically unparalleled foreshortening of the translation of an idea into the practical reality of a bomb within a timespan of one tenth of the time that so many scientists and practical men thought in those crucial years that it would take.

By way of extended light on the later phases of the project, and more particularly the use of the bomb, I would draw your attention to an article that was written by Mr. N. S. Finney of the Cowles Organization and published in Look for March 1950, based upon his independent examination of my own preserved records and supplementary sources. (Enc. 3) Inasmuch as a major emphasis of his article is on the warnings I had given regarding what I had already discerned and stressed from 1943 on as the growing danger from the Soviet and on my advice against bringing in Soviet Russia in the closing phases of the Far Eastern War, - in view of all that, you will be interested in my memorandum of October 1944, entitled "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War." (Enc. 4) This memorandum, which forecast the timing of the end of the European and Far Eastern Wars, had as follow-up the warning by me at the end of February 1945, or immediately upon the President's return from Yalta. (Enc. 5) These and other documents came within the ken of and elicited a comment from Mr. John Foster Dulles, as per accompanying correspondence. (Enc. 6a-b)

As for the postwar developments and contemporaneous interpretations by this writer, for the sake of your time economy only two memoranda are included. The first is an interpretation of the forward significance of the Soviet atomic explosion at the end of September 1949, together with a forecast, timed for mid-1950, of "Far Eastern aggressions pivoted on Korea"; and the second is a memorandum of a year ago regarding "British Atomic Developments and Plans Keyed Into a New Orientation on a Nuclear Industrial Age." (Enc. 7-8)

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Finally, with regard to the common emphasis by Mr. Robert LeBaron and myself on opportunities in Brazil specifically and Latin America generally, I am adding a separate note summarizing views, in general concordance with those that I voiced to you yesterday, that were conveyed independently by a high official in one of our major automobile companies concerning the situations in Latin America and Europe. (Enc. 9)

Sincerely yours,

Mr. G. H. Walker, Jr.
One Wall Street
New York City

121
[Bloch, Dr. Kurt
Corresp.]

BARRON'S
National Business and Financial Weekly

50 BROADWAY
NEW YORK 4, N. Y.

February 20, 1957

Dr. Alexander Sachs
25 Broadway
New York, New York

Dear Alex:

Please find enclosed copy of a letter to Mr. James A. Lane, whose contents, I am sure, will interest you.

As always,

Cordially,

Kurt

Kurt Bloch
Associate Editor

KB:BW
Enclosure

February 20, 1957

Mr. James A. Lane, Director
Reactor Experimental Engineering Division
Oak Ridge National Laboratory
Oak Ridge, Tennessee

Dear Mr. Lane:

In leafing through the program for the Nuclear Congress of 1957, to be held next month in Philadelphia, I came across the plan for the round table discussion of March 15, to be chaired by you, which states that "it is believed that a power reactor using natural uranium would be ideal for foreign export."

If you happen to be in possession of any memoranda elaborating on this belief, I should be very grateful, indeed, for letting me have them. Would you, incidentally, hold that it is exaggerated to say that the view expressed in the program represents a departure from a previous tendency in the U.S. nuclear power program favoring reactors utilizing enriched uranium?

Being far from well-versed in technical matters, I was surprised at noting that Argentina's uranium refinery has been built by a large German metallurgical concern, indicating that technologically and economically, the production of natural uranium metal is far less difficult than that of enriched uranium. Hence plants utilizing natural uranium would seem to offer "have-not" nations as far as atomic energy is concerned, easier access to nuclear power than reactors depending on enriched uranium. I wonder how far increasing realization of that situation would affect such projects as that of American & Foreign Power which has announced its intention to build three 10,000 kilowatt power reactors in Latin America.

Furthermore, am I quite off the beam in thinking that generally speaking, the economics of scale play a greater part in the construction of natural uranium reactors than in that of facilities utilizing enriched uranium? The idea stems from the obvious British tendency towards units larger than those projected in the U.S.

Mr. James A. Lane, Director

-2-

February 20, 1957

I realize that these are questions of a rather large order. Still I should be grateful for any answers you could give me yourself or possibly through one of your assistants.

With best regards,

Yours sincerely,

Kurt Bloch
Associate Editor

KB:BW

(Consp Norden)
File
N

March 25, 1957

Dear Mr. Norden:

I am transmitting herewith photostatic reproductions of main excerpts from a report entitled, "Early History Atomic Project in Relation to President Roosevelt, 1939-40". It is an assemblage from original documentary material, prepared in the immediate aftermath of the bomb and just prior to the end of the war, with a view to placing at the disposal of the governmental authorities influentially concerned the historic record from preserved notes and in full freshness of memory. - At the time I deemed it necessary to effect the dictation in sections and even fragments to different people to assure complete confidence. Though this complicated the tasks of revision and coordination it did fulfill the then primary objective of confining the knowledge and the comprehension to the high governmental and other personalities to whom it was specifically addressed.

As for the bearing of this document on my testimony, it is noteworthy that the late Senator McMahon, having learned directly from the governmental personalities and sources of the pivotal role played, had insisted on my providing the overture to the hearings in preference to General Groves. In my then interchange with the Senator the document was shown to and examined by him. The ensuing opening testimony was delivered by me extemporaneously and was keyed into this original report. Only as members of the audience at the original Senate hearings and as later readers of the final testimony did or could other persons who were in the margins or between the lines of the history-in-the-making secure for themselves the thus disclosed historical documentation.

In the light of the prolonged striving on my part to preserve the confidential character of the account, I feel it necessary to state that any disclosure or utilization by you of this material should be talked over with me at your earliest convenience.

With kind regards,

Sincerely,

Mr. Heinz Norden
35 Boulder Lane
Hicksville, L. I.

File N



THE MEDIMETRIC INSTITUTE
INCORPORATED

HEINZ NORDEN

EXECUTIVE DIRECTOR

11 EAST 26TH STREET
NEW YORK 10, N. Y.
MURRAY HILL 3-6191

December 17, 1957

Handwritten:
The
problem
is
O.R.O.
Johnson

Dear Dr. Johnson:

It was a great pleasure to have met you this past Friday and the conferences with you prior to and during the luncheon were exceptionally stimulating and enlightening.

The theme of time-leads moves within the resonance-range, to borrow a term from nuclear physics, of all my labors since the late-Thirties. The enclosed reprint of my testimony on the early history of the Atomic Project features my Cassandra warnings as they accumulated from the mid-Thirties on and the climactic warning in March 1939 that the "Imminence World War in Perspective Accrued Errors" left primarily the United States barely sufficient time-margin, or what I called the "time draft" that could still be made on the "Bank of History" for preparedness. The specific sentence, which was quoted in my testimony before the Senate Committee on Atomic Energy in November 1945, is as follows:

"There is still time for Western Civilization, and especially for the exceptionally and fortunately situated United States, to use the time drafts that can still be made on the 'Bank of History' for the preparedness that has and will become more and more urgent and inevitable for all members of Western Civilization as a result of the past errors committed and in the course of the prospective unfolding aggressions of Nazi Germany."

As to the specific methodology referred to by me by which the President was kept advised throughout the war of the progress of accumulating strength by us and the consequential shift of preponderant strength to us and to our side, I am submitting herewith a set of charts and tables under the general heading "National Production and War Efforts of Our War Alliance Against the Axis." I am presenting this as a special documentation to the O.R.O. and should appreciate receiving formal acknowledgment of it. This operational methodology played a significant role in Presidential policy decision. A preliminary one was the counsel therefrom elicited to the late Wendell Wilkie when he returned from his visit to Russia in 1942 and became an advocate of immediate operations on the Continent prior to the attainment of adequate strength on our part. The build-up of strength in order to overcome the still available lead that the enemy has also counseled caution after the collapse of fascism in Italy and the fascist military power in the late-summer of 1943. Only by early-1944 did the "lead" pass decisively to us on a cumulative basis, as is evident from the attainment of a ratio one and one half times the Axis on the chart entitled "Combat Munitions Ratio United Nations Vs. Axis on

Basis Cumulative Quarterly Data." On a current basis, according to the subsequent chart, we approached the figure of three times their strength by the spring of 1944. For climactic and logistic reasons the decisive great invasion of the Axis fortress from France into Germany was fixed for late-spring 1944 because of the accomplishment by then of dominance to predominance by our Alliance as represented by this cumulative ratio of one and one half times. Thence it was deemed there should be smooth sailing for the geographical sweep from France into the Ruhr, and when the Ruhr was conquered it was held that that would mark the end of the war.

By way of documentary light on the elucidation of these operational hypotheses and their utilization by the President, I am similarly submitting to you in your official capacity the document of end-October-early-November 1944 as confidential evaluation to President Roosevelt, entitled "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War." The following excerpt from page 2 of the memorandum highlights the utilization of the graphs for the timing of the end of the war in Europe as spring 1945:

" ... The pulse of the plight and the prospect of the salvation for our cause has been registered on the counterpointed readings of the War Expenditures in Total and in Combat Munitions of the Members of the Alliance Against the Axis Since 1938. We have thus far timed with extraordinary closeness the periods of the major offensives related to current and to cumulative comparative positions.

"The projected date for the end of the war as spring 1945 has been related to an attainment of accumulated munitions expenditures by the Allies in excess of double the accumulations by the Axis since 1938."

In this connection it is of salient historical significance that this very orientation and methodology were utilized to relieve the President's mind of panicky fears and plans by high military authorities that Nazi resistance would continue for a long period after in the form of guerrilla warfare. The answer to that was set forth by me in the very sequel paragraph as follows:

" ... The great fact about Nazism, as this writer succeeded in crystallizing it in the very midst of the 'White War' victories of Nazi Germany, is that it is the first attempt in history at deliberate de-politicization, de-societization, and de-civilization of a people and a culture. The Nazi system is: 'Tyranny implemented with modern technology, deliberately uprooted from the Western tradition of an ecumenical order of law and conduct that in principle has been common to the variously articulated societies that compose the Great Western Society

particularly since the Renaissance.'

"This retroverted primitivism via tribalism fused with technology will, once the control over technology is broken, literally fold up. The top Nazis may personally want to go through a Wagnerian immolation. But there can be no transmissible power from them to hold Germany together because there is no social system and no cohesiveness that is independent of coerciveness. Hence there will really be nobody to surrender, but only commandants in selected areas. Thus the understanding of Nazism is itself a clue to the final liquidation of Nazism and the Nazi State."

In connection with the task and the methodology of timing you might find interest also in reexamining the memorandum of October 1944 to the same President that utilized the measures of comparative munitions efforts for timing the end of the war in Europe as April 1945 and urged the immediate undertaking of negotiations with Japan's Imperial Household counter to the unconditional surrender formula, for reasons set forth therein. This document, copy of which was sent by President Van Dusen to Mr. John Foster Duller, elicited Mr. Dulles's comment of June 10, 1946.

* * *

Turning to another effort of a technical rather than intellectual nature that was made to reverse a time-lead held by the Nazis, I draw your attention to a memorandum submitted to high Navy officials regarding the invention of Dr. Emil Mayer and myself which we gave to the Navy in the summer of 1941. The memorandum is entitled "History of Proposed Anti-Submarine Protection Through Establishing Radio-Detector Buoys Coordinated with Maintenance and Supervision."

* * *

Finally, there is submitted a document with chart of January 1950, revised in April 1950, entitled "Approach to and Methods for Projecting Comparative Atomic Weapons Accumulations." By the therein adopted assumptions of rates of atomic weapons accumulations, in the wake of the Soviet's emulation the prior late-September of an atomic bomb, the successive stages of weapons growth led me to forecast what was denominated in the subtitle as "Soviet Testings in Far East, Blackmail Pressures on Europe and Middle East, and Accruing Threats to the United States." The concluding page 5 submitted a table on "Conjectured Estimated Timing of Successive Dangers," of which the first was "Mid-1950 for Far Eastern Aggressions Pivoted on Korea."

Now while the rest of the timetable was deferred by our subsequent advance and enhanced leadership through the hydrogen bomb, the situa-

tion symbolized by Sputnik reinstates the second and third stages of "Blackmail Pressures on Near East and Western Europe" and threatened "Neutralization of U.S. Effective Atomic Weapons Leadership" for ourselves and for Western Civilization. -- If space and time permitted, a parallelism could be drawn between this situation and the position of Europe in the mid-Thirties, when it had been confronted by Hitler's reoccupation of the Rhineland and had unfortunately shrunk from the pressing urgency of immediate reorganization of its defense postures and weapons technology.

* * *

The operational ~~goals~~ represented by all the foregoing have, through the acceleration of ~~time~~ tempo of history as well as technology, become all the more timely and urgent, or what the Scots call "timeous". -- Hence the importance of our exploring together the appropriate procedures for gathering these ideas and applying them to the contemporaneous crisis which - in keeping with the original meaning of the word "krisis" as reflected by the great Johannine verse, "Now is the judgment of this world ..." - is truly a challenge to judgment.

With high regard,

Sincerely yours,

Dr. Ellis A. Johnson
Operations Research Office
7100 Connecticut Avenue
Chevy Chase
Washington 15, D. C.

[Copy]

Frank
(Lawrence)

August 12, 1958

Dear Mr. Lawrence:

A considerable time has elapsed since I had the pleasure of meeting you in mid-May at that luncheon conference in honor of our friend, Dr. Urey, and the other physicists and chemists who were proceeding to Israel and the Weizmann Institute's conference on nuclear developments. Your request for some unpublished material in my possession on the origins of the Atomic Project has been much on my mind. But on top of pressures from my professional tasks there have latterly emerged types of national emergency that one who is within perimeters of consultancy has had to labor on. Thus the expected margin of leisure for the kind of responsive presentation that I had hoped to make for you has been completely unavailable. Yet there is the sense of obligation because of the promise made to you, all the more telling because of my admiration for your penetrative and sustained work on our nation's role in atomic developments.

Accordingly, I am submitting to you a reproduction of contemporaneous notes of mine on the prelude to the history of the Atomic Project, namely notes on conferences that I had had at the Institute for Advanced Study in Princeton from February through May 1939. For your convenience, one of my aides in the office has transcribed in typed form my recorded handwritten notes, - which transcript bears some slight revisions by me. I am also enclosing copy of a covering page to my handwritten notes that I put in my looseleaf book late that very year. The page bears the marks of the aging of the paper over that near twoscore years of elapsed time. Parenthetically, the assembled pages in the photostat from that old notebook of folded pages bears the marks of the clips of the looseleaf.

All these are prior to the developments of the summer of that year. These, so far as the publicized and popularized but only partially correct history, pivoted on Dr. Einstein. But in point of historical fact it was a group of physicists that included and for a while was dominated by Dr. Szilard, who had sought to interest the Navy in the need for fostering research in atomic physics and who had brought this matter to the attention of Mr. Ross Gunn, a scientific adviser to the Navy, at the meeting by the Physical Society in Princeton in June that year. The negative attitude voiced by him in a letter, of which I have a copy, prompted independent efforts by individual members of the group. They realized that only the President as the Commander-in-Chief could, in the grammatical mood of "Might", be able to overcome this indifference and potential hostility of the Services to becoming involved, especially in view of the then negligible budgets for defense under which they were operating. The approach to me by Dr. Szilard and others was based not

only on my known close relationship to the President but also on my reported long-maintained interest in scientific developments and the intimations that were given by my Princeton friends of my having already been au courant with those researches that Niels Bohr had reported on to his Princeton friends. Since throughout my association with the President, harking back to 1932, I had succeeded in eliminating leakages to the public, including people who were in corresponding closeness of touch with him on other matters, I naturally avoided conveying to those physicists anything more than that in some way the President was already apprised of the deemed far-reaching significance of that emergent new science and technology. What was deliberately not conveyed to them was my independent and complementary relationships to the problem. Thus Dr. Walter Stewart was then the Chairman of the Rockefeller Foundation as well as a Professor at the Institute for Advanced Study and Dr. Aydelotte was not only the new Director, but, by virtue of his representing on this side the Rhodes Scholarship Fund, had important contacts in Europe.

In the course of my conversations with Dr. Szilard, I apprised him of and secured his collaboration in a program for systematic presentation of the problem to the President so as to establish an auspices under which the White House could give the matter a consideration independent of the Services. The only agency that could thus serve as an originating center for scientific problems was the Bureau of Standards. In furtherance of this idea of an independent auspices a scientific personality had to be made a point of origin. It was then already known to me that Dr. Pegram had approached the Navy by a letter with Dr. Fermi as the emissary and that that mission was a failure. That failure stemmed in part from Dr. Fermi's own expressed skepticism about the probabilities of a chain reaction, and also in part from the then preoccupying interest of the Services in American neutrality as distinguished from intervention. This latter element, which was known to me from my interchanges with the President, was held in such confidence by me that not a word of it was breathed by me to Dr. Szilard or the other physicists, for the very simple reason that that was a strict confidence between me and the President. We now know what a long and arduous struggle the President had not only to get the Neutrality Act modified but to get the heads of the Services in a frame of mind to pursue programs on the assumption of having to intervene as the war progressed.

With the elimination of Dr. Pegram as the then head of a physics department wherein atomic research was progressing and with the coordinate elimination of Dr. Fermi as a recognized experimenter in the field, another personality as a center had to be found. I thus hit upon Dr. Einstein. A proposed letter for ~~his~~ signature was

drafted by me with the aid of Dr. Szilard, and the text of the letter reverberates with references to the international situation and with references to the potential economic applications that linked up to my submissions to the President in March that year, - submissions dealt with in the enclosed reproduction of my contemporaneous notes herewith. It was my then plan to submit the draft-letter to Dr. Einstein myself, but intervened calls to Washington prevented it and so the task was entrusted to Dr. Szilard. But the good offices of Dr. Aydelotte had already been secured by me and he had already apprised Dr. Einstein that his signature would be required to a letter to the President that had been dictated by me. This disposes of the mass of apocryphal stuff about how the emissaries worked on a letter by Dr. Einstein which they revised. As you know, it was never difficult to get Dr. Einstein's signature to appeals for or letters about good causes, and such appeals and letters were not authored by him but for him.

Again, the role of the letter, like the role of reprints of articles on progress in atomic physics, was that of an element in a dossier for the auspices for consideration by a group in Government independent of the Services. The rejection by the Services was so final that their opposition could be relied upon. It is important to recall and recapture the state of pre-World War II Washington, still preoccupied with a domestic economy with huge unemployment and merely worried about Europe as a source of annoyance and disturbance to the progress of a very slow and inadequate recovery. In that setting and in view of the unpopularity of even the mere idea that the United States would become an active intervener in the war, it is understandable how powerful would become any active opposition by the Services to letting their very meager appropriations get sidetracked into such futuristic scientific research as atomic physics. Once more, all this could not be told to the scientists who were talking to me, whether or no they were native or new Americans. That side of the rejection by the Services just had to be kept confidential, and I must most emphatically state here that I do not want you to deal with that. I am telling it to you in confidence at this long distance from the events so that you understand the why and the how of the unfamiliarity on the part of Dr. Szilard and others with the then governing obstacles to securing a re-consideration.

Between the accumulating of the dossier - of which the Einstein letter was merely an element for the record - and the submission of the project for the President with the express objective of his authorizing the Bureau of Standards to act thereon there intervened the preliminaries to the Hitler march on Poland. These absorbed all the time of the President and, aware as he was of the improbabilities of preventing Hitler's aggression, his then gravest concern was

with removing the shackles on American defense and liberty of action in favor of the Allies as a result of the Neutrality Act still on the statute books. It was available for me to take up with him this problem in the months of August and September, as I was making visits to Washington during that period on other problems in connection with domestic and international affairs. But under the pressure of those other concerns of his the odds were overwhelming that his decision would be negative: that he would be averse to challenging the negative attitude towards and rejection of the project by the Services. That is why my letter to the President that I handed to him on October 11, 1939 opened with the following sentence: "With approaching fulfillment of your plans in connection with the revision of the Neutrality Act, I trust that you may now be able to accord me the opportunity to present a communication from Dr. Albert Einstein to you, and other relevant material bearing on experimental work by physicists, with far-reaching significance for national defense."

