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J. Robert Oppenheimer: Niels Bohr and Atomic Weapons

Roger A. Meade, ed.

On May 18th, 1964, J. Robert Oppenheimer made his last visit to Los Alamos, highlighted by a talk on Niels Bohr before a full house at the community's civic auditorium and by an audio link to another full house sitting in the Laboratory's main auditorium. Although recorded, his talk was not transcribed. Later, Oppenheimer sent a transcript to the Laboratory, which is reproduced below. His talk, of course, deviated from time-to-time from this written version. Unfortunately, the transcript was microfilmed, the original disappearing. Hence, all of Oppenheimer's marginal notations and handwritten edits are unreadable. A poor quality recording is being converted to a modern format and will be available for listening.

Introduction by Norris Bradbury[1]

From the [early] grueling days of the atomic age, in the middle of the war, at the time when it was rapidly being discerned that an atomic bomb would be possible, a laboratory had to be established somewhere, and a man had to be picked to direct that laboratory. The place was called Project Y, and its address was a classified address, Box 1663, Santa Fe, New Mexico, because Los Alamos was a classified word. [The civilian] and military people who came to this Project could not reveal their location here. Their scientific journals sent to some other box in Los Angeles. And the director of that laboratory, chosen from among the outstanding citizens of this country, was kept deeply secret - Dr. Robert Oppenheimer, professor of physics at the University of California. In two and one half years, from the early part of 1943 and the middle of 1945, he put an extraordinarily diverse team of people together, people of capability, people of dedication, people of objective, but people of diverse personalities. And by sheer talent and sheer technical ability, by sheer force of personality and character made them into a team, which constructed the first nuclear device at Alamogordo, and which was shortly thereafter used in war. This man, known to all of you as Mr. Los Alamos, is again with us tonight. He is now Director of the Institute of Advanced Physics at Princeton. But I think to him, and to us, Los Alamos and he are inextricably intertwined. To me and to all of you it is the greatest of pleasure to welcome him to Los Alamos to speak to us tonight.

J. Robert Oppenheimer

Even in the early Manchester days, in 1912, Bohr was of course interested in the atomic nucleus, both for its own phenomena and for the implications of its existence for atomic structure; in the first months he was probably as concerned with nuclear problems as with atomic problems; but after that the very great engagement of understanding the quantum theory of the atom occupied

[1] Bradbury's introduction was transcribed from the audio recording. Also have made some minor edits to the transcript. Any errors are mine.

him almost fully. He had returned to Copenhagen in 1916; in June, two years later, Rutherford[2] wrote to him of the war, *"It is a long worm that has no turning.* "At the war's end, Rutherford wrote to tell about his success in making artificial nuclear transmutation. This is the historic reaction: $N^{14} + He^4 \rightarrow O^1 + H^1$: he bombarded nitrogen with alpha particles and produced long-range protons and oxygen 17. They wrote to one another of the end of the war, Bohr in a way which was rather melancholy-prophetic of what would happen a quarter of a century later: *"All of us here are convinced that there will never more be a war of such dimensions in Europe"*, and of *"a new era in history."* When Rutherford's paper on the transmutation of nitrogen was published in 1919, Bohr and Sommerfeld[3] pointed out that they were probably seeing a useful application of Einstein's relation between the change of mass and the release of energy.

After the quantum mechanics was developed in 1925, the greatest interest lay of course in its application to atomic and molecular structure, and in understanding what sort of description of nature it was. But there were two points, among others with which I had a little connection, and which Bohr liked very much. They had to do with nuclear puzzles, but they also illustrated new and paradoxical features of the quantum theory. One is very simple indeed. When Rutherford scattered alpha particles on nuclei, he could describe the results in terms of the electric repulsion of the nuclear charge. For very high energy particles and rather light nuclei, there would be anomalies because the forces of specific nuclear nature would give large angle scattering; but in the case of helium, there were anomalies which were not present at low energy, which occurred at somewhat higher energy, but which did not at all fit with any effect due to nuclear interaction. I did not then know anything about that; but in another connection was formulating the effect of the interference of the wave representing the scattered particle and the wave representing the target particle, the recoil article, because with the scattering of alpha particles in helium, the two are identical, and the waves do interfere. Mott[4] picked that up and showed that the anomaly in the scattering was completely explained by it.

