"BOOK GHS 1960"

ROUGH DRAFT OUTLINE June 14th. 1960. 1

Childhood (1000 words) 1898–1908

1. Very often it is ~~difficult~~ difficult to know where once a set of values come from but I have no difficulty in tracing ~~my set of values~~ mine to the children's tales which my Mother used to tell me. My addiction to the truth is traceable to these tales and so is my ~~pre-occupation with~~ predilection for ~~the~~ "Saving of the World".

Adolescence (1500 words) 1908–1914

2. Apart from my Mother's tales the most serious influence on my life came from a book which I read when I was 10 years old. It was a Hungarian classic, ~~which in Hungary is~~ taught in the schools, "The Tragedy of Man". I read it much too prematurely and it had a great influence on me, perhaps just because I read it prematurely. Because I read it, I grasped early in life that "it is not necessary to succeed in order to persevere".

The first world war (3000 words) 1914–1920

3. As far as I can see I was born a scientist. I believe that ~~very~~ many children are born with an inquisitive mind, the mind of a scientist, and I assume that I became a scientist because in some ways I remained a child. The set of values of the society in which I lived in Budapest was conducive for a young man to dedicate himself to the pursuit of science and the poor quality of the teaching of science at the universities in Hungary furnished stimulation to independence of thought and originality.

I was 16 years old when the first world war broke out and I rebelled against it

~~1920~~ Berlin 1920–1933 (3000 words)

4. ~~Anyone~~ No one in his right senses in Hungary, no matter how much he was interested in physics, would major in physics in Hungary. I myself majored in electrical engineering but when after the First World War I went to Berlin to continue my studies, the attraction of physics became so great that I dropped my studies of engineering and set out to obtain a Doctor's Degree in physics at the University of Berlin. I very nearly did not make it, but then, suddenly, my work began to turn out very well. I got my degree and I was regarded as a yong man of great promise by ~~the man whom~~ those whose opinion I valued most highly. In the 1920's physics was the king of the sciences and Berlin was a great centre of Physics.

B.P. Ever since I was 13 I was interested in physics and in public affairs but I kept these two things sealed in water-tight compartments and it never occured to me that

these two interests of mine would ever meet. Because of my interest in public affairs, Nazisism in Germany did not come as a surprise to me. In 1933 when Hitler took office I kept two suitcases packed in my room in the Harnack House, the Faculty clubhouse of the Kaizer Wilhelm Institutes and after the Reichstag was put on fire I ~~xxxx~~ picked them up and I took a train to Vienna. There I tried to appraise what may be in store for those ~~xxxxxxx~~ scientists and scholars on the staff of German universities who would be unacceptable to the Hitler Government. Some organization ought to be set up abroad so I thought, preferably in England, which would undertake to find positions for those who will be forced to resign from their university positions in Germany. A chance encounter with Sir William ~~xxxxxx~~ Beveridge was instrumental in the setting up of such an organization in London and it also landed me in London in the late spring of 1933.

~~1933 - 1938~~ England 1933 - 1938 (10000 words)

6. One of the last books which I read before I left Berlin was "The World Set Free" by H.G. Wells. This book, which was published in 1933, predicted the liberation of atomic energy on an industrial scale and the development of the atomic bomb. It did not occur to me when I read it that I was reading a prophesy that might come true but while strolling through the streets of London in the Fall of 1933 I was pondering upon the statement made by Lord Rutherford who said that "he who talks about the large scale liberation of atomic energy is talking moonshine", it ~~It~~ suddenly occurred to me how in certain circumstances it might become possible to set up a nuclear chain reaction, liberate energy on an industrial scale and construct atomic bombs. The thought that this might be, in fact, possible became a sort of obsession with me. It lead me to go into nuclear physics, a field in which I have not worked before and the thought stayed with me even though my first hunches in this regard turned out to be wrong. When the German troops moved into the Rhineland and England advised France against invoking the Locarno Pact, I knew that there would be war in Europe and I came to America at the end

of 1937 under an arrangement which permitted me to divide my time between America and Europe.

America 1938 - 1945 (60,000 words)

7. During the Munich crisis I happened to be in Urbana, Illinois and I spent one week listening to the news over the radio. When it was all over I was convinced that there would be war within a year and I resigned my position at Oxford.

America 1945 - Korean war (10000 words)

America Korean War to Pugwash (2000 words)

America after Pugwash (10000 words)

OUTLINE (Continued) 6/22/60

7. AMERICA(1938 through November, 1940) /20,000 words/

During the Munich Crisis, I happened to be in Urbana, Illinois, and I spent one week listening to the news over the radio. When it was all over, I was convinced that within one year there would be war. I resigned my position at Oxford.

In January, 1939, I learned of the discovery of the fission of uranium by Otto Hahn. I saw at once that if neutrons are emitted in the fission process it might be possible to set up a chain reaction. "H.G. Wells, here we come!" I said to myself. Immediately I was obsessed with two thoughts: To do, as quickly as possible, an experiment to discover whether or not neutrons are in fact emitted in the fission process, and to contact those laboratories in America, England and France where such an experiment conceivably could be thought of, and performed. This I thought had to be done with a view to reaching an agreement that if neutrons were in fact emitted, this fact should remain a secret of the three countries involved, lest the Germans developed the atomic bomb first and used it in the impending war. I was not affiliated at the time with any university, but after scouting around I borrowed $2,000.00, rented a radio, and teamed up with Dr. Walter Zinn, at that time instructor at City College. The experiment was actually set up at the Physics Department of Columbia University, and was performed on March 3, 1939. When I saw the neutrons emitted in the fission of uranium, I knew that the world was headed for trouble.