Once more, in mindfulness of the delicacy of the situation in Washington, my defined first and second targets of its utility for the United States were "the creation of a new source of energy which might be utilized for purposes of power-production," and as the corollary the uses of "new radioactive elements ... in the medical field." Only after that did I feature the possibility of a bomb: "the construction, as an eventual probability, of bombs of hitherto unenvisaged potency and scope." Then came a re-statement of the then pertinent international situation and its rapidly emergent changes: "In connection, then, with the practical importance of this work - for power, healing, and national defense purposes - it needs to be borne in mind that our supplies of uranium are limited and poor in quality as compared with the large sources of excellent uranium in the Belgian Congo, and next in line Canada and former Czechoslovakia ..." It is further noted in my testimony, enclosed herewith (page 557 of the reprint), that I had conveyed to him in that conference a brief report on "the successful experiments of Drs. Hahn, Strassmann, and Meitner," and also about experiments proceeding on this side by "the people who had been at work on and who had been consulted."

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To this admittedly selected and also foreshortened account I am adding a reproduction of an article in Look for March 1950 that was written by Nat Finney who had been given access to my notes. The early part of that article contains in much briefer form a statement of the origins of the Project and places in the perspective of my initiatory explorations of the idea with the President the subsequent

collaboration with the scientists and adumbrates the above detailed description of the role of Dr. Einstein.

The specific confirmation by Mr. Finney of the foregoing is found in the paragraphs between the arrows on the penultimate page of the enclosed reproduction of that article. The salient elements of those paragraphs can be condensed as follows, with utilization of Mr. Finney's words:

- (1) The direct knowledge of the lectures by Lord Rutherford and F. W. Aston in 1936-7 that forecast atomic energy and the potentiality of an atomic bomb.
- (2) The contemporaneous knowledge, including the securing of a translation of the Hans Strassmann report.
- (3) The direct and cumulative contact with refugee scientists in atomic physics.
- (4) The supervened contact with Dr. Szilard "in the summer of 1939 when the Navy Department flatly turned down proposals for an Atomic Project."
- (5) The writer's efforts prior to that "to familiarize President Roosevelt both on the scientific possibilities of atomic energy, and the political possibilities if Germany became able to terrorize the democratic world with an atomic devastator."
- (6) Having become "F.D.R.'s personal Jeremiah on technological warfare" along with "fear of Germany's terrorizing the whole world," it was "during the late-summer of 1939" that "the subject was broached with President Roosevelt about starting an Atomic Project."
- (7) But "the President was so preoccupied ... that he could not then give the matter attention."
- (8) Thus emerged the arrangement for awaiting the opportune occasion for convincing the President and the corollary preparation of a "dossier of material," which would contain "a supporting letter by Dr. Einstein."
- (9) In the presentation on October 11, 1939 the overture consisted in the reading to the President of "a long letter-memorandum ... along with the letter signed by Dr. Einstein and a joint memorandum signed by Dr. Szilard and himself" (this writer).

(10) But that did not suffice, for the broad reasons first stated in the foregoing passages of this communication which I had not vouchsafed to Mr. Finney and for another reason that is featured by Mr. Finney. The latter one is the intimation, as an initial guess-estimate, that it could be a billion dollars, and that was later rendered the more specific as a probable cost of \$2 billion on the basis of the "telescoped cost of electrical power in the generation before World War I."

(11) The crucial and decisive overcoming of the negative attitude occurred the following day by means of the historical analogy of Napoleon and steam power, as recounted on the closing page of the article.

As still further documentary support on the independent and initial concern of the writer with the potentialities of atomic research for power and for weapons, your attention is drawn to pages 554-556 of the enclosed reprint of my testimony.

It remains to add that the intense and poignant concern of the writer over the potentialities of Nazi Germany's exploitation of atomic power for bombs came from the experience of missed opportunities by the West through the early years of the Hitler regime, and the exchanges of views between the writer and the President on the consequential accumulation of Nazi power and its leadership in the air, - all of which from 1937 on was serving to intimidate and to terrorize small and intermediate powers, and at the time of Munich the major European powers. It was in the wake of all that that the writer's exposition on March 10, 1939 of his thesis on "Imminence World War in Perspective Accrued Errors and Cultural Crisis of the Interwar Decade" led him to conclude that the United States just barely had time for what he called "the time-drafts that can still be made on the Bank of History for the preparedness that has and will become more and more urgent and inevitable ... in the course of the prospective unfolding aggressions of Nazi Germany."

With kind regards,

Sincerely yours,

Mr. William L. Laurence
The New York Times
229 West 43rd Street
New York, New York

Weekend February 3 - 4 1939

Meeting with Frank Aydelotte Director Designate (successor to Abraham Flexner) Institute for Advanced Study, Princeton, N. J. Also with Robert B. Warren and V.W. Stewart at the Institute.

Continued by Cassandra warnings of imminence around the idea of March new Nazi-timing for aggression by a coup in Czechoslovakia, knocking the pins under the ramshackle Munich "settlement." Testing of ideas of imminent outbreak World War around August - September - in connection forthcoming Report for FDR arranged early March.

The approaching all - encompassing eclipse of civilian life for a most terrible technological war with unspeakable Nazi cruelties coincides in mysterious Providence with hints new scientific revelation through convergent discoveries in atomic physics. Under spell of just received book Background to Modern Science (Cambridge University Press late 1938) which includes the lectures of Lord Rutherford that I had heard in 1936 - With what modesty the late Lord Rutherford calls "the work of Bohr to be one of the greatest triumphs of the human mind." That applies to himself and not only to those giants but to the whole assemblage of workers in Europe on atomic physics, many of whom have been scattered(?) to our country and struggling to get laboratory spots and to pierce the surrounding indifference from the establishment of science research, as already well known from people within my ken among the refugee scientists.

The sequel lecture by P.W. Aston on Forty Years of Atomic Theory concludes with this terrifying challenge: "Personally I think that there is no doubt that sub-atomic energy is available all around us, and that one day man will release and control its almost infinite power. We cannot prevent him from doing so and can only hope that he will not use it exclusively in blowing up his next door neighbor" - What terrible portend if Nazis should get it !!!

On return Sunday February 5 1939

By strange coincidence the subject uppermost in my mind been found dominant in the whole Institute Community under the impetus and inspiration of Niels Bohr. F.A. showed me copy of a letter written by Niels Bohr while at the Institute bearing the date of January 30, 1939 to the Editor of Nature, entitled "Disintegration of Heavy Nuclei." The letter opens: "Through kindness of the authors I have been informed of the contents of the letters recently sent to the Editor of Nature by Professor L. Meitner and Dr. Frisch." It ends with a plea for "the prompt continuation of the experiments in the new type of nuclear disintegration."

The communication to be published presently refers to the revolutionary accomplishment by two German chemists, Hahn and Strassmann, of atomic fission, and carries this further in the light of their cooperative experiments in the Stockholm Institute.

Follow-Up Weekend Visit, March 17-18 -

At the Institute the since received copies of Nature containing the Meitner-Frisch communication was more of a resounding subject than the fulfillment of my General Cassanira forebodings voiced in my early-February visit. In the same Nature for February at pages 239-40 there appears the letter-type of communication under the heading "Disintegration of Uranium by Neutrons" by Meitner and O. R. Frisch under respective pseudonyms academic auspices in Stockholm and Copenhagen. Interestingly, the same issue contains a leading article which features the Hahn-Strassmann experiments of the turn of the year, 1938-1939, based on two reports in Naturwissenschaften: "Isotopes of the Alkaline Earth Metals from Uranium." The issue received also contains report by Professor Otto

Mahn and F. Strassmann: "Chemical Properties of the Trans-Fission Elements."

Read the paper which had the sympathetic audience of the Chief - the prior week and which had been presented in full before the ex same St. John's College on March 10th. Also, confirmed on the Idea of March through the Nazi seizure of conservative Pragan Government and the introduction of a mere entry, Kochs. The address which became the center and circumference of the Institute conferencing is entitled "Imminent World War in Perspective - A Survey of Errors and Cultural Crisis of the 19th Interwar Decades." The emergence of the new crucial scientific experimentations needs to be brought to the Chief's attention, but he must remain preoccupied with the shadow activities on the assumption that war Nemesis is avertible - and then will have to remove the self-imposed shackles of the Neutrality Act.

Further Visit With RW and FA, Weekend April 21-22, 1939

The Princeton community continues to resound with new disclosures in the scientific world through the medium of Nature. The issue of March 18th contains another communication from L. Meitner and G. H. Frisch entitled "Products of the Fission of the Uranium Nucleus." The sensation of the issue, however, to the scientists of the Institute and Princeton University, was especially the subsequent refugee Professor Wigner, is the communication from the Joliot-Curie group. His collaborators as listed are Dr. L. Kowarski and Hans von Halban, Jr. (the extraordinary mixture of man-of-the-world, well-married to a Rothschild and inventive experimenter). Their communication from the Radio Laboratoire de Chimie Nucléaire is entitled "Liberation of Neutrons from the Nuclear Explosion of Uranium." Wigner always thinks the Joliot-Curie group further advanced than any in the world - in view of Fermi's continued skepticism about reinforcing nuclear energy processes.

Origins of Concern with
Significance of Atomic Research
for U.S. Defense and for
U.S. Role in the Overriding
World Crisis from Beginning of 1944

[Consp]

The New York Times

TIMES SQUARE NEW YORK 18 NY
LACKAWANNA 4-1000

August 26, 1958

Dear Dr. Sachs:

Many thanks to you for the invaluable and highly interesting material you have sent me. It is much more than I expected and I shall certainly make good use of it in the future.

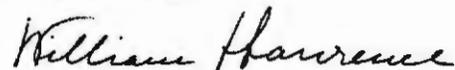
There are two items which surprise me greatly.

One is that Enrico Fermi did not have faith in the practicability of a chain reaction. That appears to be contrary to the information from many other sources on the subject, including Mrs. Fermi's book, "Atoms in the Family," as well as the well-known fact that as early as March 17, 1939, Fermi went to Washington to interest the Navy in the possibilities of fission in naval vessels and in explosives. This fact is reported in the Smyth Report, as you know, as well as in Mrs. Fermi's book.

The second item that surprises me is the information that President Roosevelt had intended to make a public demonstration of the atomic bomb on some uninhabited spot to which he was to invite representatives of Japan and others. It seems surprising to me that Roosevelt did not confide his intention in this respect to any one, including such intimates as the late Secretary of War Stimson. It is possible, of course, that Roosevelt intended to do so, but if he had, it would have been to someone higher up than his military aide. I would like very much to have a copy of that memorandum mentioned in the Look article by Finney, if you still have it. In case you have destroyed it, I think it would be highly important if you were to resurrect it from memory. Such a memorandum would be a highly valuable historic document, and it would be a pity if it were to be lost for posterity.

Congratulating you belatedly on the important rôle you have played, and thanking you again most profoundly for the wealth of historical material you have supplied me with, I am

Sincerely yours,



William L. Laurence
Science Editor

Dr. Alexander Sachs
25 Broadway
New York 4, N.Y.

[Copy]

File L
(Laurence)

August 28, 1958

Dear Mr. Laurence:

In view of the salience of the two issues raised by you, the best form of thanking you for your letter of August 26, is to hurry off some reply, as next month will be very crowded with other tasks.

Taking up the second point first, let me invite you to reexamine the proposal regarding the issue of the atomic bomb as it was transcribed by Mr. Finney in the article. That proposal, as set forth in the middle section of the third column on page 24, distinctly limited the participants in "rehearsal demonstrations" to "representatives from all Allied countries...neutral countries..., ((and))..the major religious faiths)". There is, therefore, no basis for raising questions about whether this or that enemy would be included, specifically your reference "representatives of Japan and others".

As to derivative questions raised by you on that point, your reperusal of the relevant paragraphs will elicit an explanation for the misimpression received regarding Japan. The group of paragraphs had as a heading "Our Enemies Would Be Warned". That "warning" as provided by the plan, was to "be issued by the United States and its Allies in the project to our enemies in the war, Germany and Japan". Along with such warning there was to have been included in significant measure the "report on the nature and the portent of the atomic weapon", as previously "prepared by the scientists and other representative figures".

Furthermore, you will note from the text of the article that the then record that I had was included in a letter that I had written to Secretary of War Robert P. Patterson in the summer of 1946. That letter I still have and also have Secretary of War Patterson's acknowledgment and the material of the special dossier that was prepared for and used in my conference with him. Inasmuch as the impaired state of health of Secretary Stimson in the closing year of the war brought about extensive and increasingly extensive devolution of responsibility on his assistants, Secretary Patterson's acceptance of my account is of decisive historical significance. Secretary Stimson's book entitled On Active Service, while written largely by Mr. Bundy, bears recurring witness to Stimson's increasing dependence on Patterson as the war continued. Still furthermore, if you will be good enough to reexamine the early section of the Look article entitled "A Plan to End the War with Japan" you will find further confirmation, in turn supported by Stimson's book, of the fact that the Army High Command counted on resistance by Japan. The relevant chapter in Stimson's book, chapter 23, was quite clearly written by Mr. Bundy. Indeed, Mr. Bundy is designated on page 614 as having been "Stimson's direct agent...and contact with the work of General Groves". The Army's plan for the invasion of Japan, as set forth on page 618, appears not to have had direct collaboration from the

Secretary of War. Indeed, it is stated by Mr. Bundy, on page 619, that "these plans did not bear any significant impress from Stimson, who was never directly concerned in the handling of Pacific strategy". But it is added that he accepted them, and that he deemed them "wholly sound". The implication from that is that he had thought the invasion was indispensable.

While only marginally interested in the atomic bomb and while sceptical that it would succeed in rendering unnecessary invasion--and the enormous casualties of such invasion--Secretary Stimson did accept the project, as is apparent in his report of a conference with the President on March 15, 1945, the last one that he had had, but not other advisers. Incidentally, the composition of the committee, as given in the footnote to page 616, contains another confirmation of the position I have taken in my submissions to you that in those closing months Secretary Stimson delegated more and more of vital work to his assistants. That footnote states "the principal labor of guiding its extended deliberations fell to George L. Harrison, who acted as chairman in my absence".

Finally, the minutes as preserved and recorded of mid-March with President Roosevelt, contained an indirect reference to my plan. The middle paragraph on page 616 concludes as follows: "Third, we must face the problem that would be presented at the time of our first use of the weapon, for with that first use there must be some public statement".

* * *

Having taken considerable space for furthering to clarify and, if you will allow me, to correct inferences drawn by you with respect to "the second item", I will leave for a later communication the substantiation of my statement regarding the attitude of Fermi in 1939 and, particularly, during his conference with the Admiral. Here, too, the decisive consideration is that in my records there is preserved a completely contemporaneous account and not merely the retrospective ones included in the later books and articles that have been published by others or that are contained in what might be called the "oral tradition". The contemporaneous record was, of course, based on information that was given to me by the President and his direct assistants. In this forthcoming letter I shall provide the vindication of the statement I have conveyed to you.

Sincerely yours,

Mr. William L. Laurence
Science Editor
The New York Times
Times Square
New York 18, New York

[Correspondence]
7/22 L

September 3, 1958

Dear Mr. Laurence:

Resuming consideration of your letter of August 26th in the light of my prior letter to you and the follow-up one of August 28th, the question at issue can be accorded corroboration by certain independent sources. But unlike the document presented to you, those sources - apart from the obviously rough and incidental references in Smyth - are much later than Smyth, and all of them long after the accomplishment of chain reaction and the invention of the atomic bomb. They are thus not directly revelatory of the original uncertainties and anxieties that were in ferment throughout the sequence of crucial experiments and the reflections upon them by the scientists concerned. Two of the sources are the late Professor Arthur H. Compton's Atomic Quest (published by the American branch of Oxford University Press in 1956) and the book published two years earlier by Mrs. Enrico Fermi (Laura Fermi), Atoms in the Family (University of Chicago Press, 1954).

Professor Compton's book is extremely defective and variously erroneous on the processes and the progress of the labors under my direction for securing the support of the White House and the conversion of the Bureau of Standards and then of the Government to the unlimited support and prosecution of the Atomic Project. But Professor Compton's book provides some major lights on why it took Professor Fermi considerable time to change his attitude of initial skepticism, by reason of the prerequisite self-correction that Fermi had to undergo. That process is described by Professor Compton in Chapter 1, particularly the sections from page 16 on regarding what Professor Compton calls "Fermi's mistake in 1934," and again at page 19, after Bohr's report of the Meitner-Frisch confirmations of fission in the conferences at Princeton in the early-spring of 1939: "Enrico Fermi ... now saw how he had come to make his famous mistake." Conversely, the book of Mrs. Fermi is much clearer and correct on personal matters and completely silent on the intellectual ferment that was going on, chiefly at Princeton and Columbia.

Inasmuch as my contemporaneous notes - a reproduction of which was vouchsafed you - do reflect that ferment, you will find it interesting that Professor Compton, at page 23, confirms my contemporary interpretation of the importance of the then French work: "Already in 1939 von Halban, Joliot, and Kowarski, working in Paris, reported evidence that if uranium is dissolved in water in which a source of neutrons is immersed the number of neutrons is increased. This was a first step toward producing a chain reaction." -- Now that reference serves as prelude to a statement of the then prevailing concept

of the problem of chain reaction: "But the smallness of the increase indicated that no extension of this mode of approach would make the reaction self-sustaining."

Later on in this chapter, or in Section 3, called "Early Attempts to Put Atomic Energy to Military Use," an account is given of the attempt by Professor George B. Pegram of Columbia to interest Admiral S. C. Hooper, then Director of the Technical Division of Naval Operations. That letter specifically contained the reference to "the possibility that uranium might be used as an explosive" and the immediate expression of skepticism as follows: "My own feeling is that the probabilities are against this ..." That is the letter of the 16th of March, 1939 which, by footnote acknowledgment, was taken by Professor Compton from the 1954 book by Laura Fermi. This is followed by an ex post facto rationalization to the effect that the language employed was "the professorial way of saying, 'Though I'm not sure of its full meaning, here is something vital to the nation's safety.'" Then follows another statement to the effect that though "the Navy's reply to Fermi seems to have been simply a polite 'thank you'," the Naval Research Laboratory was "actually" proceeding with "the possibilities of uranium fission" as "already under discussion." This is incorrect and is refuted by the letter from the technical advisor to the Navy which I included in my material to you, by my whole testimony on November 25, 1945, and the documents that I had submitted to the War Department, which if in any way incorrect would have been questioned.

Returning to the emergence of divergent attitudes towards the possibility of a chain reaction, the discussion earlier in that chapter of what in a sub-section is called "The Possibility of Chain Reaction" (as in the original at page 19), admits the following:

"Through all this preliminary period, however, no suggestion had arisen of any way in which the energy in the atomic nucleus could be released in practical quantities ... No one saw how such atomic processes could affect any substantial number of atoms. Many felt such hope futile. There were others like myself who still hoped and kept on working."

The then emerged possibility that "the nucleus of the uranium atom should have in it several more neutrons than the nuclei of the two atoms in which it divides" was being worked upon in the summer of 1939, according to the later account. It is that later work which led particularly Szilard and Wigner to adopt an attitude bordering on optimism while others remained skeptical. To pursue that work would, it was realized by them, entail what Compton correctly designates at the end of page 20 as "an effort ... enormous and perhaps

prohibitive." The impetus to it came from concern over the implications for the national safety of Western Civilization in the event that the Germans would be pursuing theirs with no limits while we would be working on a mere university laboratory basis. That is the point of view articulated in my testimony which inaugurated the hearings of the Senate Committee on Atomic Energy at that time. This is indirectly confirmed by Professor Compton at the middle of page 20 as follows: "Szilard and Wigner were greatly concerned over the military danger opened up by these studies. They knew by now that the German scientists were working actively towards a nuclear chain reaction. The added strength that this might give the Nazi power could mean world tragedy." It was at this point and through reciprocal awareness by the scientists of my concern and my awareness of their concern that the liaison was effected by me and also for me with the scientists at Princeton and at Columbia.