An episode more important from the point of view of history had to do with the way in which alpha particles come of the nuclei, radio-active nuclei. That started in a very different way. In the winter 1926-27, I was looking at the Stark effect in hydrogen, the effect of an electric field on the stationary states of an electron in the simplest of all atoms. It was already known that the energy displacement, the splitting and shift of some levels, came out right; but there were not any stationary states. This is completely obvious once you pick up a pencil because an electric field produces a potential, V, that is simply a straight line with slope proportional to the field. And if you have a nucleus, the nucleus of a hydrogen atom, there will be a big dip in the potential corresponding to its attraction for the electron. If the electron is in a state with an energy E, then because of the complementarity between energy and location, there will be a small probability that the electron will be out of the atom, and once it is out there it will keep going, and it will escape. When the transmission through such a high potential hill is very low, it is easy to give a general formula for it. I did not know what use this would except for the auto-electric effect,

[2] Sir Ernest Rutherford: Nobel in Chemistry, 1908. Postulated the nuclear structure of the atom, discovered alpha and beta rays, and proposed the laws of radioactive decay.

[3] Arnold Sommerfeld: German theoretical physicist. Doctoral supervisor for Werner Heisenberg.

[4] Nevill Mott: Nobel in Physics, 1977.

which was then being studied by [Charles] Lauritsen and Millikan,[5] and where it fit. But in alpha-decay there had long been a paradox. If you have a heavy atom with atomic weight *A* and atomic number Z, and it undergoes alpha decay, then you get a nucleus with a reduced atomic weight, *A-4,* and a reduced atomic number, *Z-2*, and an alpha particle with some energy. If you now take these alpha particles and bombard this remaining nucleus, all you find is this strong electric repulsion of the nucleus. The alpha particle does not feel anything else. And the paradox was: how does it come out when it cannot get through? [George} Gamow knew about this, and [Ronald] Gurney also, and with [Edward] Condon they saw that one could explain alpha decay just as such a leakage through a potential obstacle, such a potential mountain, a typical wave effect of the alpha particle. These things had a light encouraging effect, because they made one confident that at least some aspects of the nuclear problem would be describable in terms of the, by then, rather familiar quantum mechanics. The reason the alpha particle shows no anomalous scattering is that the transmission of this mountain is very, very feeble. It varies with the energy of the alpha particle but is something like one in 10^{30} or one in 10^{40} impacts, and that is beyond any measurement of scattering.

This has some historical importance. On the one hand it led to the beginnings of a theory of energy generation in the sun and stars, a theory which was largely rounded out a decade later, but is still not quite finished; and it also gave courage to a number of people to build accelerators which would give protons in the hundred kilovolt region, which could presumably get through the potential barriers around nuclei, even if they could not get over them. This program succeeded. In 1932 there were far greater things. Rutherford, in April of that year, wrote another letter to Bohr. He said that he was convinced by Chadwick's[6] experiments that the neutron was really there; and even more excited, he said that the Cockcroft-Walton accelerating tube was working, and that even at 125 kilovolts one began to see that protons could transmutate lithium7 and produce two high energy particles. Rutherford even noticed that similar things happen at higher energies with target nuclei which had an even number of neutrons, and an equal number minus one of protons; and that they were the only ones. With this, one had accurate measurements and could confirm precisely the equivalence between the change of mass of the nuclei and of the energy liberated in the reaction. That same year the positron was discovered, and one had an even more overwhelming example of the change in energy into mass, because from nothing but electromagnetic radiation one got two electrons of opposite charge and from them one got nothing but electromagnetic radiation.

It was with the neutrons, and in the mid '30s, that Fermi began his justly famous experiments to see what happened when the neutrons were slowed down. Off hand, he thought that by putting material rich in hydrogen in the way of neutrons, he would let them lose energy easily and that they would lose their vigor. But he found in many substances, which he called water-sensitive, a great enhancement of the radioactivities produced and the nuclear reactions created. The typical reaction is that the nucleus captures the neutron and loses the excess energy in gamma radiation; if the product nucleus is radioactive, it is very easy to find out what is going on. The cross sections for these reactions were very large, not as large but of the order of the square of the wavelength of the slow neutron, in some cases, later discovered, a million or ten million times as

[5] Robert Millikan: Nobel in Physics, 1922.

[6] James Chadwick: Discoverer of the neutron; Nobel in Physics, 1935.

big as the nucleus itself; this is of course a typical wave effect, for which quantum theory had a place. But much more startling was the fact that different nuclear species showed at certain, well-defined energies, typically a little bit above thermal energy, an electron volt, twenty electron volts, absorption in a very narrow energy range, very sharp resonances, very well-defined energies, and sometimes several for a given nuclide. In many cases, the nuclide would have its characteristic spectrum.