At that point I thought quite mistakenly that from this point on there should be no difficulty about obtaining financial support for this work. But in this I was quite mistaken.

My attempt to keep the neutron emission of uranium secret ran into difficulties, and it collapsed when Joliot in Paris published his results and declined to cooperate. The circumstances surrounding this collapse are not without human interest.

Fermi and I teamed up and performed an experiment which we thought would show that a chain reaction could be maintained in a system composed of water and uranium. We actually thought that we had shown this when the experiment was completed, but then George Placzek dropped in for a visit and showed us that we were in error.

In July, when I was left alone in New York, I recognized that a graphite uranium system would have a much better chance to sustain a chain reaction, and that accordingly the liberation of nuclear energy on an industrial scale was at hand. My first concern was to warn the Belgian Government of this possibility, lest being unaware of it, they make available uranium to Germany from the Belgian Congo. It was this consideration which brought me into contact with Einstein, a contact which resulted in Einstein's historic letter to President Roosevelt.

6/23/60

In response to Einstein's letter, the President appointed a committee which met for the first time on October 21, 1939. We did not look to the Federal Government for funds but rather for official recognition that would have enabled us to obtain private funds. During the meeting, through what might be described as a comedy of errors, the issue of funds came up and the committee promised to provide us with $6,000.00 in order to enable us to buy a few tons of graphite. By February, 1940, we had not heard anything further from the Government, and in the period from June 1939 to April 1940, not a single experiment was under way in the U.S. that was concerned with the possibility of setting up a chain reaction.

In February, 1940, I decided to take some drastic action. I sent a paper to the Physical Review, describing how a chain reaction may be set up in a graphite-uranium system. And I took a copy of this paper to Einstein in Princeton. Einstein wrote a letter saying that if the Government was not interested in pursuing this matter, my paper would be published in due course of time.

This provoked another meeting of the Uranium Committee at which I was asked to defer publication of my paper. In the mean time, the $6,000.00 promised to us were received by Columbia University, and some of the most urgent expenditures got under way, But nothing remotely resembling the scale that was needed.

At this point I received a letter from Turner in Princeton, who pointed out that in the chain reaction which I hoped to be able to set up there would be formed a new element which might be capable of undergoing fission. As we now know, this is in fact the case, and the element formed in the chain reaction is now called plutonium. Neither Fermi nor I had thought of this possibility, which was obviously of the utmost importance, and this realization increased my sense of urgency.

On Rabi's advice, I enlisted the help of H. C. Urey, who prevailed on the Chairman of the Uranium Committee to appoint those of us who were actively interested in this problem to serve as a technical subcommittee of the Uranium Committee. We thought this would put us in a position to approach various laboratories in the U.S. and to enlist their cooperation in pursuing the various aspects of the problem, including the possibility raised by Turner's suggestion.

The Committee, having been duly appointed, met in Washington and when the meeting was opened by the chairman, he told us that the committee would be dissolved upon termination of the current meeting, because if the government were to spend a substantial amount of money--we were discussing sums of the order of a half million dollars--and subsequently it would turn out that it is not possible to set up a chain reaction based on uranium, there might be a Congressional Investigation. If this were the case in such a situation, it would be awkward if the Government had made available funds on the recommendation of a committee whose membership comprised men other than American citizens of long standing. Fermi and I were not American citizens. Though Wigner was an American citizen, he was not one of long standing. Thus the work on uranium in the United States was brought to a standstill for the next six months. Mr. Wigner wrote a very polite letter to the Chairman of the Uranium Committee saying that he would hold himself in readiness to work for the Government on all matters related to defense, with the exception of uranium.

After reorganization in Washington, which put the Uranium Committee under Dr. Vannevar Bush's committee, Columbia University was given a contract in the amount of $40,000.00 to develop the Fermi-Szilard system. On November 1, 1940, I was put on the payroll of Columbia University under this contract. Since I was instrumental in inducing the Government to assume expenditures for exploring the possibility of setting up a chain reaction, and with a view to the possibility that our efforts might come to nothing, it was deemed advisable to set my salary at a low figure, i.e., $4,000.00 a year.

November 1940 to December 31, 1941

(5,000 words)

While up to this point we had suffered from the lack of official recognition, during this period we were suffering from having official recognition. H.C. Urey was under orders not to discuss with Fermi and myself the possibility of preparing substantial amounts of Uranium 235. Because of this compartmentalization, we failed to put two and two together, and at no time were we or any other physicist able to say to the American government that atomic bombs could be made with amounts of Uranium 235 which it was practicable to obtain. Thus our project and Urey's remained projects of low priority until the British colleagues, who were not so compartmentalized (hamstrung?), pointed out that making atomic bombs of Uranium 235 must be regarded as a practical proposition.

This led to a reorganization of the project and the group working at Columbia University was transferred to Chicago.

December 31, 1941-to December 2, 1942

(10,000 words)

A chain-reacting uranium-graphite system was put into operation in Chicago on December 2, 1942.

December 2, 1942-to August 6, 1945

(20,000 words)

A. The building of the Hanford Plant.

B. The fight against the use of the bomb.

August 6, 1945 to the Passing of the Atomic Energy Act

(20,000 words)

A. The scientists become vocal on the issue that the U.S. will not have a monopoly of the bomb for long.

B. Fight of the scientists for civilian control of atomic energy.

(total 72,000)