* * *

Further corroboration is found in Laura Fermi's already mentioned book. After narrating in Chapter 17 how she came upon the letter ten years after it had been written and after setting forth, with rather few omissions, that letter to Admiral Hooper of March 16, 1939, she records at the bottom of page 163, "Enrico had seen the Admiral ... The interview had yielded little result." Then at the top of page 164 she reports the question she put to her husband: "Couldn't you arouse the Admiral's interest in the atomic bomb?" The late Professor Fermi's answer is important and crucial for the question put by you:

"You are using big words. You forget that in March 1939 there was little likelihood of an atomic bomb, little proof that we were not pursuing a chimera."
(italics added)

She then proceeds to give her own reflections: "It is not surprising that this attempt should have been inconclusive ..." That Fermi should wish to contact Admiral Hooper because he happened to be in Washington, that he shouldn't plan his trip in order to see the Admiral further minimizes that "bare possibility of atomic explosives that sounds in itself over-casual, now that atomic weapons are a fact." (italics in original)

Thus far, then, the source accepted by you, the memoirs written by Mrs. Fermi, contain a most telling and decisive confirmation of my position in the form of a recalled and recorded statement by Enrico Fermi himself that at the time he had met and conferred with Admiral Hooper "there was little likelihood of an atomic bomb, little proof that we were not pursuing a chimera." Such a quotation is a near

equivalent to a contemporaneous statement by Fermi of what he had thought in the spring of 1939, namely that he was skeptical of the reality of a chain reaction and hence derivatively the produceability of atomic explosives. Secondly, Mrs. Fermi anticipatorily challenges and contradicts the retrospective rationalization of the Pegram letter that was made by Professor Compton in his book published in 1956, called Atomic Quest. Whereas Professor Compton attributes the language to academic "super-caution", Mrs. Fermi maintains that the incidentalness and the over-casualness of the appointment made with Admiral Hooper "further minimizes that 'bare possibility' of atomic explosives." That, too, is substantiation for those contemporaneous notes of mine in the spring of 1939.

And by way of reinforcement, let us proceed to examine Mrs. Fermi's further clarification. That further clarification amounts to an affirmation by her of the position taken by me that at that time both Professors Fermi and Pegram had an attitude of "skepticism". The account proceeds as follows:

"Professor Pegram's attitude was due to his cautious judgment that warned him against jumping to premature conclusions. His skepticism about the outcome of the work in his own laboratory was shared by many other scientists, and was probably caused by a hope that nuclear weapons would prove unfeasible." (italics added)

Now the expression in the above sentence "many other scientists" encompasses Fermi, and particularly so as articulated and confirmed by the immediately ensuing sentence:

"And Enrico himself, when talking to Hooper, doubted the relevance of his predictions." (italics added)

There just is no room for interposing any doubt about such affirmations that when Dr. Fermi had talked to Admiral Hooper he did, and expressly so, voice doubts regarding those predictions which had applied to what Professor Pegram had called "the bare possibility" of atomic explosives, which could only come from the experimental proof that a chain reaction could be produced from atomic fission.

To be sure, other questions do arise, such as, 'Then why did Pegram write Hooper?' That is also answered in these memoirs, and that was part of the struggles in which I had participated in advance of Szilard in the course of my independent conferences at the Institute for Advanced Study. The position that I took ab initio was that so long as there was a "bare possibility" the United States and the Free

World had to explore it and become an independent co-discoverer in order to prevent Nazi Germany from becoming the one and only power that could have atomic explosives, lest out of that would come a terrorization of the United States and Western Civilization. That such a view had harmonic echoes among the physicists other than both Pegrarn and Fermi is averred by Mrs. Fermi in the sequel to her account of the expressed doubts of the probability of atomic bombs by Fermi:

"Skepticism and doubt, however, did not lighten the burden of responsibilities on the physicists' shoulders ... Hungarian-born physicist Leo Szilard felt more sharply than anyone else in this matter of double responsibility of the scientists toward the Government and of the Government toward that part of science that might become useful to the military. Szilard talked repeatedly with his friends and aroused some of them. In July 1939, he and another Hungarian-born physicist, Eugene Wigner, conferred with Einstein in Princeton ..."
(italics added)

This, in substance, conforms to my contemporaneous record. Whereas Fermi as the leading experimental physicist involved in nuclear research was at the time skeptical and had indeed, by Mrs. Fermi's revelation, accentuated the doubt conveyed by that letter of Pegrarn, there were others who were more sensitively perceptive of the political implications. These on the non-physical science levels included the present writer as independently and originatively concerned, and also his friends, Professors Walter Stewart and Robert B. Warren, and, more marginally, the Institute's new Director, Frank Aydelotte. From the point of view of potential influence, the significance of Walter Stewart was that he, in addition to being a Professor of Economics at the Institute, was also Chairman of the Rockefeller Foundation. And the importance of the other two lay in their close following of international developments.

Thanks to the visits and conferences deliberately made by the writer with them, that stream of concern effected a confluence with the stream of concern from such physicists as Eugene Wigner at Princeton and those who were seeing those physicists, - preeminently Leo Szilard, who, while not yet recognized as of corresponding standing, was very active in his agitation. Now at the very time that I was about to meet Eugene Wigner, Leo Szilard was independently seeking contact with me and secured it through a friend. As a result of my first meeting with him in July, I proposed the strategy of an independent presentation to the President by me, for which I wanted a dossier. It was in implementation of that plan of mine that Szilard

and Wigner arranged to see Einstein and advised him that there would be prepared for his signature a letter. The selection of Dr. Einstein, in which they readily concurred, stemmed from a three-fold realization: the first part is covered by all the foregoing and constitutes the reason underlying the importance of a correct instead of the conventionalized history. That first part revolves around the then already recorded and disseminated information from the Navy to the White House that both Drs. Pegram and Fermi had been extremely skeptical about the "bare possibility" of chain reaction and therefore atomic explosives. That information was within my ken, but, as a result of my very special relationship, was not so characterized by me to that conferee or other conferees. Furthermore, while Dr. Szilard was not thus fully advised, he did have the negative reaction of Mr. Ross Gunn, as per the document transmitted by me. Secondly, in view of that attitude of those in a specially authoritative position towards the laboratory work being done, one couldn't, with any certainty, rely on being able to enlist support of any other recognized American physicist. And, thirdly, so far as my self-set, well-nigh impossible task of outflanking the by then crystallized negative attitude of the Services was concerned, it was decidedly best and most fitting to use the name of a scientist to whom the President naturally responded with awareness and admiration, namely Dr. Einstein. And to envelop all these considerations into a single characterization, the leitmotif adopted by me for the presentation was the threat of intimidation from Nazi monopolization of atomic research, which transcended the issue as to the degree of probability of the scientific break-through, as we now say, involved in the issue over the prospects of a chain reaction.

* * *

Such, then, is the re-evocation by me of the fullness of my thought at the time. I am sure you understand that an exposition of this kind is confidential, and as it is intended for utilization by me at the proper time, I should appreciate your letting me know in advance regarding your possible desires in that connection.

Continuing with this exposition, it is noteworthy that the account by Mrs. Fermi comes closer to the fullness of the original facts than Professor Compton's book or the prior publicistic accounts. There remains in the account echoes of the dramatized stories about the bringing of the letter. But the crux of her account is that the letter was prepared for Dr. Einstein and that he was expected to sign it; and that he was to sign it in the role of "being by far the most prominent of all scientists in the United States." Again excluding the apocryphal element regarding discussions of the content of the letter "by several physicists," the account is correct in noting that the letter was made "ready" and that it was sent to

him through Dr. Szilard. That letter, as previously noted, was dictated in my office and was taken by Dr. Szilard for securing Dr. Einstein's signature, as I could not then go. At his then vacation place, Dr. Einstein, as correctly stated, "read the letter." The content of the letter that was drafted by me was keyed into the considerations spelled out herein and in the prior communications, and in mindfulness of the then political climate of the country, the potential civilian applications were thrown into sharp relief instead of exclusive or even preponderant emphasis on the military applications which were still enmeshed in uncertainty.

With kind regards, and with the desire that I be advised regarding your desires towards the elucidation of this subject matter,

Sincerely yours,

Mr. William L. Laurence
Science Editor
The New York Times
Times Square
New York 18, New York

January 16, 1959

Dear Robert:

In the course of our conversation this morning allusions to work on the inauguration of the Atomic Project and on the guidance of strategic and diplomatic policies led to the expression of interest on your part in seeing the documents involved. Accordingly, I am enclosing a photostatic copy of the Einstein letter, - which, in historical fact, was composed by me on the sixth floor of the One William Street office in the presence of Dr. Szilard and handed to him for taking to Dr. Einstein at the summer place designated, in pursuance of arrangements that I had made. The precise historical setting and course of events for my presentation to the President of the case for a reversal of the expressed disinterest of the Services in any aid whatsoever, - these were set forth in the article by Mr. N. S. Finney in Look for March 14, 1950 (page 26, from the central section, called "He Saw War Coming" and "1939: Year of Decision," through page 27, with the story of Lord Acton and Napoleon's blunders).

As to the reference to Mr. Dulles, I am enclosing reproduction of the exchange of correspondence between him and President Van Dusen. The document that elicited the greatest interest on Mr. Dulles' part was the memorandum of End-October-Early-November 1944, entitled "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War." While there is no hurry about returning these documents, I should like to have them back after your having effected such reproduction as you desire.

Sincerely yours,

Mr. Robert Lehman
One William Street
New York 4, New York



UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

[Corresp.: Atomic Energy Com
[Repts, Studies, Memoranda
AEC]
[Subj. File: AEC]

HA 7-1831
Ext.

July 24, 1959

Mr. Alexander Sachs
72 Wall Street
New York, New York

Dear Mr. Sachs:

As you may know, the Atomic Energy Commission last year established in its Washington headquarters a small staff of professional historians and assigned them the responsibility of preparing an official history of the Commission. During the past year intensive research in the records of the AEC and other agencies has produced most of the specific information needed for Volume I, which will include the wartime backgrounds and the establishment of the Atomic Energy Commission in 1946.

In our study of the origins of the atomic energy program in the United States, we are of course aware of the important part you played in securing Government support for this work. We have examined the report which you prepared in 1945 for the Secretary of War, and have discussed these matters with Dr. Lyman J. Briggs, and Colonel Adamson. There are, however, a number of points which we would like to discuss with you personally.

I wonder if it would be possible for Dr. O. E. Anderson, the Assistant Historian, and me to talk with you for an hour or so at your office on Tuesday, August 4. We expect to be in New York all that day and would be glad to meet you at your convenience.

Sincerely yours,

Richard G. Hewlett
AEC Historian

P.S. This letter is late in reaching you, for it was first sent by mistake to your old office at 72 Wall. I hope that the short notice will not prevent you from seeing us. I shall try to reach you by telephone Friday to learn if you will be available August 4. RH

File A

(Hewlett)

Sept. 29, 1959

Dear Messrs. Hewlett and Anderson:

Though conscious of a greater immediacy and urgency attaching to a report in progress for the Secretary of the Treasury, - in pursuance of exchanges alluded to in the Under Secretary's letter of July 20th attached hereto - I find it necessary to effect immediate follow-up to our conference of late-yesterday afternoon, in order to gather the benefits from the memories reactivated by the effort at recapture of the original coming-into-being or historicification of the Atomic Project. What the scientists call "resonance range" also applies to this enterprise of historical recapture by an original agent of the historical process.

With respect to the origination of the Atomic Project, there has evolved and become crystallized, at first at the hands of journalists and later at the hands of scientists who were connected with the Project, an exceptionally oversimplified, inadequate, and even fictional version. Since there was no historian attached to the White House and since not only President Roosevelt who had made the great decisions had passed away but also his assistant and official confidant in connection with the Project, General Watson, had also died without having left a record of his own, Dr. Smyth, when called upon in the finale phase of the war to write primarily and preponderantly on the scientific end of the Project, had to rely on such pieces of correspondence as could readily be assembled from the White House files. The conditions of secrecy were especially decisive for me because of an oath I had given to the President that no intimation whatsoever was to be given regarding the Project until the release by him and the submission to him, after the use of the bomb, of a report, which would be followed by a statement from him regarding my role as the originator of the Project and as his constant adviser on it. Therefore, only after the use of the bomb did I undertake the preparation of a preliminary report. The covering page of that draft of August 8-9, 1945 - a photostatic copy of which is attached - is contributive to a correction of the mis-history that has become accepted. For after the summary heading as "Early History Atomic Project in Relation to President Roosevelt, 1939-40," there follows an attempted condensation of the true course of events and of the process by which the President was converted to undertake the Project. While in the footnote below* the subheadings are given

* "From Inception and Presentation of Idea to the President and his Conversion to it by Alexander Sachs in the Context of the Aggravating World Crisis" and "Through the Overcoming of the Prior Definitive Disinterest of the Services and the Testing of the Proposed Project's Advisability by a Presidential Committee Inclusive of the Writer and Lyman J. Briggs, Director, Bureau of Standards, as Chairman, to the Transfer, in Accordance with the Writer's Recommendation, of the Project for Execution to the National Defense Research Committee established by Presidential Order June 15, 1940."

verbatim, it is desirable for the restoration of correct historical perspective to translate those subheadings into statements. The first statement is that the conversion of the President was effected "in the context of the aggravating world crisis." This was intended to make clear that the embarking on the Atomic Project not only occurred in the midst of the aggravating world crisis, but involved profound and pervasive considerations and accrued convictions by the President as to what, assuming that the optimistic view about a chain reaction would be fulfilled, American leadership, in promoting not merely the bomb but also the new science and technology, could mean for the United States and the world in the sequel to military victory. The second and fuller subheading brings into sharp relief what the thus far published histories have either omitted totally or have, even to the limited extent acknowledged, misconceived. The initial and at the time apparently insuperable hurdle that had to be surmounted was that the Services, and particularly the Navy, - as the only one that had any research organization and funds - had already indicated that they were not interested in doing anything to aid research on the Atomic Project.

While the very role of the Presidency under Roosevelt and since has become dominant in national administration and national life, the condition of national defense prior to World War II was small in scale, without any margins, and the military heads guarded jealously their meager funds; and due to their close linkage with the Congressional Committees of the House and the Senate, they could not be ordered about by the President to engage in any reallocation of their all too meager appropriations. Therefore, having turned down requests for aid in March and in July that had been made by Columbia University Physics Department members, the effort to revive the idea of aid through White House intervention required, first of all, the conversion of the President to a sense of the unique historic importance of the Project, and thereafter the devising of resourceful organizational strategy. The first strategy evolved and pursued by the writer was to shift the focus from the Services to the Bureau of Standards. Then came the struggles to secure favorable recommendations from the Committees constituted. While, through heroic efforts, acceptable reports - in the limited sense of being non-rejected - were secured, the Bureau of Standards scientists wanted assurance, that at the time could not be given, with respect to the availability of a chain reaction from atomic fission and the strength of that chain reaction to produce an atomic bomb. The recognized leader in the research, Professor Fermi, continued the skepticism that he had voiced to Admiral Hooper in his mid-March 1939 conference with him, as arranged by Dean Pegrum of Columbia. And that continued skepticism was related to a position that he had taken as far back as 1934, - a position which the late Professor Arthur H. Compton, in his book The Atomic Quest (published in 1956), called "the Fermi mistake." Indeed, as noted by Professor Compton, the intimations received from

the French researchers regarding a designated "first step towards producing a chain reaction" was not regarded by Fermi and others as bearing on their skepticism because, in Professor Compton's words, "the smallness of the increase indicated that no extension of this mode of approach would make the reaction self-sustaining." Thus there hovered over the organizational medium, through the Bureau of Standards, doubts which could not be resolved until the accumulation of the work in Chicago that was brought to a climax by the group under Fermi at the end of 1941.

Yet the complex considerations that impelled this originator-proponent and also the President, in the wake of his conversion, rendered advisable to the point of imperative that the research should be facilitated. The two primary purposes were these: (1) that the United States should not run the risks of the grave consequences from a Nazi success built upon a successful bomb or other type of military success built upon the experimental work on atomic fission by Hahn and Strassmann; and (2) that whereas from the mid-Thirties on the Nazis were in the forefront of the utilization of science as an instrument of military aggression and also psychological warfare, - from "softening-up" to terrorizing - the expulsion by Nazi Germany of scientists who had labored on atomic research and the arrival and acceptance into our society as well as in Great Britain of the atomic scientists constituted a sort of providential opportunity for the United States and the West to catch up and surpass the regime of oppression and terror and to become the world leader in a type of applied science that during the war was to be polarized on weapons, but thereafter for peaceful uses and human welfare. It is the second and longer-range constructive purpose that inspired President Roosevelt and led him to treat the fostering of the Atomic Project, despite the institutional resistances of and within Government, as a dominant theme of his wartime Presidency, surcharged with hope of transcending defense and becoming organic in the postwar science-infused economic and human progress.

Imbued by that great purpose, the President was very glad to accept and make his own proposals, evolved by me in early-March and formulated in letters of May 11th and May 15th, regarding "the establishment of a Scientific Council of National Defense, composed of executives, engineers, and economists, acting in behalf of the Government, who should be invested with administrative powers for the testing and execution of technical projects of utility for national defense." It is to this new body that the Atomic Project was to be transferred. And be it stressed that at that time the scientific research had not yet crystallized the affirmative answer as to the availability of a chain reaction. What the President did accept and agree to was that the promotion and the fostered pursuit of the research until there was a decisive answer was to become a major responsibility of the new organ. Moreover, the idea was suggested and the hope was held out that the assembled scientific workers would take up the tasks of solving the hitherto intractable problems in meeting the dangers from Nazi war technology and try to come up with new devices of their own. (Incidentally, the writer collaborated with a German refugee-technologist, Dr. Emil Mayer, in a project of a radiosonic buoy and a protective scheme against submarines.)

that the assembled scientific workers would take up the tasks of solving the hitherto intractable problems in meeting the dangers from Nazi war technology and try to come up with new devices of their own.*

* * *

As documentary support for the position taken by me at our conference, as re-summarized in the immediately preceding paragraphs, I am submitting a trio of letters that was written a little over a year ago in response to questions that had been put to me in mid-May 1958 by Mr. William L. Laurence, the Science Editor of the New York Times. With a view to time-economy at this stage and with a view to affording you the opportunity to go over my statements of a year ago, it seems best to delimit this communication to the transmittal of that recapture and to limit the supplement to providing sorts of italics for corresponding points raised by you yesterday, rather than attempting to extend that trio of letters. As indicated to you, there have been institutions of learning which have sought to arrange a series of lectures, that could afterwards be put into book form, on that very novel emergence, in both international affairs and international science, of a government's providing the aegis and the financial resources for the accelerative genesis of revolutionary science and revolutionary technological application of the atomic physics and the atomic bomb. I expect that at a long last I shall be enabled to take the time for adequate preparation of the material for such a lecture series and for rendering a definitive historical account by the one who was vouchsafed by history to have played the role of the originator of the Atomic Project through conversion of the President to it after all interested Government elements had rejected it.

With respect to the collect submitted herewith, there is added a photostatic reproduction in white of the contemporaneous notes that were made by me from the beginning of February 1939 through late-April 1939, or the period during which, by reason of my contacts with the Institute for Advanced Study at Princeton, under the then Director, Frank Aydelotte, I became cognizant of and concerned over the deemed far-reaching significance of the Hahn-Strassmann experiments and the echoed reactions to them by Niels Bohr, Frisch-Meitner, and Joliot-Curie. The reproduction of the original notes is preceded by a reproduction of the covering page made thereafter.

* Incidentally, the writer collaborated with a German refugee-technologist, Dr. Emil Mayer, in a project of a radiosonic buoy and a protective scheme against submarines.

That full description - "Origins of Concern With Significance of Atomic Research for U.S. Defense and for U.S. Role in the Overwhelming World Crisis from Beginning 1939" - is indicative of my orientation of the period and of my consequent dedication to the task. That self-set task was in the perspective of that whole half dozen years from 1933 on, during which Germany, under Nazi rule and the daemonic ambitions and labors of Hitler, succeeded not only in eliminating the disarmament controls but in effecting such rearmament as wrested the prior dominance of the Free World in technological warfare and, furthermore, continued to project to that world the encompassing dread of annihilation by air power, submarines and Panzer divisions, and also through the subversion and undermining of the morale of peoples.