Bohr did not need to look it up in a book. A sharply defined energy means a long-lasting state. Thus in 1937 Bohr wrote a paper, saying that the reason the states lasted so long was that the incoming neutron rapidly communicated its energy to the other nuclear particles; there are a lot of them; this dissipation is quick and effective; and it takes a long time either for radiation to occur or for the energy to concentrate back on one particle which could come out of the nucleus again. This is, of course, right, and it was immediately recognized as right; but we know, and you know better, that in many nuclear reactions with somewhat higher energy particles one has a series of stages in which the colliding particle only gradually gives up the ghost to all of the other particles in the nucleus.

The discovery of the radioactivity of uranium when bombarded by slow neutrons was naturally interpreted by Fermi in analogy with all other radioactivities. It was somewhat puzzling that one of the radiations came from something that appeared to be radium; as you know, [Otto] Hahn and [Fritz] Strassman took some care to make sure of this identification, which was a bit odd, and they found something even odder: they found it was not radium, but the chemically similar but distinguishable barium. Bohr brought this news to the United States, together with the suggestion, rather the conviction, of Otto Frisch in Copenhagen, and Lise Meitner in Stockholm, that the nucleus of uranium simply came apart into two not quite equal pieces, one of which sometimes was barium. Then immediately the fission fragments, which have a lot of energy, and their energies, were detected in many places; I happen to remember Columbia, Copenhagen, and Berkeley.

In addition to fission by slow neutrons, uranium absorbed as an epithermal energy with a sharp resonance, very much like all other elements, producing, as we now know, uranium 239 and, as a result of beta-decay, neptunium. Bohr had a look at that. He was clear that when fission was possible, radiative capture would not compete with it, gamma-ray emission would not compete with it. He was clear that the rare uranium isotope could not possibly have such a big capture cross-section at that energy, and the capture must be a characteristic feature of the abundant uranium 238, and not of the rare one; and that the rare uranium 235 was the one that fissioned with slow neutrons. This he published; it had of course the implication that 235 was a much more active candidate for fission chain reactions, that enrichment in 235 would increase the fission activity and that pure uranium 235 would presumably be a very much more active material. Bohr with [John] Wheeler made a systematic survey of what made an element, a nucleus, undergo fission, either with neutron bombardment, or spontaneously; spontaneous fission had been discovered by [Georgy] Flerov and [Konstantin] Petrzhak. From these arguments it would have been reasonable to conclude that uranium 235 and the nonexistent elements uranium 233 and plutonium 239 would probably come apart in fission with slow neutrons. But the last two did not exist, and the first was quite rare and hard to separate, formidably hard to separate, so that when Bohr left the United States for Copenhagen, he did not expect that explosive application of the

fission process lay very close at hand. Indeed, it was some time before one knew that enough neutrons were liberated to sustain a chain reaction, and it was not until 1943 and ’44 that one knew that in fast neutron fission the number of neutrons was adequate for a chain reaction, and the time delays involved in their emission small enough so that explosions could take place.

The home in Copenhagen to which Bohr returned was in a very different world, and it was in some ways a very different place, from the Copenhagen of the 1920s. For seven years, his Institute, and, to a moving extent, his home at Carlsberg, were a refuge, often temporary, often of longer duration, of course in the first instance for colleagues from Germany, and later from Austria. When Fermi came up to get his Nobel Prize, he went to Bohr instead of going back to Italy, and then to the United States. From Russia came Charlotte Houtermans, whose husband [Fritz] remained in jail until the Molotov-Ribbentrop Pact enabled him to be exchanged, God knows for whom, and [George] Placzek and [Victor] Weisskopf, and many others besides. Thus, Bohr had, in addition to his deep devotion to Denmark, which had kept him there when he was asked to go to England more than twenty years earlier, also a sense of responsibility for his wards, and for his Institute. The terrible years 1940-43 are, as far as I know, not publicly documented. The Institute itself was closed in ’40. Heisenberg and [Karl Friedrich] von Weizacker, and others as well, came to Copenhagen. Bohr had the impression that they came less to tell what they knew then to see if Bohr knew anything that they did not.