It is advisable to pause over the nature and the import of the fact of concern over not only what was happening to Europe, but what did happen to the Continent and what it all portended for the Free World and for Western Civilization. What the people who have written on this subject have missed is that the original experimenters in the respective countries were pursuing the work with that concentration of attention upon what was regarded as pure science and, on the whole, without any perceptiveness of the emergent portent from Nazi Germany of the exploitation of science for what we now call "weaponry" and what the Germans then called "war science." Only the scientists who became refugees were induced in varying degrees to emerge from the quasi-monastic attitude towards science and to react against the perversion of science and systematic exploitation of science by the Nazis. This encompassed scientists who emigrated to Britain and to countries on the Continent, and also others who came to the United States after such stopover in England, France, or some of the neutral countries. In this connection it is worth referring to a personal historical fact that this writer came into contact with such scientists during the late-Thirties in England, as it had been his custom during the Thirties to spend his vacations there. Chief among scientists who were thus keenly and continually responsive to the larger and deeper political aspects I would place Niels Bohr and Albert Einstein. In that perspective, then, it is of a sort of providential historical sequence that the very first one to have articulated the larger meaning of the Hahn-Strassmann experiments was Niels Bohr; and that this writer, having read about those experiments in Nature, was impelled to confer about them with his friends at the Institute in Princeton at the very time of Bohr's visit, and in the wake of all that to have discussed that problem with three personalities, namely the then new head of the Institute for Advanced Study, Frank Aydelotte; a fellow-economist, Walter Stewart, who was also Chairman of the Rockefeller Foundation; and Albert Einstein.

In what I call "historifying sequence," - that is the way the historical eventuations proceeded in shaping the evolving history - my

February meetings at Princeton gave impetus to my effort to arrive at a conceptual preview of that evolving history. As is noted in my contemporaneous notes and as was referred to in the testimony of November 1945, there had been scheduled an address at St. John's College, Annapolis, for March 10, 1939. In anticipation of that, I prepared certain notes, copy of which went to the President. Moreover, prior to my speaking, I had a conference with the President in which I reviewed the world situation; and in the course of that review brought to his attention the article that had appeared in Nature on the Hahn-Strassmann experiments and also the interpretations by Niels Bohr and gave also a summary of my conversations at Princeton. In the course of that conference with him, I gave my forecast that the probabilities were overwhelming that Germany would convert the prior Munich settlement (concluded on the 29th of September 1938, by which the Sudetenland was given to Germany) into a domination of Czechoslovakia. This forecast was in keeping with a pattern of aggression in the spring and autumn, - which started with the re-militarization of the Rhineland in March 1936, followed by the embarking on the fostering of the Civil War in Spain from late-summer on that year, followed, in turn, by the annexation in March 1938 of Austria, and the Munich settlement in September. The virtual annexation of Czechoslovakia occurred within a week of the address, - that is occurred on March 15, 1939. Interestingly enough, the letter that was taken by Professor Fermi to Admiral Hooper, then Director of the Technical Division of Naval Operations, was dated March 16, 1939. Thus the advance briefing of President Roosevelt about not only the aggravating world situation but about the new "break-through," as we now call it, in science, antedated the attempt by Dean Pegram of Columbia to acquaint the Navy with experiments in atomic research going on at Columbia.

After having learned of the totally negative outcome of the Pegram-Fermi approach to the Navy, the problem confronting me, as one with that dedicated concern to secure Presidential approval and thereby American leadership, became terribly difficult. For it became known to me that the original interview of Professor Fermi and Admiral Hooper was in the context of the voiced skepticism. The quotation given in my letter of September 3, 1958 to Mr. Laurence is: "My own feeling is that the probabilities are against this," - the "this" being what the letter earlier referred to as "the possibility that uranium might be used as an explosive." That letter of mine also quotes from Professor Compton that, "Many felt such hope futile." Moreover, Mrs. Fermi's book contains, on page 164, Enrico Fermi's own recaptured explanation for the failure of his interview with the Admiral. In Fermi's own words: "You forget that in March 1939 there was little likelihood of an atomic bomb, little proof that we were not pursuing a chimera." (italics added)

The next development was the definitive turn-down by the Naval Research Laboratory, in a letter dated July 10, 1939, of a proposal

that had been broached to the Technical Advisor, Ross Gunn, for some kind of a contribution by the Navy so that the experiments at Columbia could go forward. The device is one that has become so pervasive and conventional that an ordinary historian could be excused for thinking that it had been in operation even prior to our embarking on the Atomic Project. But, as I reported to you yesterday, one of the recurrent remarks of President Roosevelt to me was that, 'Neither the Departments nor the President has any money for any such purpose.' Mr. Gunn's letter, of which a reproduced copy inclusive of signature is attached hereto, stated, "... it seems almost impossible, in light of the restrictions which are imposed on Government contracts for services, to carry through any sort of an agreement that would be really helpful to you. I regret this situation but see no escape."

It was after that that Dr. Szilard came to see me as a result of what he had heard about my combination of interests in international affairs and science and my long sustained role as a confidential advisor to President Roosevelt on both economic and international problems. While I was informed by him of the complete turn-down, an exploratory inquiry that I had made of Dr. Szilard led me to be very doubtful as to whether he had full information regarding the Pegrum - Fermi attempt and failure to enlist Admiral Hooper's interest. Therefore, I did not think it appropriate to advise him that such an attempt had been made and that Mr. Gunn's refusal was in line with and, in a sense, was pre-determined by Admiral Hooper's declination of any support or active interest.

Between visits by Dr. Szilard at my office, I evolved the idea that, instead of trying to secure re-consideration by the Services, I would pitch the Project into two keys, like foci of an ellipse. The first key was that of the potential civilian applications of atomic power for power and healing; and the second was the value for defense in order that, independently of the 'whether' and the 'timing' of our entry into war, we should be on top of this problem, - that we could not be bullied or, what would be worse, terrorized by Nazi reports of their progress in atomic research. Hence came the shift of venue from the military to the Bureau of Standards. Specifically, not only was it imperative for me at the time to be mindful of the President's preoccupations with the prelude diplomacy to the inevitable war outbreak for the autumn of 1939, but it was indispensable that the Navy and the Army be, in the preliminary phases, excluded from the question of reconsidering their prior positions, - though it was only one Service which was involved, namely the Navy, which alone had a Research Division and Laboratory. In order to prepare the ground for a submission to the Bureau of Standards, it seemed essential that a dossier be made up. The primary material for that dossier was, naturally, the publications in scientific journals, which encompassed what had been published in England and Germany as

well as on this side. For the material on this side Dr. Szilard could readily provide the reprints of the articles in the Physical Review by Dr. Zinn and himself. The articles in Nature were accessible to me as a subscriber. Then came the question of securing an opinion from a scientist to whose position and name the President would respond with a chord of more than ordinary recognition. Dr. Fermi, as an atomic physicist and Nobel Laureate, would normally be regarded as the person to use. But the President had known of his interview with Admiral Hooper and had known that Fermi thought that an atomic bomb was a chimera. Difficult as it is for us to believe it, that is the recorded fact, and that was known; and Fermi's negative attitude was also known to me. Besides, in view of that negative attitude, any kind of a testimonial from the Pegrarn-Fermi group would have brought the whole subject within the purview of that Navy Department, and that would have been a sure way of giving the Project a total and definitive blow. Accordingly, it was decided by me that the President should be reoriented to the problem in terms of his own conception of the probable leadership of the United States at some date in the progress of World War II, assuming its inevitability and accepting the consequential near-inevitability that America would more than parallel the decisive role in World War I.

This, therefore, made Dr. Albert Einstein the most logical figure, by reason not only of his anti-Nazism but of attunement to what the President and I had been in the habit of referring to as 'the social significance of science.' Thus the dossier proposal of mine required a letter to be signed by Dr. Einstein. Such a letter was drafted by me at my office at One South William Street, with assistance by Dr. Szilard. The matter of securing the signature was a routine one. As I then occupied the position of Vice President in Charge of Economic and Investment Research for the Lehman Corporation, the mounting threats to peace and the imminence of war not later than September - the timing dictated by the climactic geopolitics - I found it inadvisable to absent myself from a scheduled portfolio meeting. The crucial character of August was against the background of the sequel of aggressions by the Nazi-Axis. Following the Ides of March, or the annexation of Czechoslovakia on the 15th, and the annexation on the 20th of Memel by Germany for the control of the Baltic, there came the bilateral agreements between Great Britain and Poland in early-April. There also took place in that month the annexation of Albania by Italy for prospective domination of the Black Sea by the Axis. At the very end of April came the annulment by Nazi Germany of the German-Polish Non-Aggression Pact. And from late-July on there were the preparations for the British Mission to Soviet Russia in what turned out to be a futile effort for collaboration by Russia in resistance to German aggression. It is worth recalling and featuring that the closing week of August witnessed the announcement of the Russo-German Pact, while the British Mission was still in Moscow.

Thus the preparation for the documentary side of the new approach to President Roosevelt with regard to the Atomic Project was completed at an already advanced stage of the mounting accentuation of events that culminated with the mobilization of all Germany on August 25, 1939. While the White House and the State Department went through the motions of trying to halt the onrush of the war catastrophe, this writer, as one in the circle of confidential advice, submitted the reasons for his strongly negative attitude towards the possibility of any success for the British Mission in Moscow and correlatively for the conviction at the time that the Soviets would not only stay away from help to Poland but would, through some diplomatic device, such as a non-aggression pact, be an aid to the Nazi aggression plans.

This entailed re-discussion with the President of a theory propounded in late-1937 as to the significance of the Soviet purges as a preparation for a neutral position between Hitler and the West. The continuing importance of that orientation reemerged later in the war when, in April 1943, the writer prepared and submitted an analysis of "Sovist Foreign Policy and Inevitable Russo-Allied Rifts," which was utilized, in turn, in the memorandum submitted after the President's return from Yalta in late-February 1945. The inclusion at this stage of these forward references is for the sake of exhibiting the type of interchange and advice submitted to the President, since the working hypotheses were thereby accorded continuing test as to their validity and utility for the ensuing stage of the developing crisis. Thus involved in thinking about the issues that came to a head in the climactic month of August 1939, this writer was poignantly aware that it was out of the question to be submitting to the President in August that dossier, with the letter that bore Dr. Einstein's signature. And the month of September opened with the German invasion of Poland on the 1st, the war declaration by Britain and France on the 3rd, and, notably enough, as a confirmation of the views that had been submitted prior to the Russo-German Pact of the 23rd of August, Soviet Russia itself invaded Poland on the 17th of September. From late-September to mid-October, the President's preoccupation was with the securing of Congressional approval for radical revision of the Neutrality Act. Only when it appeared that this aim was being fulfilled did it seem right and timely to reopen, in terms of my new framework, the whole question of interest by the White House in furthering the atomic research. Accordingly, the letter which was brought to the White House on October 11, 1939 opened with the words, "With approaching fulfillment of your plans in connection with revision of the Neutrality Act, I trust that you may now be able to accord me the opportunity to present a communication from Dr. Albert Einstein to you, and other relevant material bearing on experimental work by physicists with far-reaching significance for national defense."

From here on the testimony, as given by me before the Senate Committee, provides considerable but not comprehensive detail. There is thus inadequate treatment of the correspondingly adverse attitude on the part of a physicist with the Bureau of Standards. While there was never any question about Dr. Szilard's zeal in entertaining the hypothesis about the chain reaction, the Bureau of Standard's scientist insisted on and secured Dr. Briggs' concurrence in a demand for much more positive results, to which Dean Pegrum of Columbia would lend his signature. But while Dr. Szilard had laboratory space, he was not an official member of the Physics Faculty and clearly was in no comparable position in the scientific world to that of Dr. Fermi. In the conferences that were arranged, there was a greater readiness on the part of Dr. Wigner than others to entertain the hypothesis of a successful outcome in respect to a chain reaction.

Dr. Wigner, who was then a Professor of Theoretical Physics at Princeton University, had not yet embarked upon and become known as also an eminent experimenter in atomic research. It is worth noting that a then young participant in the conferences at the Bureau, Dr. Teller, - another of the refugee physicists working in Washington - voiced a willingness to suspend disbelief and skepticism. However, from the point of view of the physicists at the Bureau of Standards, Fermi was the leading experimenter and so his attitude of skepticism and awaiting developments influenced the Bureau far more than the hospitable attitude of other physicists. Dr. Fermi's skepticism is dealt with at considerable length in my letter of September 3, 1958. At the time of composing that letter I was also aware that he had retained his skeptical attitude in 1940.*

The just alluded-to difficulty was covered by me a little more in my talk with you yesterday. The plain fact is that the first half of 1940 was occupied with efforts on my part to overcome a very substantial obstacle. Though pressure that was exerted by me via General Watson, the representatives of the Army and the Navy did make a report very early that was, to put it with some precision, not opposed to going ahead with exploring the project of the utility of atomic research for defense. But subsequent to the report of Colonel Adamson and Captain Hoover, - as I recall the names - the Bureau of Standards itself sought assurances for results which the course

* It is my impression, though I have not re-checked the source, that there are echoes of that, though not as strong, in an address that he delivered at the University of Pennsylvania on the occasion of a bicentennial celebration.

of the experiments did not yet provide. As I recall, from the reports that had been published in Nature in the course of the summer of 1939, the Joliot-Curie group was almost positive that a chain reaction of significance would obtain. In addition to Joliot-Curie there were also Dr. von Halban and Professor Leo Kowarski, - whom I got to know later and who thought there was a family relationship between us. I did show the President those articles from Nature, and since he shared my orientation as to the combined national and international importance of atomic research for the West, he accepted the positive approach. But it was essential that the Bureau of Standards gain sufficient conviction as to communicate its acceptance of that attitude over what was the continued opposition of its leading physicist. So far as Dr. Briggs was concerned, he wanted to have Dr. Einstein attend the meetings and be able to discuss this in person with staff members. But on the evidence submitted by me, Dr. Einstein's signature was related primarily to his anti-Nazism, and throughout that period the letters that were drafted by me for his signature were keyed into the encroaching threats from Nazi progress. Hence the featuring in those letters of that Nazi progress in research.

In that perspective there was a correlative interest on my part, featured in my very first letter to President Roosevelt, namely the deflection of the Belgian inventory of uranium from the vulnerable storage depots in Belgium and France to the United States. This is a chapter by itself, of which a full account cannot be given in this setting. Suffice it to mention that the representatives of Union Miniere in this country did not want to carry out that proposal unless it was treated as a purchase or unless they were compensated for the costs of transfer and also for the call on the supply, with additional insurance charges in the event of a quick end to the war, - to which the politically naive manager on this side looked forward.

In the setting of continuing yet varying problems, it was decided by me to propose to the President the establishment of an Office for Research and Development for all defense activities, and to place within that Office the supervision of atomic research. By that time the accruing expositions on my part of the role of science in the war and for victory had come to be accepted by the President, and thus the residual objections and hesitations of Dr. Briggs were not so much answered as set aside.

It remains to refer here to a line of argument mentioned in our conversation yesterday, which exercised rather decisive persuasion on the President. This is what I called then and re-called to you "Bishop Butler's Theory of Probability." The opening of the introduction to Bishop Butler's "Analogy of Religion, Natural and Revealed," is an introduction on the nature of probability. The very first sentence of the introduction is this: "Probable evidence is essentially distinguished from demonstrative by this, that it admits of degrees;

and of all variety of them, from the highest moral certainty, to the very lowest presumption." The most distinctive part of his theory is the later defined assertion or - as I have been accustomed to call it - the recognition of the validity in life of weighing probabilities not merely by their internal degrees but by the estimated sense of importance and concern that the man making a judgment attaches to what is at stake in the probability choice. This goes back to Blaise Pascal's wager, promulgated a century before. But Bishop Butler's formulation is fuller and clearer, and it represents the affirmations that man, as a moral and spiritual being, makes for himself and as a trustee of interests under his care as to the objects of his great solicitude. Bishop Butler's formulation is as follows:

"For surely a man is as really bound in prudence to do what upon the whole appears, according to the best of his judgment, to be for his happiness, as what he certainly knows to be so. Nay further, in questions of great consequence, a reasonable man will think it concerns him to remark lower probabilities and presumptions than these; such as amount to no more than showing one side of a question to be as supposable and credible as the other: nay, such as but amount to much less even than this. For numberless instances might be mentioned respecting the common pursuits of life, where a man would be thought, in a literal sense, distracted, who would not act, and with great application too, not only upon an even chance, but upon much less, and where the probability or chance was greatly against his succeeding." (italics added)

The net import of the foregoing considerations on probability as a guide to vital choices was that it is the office and the duty of the statesman in relation to this kind of research project to make a greater venture of faith than was permissible on a calculus of business probabilities. This attitude was carried further into the thesis that our utilization of this unique assemblage of creative scientists constituted a unique dual opportunity for enabling us to get ahead of Nazi research in this potential weaponry and for influencing the whole tone and sweep of work in the frontiers of science for overcoming the Nazi advantages in technological warfare and thereafter for maintaining leadership in applied science. The political-social setting of the accrual to the United States and Britain of this creative science entailed what I called a mutation in the history of the relationship between the pursuits of pure and applied science and the bodies politic and economic, namely whereas previously the prosecution of pure and applied science was delimited in scope and piecemeal in process, henceforth they would have these new dimensions of national and international welfare.

Now, during that critical spring and summer of 1940, the control of Britain passed from Chamberlin to Churchill, and when Churchill on May 10th assumed control, he delivered himself of his now famous utterances regarding the ultimacy of the struggle at all costs. In effect, the President and I agreed that Churchill had affirmed and proceeded to enact the Butlerian Theory of Probability that, however low the degree of probability of victory against the Nazis seemed then, the supreme importance of victory against the Nazis rendered it necessary to conduct the struggle in the conviction that the degrees of the probability of victory would accrue with the progress of the struggle. -- It is necessary to recapture still another support for the Butler Theory of Probability that was evoked by me in the conferences, namely William James' analogous formulation in his Psychology:

"The world puts all sorts of questions to us, and tests us in all sorts of ways ... But the deepest question that is ever asked admits of no reply but the ... turning of the will and tightening of our heartstrings as we say, 'Yes, I will even have it so.'"

In sum, in marked divergence from the simplistic notions, which have become a kind of popular myth, that the President embarked on the great gamble of atomic research under the impetus of a highly esteemed and admired world figure in science, as later supported by a recommendation from the Bureau of Standards, the true history of the origins of the Project involves an accumulation of profound considerations and decisions that, for all the contributiveness of the originator of the idea, in the final analysis were made on prayerful reflection by the President himself, - made as part of the venture of faith and struggle for victory in World War II.

Yours sincerely,

Drs. Richard G. Hewlett
and
O. E. Anderson
U. S. Atomic Energy Commission
Washington 25, D. C.

P. S. As a final element in amplification of our conference, I am enclosing copies of other documents to which I referred. These are evidentiary of my recurrent pursuit of projecting the course of events, with a view to utilizing such advance history or "pre-history," in my technical sense of the word, for guiding historical agents. The first two are a memorandum to President Roosevelt in late-October 1944 and a preserved extract of submissions to him upon his return from Yalta. These documents were seen by the late John Foster Dulles, and his reaction to them is conveyed in a reproduced letter to President Van Dusen of the Union Theological Seminary. Finally, I am enclosing copy of a memorandum with a hypothetical graph, prepared in January 1950 and revised in April 1950, forecasting the high probability of Soviet indirect aggression operations against Korea. This document is entitled "Approach to and Methods for Projecting Comparative Atomic Weapons Accumulations."

Enclosures

1. Letter from Julian B. Baird, Under Secretary U. S. Treasury, July 20, 1959.
2. Photostat title page August 8-9, 1945, "Early History Atomic Project in Relation to President Roosevelt, 1939-40."
3. Letter to William L. Laurence, Science Editor, N.Y. Times, August 12, 1958.
4. Letter from William L. Laurence, August 26, 1958.
5. Letter to William L. Laurence, August 28, 1958.
6. Letter to William L. Laurence, September 3, 1958.
7. Photostat title page notes A.S., "Origins of Concern With Significance of Atomic Research for U.S. Defense and for U.S. Role the Overwhelming World Crisis from Beginning 1939."
7. Phostats A.S. notes 1939, together with typewritten transcript.
8. Copy letter to Dr. Leo Szilard from Ross Gunn, Technical Advisor, Naval Research Laboratory, Anacostia Station, Washington, D. C.
9. Photostat memorandum by A.S., end-October-early-November 1944, "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War."
10. Extract from memorandum by A.S. to President Roosevelt upon return from Yalta, end-February 1945.
11. Photostat letter from John Foster Dulles, June 10, 1946.
12. Memorandum April 1950 (revised from January 1950) by A.S. on "Approach to and Methods for Projecting Comparative Atomic Weapons Accumulations."



UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON 25, D. C.

[Corresp.: Atomic Energy Commission
[Reports, Studies, Memos: AEC]
[Subject File: AEC]

October 5, 1959

Mr. Alexander Sachs
25 Wall Street
New York, New York

Dear Mr. Sachs:

I want to thank you on behalf of Dr. Hewlett as well as myself for the time you set aside for us last Monday afternoon. It was a pleasure to meet you and to gain a clearer picture of the part you played in alerting the federal government to the uranium problem.

Your letter of September 29 with its attachments came in the morning mail. It puts us further in your debt. I have already read it through, as will Dr. Hewlett when he returns to Washington. I realize how much time and effort such a communication requires. You may be sure that it will have the closest study as we prepare the chapters that deal with those significant years when American science and government were groping toward cooperation.

Sincerely yours,

Oscar E. Anderson
Oscar E. Anderson
Acting AEC Historian

[Copy. 4]



HEADQUARTERS
DEPARTMENT OF THE ARMY
OFFICE OF THE CHIEF OF MILITARY HISTORY
WASHINGTON 25, D.C.

IN REPLY REFER TO
OCMH

MAY 20 1960

Mr. Alexander Sachs
25 Broadway
New York, New York

Dear Mr. Sachs:

As part of the official series THE U.S. ARMY IN WORLD WAR II, the Office of the Chief of Military History is preparing a volume on the role of the Army in the development of the atomic bomb. Dr. Stanley L. Falk, one of the historians assigned to this volume, would like very much to discuss with you your part in initiating work on the bomb.

Dr. Falk is a careful and trustworthy scholar. He would like to visit you at your convenience during the week of June 6th, or, if you prefer, will be glad to come to New York at any other time you specify. I will appreciate any assistance that you can provide him.

Sincerely yours,

James A. Norell
JAMES A. NORELL
Brigadier General, USA
Chief of Military History

[Convoy. U.]

File
OS [unclear]

June 8, 1960

Re: OCMH

Dear General Norell:

In acknowledging after this time interval your kind letter of May 20th, allow me to note first that I have been under pressure on problems of public policy for the Treasury and the Federal Reserve, as you will note from the enclosed copy of a recent acknowledgement from the Under Secretary. The culminative actions of the authorities on June 2nd and June 6th entail further implicit tasks for them, and thus this current week would be impracticable for a conference with Dr. Stanley L. Falk, as suggested by you. Accordingly, I would suggest that I be contacted in the closing week of the month to effect a mutually convenient appointment.

It remains to add that about a year ago, or precisely July 24, 1959, Mr. Richard Hewlett, the Historian for the Atomic Energy Commission, inaugurated a similar inquiry of me. This led to conferences at my office with him and Dr. O. E. Anderson and to a supplementary historical conspectus of mine on September 29, 1959. What was remarked by me at near the opening of that communication remains applicable to even the since accrued publications, to wit, "With respect to the origination of the Atomic Project, there has evolved and become crystallized, at first at the hands of journalists and later at the hands of scientists who were connected with the Project, an exceptionally oversimplified, inadequate, and even fictional version." From the accompanying transcript of my testimony at the end of 1945 and from the article by Mr. N. S. Finney in Look for March 14, 1950 you will realize the special status towards President Roosevelt occupied by me prior to and during the effectuation of the Atomic Project, in contrast to the later participants in the Project, let alone the subsequent writings by those who had not even tangential relationship to the policy-making. Accordingly, the tendency in recent years for mere compilatory assemblage of views has led to the encrustation of journalistic and related versions. Because the Army played the decisive role in the effectuation of the Project, it is all the more important that a re-thinking be undertaken for a fresh recapture of the historical truth. As reinforcement of this defined high advisability, let me direct your attention to the following continuance of the quoted sentence from the pertinent opening part of that letter of September 29th:

"Since there was no historian attached to the White House and since not only President Roosevelt who had made the great decisions had passed away but also his assistant and official confidant in connection with the Project, General Watson, had also died without having left a record of his own, Dr. Smyth, when

called upon in the finale phase of the war to write primarily and preponderantly on the scientific end of the Project, had to rely on such pieces of correspondence as could readily be assembled from the White House files. The conditions of secrecy were especially decisive for me because of an oath I had given to the President that no intimation whatsoever was to be given regarding the Project until the release by him and the submission to him, after the use of the bomb, of a report, which would be followed by a statement from him regarding my role as the originator of the Project and as his constant adviser on it."

Sincerely yours,

General James A. Norell
Chief of Military History
Headquarters Department of the Army
Office of the Chief of Military History
Washington 25, D. C.

[Comp. 4]



HEADQUARTERS
DEPARTMENT OF THE ARMY
OFFICE OF THE CHIEF OF MILITARY HISTORY
WASHINGTON 25, D. C.

IN REPLY REFER TO
OCMH

Mr. Alexander Sachs
25 Broadway
New York 4, New York

Norell - Oxford 5-2064

Dear Mr. Sachs:

Thank you for your letter of 8 June 1960 and for the materials that you were kind enough to send with it. These materials, together with other information in the official records, provide the answer to most of the questions that Dr. Falk desired to ask you.

There are still, however, one or two points about your initial meeting with the President and the establishment of the Uranium Committee on which the record is not clear. Dr. Falk would like to discuss these with you briefly and at your convenience. It would be helpful to him if he could visit you during the month of July, but he will be glad to come up at any other time that you may desire.

Thank you again for your aid and interest in our project.

Sincerely yours,

James A. Norell
JAMES A. NORELL
Brigadier General, USA
Chief of Military History

L. Conroy. U]

June 29, 1960

Dear General Norell:

Thank you for your acknowledgement, dated June 28th, of my letter of the 8th, along with enclosures.

I should be glad to see Dr. Falk in the course of the coming month if he will call me at HA 2-5390 to ascertain and arrange a mutually convenient date.

Sincerely yours,

General James A. Norell
Department of the Army
Office of the Chief of
Military History
Washington 25, D. C.

Re: OCMH

2 Copying U.S. File

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NO. WDS.-CL. OF SVC.	PD. OR COLL.	CASH NO.	CHARGE TO THE ACCOUNT OF	TIME FILED
			Alexander Sachs, 25 Broadway, NYC 4 HA 2-5390	

Send the following message, subject to the terms on back hereof, which are hereby agreed to

July 14, 1960

Dr. Falk
 c/o Office of Chief of Military History - Gen. James A. Norell
 Department of the Army
 Washington 25, D. C.

In view interposed necessity luncheon conference client from ~~and~~ abroad wonder whether feasible postpone to Tuesday our meeting at 11 with luncheon extension STOP Alternatively could retain our meeting 11 with view to finishing by 12:30 STOP Please telephone me tomorrow at HAnover 2-5390.

Alexander Sachs
 25 Broadway
 New York City 4

[Falk, Stanley
Convers. File]



HEADQUARTERS
DEPARTMENT OF THE ARMY
OFFICE OF THE CHIEF OF MILITARY HISTORY
WASHINGTON 25, D. C.

IN REPLY REFER TO

22 July 1960

Mr. Alexander Sachs
25 Broadway
New York 25, New York

Dear Mr. Sachs:

Thank you very much for taking time out to speak with me last Monday. I am enclosing a copy of my notes on our conversation. As you can see, these are fairly brief and sketchy and are useful to me mainly as a "jog" to my memory. Used in conjunction with the other material you have so kindly made available to us, as well as your "Early History of the Atomic Project," available in the official records, they will enable me to present what I trust will be an accurate picture of these early events. Any comments you may wish to make on these notes will be welcome.

Incidentally, would it be possible for you to let me have copies of your correspondence with William L. Laurence in August and September, 1958, which you list as enclosures 3 through 6 to your letter to the AEC historians? The extract from your memorandum to the President upon his return from Yalta (enclosure 10 to your letter to the AEC) would also be helpful.

Thank you again for your time and interest.

Yours sincerely,

Stanley L. Falk
Stanley L. Falk



HEADQUARTERS
DEPARTMENT OF THE ARMY
OFFICE OF THE CHIEF OF MILITARY HISTORY
WASHINGTON 25, D.C.

IN REPLY REFER TO

Notes on a statement by Alexander Sachs to Stanley L. Falk, 18 July 1960.

As early as February 1939, Mr. Sachs became aware of developments in the field of nuclear science through the article in Nature reporting the Hahn-Strassman experiments. He had, even earlier, been familiar with the general concepts involved, but the Nature article led him to discuss the work and its implications in detail with Frank Aydelotte, Director of the Institute for Advanced Study at Princeton, Walter Stewart, an economist, and Albert Einstein. Sachs was a longtime confidant and adviser of President Roosevelt and, in early March 1939, while discussing with the President the increasingly dangerous international situation, brought the Nature article to FDR's attention. He explained the importance of the scientific advances in the context of the world situation.

In March 1939, meanwhile, the scientists approached the Navy for support in their work and were turned down. A subsequent approach in July by the scientists to the Naval Research Laboratory was turned down by Mr. Ross Gunn, Technical Adviser at the Laboratory. (A copy of Mr. Gunn's letter to Szilard, dated 10 July 1939, was given to me by Mr. Sachs and is attached herewith.)

Having been turned down by the Navy, which had the only research organization in the military services, the scientists turned to Sachs. They were aware, because of his visits to Princeton and his general reputation, of his interest in science and international affairs and of

his relationship with President Roosevelt. They hoped that Sachs could get them presidential support for their continued research. They did not, as Sachs did, see their work in all its international implications. Sachs concluded that, given the turn-down by the Navy, there was not much point in trying to get support from the military services. This, combined with the general scarcity of governmental funds for research, led him to think that he should ask the President to turn the matter over to the Bureau of Standards, which had the necessary funds and organization to promote continued atomic research.

To this end, it was necessary to put together a "dossier" on the subject, including reprints of the articles in Nature and the Physical Review and other related scientific materials. Sachs also felt the necessity of presenting this with a covering letter from a well-known scientist. Fermi and Pegram would not do, because they had been associated with the ill-fated attempt to get support from the Navy. Szilard's position was one of lesser prestige at this time, and this ruled him out. Accordingly, Sachs concluded that Einstein was the one whose name should be signed to the letter to the President.

On August 2, 1939, in his office at One South William Street, New York, where he was a director of the Lehman Corporation, Sachs dictated the famous letter to the President. (Sachs stated that the version of this in Jung, Brighter Than A Thousand Suns, is, for the most part, correct.) This letter as written by Sachs was "identical" with the one delivered to FDR. The scientists then took the letter to Einstein, got his signature, and returned it to Sachs.

Sachs then waited for just the right opportunity to present this to the President. Because of his special relationship with FDR, Sachs was

aware of the President's preoccupation with the oncoming war in Europe and with the problem of revising the Neutrality Act. Accordingly, Sachs bided his time through the fast-moving events of August and the early weeks of the war. Thus, although he spoke with the President about other matters during this period, it was not until early October that he deemed the time ripe to bring up the matter of atomic energy. By this time Sachs knew of the "approaching fulfillment" of FDR's plans for revising the Neutrality Act and felt that he now could get the President to devote his "full attention" to nuclear prospects.

Although he had intimated earlier that he wished to speak to FDR on an important matter, it was not until 10 October 1939 that Sachs telephoned the White House from New York to make an appointment for the following day. In answer to my question, Sachs stated that the absence of his name from the copies of the White House appointment calendar that I had seen was due to his special relationship with the President and the fact that his visits would not normally be listed on such a record.

The initial reaction of the President on the 11th, as indicated in the published accounts by Jung and Finney, was skeptical. He felt that there were other matters which deserved a higher priority of attention, such as the revision of the Neutrality Act. FDR was also skeptical about the availability of funds for atomic research -- which is why Sachs hoped to bring in the Bureau of Standards, the only organization with money for research. On the morning of the 12th, however, the President made his favorable decision.

It was on the afternoon of 12 October that Sachs met with Colonel Adamson and Commander Hoover at the White House. He is positive that he

saw no service personnel, other than General Watson, on the 11th. Sachs does not recall the presence of Mr. Adelman, although he confirms the description of the meeting as written by Adelman. Nor does he recall anything about Colonel Shekerjian or any other officer, although he feels it is possible that they were there.

Sachs describes the attitude of the Army officers both at this and subsequent meetings as "skeptical." He says that this attitude was evident at the 21 October 1939 meeting (reference to p. 7 of his history), that it continued, and that service "hostility" was one factor behind his recommendation for the organization of OSRD.

Sachs claims credit as the "originator" of OSRD, pointing out that the terms of the announcement of its organization "overlapped language of my own" in his letters of 11 and 15 May 1940.

During this period Sachs talked with the President about future Army participation "the moment" the U.S. entered the war. He felt this would be necessary for three reasons: 1) the great costs; 2) the fact that the Army would probably be the service with the greatest initial participation in the war -- he foresaw the need for an African invasion -- and therefore would have priorities, etc.; and 3) the need for the Army Engineers to undertake the necessarily fast construction connected with the project.

Sachs discussed these matters only with FDR and General Watson during the war, and the President kept Sachs' connection with the project secret.

The following documents were given to me by Sachs and are attached herewith:

- 1) A photostat of portions of Sachs' diary for February and April 1939. (Unfortunately he no longer has those pages of the diary for October.)
- 2) A thermofax copy of Ltr, Ross Gunn to Leo Szilard, 10 July 1939.
- 3) A thermofax copy of Ltr, Sachs to Drs. Hewlett and Anderson, the AEC historians, 29 September 1959, which contains a detailed version of much of the above.
- 4) A carbon copy of an outline of a book Sachs proposed to write.

July 25, 1960

Dear Mr. Falk:

In reply to your letter of the 22nd, this is to let you know that Mr. Sachs is in England this week.

He should be returning some time next week, when he will be writing to you.

Sincerely yours,

Secretary to Mr. Sachs

Mr. Stanley L. Falk
Headquarters Department of the Army
Office of the Chief of Military History
Washington 25, D. C.

[Convosp. - Strauss, L.]

[Convosp - 12/60]

File

C. Strauss

December 27, 1960

Dear Lewis:

In furtherance of our telephonic conversation on Friday morning, just before my departure for the holiday, I am transmitting herewith reproduction just made from my only available file copy of my extensive submission to the Historians of the Atomic Energy Commission in late-September 1959. This report, as you will note from the closing page, had extensive and varied documentation. Considering the extent of this collect of enclosures, it seems best to let you judge for yourself which would be useful to you for your own purposes in connection with the book that, as you indicated, you are preparing for Doubleday. The enclosures could be secured by you readily from the Chairman of the Commission. Or in the event that that might involve difficulty, a note from your secretary, indicating the items desired, would be attended to by my office. There is also transmitted herewith reproduction of the letter written by me, as described in the text of that enclosure, for signature by that great genius and worthy as a cooperator in worthy projects, the late Professor Albert Einstein.

In view of the journalistic embroideries and the web of myths that has been woven by scientific and other writers without original contact with the governmental sources of decision, it is advisable to draw your attention to the earlier part of my opening testimony before the Senate Committee on Atomic Energy on November 27, 1945. That testimony, before the Committee that was presided over by Senator McMahon, had been reviewed by me in its main elements in conferences in Senator McMahon's office and also with the then Secretary of Commerce, Henry Wallace, who as Vice President in 1939 and still confidant of Roosevelt, did know a considerable amount of the confidential parts, though not as much as General Watson. In that outline given by me at those offices there were present Szilard and others of the scientists who began their crusade against General Groves, which I opposed most emphatically. Thus they had advance notice of my disclosure in my testimony regarding my original concern with the potential import of atomic energy as far back as the Cambridge University Lectures in 1936 and 1937. Thus, too, they had learned that I had brought to the attention of President Roosevelt the import of the articles in Nature regarding atomic fission that were published by the spring of 1939. The connection effected by me was in the course of the sustained advice to President Roosevelt and proffered forecasts on the inevitability of a world war from the mid-Thirties on and the urgings of entry on defense. This is further developed in the utilization by Nat Finney in the Look article of March 14, 1950, - parts of which, and particularly page 26 of the enclosed reproduction, are pertinent for your historic reconstruction.

Early in 1946, an important publisher had discussed with me the preparation of a book. But the position that I took against transmittal of the secrets to Soviet Russia were, in that heyday of collaborationism, quite unpopular, and that business publisher said quite openly to me that he was afraid of what fellow-traveler book reviewers would do to such a book. What I have preserved from the material submitted to him is an outline of April 1946, also enclosed.

Finally, it is advisable to record here that Brookings has prevailed upon me to deliver a series of talks in the framework of a seminar for Government officials and specially invited guests, as you will note from the enclosed copy of a letter of mine acknowledging their acceptance of the setting and procedure as recommended by me. The subject matter of the eventual book, though not the necessarily more abbreviated title is as follows: "Nuclear Age's Emergence and Development - New Modes and Tempa in Inaugurated and Progressing Interactions Between Science and Society."

* * *

Looking back on the accrued myths and the subsequent injudicious policies recommended by eminent scientists who underwent psychic upsets from the experience of association with this "exponential weapon," as I called it in my memorandum to President Roosevelt of October-November 1944, also enclosed - the most challenging question, provocative of humility all around, is the bewilderment of the scientists and the all-around balance of the policy-makers utilizing the gifts of the scientists. As an epigraph to an article for Fortune at the end of 1945, - which also came up against the then philo-Russianism - I used the Sixtieth Psalm, Verses 2 and 3, in the Authorized Version:

"Thou hast made the earth to tremble; thou hast broken it: heal the breaches thereof; for it shaketh.

"Thou hast showed they people hard things: thou hast made us to drink the wine of astonishment."

With best wishes for the New Year,

Sincerely yours,

Hon. Lewis Strauss
Mercury Building
1925 K Street, N. W.
Washington 6, D. C.

[Encls. - Strauss, L.]
[Chrm 1/61]

LEWIS L. STRAUSS

Mercury Building
1925 K Street, N.W.
Washington 6, D.C.

6 January 1961

Dear Alexander:

I am most grateful to you for the letter which you sent to me and its informative and inspiring enclosures. Just one more question about the Einstein letter of August 2nd as I am anxious to help clear up some of the myth which has gathered around this historic communication.

It is my belief that the letter was not written by Einstein, nor by Szilard, but by you and that it was taken to Peconic, Long Island, where Einstein signed it on August 2nd and that it remained in your keeping until your famous interview with the President in October. There are one or two peculiarities about the letter, however, which I think would not have been your language. For instance, on the 2nd page, the 8th line, there is the word "inofficial" where I think you would have used "unofficial". It is certainly not material. Furthermore, it seemed to me that if anyone other than yourself had composed the letter, you would have been specifically suggested as the person referred to in that paragraph.

I assume that you have the original letter since you were so kind as to send me a photostat, but I have been told that the original is in the Roosevelt Library at Hyde Park. Is this correct?

With warm regards, I am

Faithfully yours,

Lewis

Mr. Alexander Sachs
25 Broadway
New York 4, New York

[Corresp. Strauss, L.]
[Chrm 1/61]
} 12
S
Strauss /

January 11, 1961

Dear Lewis:

On return from delivering an address on the New Order of Science and Technology in Relation to Dynamic Defense and Growth Economics before a special conference of high officials arranged by Brookings, I find your characteristically gracious and thoughtful note of the sixth of January, and hasten, in view of your manuscript urgencies, to provide the further light desired.

The letter as written by me at the end of July, in the setting of the prior exchanges with the scientists involved, was to have been taken by me for re-transcription and signature by Dr. Einstein. As stated in my account for the Historians of the Atomic Energy Commission, the requirement for my attendance at a scheduled Portfolio Meeting after having applied so much labor to public tasks rendered it necessary to assign the task to Dr. Szilard and a colleague. My instructions for that purpose were in accordance with my original plan, namely that my letter was to be re-transcribed to bear the locus and the date of the signature. Thus it came about that on blank sheets of paper, rather than personal stationery, my letter was thus re-transcribed. A typographical error, such as noted by you, did occur in the re-transcription. The re-transcription at the place where Dr. Einstein stayed had, as you will note from my reproduction, another defect, namely that the margins of the first page and the second page diverged.

Upon return of the original letter to me, I treated it as one of the elements in the dossier, with the scientific part naturally deemed of primary importance. The comments on that dossier are, as noted in my previous letter to you, found in the closing column of page 26 of the Look article of March 14, 1950. From a re-reading of that account it will also emerge that the emphasis in my conference with the President was on the combined scientific and defense situations.

What has been outside of the ken of and therefore completely glossed over by the writers on this subject, inclusive of the late Dr. Compton, is that in my role as adviser who had the President's confidence, he and I knew that the Services had turned down the pleas for aid, and that the only branch of the Services which had research money, the Navy, had turned it down twice, as set forth in my special communication to the Historians of the Atomic Energy Commission. It therefore initially appeared hopeless to secure reversal. The matter of endorsement by any scientist could not possibly be decisive, for scientific endorsement by the head of the Columbia Physics Department, Dr. Pegrarn, had been brought to the Navy Admiral by Professor Fermi and was completely ineffective for the reasons set forth in my account. Whether if Dr. Fermi had not been so skeptical the Navy's attitude would have

been different it is impossible to say, but the President knew of the interview and its results. Conscious as I was of that apparently insuperable hurdle, - as distinguished from the scientists who were only partially and superficially aware of what had happened through the Ross Gunn letter - I had to await a time when the President could accord a type of consideration that by its very nature had to be reorientation in terms of high polity and high strategy. It stemmed from that that the scientific work too had to be cast in the mold of the struggle between Nazism and democracy. That explains the tenor and the content of the letter that I prepared for Einstein's signature. On re-reading you will find that it contained a point about uranium supply and the quality of uranium in the United States as against Czechoslovakia, the Belgian Congo, and Canada. That consideration was also outside of the ken of the Columbia and Princeton physicists and would scarcely be thought of as an item to feature. It was thought of by the one who was the President's Cassandra and Jeremiah from 1937 on and the correct forecaster of the failure of economic sanctions, the invasion of Austria, the Czechoslovak and Munich disasters and, at the beginning of 1939, the absorption of Czechoslovakia. Thus in anticipation of the eventual conference with the President at the deemed opportune time, in October, I attributed cognizance of that by the noble physicist who was to sign the letter, and had him make reference to the quality of the uranium ore available to us as against what would be available to the Nazis from Czechoslovakia and prospectively from the Belgian Congo via the forecasted Nazi occupation of Belgium.

This passage has a double import for and challenge to the mis-historification that has been effected by the journalists who fixed on the justly eminent physicist and by the physicist who was the bearer of the prepared letter. Since in my advisory role on the international developments I had been predicting for the President the course of the nemesis, I sought to focus his attention on the crisis that would emerge for the United States from the fulfillment of the trends even in connection with that letter that was to be signed by Dr. Einstein. The agreement for the signature had been secured by me prior to the transmittal of the letter and it was known by me that Dr. Einstein would not be reading it and passing on the details. For that matter, had he and my emissary been examining the letter, they would have shrunk from associating themselves with my comments on the threat of unavailability of uranium supply to the United States because of the forecasted closure of the Belgian supply, which was designated as the best in the light of my prior studies on Union Miniere. As scientists they would have wanted to contract the scope of the letter to what was within their ken as scientists. But I insisted that my emissary should not act to make any changes in the letter and merely carry out the pre-arrangement with Einstein for his addition of a signature. -- With respect to the specific and related international aspects of the problem, it is noteworthy that in the eventually carried-out presentation by me in conference with the President, the letter which was accorded the greatest attention by him was the re-

statement of these and other aspects as contained in my covering letter for the whole dossier.

* * *

The foregoing larger setting for the letter shows that the letter as such was subordinated to the problems and the exposition of developments, political as well as scientific. The original of the letter as signed was left with the President, and thus has come down to the Roosevelt Library. The reproductions of the original letter with the signature were included in my special reports to the War Department and later in my presentation before the Senate Committee on Atomic Energy in November 1945.

It further follows from all the foregoing that the problem as it figured in the President's mind as well as in discussions between him and myself on that day and on the following day were not concerned in the slightest with scientific endorsement or the personality of the endorser, but with the whether and the how of the reconsideration of the whole problem and the possibility of taking a positive stand in face of the already structured turn-down by the Armed Services of any support for the Project. How those obstacles were surmounted is recorded not only in my testimony and the Look article, but in the fuller historical reconstruction for the Historians of the Atomic Energy Commission. Even the last is an incomplete account and will be accorded further development in the material that I have been requested to give by that important social science institution, the Brookings, in Washington.

Sincerely yours,

Admiral Lewis L. Strauss
Mercury Building
1925 K Street, N. W.
Washington 6, D. C.

[Comrap. Strauss, L.]
[Chron 1/61]
JL

January 12, 1961

Dear Lewis:

With a view to your time pressures in completing your book, it occurs to me that it would be advisable to anticipate a question that might be elicited by this second and fuller letter to you of January 11th, even though your reflective turn of mind would anticipate the answer that such a question would evoke.

On the indicated and amplified historical record submitted by me, the fact of the letter by a scientist in endorsement of the Project was historically of a minor and indeed merely incidental order as part of the dossier. For the President knew directly, and in turn conveyed to me, the details of the episode involving Dean Pegrum of Columbia, Professor Enrico Fermi, and Admiral Hooper of the Navy, and also knew that the Navy's Research Consultant, Ross Gunn, later on reaffirmed and rendered definitive the Navy's rejection of the proposal for Government support of atomic research. Accordingly, with the knowledge available to the President that eminent physicists endorsed the idea of Government support but that the Armed Services did not deem it advisable, another letter from a physicist, no matter how eminent, performed a function of completing the record or the dossier. Yet it had a supplementary role, which I did spell out in my testimony before the Senate Committee on Atomic Energy when the Chairman, Senator McMahon, deliberately made me the first witness instead of General Groves.

Accordingly, I desire to amplify my account further by specifying the supplementary significance that I originally attached to a letter from Dr. Einstein, even though, due to my extremely confidential role to the President, I did not at the time deem it proper to make full disclosure to the emissary of my proposed letter for Dr. Einstein's signature. That had to do with the unique significance of the refugee scientists as a whole and the relationship to them of Dr. Einstein, who came of his own free will much earlier because of the revulsion of his noble nature against all forms of oppression. For the sake of brevity I will not at this juncture reproduce the text of my comments that you will find in the earlier pages of my testimony. Nor will I set down here what, for the sake of time-economy, in my original exceptionally long presentation to the Senate Committee was ~~the~~, namely a recapture by me of the role of refugee scientists in the seventeenth and eighteenth centuries. For what A. N. Whitehead called "the Century of Genius," the seventeenth, as the first great efflorescence of combined theoretical and technological science was mediated by the havens of refuge to oppressed scientists accorded by Holland and England; and be it added - as it was in my notes - by the United States, which provided such refuge for a technological student of science called duPont.

Now because Dr. Einstein had challenged Nazi ideas before Nazism took over Germany, it was proper and justifiable for me to interpret what could be in his thought and therefore to attribute to him the larger political consequences of aggrandized Nazism potentially on the march when I drafted the letter at the end of July, and regarded by me as inevitably headed for the whole engulfment of Europe, and thereby threatening such things as sources of supply of uranium from Belgium. In a word, the larger political perspective telescoped in the draft-letter for Dr. Einstein came from my psychological-political empathy with Dr. Einstein and was not a mere piece of ghost-writing for him. As I stated in the very last letter, had the scientists drafted a letter, they would not have thought of and for that matter would have been unable to formulate these larger, deeper political and moral meanings; and it is these meanings that had as their practical consequence the problem of uranium supply summarized in my draft for Dr. Einstein and articulated more fully in my covering letter, which though written much later had already been foreshadowed in notes that I had made.

Focussing once more the searchlight of historical truth, the hints contained in the letter for Dr. Einstein's signature and in my fuller letter regarding refugee science as such in that crisis of human history were vital to that extremely difficult task that I undertook of reversing the structured negative position of the Armed Services. With the Chief Executive at that time completely lacking in the varied and maneuverable financial resources now available to the President and with the regard that he had to show for the attitudes of the Navy and War Departments in connection with defense problems, it was most important that the President should be inspired to a great feat of will and decision. For as advisor to him I had to take cognizance in advance of what he afterwards described as the nature and the weight of the objections that would be raised by the Navy and other highly esteemed heads with their weighty Congressional support. Since our defense had been so neglected and since in the hoped-for event of a lifting of the Neutrality Act there could be appropriations for improving that defense, why should the President get concerned about the fact that scientists wanted money to carry on research, for which the proper sources would be regular university funds and foundations. It was therefore vital to recast the whole idea so that the rejections by the Navy would not be relevant to the new proposal. One concrete instrumentality was devised by me, namely to shift the Project from the Armed Services to the civilian organ of the Bureau of Standards. But that could only be an instrumental corollary once the Project was reoriented in the larger and deeper terms of the fundamental struggle between Nazism and democracy and the attempt by the tyrannical systems to enslave the human spirit, including the spirit of the scientists. In the event of the conversion of the President to the Project as I envisaged it, the United States would have the great opportunity of acquiring through the refugee scientists the leadership in the hoped-for most fundamental new discoveries in the

event atomic fission, as realized by the two German scientists, were to open up through the chain reaction fertile discoveries and technologies over the whole gamut from weapons and defense to economic-technological innovations, health and other human welfare.

With high regard,

Sincerely yours,

Admiral Lewis L. Strauss
Mercury Building
1925 K Street, N. W.
Washington 6, D. C.

January 18, 1961

Dear Lewis:

Of course you have my consent to quote from my letters, but please effect contextual quotations, - that is, avoid extracting parts of sentences, which happens to be a tendency in recent book-making. Only through integral quotation of the sentences and preservation of the context will there be a correct preservation of the historic record with regard to the Atomic Project. For it is that epochal project which has suffered terrible mis-interpretation and distortion by writers, not merely in the press but eminent scientists like Dr. Compton, and might by an eminent administrator like yourself suffer from the type of foreshortened, refracted, and distortive observations that have masked the present accumulated mis-history.

Sincerely yours,

Admiral Lewis L. Strauss
Mercury Building
1925 K Street, N. W.
Washington 6, D. C.

[Copy]
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File
Pegram's
(original)
Dr. Strauss
5/10/61

May 8, 1961

Dear President Calkins:

With regard to the projected series of lectures on "The Nuclear Ages - Rediscovery of the Past and Reorientation on Present and Future," the approach and content follow the expositions given you within the past year. The original act involved a totally unprecedented confrontation in and by Government with problems and the devising of a new repertory of research procedures.

As had been adumbrated by me in that original delightful luncheon conference with you, there are quite a number of neglected aspects of that original enterprise as an adventure in thought and governance up to the Atomic Project. The emergence from the stage of scientific contemplation of potentials to the stage of technology in operation took generations, and that timespan was illustrated by electricity and radio as the two major incarnations of idea into practical reality before World War II.

It was the combination of a statesman and a social scientist which broke the previous historic trend and, in the language of my testimony before the Senate Committee on Atomic Energy, reduced to a decile the time for the emergence and maturation of the new technology. As the threats to our Civilization were without precedent, such miniaturization, if you will, was the prerequisite for the Government's consideration of the problem. Moreover, apart from summary statements to the historians of the Atomic Energy Commission and early this year to Admiral Strauss, there has thus far been no light thrown by the marginal personalities who have written so much on the subject as to how it came about that the Government's undertaking was cast in a frame tremendously surpassing the scope that the scientists thought would be involved. Indeed, as those pressing for Government support had in mind the continuance of their researches, the original scale, as suggested by Dr. Pegram, and even by Dr. Bush in his then capacity with the Carnegie Institution, was in areas below a decile of what had been computed by me as the eventual cost, and which happened to have been vindicated. Thus there was infused ab initio a concept that not only would it be contributive to a foreshortening of the war, but that there would be opened up an outstandingly new era of the role of Government to science acceleration as an instrument in economic progress and world peace.

After the successful use of the bomb for the tremendous life-saving, as estimated contemporaneously by both late Secretary Stimson and by Sir Winston Churchill, in his ensuing war records, I prepared as a sequel to my November testimony before the Senate Committee a special report entitled "Perspectives on Atomic Energy and World Security." The accompanying chart is, so far as I know, the first venture at

discernment that the tremendous destructive potential of atomic weapons would act as a deterrent to world wars and would confine conflicts between powers to limited wars. That chart of November 1945 was entitled "War Outbreaks in Time as Experienced, Compared With Distribution Under Poisson Law of Rare Events, With Probability Latest Peak Frequency May Be Contracted as After the Thirty Years War by More Infrequent and More Limited Wars."

But gradually in that postwar there did emerge a series of efforts which became entangled in the internal and international struggles between emotional to utopian pacifism and more practical-minded disarmament proposals. Fortunately, in this postwar there did not take place the drastic demobilization and rapidly eroded defense that characterized the Allies after World War I. The dangers of such a repetition of history were long in advance discerned by the instant writer in a submission to the President immediately after Yalta, in a memorandum which has since been accorded publication. It was as a sequel to a memorandum in April 1943 on "Soviet Foreign Policy, Totalitarianism, and Russo-Allied Rifts" that this writer submitted to the President a document that was to have been part of the re-thinking of his policies that, alas, could not be effected by him. The opening and refraining thesis of that document was as follows:

"With the Nazi danger of 1939-41 converted into a potential military mastery over Central Europe, there is a high probability approaching certainty that Soviet Russia will return to introverted insulationism and extroverted imperialism, and that it will embark upon splitting the survived triumvirate of the major powers."

The first and last inferences drawn from it were as follows:

"The requisite reorientation in American political strategy is, with respect to Russia, a policy of diplomatic firmness backed by maintenance of reserve power as against the tendency after military victory towards rapid and sweeping demobilization. ...

" ... we must devise policies and instruments calculated to circumvent in time the ideologically more pervasive and internationally more subversive challenger of Western political and economic democracy that after military victory may be the adopted role of the war-aggrandized Byzantine totalitarianism.

The foregoing may be taken as having foreshadowed not only the phenomenon of a designated "short-of-peace postwar" - since called the "cold war" - but the re-embarking in 1950 on defense in connection with the limited Korean War and also the overlapped Marshall Plan.

Yet it is noteworthy that this postwar period entailed dampening of the Atomic Project in its civilian implications and applications. It is the meaning of the original Atomic Project for the resurgence of civilian or economic atomic development that needs to be evoked and sketched. And this entails that counterpointing of present and future with the past that interested you in my talk with you while in Washington recently, as it had previously also interested Ralph Watkins. This does not entail an interjection into high politics or diplomacy of test bans, but rather in the drawing of a lesson from the past for the present to assure progress in the future. And this is a lesson that can be italicized with propriety as well as profit by the original utilizer of civilian applications as an element in the conversion of the President to the value of the Atomic Project, notwithstanding the complete rejection of the idea by the Armed Services in April 1939 in that mission that had been sponsored by Dean Pegrum of Columbia and by Dr. Enrico Fermi, who on the record had regarded ^{the} practical applications of nuclear research as so extreme as to be called by him a "chimera". That a non-natural scientist did discern the potentialities and deemed them worth pursuing is a salient illustration of the value in national economic management as well as governance of other insights than the strict pursuit of empirical science.

Finally, as to contemporaneous pertinence and value, there are certain applications that have been hinted at in that latest formulation to you in connection with the importance to our nation of recapturing leadership in practical economics and human welfare. It is from that setting that scope for efflorescence emerges in two major areas. The first is the prospective economies, with far-reaching implications for undeveloped countries in electricity generation. As a close follower of these subjects, material has been accumulated by me along the very lines of the just published article in Nature for April 22nd, entitled "Generation of Electricity Without the Use of Rotating Machinery." This encompasses such methods as the fuel cell, magnetohydrodynamic (MHD) generation, thermionic generation, and thermoelectricity. Of decisively pressing importance for our nation is the role of atomic power in solving the problem of the dropping tableland and water levels, - involving over time magnitudes of potential costs in the neighborhood of the national debt.

* * *

The foregoing amplification of the outline in the previous letter suggests the import of the major theme on the Two Nuclear Ages and the entry upon a renascent Nuclear Age. Through unprecedented vision and will, our nation, which had been very much in the background of atomic research prior to World War II, did utilize not only the Saving Remnant of such scientific talent from the cruel totalitarianism of Central Europe, but called into being a new American generation of scientists. As a collateral result, the temper and tempo of the

interactions between science and technology have been radically transformed and accelerated. For reasons hinted at there have been subsidence and partial eclipse of progress in the civilian atomic age. At this juncture we are again challenged by history to live up to that motto of the Great Seal of our country in noting that we have been established to be a new order of the ages ("Novus Ordo Seclorum").

With kind regards,

Sincerely yours,

President Robert D. Calkins
The Brookings Institution
1775 Massachusetts Avenue, N. W.
Washington 6, D.C.

cc Dr. Ralph J. Watkins

(Duplicate)
Corresp - Lapp
[Chrom 7/64]

July 21, 1964

Dear Dr. Lapp:

In accordance with the request made by you in the course of your telephone call to my office late yesterday afternoon, I have assembled a collect of the documentary confidential material to which allusions were made in our conversation. This material covers my labors during my extended service as confidential advisor to the late President Roosevelt, with special and varied bearings on my initiatory advices to him on the interacting developments from the mid-Thirties on concerned with (a) the accumulating war threats from the fusing Nazi, Fascist and Nipponese totalitarian systems, (b) the heightening technology of war materiel, and (c) the progress of atomic physics and the emergence at the beginning of 1939 of the potentiality of atomic weapons. As a complete assembly would itself constitute a book, I am compressing the collect to the more convenient size of a large folder.

As to the interlinked roles of (a) the forecasts of stages of totalitarian aggression and (b) the crucial acceleration of atomic research in the very aftermath of Italy's aggression against Abyssinia and Allied humiliation through the defeat of economic sanctions, - all that is covered in published form from pages 553-58 of my opening testimony before the Senate Atomic Committee on November 27, 1945. (Enc. 1) A contemporaneous document, which has not been published, was disclosed in a communication of September 29, 1959 to Messrs. Richard G. Hewlett and O. E. Anderson, then on an assignment of the U.S. Atomic Energy Commission. This document is a reproduction of handwritten notes from February through April, 1939, out of then already sensed discernment of unfolding history in its Nemesis. It was then designated "Origins of Concern With Significance of Atomic Research for U.S. Defense and for U.S. Role in the Overwhelming World Crisis From Beginning 1939." To the reproduction is added a typewritten transcript made by a secretary then, as herewith reproduced. (Enc. 2) Supplementing my own testimony towards the end of 1945 there have been articles by publicists. The first was by Mr. Nat S. Finney of the Look editorial staff on March 14, 1950, entitled "How F.D.R. Planned to Use the Bomb." (Enc. 3) The second is an article by John Gunther for the twenty-fifth anniversary of Look in February 1962 on the great decisions of the intervened period. The illustration on page 69 and the text on page 71a. are devoted to my conversion of President Roosevelt to the Atomic Project. (Enc. 4)

I come now to another set of undisclosed documents that are concerned with the projections by President Roosevelt for the use of the bomb when completed, for the purpose of accelerative closure of the war,

especially in the Far East. It was at the end of October and the very beginning of November, 1944 that, in keeping with the pattern of periodic evaluation of the progress of the war, I submitted to President Roosevelt a long memorandum entitled "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War." The very first page, as you will note, under point 3 forecasted that, "The end of the war will be accelerating piecemeal operations" and, "The timing should be between April and May" (meaning 1945). (Enc. 5) Section B. 1 repeated that: "The projected date for the end of the war as spring 1945 ..." Then after an extensive exposition of the high advisability of utilizing the Imperial Household for ending the war with Japan without invasion, the following summary advice is given at the top of page 7:

"The talk so glibly indulged in by military commentators of two more years of war against Japan is as unrealistic as the talk of a Hitler-directed guerrilla warfare after we will have taken the Ruhr. Moreover, with the augmenting military operations and exponential weapons, the coup de grace should be delivered by the existing coalition," - meaning the United States and Britain without Soviet Russia.

A supplementary urging was submitted early in November, 1944 before the President's Foreign Policy Association speech. Its purport and import were for interlinking the designated "exponential weapon" - our own code word for the atomic bomb - with a separate political strategy for Japan. (Enc. 6) This formulation included a scheme of advance warning before using the bomb. With respect to these documents, long after the events - more precisely in the summer of 1946 - they were shown by me to the then President of the Union Theological Seminary, Dr. Henry P. Van Dusen, who in turn passed them on to the late John Foster Dulles, who transmitted to him the comments contained in the letter of June 10, 1946. (Enc. 7) While still on the subject matter of the atomic bomb and its potential military uses, it would seem appropriate to anticipate history and submit a memorandum of April, 1950. By developing a methodology for projecting comparative atomic weapons accumulation, - in the wake of the Soviet's duplication of the bomb in September, 1949 - my study of April, 1950 was bold enough to have set the following target dates of aggression: (1) mid-1950 for Far Eastern aggressions pivoted on Korea" and blackmail attempts from mid-1951 to the mid-1950's. (Enc. 8)

Coming to briefer and more summary description, the recapture of the origins of the Project is set forth in detail in the previously mentioned communication to Messrs. Hewlett and Anderson of September 29, 1959. (Enc. 9) This document sets forth in detail the historifying sequence - that is how history was made - and throws into sharp relief

the hitherto totally neglected fact that the military services, and particularly the Navy, had in two salient acts rejected any governmental aid to the Atomic Project. One of the constituent elements in it was that Professor Enrico Fermi thought, in the language of the memoirs written by his wife, that "an atomic bomb" would have been "pursuing a chimera." In the course, then, of the labors of persuasion directed at the President, there could have been no prejudging of the attainability of a chain reaction, out of which would have come the bomb. Consequently the urgency was related to (a) the prevention of the possibility of Nazi terrorization of elements in the United States - in keeping with a pattern that had up to then worked with people on the Continent - and (b) the potentialities of such epochal research and invention for transformative civilian economics. The argument set forth in the closing section of the letter, from the bottom of page 11 through mid-page 12, recaptures a discussion that had a profound impression on the President, namely in the light of Bishop Butler's theory of probability, that even when the chance of attainment of an end is regarded as small, the greatness of importance of the end sought over-balances the low weight of attainability. President Roosevelt, it must be stressed, was a profoundly religious and ethical person as well as leader, and this argument contributed to winning him over to (a) the assurance of prevention of Nazi use of potential bomb and (b) the potential benefits that might come to us from progress in atomic research.

In the following year came supplementary efforts to dispel the mythopoetic journalism that passed for contemporary history. Admiral Strauss had already repeated it in the draft of his book. As his book was being readied for the press, he called me for statements on the true origins. A set of that correspondence constitutes a special Exhibit 1Ca-f.

As a final supplement to the explication of the origin of the Atomic Project, I am enclosing the letter to General Groves of September 12, 1963 by way of correction of an impression that he had received from the Hewlett book. (Enc. 11)

As culminative evidence of the continued advisory role to the President on international policies as well as the Atomic Project, I am enclosing copy of a memorandum to him at the end of February, 1945 upon his return from the Yalta Conference. It foreshadowed the entry of the imminent postwar on a short-of-peace or, as we now call it, "Cold War" epoch, and the memorandum adumbrated an orientation and a policy that was later incarnated in the Marshall Plan. (Enc. 12)

* * *

Now we have to turn to acquire for ourselves some lights on the prolonged genesis from the beginning of 1939 until the symbolic date in October, 1939 of a decision by the President to inaugurate procedures outside of the military for fostering of research that might produce a weapon, - which weapon the one outstanding world-recognized experimenter, Enrico Fermi, thought so unlikely as to have later attached to it the descriptive designation of "chimera". Though the collect submitted herewith is varied and ramified and although the documentary material as submitted to the two other main inquirers was replete with historical roots, we have to be mindful of the aggravated shortcomings of our contemporaneous institutionalism in dealing with emergent history. Here we are up against the complex processes that tend to deprive us of the raw material of emergent history and to replace that by reprocessed pseudo-events and surrogate events for the utilization by the apparatuses of the movies, press, radio and television, with the new technological media dominant over the older discursive media wherein publicists concerned with the truth of events-in-process used to refine away the dross from the received crude material. As a result, we suffer not only from the foreshortening of perspective but the visual and vocal amplifications of the introduced substitutes for the events, written up and presented like scenarios keyed into dramatizable top-billed figures and situations.

Fortunately, for the Second World War, Winston Churchill, as a self-trained historian of earlier new history in which he was a participant, gave our world a sense of the true flux and ferment of the newly accruing history of the war. But on our side, the corresponding history-shaper was too burdened to engage in memoir-writing, though he often mentioned to me that after retirement he would want to give his version with the aid of this and other confidential aides. The lesser figures who have issued books on it, with the exception of Secretary Stimson, have given us products that reflect the scissors-and-paste jobs of the assemblers of the material from contemporary chronicling along with some official documents. And as for the professional historians, their training and activities have been, in most instances, prefixed by labors on the accumulated deposits of writings by others and previously crystallized orientations. With such notable exceptions as Professor Samuel Morison on the Navy and A.M. Schlesinger on the thus-far recorded early New Deal, the typical academic historian, when self-directed to contemporary history, has shown, in areas that I happen to know first-hand, not only little ability but little interest in discovery of the deeper layers of the historical determination and the removal from the publicized version of the distortions and falsifications that the original apparatuses have effected and thereby placed into circulation and acceptance.

Hence for the recapture of what had really occurred, the very first task is to realize that at the beginning of 1939 nearly all of the now recognized leading figures in atomic research were in or beyond the margins of those activities, particularly as to the stages to flow from the epochal experiments in Germany. There was tangential work by Arthur Compton, and then there were workers at Columbia, - that is a few refugees who had been accorded laboratory space. The most eminent at Columbia, Enrico Fermi, was not then working on the problem, nor was Urey. Utilizing conceptual terms of my invention necessary for this recapture, the basic process in history is not, as we so erroneously and with encrustedness say, the past, but eventuations within the present out of the past looking to a future. (Summarizing from a forthcoming work on Emergent History's Modes and Dimensions -) That process I call historification. But both for historical drift into the future and the transformation of the drift by historical agents there is a process of pre-historification, where - in elements that can influence the drift consider whether they should or should not. If in the flux the decision runs against intervention by the historical agents, a good deal of such potential history gets lost, and only when, as in the case of the sequel to appeasement, other historical agents enter to transform the prior action and in-action does history take cognizance of what might have happened.

Now at the beginning of 1939, the crucial pre-historification was the visit of Niels Bohr to Princeton and the intellectual-spiritual seismic disturbance fostered by him through the two communities at Princeton University and the Institute for Advanced Study. Due to my original links with Abraham Flexner and his then prospective successor, Frank Aydelotte, - both of whom were co-guests with me at Lord Lothian's estate in 1934 - and due to my personal-professional link to Economics Professors at the Institute, Walter Stewart (who was then also Chairman of the Rockefeller Foundation), and Robert Warren, a philosophically and historically highly reflective person, - due to these links I was at the time within the resonance of the reactions in progress to the experimental events of the Hahn-Strassmann atomic fission and the inferences from them by Frisch and Meitner.

The just given 'additive' is significant because the two plus Bohr, and along with them the Joliot-Curie group, regarded chain reaction as probable, contrary to Fermi. The two marginal experimenters at Columbia, Szilard and Zinn, leaned to that view, but they did not at the time have the standing, nor did they come close to saying it the way the partial-Scandinavians and French did in the letters that were published in Nature, considerably ahead of Szilard's visit with me. Inasmuch as I had been engaged for the President throughout the spring of 1939 on timing and defining the transition from the piecemeal aggressionism by Hitler to imminent war declaration, I kept the President advised of the Princeton developments and of the publications in Nature at the very time when I presented the March lecture, which

opened my testimony for the Senate Committee. It was as a sequel to that disclosure that I learned of the Fermi-Hooper meeting in Washington and the turn-down by the Navy of any aid to the experimental work at Columbia, which had received a new impetus from the Princeton repercussions. Concurrently, my contacts at Princeton conveyed to me some idea of the experimentation going on at Columbia. For in the accumulation of my interest in atomic physics, as reported in my contemporaneous notes, my focus from the inception had been on the terrible threat embodied in atomic fission to the fulfillability of what Aston had adumbrated in that Cambridge lecture, as reproduced in my initial testimony before the Senate Committee.

It is in that conjuncture that I started inquiring of influential refugees in the New York area, who simultaneously were advising one Columbia member, namely Szilard, to contact me. When a meeting was finally arranged for Szilard at the end of July at my office at The Lehman Corporation, One William Street, I learned of the financial difficulties that such researchers had; and in the specific case of Leo Szilard, his awareness of the political situation. But what scared me from the start - remember that was July and months prior to the Hitler-Stalin Pact - was his mis-reading of the immediate prior history and assumption that Soviet Russia was the innocent party in the Munich episode and that Russia would then be the next victim instead of Poland. Indeed, even after the Stalin-Hitler Pact, that mis-reading of immediate history and mis-orientation on evolving history made it clear to me that I had to be on strictest guard; and so I was.

From here on the material in the collect takes up and carries forward the historication. Again, the complete foreclosure of any aid by the military-naval services and their leaders required inventing a very different approach to the President and evolving not just a response to influential endorsers of an idea, but the evolving of a strategy for the protection of the United States in the prosecution of the war and for the opening-up of ~~new~~ ~~vistas~~ for U.S. science leadership. That is the truth against the fantasticated myths that have been popularized.

Finally, it must be borne in mind that at no time did the invented channel of the Bureau of Standards come out positively and that the labors with its late Director, as well as with the two attached representatives for the Army and Navy, were to prevent their negating the Project. The way I finally saved the Project from submergence was through a supplementary conversion of the President to his adoption of a new medium for fostered scientific research and technology for military weapons, - about which a good deal of material is found in my published testimony. The channeling of such letters was the securing - in those later instances by myself - of Dr. Einstein's signature to other letters that I drafted for him that went

to Dr. Briggs. There were also direct letters by me to General Watson. In one of those letters I apprised him of some further progress made by the experimenters at Columbia, but, as you will note from the text on page 572 of my printed testimony, that progress, contrary to some subsequent allegations by Dr. Szilard, was far remote from the much later accomplishment and demonstration of a chain reaction. The main aim of that letter of May 15th and the prior letter to the President of May 11, 1940 was the establishment of a new instrumentality for the prosecution of weapons research and correlative activities bearing on victory. As set forth on the second half of page 572, the conversion of the President by the end of May to my proposed new organizational instrumentality of a Scientific Council of National Defense led to the establishment by the President on June 15th of the Office of Scientific Research and Development. And out of that came, in furtherance of intervened exchanges with the President, the establishment of the Manhattan Project under the auspices of the Army.

The just summarized phase of late-spring to midyear 1940 (as covered on pages 568-73 of the testimony), - that phase was the most crucial for the Atomic Project. For the Columbia University experimenters, while having made progress, had not delivered that demonstration of the chain reaction that was then demanded by the physicist-advisors to Dr. Briggs, Director of the Bureau of Standards. Therefore the whole effort of the previous year, and particularly from October, 1939, was in peril of abandonment. Recall in this connection, if you will, my conceptual instrument for policy guidance as expressed in that lecture of mine of March 10, 1939, in which the President was so interested:

"There is still time ... for the exceptionally and fortunately situated United States to use the time-drafts that can still be made on the 'Bank of History' for the preparedness that has and will become more and more urgent ... in the course of the prospective unfolding aggressions of Nazi Germany."
(quoted on page 555 of the testimony)

The rapid, compactly moving unfolding of such aggressions in the spring to mid-1940 consisted of the following: (1) April 9th: the German invasion of Norway and Denmark; (2) between April to early-May: the forced reversal of the British landing in Norway; (3) May 10th: the German invasion of the Lowlands, - with the contrapuntal advent of Churchill to the leadership of Britain; (4) mid-May: the surrender of the last of the Dutch Army; (5) late- to end-May: the surrender of the Belgian Army; (6) late-May to beginning June: the evacuation of Dunkirk; and (7) the second of June to end-June: the defeat of the French Army, the Franco-German Armistice, and the establishment of the collaborationist Petain Government.

Hence under this bombardment of calamities and the Nazi takeover of so much of the Continent, it became terribly difficult to counter the advices from the military to abandon the Atomic Project, in the more correct sense of abandoning the effort to make it an integral part of defense. Therefore the truly heroic part, in the sense of overcoming most powerful resistance, was the invention of a new instrumentality and the evocation of Presidential support for that new instrumentality through which the Atomic Project could be launched. That, as if in palimpsest, is the underlying and perduring meaning of the labors represented by the letters to the President and his intimate advisor on the subject, General Watson, in May, and the ensuing persuasions that met with success in the President's decision in mid-June to establish that new instrumentality under the name of Office of Scientific Research and Development.

The thus portrayed magnifications of the world crisis - with attendant threats to the survival of Britain, which elicited the President's transmittal of destroyers - elicited discussions on the extremity in which we were placed, and hence to reconsider the probabilities in the Butlerian sense of the enormousness of the stake and the consequentially corresponding value of attainment of leadership in weapons technology with success for the Atomic Project! Thus the then nearly climactic frustration and abandonment of the Atomic Project became the embarking upon accelerative and specialized effectuation, along the lines described in my testimony, for telescoping through concurrent pursuit of methods the invention over a time-goal of three years or so, as a decile of the previous pathfinding scientific discoveries culminating in technology in timespans of a generation.

In the thus portrayed perspectives and contexts the contributions of the great physicists from abroad, and especially Dr. Einstein, were in their incarnation of concern over the dangers from letting the Nazis be the avant-garde in science and weapons technology.

Such, then, is the recaptured historification of the Atomic Project.

Sincerely yours,

Dr. Ralph E. Lapp
1315 Park Terrace Drive
Alexandria
Virginia

April 20, 1963

Mr. Emil Gottschalk
House Orelli
Arosa, Switzerland

Dear Mr. Gottschalk:

In supplementing the acknowledgement that Charlotte already wrote you for your kind letter and enclosure, let me say, first of all, that the article in Die Zeit that the German-Swiss journalist Jungk wrote, I was ^{from} left at a loss to understand the issues in controversy with Professor Teller. The original book on the atomic project that Jungk published a long, long time ago, was on the whole a sympathetic treatment of my role. But I don't recall as to whether he drew any attention to another instance of scientific foresight by Teller. For in the early and distinctly preliminary phase of the atomic project, it was necessary for us to get from the scientist's judgement as to the feasibility of the chain reaction. As part of the irony of history, Professor Fermi himself was not only skeptical, but in the biography written after his death by his wife Laura, the prospect of sustained chain reaction for a successful bomb was declared by him as a "chimera." Teller was one of the very few in the extensive scientific group who expressed optimism instead of pessimism.

Accordingly, on the at once sequential and consequential issue of the feasibility of the H-bomb, the Teller position was, as you say in German, "konsequent." There was then a factor that not merely the journalists but the scientists who have written on the H-project have neglected to stress ^{had} nearly from the start, Teller discerned, and if you will, extrapolated continuable progress. As we lacked, at even this late date, an adequate history, and as the meager history that we have is enmeshed in excessive controversy, both professionals and lay public have failed to appreciate that, as often happens in the history of pathfinding research, there was a cleavage between those who saw the obstacles and those who saw the opportunities.

Interestingly, in early 1939 it was the ^{British and the} French headed by Joliot-Curie, and the North Europeans, headed by Bohr, Meitner and Frish, who saliently bespoke and predicted chain reaction and effectuation of the explosive application called the atomic bomb. On this side, the issue was

ajudged negatively by the greatest experimenter in the field, Professor Fermi, and by others, save for three from very different standpoints. In the case of Teller, I think it was intuitive discernment and the experimental drive. In the case of Wigner of Princeton, it was discernment, combined with concern for the danger that the Nazis would get it first. In Szilard's case, it was similar to Teller, but accentuated by the primary interest of continuing to be active in the research and to move forward into prominence with it, since unlike the other two mentioned, he had not yet "arrived" in professional status in the United States.

Having thus rescued one of the fundamentals from the obscurity that journalism has cast over the genuine but unrecorded history, it seems to me that you and your relative working with Professor Teller can appreciate that the highly emotive struggle between Teller and Oppenheimer needs to be reinterpreted. The frictions and antipathy that have been thrust to the fore in the writings about the H-bomb have distorted the original picture. The protagonists of opposing views were moved by different political attitudes. The possibility would seem to me to be that even if Teller had a different political orientation, he would still have wanted to pursue the combination of discovery and invention from fission to fusion contrapuntally. Oppenheimer may not have been pessimistic on the scientific feasibility but dreaded lifting the pitch of destruction to so much higher levels. Like many who currently have been advocating limitation of the Nuclear Club, he may have wanted to stop at the early stage with the view towards effecting some kind of agreement through the cessation of entry on larger and graver frontiers.

The ^{new} scientific article in the German paper that you sent has seemed to me unwarrantedly confused and totally unnecessarily deflected into personal issues of no consequence and relevance to the subject matter. That it was also ill-tempered indicates how bad the editing of that paper is. Throwing myself back to the period of the bitter controversy over Oppenheimer in 1954, I have not been able to understand why Teller engaged in the personal attack. Not having ^{made} any close study of the original documents, I can only fall back on the recollection that he was guilty of an excess of zeal and of misattribution of a political "arrière pensée" to Oppenheimer. I then felt, and still feel, that he ought not to have gotten himself in the bad company of the camp followers to the McCarthy persecution of Oppenheimer. He ought to have kept aloof from the politics and concentrated instead on the divergence of opinion on the future experimentation with fusion, strictly on the scientific and technological

April 20, 1963

levels. All of which would tend to place me in the group of seekers for that quality and virtue that Plato called "Sphrysonne," or balance and temperance. Stated negatively, anyone who, in respect to issues of bitter partisanship, seeks to identify himself with balance and temperance, runs the danger of being criticized by the opposing camps. With- standing that, I feel that the net effect of the doubts, to put it mildly, that were voiced by Teller regarding Oppenheimer were unfortunate and, however unwittingly, failed to serve the cause of atomic research in that the fear of persecution for associations during the later Thirties of a radical kind did scare many younger scientists into self-escapism from atomic research. I would still hold that these side consequences of the personalist intervention by Teller against Oppenheimer did not come from personal hostility towards and denigration of Oppenheimer's character and service. - You see then that I have throughout been impelled by the idea that there ought to have been far greater thoughtfulness and consideration to many people. There was a grave miscarriage of justice in the peculiar verdict of the Commission that Oppenheimer was loyal yet a security risk.

Yet at the time I was, and continue to be, in favor of continuing experimenting and invention, and maintain that there is a good deal to be said for the concomitant pursuit of diplomacy and the exploration of the possibility of agreements, but limited to vigilant retention of our rights and avoidance of any compromises with our basic security. In sum, the tenor of my own thought that not only the journalist, but the protagonists have over-dramatized the conflict. This over-dramatization and conflict-promotion have distorted the original history and have left a legacy of injustice to Oppenheimer and a mistaken attribution of jingo-ism to Teller. In a word, the correct history and its re-illumination with understanding and compassion remains to be written.

Having thus reached a finale that itself points to a prelude, I must add that it is at Charlotte's urging that I have thus hastened to re-capture the neglected fundamentals and to provide some lights, although I would have preferred to have awaited refreshment, renewed research into the original complex and into the Teller book that in turn elicited the unwarranted attack by Jungk.

Sincerely yours,

[Conroy - Hampden]

File
H

April 29, 1963

Dear Lord Hampden:

In anticipation of your return to your office by mid-May and your conferring then with Mr. Harry Oppenheimer in London, I am enclosing reproduction of a letter to me on April 25th from Under Secretary of the Treasury Robert V. Roosa. With the evincement of interest as "keen and continuing" and the characterization of the project as "constructive", it seems that we are getting close to discussibility of the project with Mr. Oppenheimer. In my acknowledgement on the 26th of Mr. Roosa's letter, I sent along the extract from the statement by Chairman H. C. Koch that appeared in the London Times for April 9th on the operations of the Daggafontein Mines, Ltd. That extract set forth in incisively etched lines the new mining technique as already in use, together with description of the adopted socially progressive measures for native labor.

At this stage we are confronted by the obstacle, mentioned by Mr. Roosa, to the U. S. Government's "participating directly in this matter." To be sure, in the experience of a financier like yourself cognizance and consequential measures have to be taken of obstacles that might arise in the event of favorable reception of a proposed negotiation. All the same, there might be some interest in an essayed review by me of a convoluted set of obstacles that confronted me in the pre-prelude stage of the Atomic Project.

As originative proponent before President Roosevelt of the Atomic Project, I had to find a way whereby I could prevent foreclosure of or great barriers to the supplies of uranium in the Congo long ahead of the decision by our Government to embark on the Project. As was stated in the aide-memoire that I left with President Roosevelt in my conference of October 11, 1939, "Our supplies of uranium are limited and poor in quality as compared with the large sources of excellent uranium in the Belgian Congo"; and therefore I deemed it essential "to lay down the lines of policy with respect to the Belgian source of supply ... as well as to solve the immediate problems of necessary materials and funds." But contrary to the journalistic dramatized accounts, the President was ab initio paralysed by the judgement that had been conveyed in March 1939 to Admiral Hooper, head of research in the Services, by the outstanding atomic experimenter then in the United States, Dr. Enrico Fermi, that a chain reaction of any potency was, in his own word, a "chimera", and what the scientists wanted was to keep abreast with pure research. Mindful of the negative attitude of the Services as conveyed by Admiral Hooper and later by Dr. Ross Gunn, the technical advisor of the Navy, I sought and succeeded in channeling re-examination of the problem to a civilian organ of the Government, the Bureau of Standards. Yet it was already apparent to me, and so voiced to the President in October 1939, that it was incumbent on us to

to treat as an imminent fact the Nazi takeover of Belgium, and therefore to take time by the forelock through advising officials of the Union Miniere du Haut Katanga that we might require uranium supplies and accelerative uranium production in the Congo. With keen and sympathetic apperception, President Roosevelt cooperated in enabling me to approach the Belgian diplomatic and business authorities in the United States. And thus I entered my plea for their redirection of supplies in transit and, more importantly, for assuring the independent functioning of the Congo. On the whole, this anticipatory intervention did prove, to put it at the very least, contributive to the preservation of the Congo's uranium resources for the Belgian Government-in-Exile and for the Free World.

May I therefore invoke for your consideration the advisability of my being included in the exploration of this project by you with Mr. Oppenheimer while he is in London? Indeed, what we are dealing with is a set of interrelated three projects, namely (1) facilitating and financing, by our governmental stock-piling, of accelerative gold production by the new technique and the recanalization of such output; (2) the Orange River Project via the World Bank and, say, a syndicate of your firm with your American affiliate and also Lehman's and the First of Boston; and (3) promoting the purchase by our Government of a sizeable quantum of gold from the South African Reserve Bank.

Do let me have your views, enhanced by your detailed advice, as to how to proceed and as to the stages to be followed. I need hardly set down that I am impelled by the conviction that in effectuating substantial parts of this project you and I would be contributing to the restoration of leadership in international finance and international balance of payments management to the English-speaking world. Though it may well be that I ought to have been more tentative in my formulations, it seems to me that thus impelled one is permitted to conform to the advice in the first Peter, - "Being ready always to give answer to the hope that is within you, yet with meekness and fear."

With high regard,

Sincerely yours,

Lord Hampden, C.M.G.
Lazard Brothers and Company
11, Old Broad Street
London, E. C. 2
England

[Curray - Groves]
[Chron 9/63]

7

September 12, 1963

Dear General Groves:

Your kind inquiry of September 3rd has been in the midst of still unresolved pressures of a confidential governmental nature superimposed upon regular professional work. The enclosed copy of a letter from a high official on April 25, 1963 serves to indicate the consequential priority thus imposed on me.

While I have not yet had a chance to look at what you correctly designate with the adjective "so-called" A.E.C. story, I assume that it echoes the theory that was started by the journalists, addicted as the profession is to melodramatizing and personalism, and that was followed up by certain scientists playing the role of amateur statesmen with a view to conveying the notion that President Roosevelt's decisions were dictated and shaped by them. The plain facts were recorded in the only available record from one who had been in close and intimate touch with President Roosevelt throughout the prewar and particularly in that prelude phase alluded to in my testimony, ^{namely} the Cambridge Lectures of Lord Rutherford and F. W. Aston and the decisive event of atomic fission by Hahn-Strassmann. At the time of my testimony, Secretary Stimson was alive. Then in the following summer his successor, Secretary of War Patterson, with whom I had close connections, was also alive. And both read my testimony and the dossier of documents placed by me with the Department.

As under the pressures of other tasks an adequate reply cannot be undertaken now by me, I trust it may suffice to allude to a since disclosed document that contradicts the popularized myth. Sensitive as I have been throughout my life to requirements of confidential labor, I avoided informing the scientists with whom I was in touch in the immediate prewar months that I had learned from the President of the interview and the exchange that took place between Professor Enrico Fermi and Admiral Hooper. As disclosed by Professor Fermi's widow, Laura Fermi, in her book, Professor Fermi had told Admiral Hooper that a chain reaction was most improbable or, to use his precise word, a "chimera". On top of that, the Navy, which was the only Service Department that had some funds for research, felt and advised the President that there should be no deflection on a type of pure science research that had such extremely remote chances of being of any significance for defense. -- In that historical context, the issues that confronted the President were not over a letter from any one scientist, but how, after such turndown by the Services, re-consideration of aiding atomic research could even come within his ken.

Thus what the mixture of journalism and politically motivated propaganda by certain scientists has accomplished has been to obfuscate the complex predicament that confronted President Roosevelt in his very approach to the problem of governmental aid to what at that stage was an enterprise in pure science that the chief experimentalist informed the Services could not in all probability be of any pertinence to defense. Professor Fermi's negativism towards chain reaction, and therefore negativism towards practical utility, was conveyed to the Services in the spring of 1939. The Technical Adviser to the Navy, Dr. Ross Gunn, specifically informed Dr. Szilard on July 10, 1939 that, "It seems almost impossible ... to carry through any sort of an agreement that would be really helpful." He noted that after the matter of appeal in behalf of the Physics Department at Columbia had been "carefully considered," that decision was reached, and so contented himself with voicing regret: "I regret this situation but see no escape." Inasmuch as those who have been relying on the journalistic chronicles and on similar sources detached from these fundamental facts have missed the essence of the original predicament and the essence of the solution that was devised by the close adviser of the President, the prerequisite of any reconsideration by the President was a fundamental reorientation, as otherwise he could not have embarked on any program that ran counter to the judgment of the only Service Department that had a Research Division and had already considered the problem and decided against it.

It is in that light that, by way of a highly condensed published source, you will find it useful to examine the treatment of the origination of the Project by Mr. N. S. Finney in the article that was published in Look for March 14, 1950. Because during the preparatory phases of the article I was preoccupied as an expert witness for a very important corporation, I placed at Mr. Finney's disposal my preserved documentary records and let him carry on on his own that historical reconstruction which the other writers on the subject appear to me to have failed to effect. Parenthetically, it should be noted that the discovery of how accruing history in crucial situations actually emerges, or history in my designated sense of historification, is not only more difficult than conventional history, but requires in the researcher qualities of mind and discernments that are rather different from conventional labors of historians. For the conventional historian already has an accumulation of previously precipitated accounts and strives merely by way of refinements to reinterpret and re-evaluate opinions regarding some episodes and the roles of certain historical figures. But what I call the historification of historification calls for skills analogous, say, to those of the high-grade original experimental scientists. And when it comes to issues and decisions of an epoch-making nature in evolving history, such labor of historianation calls for the type of attunement to significance that, let us say, a searcher for oil in a new field, or in an old field that has been passed up, has to bring to bear.

Now returning to the Finney account that has done justice to the realities, you will note from the enclosed reproduction of the Look article that on page 26 he reports my having attended those lectures that had been given at Cambridge in the earlier years by Lord Rutherford and F. W. Aston, as had indeed been stated by me in that opening presentation to the Hearings of the Senate Committee on Atomic Energy in late-1945. He further noted that at the beginning of 1939 there came to me the knowledge of the Hahn-Strassmann Report of their historic success with atomic fission. He also notes that I had then acquainted President Roosevelt with those developments and ventured to evoke the decisive political import and challenge. It was as to whether Germany would, through continuance of utilization of the results of the Hahn-Strassmann atomic fission, attempt "to terrorize the democratic world with an atomic devastator." Then referring to the emerged preoccupation of the President over the imminence of war, his account refers to the linkage that was formed with me by certain atomic scientists who wanted aid for the continuance of their experiments, yet thought on their own about the danger of Nazi leadership in the research. In furtherance of activities already embarked upon by me in the role of international advice, I awaited the opportunity to undertake the surmounting of the obstacles that had been interposed, as previously described by me herein. In anticipation of conferences with the President I assembled a dossier, and part of that dossier was a letter by Dr. Einstein. Mr. Finney refers to the Smyth Report as his basis for describing how I had "enlisted Dr. Einstein to contribute a supporting letter to the dossier of material" that I had later used.

In prior years certain academic institutions sought to arrange for lectures by me and certain publishers took up the question of a book by me for recapturing the depth of the true history. Only that way can the full and accurate account be brought forth. Yet though I started out in this letter to postpone the task to another occasion, I have in the course of trying to condense the reconstructed true situation for you provided, I trust, foreshadowings of that prospective full account. Saliently, new concepts and new action-patterns had to be evoked by me, submitted to the President, and synthesized with his reactions. The President would not, and indeed could not, in the emergence of crisis in America's defense after the outbreak of war, ^{war} gone against the decisions of the Services and subjected them to denigration. So the Project had to be reoriented to strategy in high national policy. The two foci of that reorientation were the taking of insurance against and prevention of the potential threat of terrorization by Nazi Germany with new technological weapons. That would have been in continuance of the political technique of subjection that Nazi Germany did employ successfully on the European Continent in forcing acquiescence to its aims by the smaller nations and later, in combination with Panzer surprises, reducing the political orders of Belgium and France to becoming collaborationists. The second, and in a sense most distinctive part of my contribution, was to convert

President Roosevelt to the opportunity of the utilization of research on the Atomic Project in conjunction with the infusion of science and technology with our imminent defense program. A reflection of the second and longer-term purpose accounts for my inclusion in my letter of October 11th of ideas about the uses of atomic energy in civilian economics.

In the conjoint pursuit of the two purposes the preliminary inquiries on the advisability of Government support of atomic research were, at my suggestion, carried out by the Bureau of Standards. Then considering the unavailability by the late-spring of 1940 of any proofs from the researches at Columbia of the feasibility of chain reaction, I wrote the President on May 11, 1940 regarding a new mode for fostering the research. Then on May 15, 1940, I proposed the "establishment of a Scientific Council of National Defense, composed of executives, engineers, and economists, acting in behalf of the Government, who should be invested with administrative powers for the testing and execution of technical projects of utility for national defense." It is that new organ that came to be accepted by the President and enacted on June 15th. That new organ, which was designated as "Office of Scientific Research and Development" was headed up by Drs. J. B. Conant and V. Bush. So the Briggs Committee, which never made a definitive affirmative report, was superseded by the new organ. Culminatively, that new organ, after we entered the war, - again in concordance with certain exchanges with me by the President - turned over the Project to the Army under your distinguished direction.

With high regard,

Sincerely yours,

General Leslie R. Groves
Dellwood Road
Darien
Connecticut

uadri - science, inc.

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July 30, 1964

REGISTERED

Dear Mr. Sachs:

Thank you very much for your letter of July 21 and the very useful material enclosed with it. I am returning the same herewith as Registered Mail.

In writing the New York Times story, I have relied predominantly upon the documents of early vintage. However, I am still researching the matter of the Einstein letter and its origin. I shall treat this in more detail in Chapter 1 of my new book which Harper & Rowe will publish this Winter.

Again thank you very much for your courtesy in supplying me with information and for the pleasure of your several telephone calls.

Sincerely yours,

Ralph E. Lapp

Dr. Alexander Sachs
25 Broadway
New York, N.Y. 10004

Enclosure

THE RADCLIFFE INSTITUTE FOR INDEPENDENT STUDY
78 MOUNT AUBURN STREET
CAMBRIDGE 38, MASSACHUSETTS

October 28, 1964

Mr. Alexander Sachs
Apartment 14a
1200 Fifth Avenue
New York City

Dear Mr. Sachs:

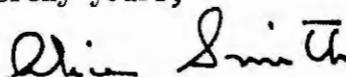
I have recently completed a book about the political and educational activities of American scientists from 1945 to 1947 which the University of Chicago Press will publish this winter. In it I should like to quote from your memorandum of autumn, 1944, to President Roosevelt as described by Nat S. Finney in Look for March 14, 1950. The publishers tell me that permission in the case of Look articles should be obtained from the individual whose words are quoted, and so I am applying directly to you although I am not sure that it is necessary to bother you about it. I enclose three pages of manuscript so that you can see the context.

I owe your address to a pleasant encounter with your niece, Mrs. Katz, at whose house I was speaking to a Mount Holyoke group about the scientists' movement. I have also known Lillian Randall as a fellow scholar here at the Radcliffe Institute.

May I venture to express the hope that you are somewhere recording in detail your part in the scientists' communication with Roosevelt or what may well have been your part in developing in the first place some of the ideas they expressed. It is a story that I should be delighted to have a share in telling. I became interested in this when writing an article, "Behind the Decision to Use the Atomic Bomb," for the October, 1958, Bulletin of the Atomic Scientists, and "The Elusive Leo Szilard," for the May, 1960, issue of Harpers. My husband was in charge of metallurgy at Los Alamos during the war, and I was assistant editor of the Bulletin in its early years, so I know well many of the people involved in these events.

I hope it will be in order for me to quote your memorandum.

Sincerely yours,



(Mrs. Cyril S.) Alice Kimball Smith

The economist Alexander Sachs who had taken the Einstein letter to Roosevelt in 1939, later reconstructed a memorandum which he had read to the President just before the November, 1944, election recommending that: "Following a successful test, there should be arranged (a) a rehearsal demonstration before a body including internationally recognized scientists from all Allied countries and, in addition, neutral countries, supplemented by representatives of the major (religious) faiths; (b) that a report on the nature and portent of the atomic weapon be prepared by the scientists and other representative figures; (c) that, thereafter, a warning be issued by the United States and its allies in the Project to our major enemies in the war, Germany and Japan, that atomic bombing would be applied to a selected area within a designated time limit for the evacuation of human and animal life, and, finally (d) in the wake of such realization of the efficacy of atomic bombing, an ultimatum demand for immediate surrender by the enemies be issued, in the certainty that failure to comply would subject their countries and peoples to atomic annihilation."*

 Quoted by Nat S. Finney, "How FDR Planned to Use the A-Bomb," Look, Vol. 14 (Mar. 14, 1950), p. 24

Sachs claimed that Roosevelt was favorably impressed.

One can only speculate about possible connections between Sachs' recommendation, the suggestion made by Bush and Conant a month earlier, and the talk of a demonstration which became common at the Met Lab, according to some recollections, very early in 1945. No written link is likely to appear, for this kind of thing had to be discussed with the utmost discretion.

As interest at the Met Lab focussed on international problems Leo Szilard, whose influence may well have sparked some of the earlier agitation, emerged as a leading figure. Szilard was a Hungarian physicist who during the 1930's

were doing enormous damage to Japanese cities, it was assumed that the war would continue in the Pacific until Japan itself was invaded and that the cost to both sides would be tremendous. Left with what had for many of them been a secondary reason for working on the bomb--bringing the war to a speedier end--an increasing number of scientists at the Met Lab now asked whether it could not equally well serve this purpose if it were demonstrated to the Japanese leaders in some uninhabited area. If this did not induce surrender, another bomb could then be used on a Japanese city. They insisted that they raised this question in the context of its bearing on the establishment of a peaceful world, not as an isolated moral or political issue, but in a small circle at least it overshadowed other aspects of the subject during the final two months of the war.

The origin of the demonstration idea is obscure. In their memorandum to Stimson of September 30, 1944, Bush and Conant had suggested that the appropriate time to disclose all but the details of bomb manufacture would be immediately after its first demonstration. "This demonstration," they had said, "might be over enemy territory or in our own country, with subsequent notice to Japan that the materials would be used against the Japanese mainland unless surrender was forthcoming."*

*Quoted in The New World, p. 329

Bush and Conant later changed their minds about the feasibility of a non-military demonstration, though Conant in particular tried to leave the way open for scientists with different views to present them to the Interim Committee.

[Covering - Sm]
[Chron 11/64]

76

(dictated) November 24, 1964

Dear Mrs. Smith:

Having been under what is, even for myself, exceptional pressure, and being now on the eve of a flying trip tomorrow morning to Europe, I cannot in the interstices between urgencies do justice to your inquiry.

It would be easy to say that since Mr. Finney's article has been published you may quote from it. Indeed, I will take no umbrage at your doing so. But you clearly are a reflective and serious researcher who desires to get at the truth of what thus far has not yet been historicized. The ordinary historian has a body of not merely received accounts, but successively processed and revised accounts of the past. What gets originated afresh or, in my terminology, is 'accrued new historification' needs to be treated as saplings taken from a nursery bed. If the roots be detached and the earth removed, they cannot flourish in the bed in which they are replanted.

Applying that to the specific inquiry, the Bush and Conant suggestion and the later Met Lab "talk of a demonstration" had no linkage to policy-making. For none of those persons were advisers to Roosevelt. The problem of the use of the bomb was in the context of co-discussions with President Roosevelt of high strategy and high policy. For this purpose I am submitting for your confidential reading a document in late-October-early-November 1944, entitled "Final Phase European War and Emerging Opportunity for Liquidating Far Eastern War." The reading of that will bear out the distinction just made. For further confirmation you can turn to Henry Stimson's autobiographical book regarding the determination of the armed services to embark upon the invasion of Japan, fixed for the late-summer of 1945. Neither of the groups mentioned by you was aware of the great and grave choices that had to be made and were made by the President. Only Stimson and Forrestal were aware that the President overrode the admirals and generals and even the civilian heads of the services. Mere intimations of the rationale of that choice are conveyed in the admirable article by Mr. Finney. Incidentally, it is erroneous to designate the memorandum, as you do on page I-41, as a 'reconstruction', because the successor to the late Mr. Stimson, Secretary Patterson, did examine the memorandum, and quotations from it were made by me in a letter to him, which is preserved.

Assuming that you will need the ensuing week or two for close study of the document enclosed herewith, I will be available for discussion with you, in person or in correspondence, as to the required

re-writing of your draft. I am the eagerer to be of service to you in view of your mentioned relationship to my wife's nieces.

With kind regards,

Sincerely yours,

Mrs. Cyril S. Smith
The Radcliffe Institute for Independent Study
78 Mount Auburn Street
Cambridge 38
Massachusetts