In ’43 the situation became acutely dangerous, and it was clear that Bohr could not stay long. He was in touch with the Danish underground, and through them by then with the British Secret Service; he had a letter from Chadwick saying that it would be good if Bohr could come to England. In the last days of September, he escaped in a small boat to Sweden. Three weeks later he was flown to England in the bomb bay of an unarmed Mosquito [bomber]. They gave him an oxygen mask, but the Royal Air Force has no such heads as Bohr’s, and it did not fit.[7]

When Bohr came to England, Chadwick shortly spoke to him of what was going on, told him of the fantastic enterprises - they looked fantastic then; they look very pedestrian today - that were under way in the United States, to separate uranium 235 by gaseous diffusion and by what were essentially mass spectrographs, and to build chain reactors, and separate plutonium chemically, these three processes to which later would be added a small liquid thermal diffusion process for enriching the U-235 in natural uranium. The English were very much involved in this. It had been raised as a possibility, the making of a bomb, by [Sir Rudolf] Peierls and [?] Simon, like the Americans refugees from tyranny in Europe; there was a committee called the Maud Committee, and a project the Tube-alloys Project. The British official conclusion was that this enterprise must be explored with all vigor even if it had, were to have, no effect whatever on the outcome of the war. It was too important for the world. They were concerned only with uranium 235, and they were concerned only with gaseous diffusion; and it very soon became clear that they did not have the resources, that things would go better if they worked in collaboration with the United States, in establishments that were in the United States, free of bombing and relatively well-off physically for those years.

The relations with the British went up and down. They were good in the beginning. We had some communication with Peierls and [Paul] Dirac on how a bomb would behave when it started

[7] Bohr passed out from lack of oxygen.

exploding. As the Manhattan District was established, things got a bit sticky. Then, in August of ’43 there was an agreement between Churchill and Roosevelt at Quebec, which provided for the participation of the British and the Canadians in the United States undertaking; for their sending missions to this country; for our sharing the political and military responsibility involved; and also for our sharing access to the indispensable uranium; and some recognition that we would talk about any industrial or commercial problems later on. This Quebec agreement had been signed when Bohr came to England. Chadwick was eager to have Bohr come the States. By that time Chadwick and Peierls had talked with us, and we understood that we had more or less the same views of more or less the same problems, and they thought that it would help very much in the weight of the British mission if Bohr would join it, and he agreed to do so. Bohr then also saw Sir John Anderson, later Lord Waverly, a conservative, dour, remarkably sweet man who was very congenial in spirit to Bohr, and who was a good friend to him. He was Chancellor of the Exchequer, and was in charge, perhaps through military intelligence, of the Tube-alloys Project. He asked Bohr’s help in maintaining and improving the position of the United Kingdom in the enterprise, as well as helping the enterprise to success.

By then Bohr had had his first good look. I will be reading you short passages from things that he said or wrote, and you will hear his own words; but I think that it is best if I rather baldly tell you what points he had in mind at the beginning, and for a long time. I run the risk of oversimplifying them by so doing, but I do so because it is easy, as history has well shown, for even very wise men not to know what Bohr was talking about. This fell on him really in a great heap, just as the whole atomic picture fell on him more than thirty years earlier.

First of all, he was clear that this was going to be an enormous change in the situation of the world, and in the tolerability of war. The word “menace,” the word “threat,” occur over and over again. When he come to Los Alamos, his first serious question was, is it really big enough? It did finally get to be.

The second point was that he knew enough of the Soviet situation to be quite confident that the wartime alliance would not endure the peace, as things stood. He spoke a great deal of different economic and social systems, and it is possible that he had India and Africa and China in mind; but it was overwhelmingly clear that he had Russia in mind. He therefore anticipated an unheard of arms race, unheard of before then, though not now, for the great weapons. He knew something about the possibility of thermonuclear amplification and wrote about it discreetly to Roosevelt and Anderson and Churchill. He expected, perhaps rather more than has, in fact, occurred, that what looked so formidable in 1943 would not look so hard in 1953 or ’63, in the way of getting materials and assembling them; he thought that it was necessary to start then to try to prevent this arms race. He was clear that one could not have an effective control of what was then called atomic energy, which would permit some useful application, and a free scientific spirit and enterprise, without a very open world. And he made this quite absolute. He thought that one would have to have privacy, and some respect for individual rights and the quiet processes of government and management, but in principle everything that might cause a threat to the security of the world would have to be open to the world.

He knew that this would not be quite in character for the Soviet Union, that it would be very hard for them. He therefore felt that it was essential to attempt to engage the Soviet Government by

very early consultation, by consultation as an ally that had been invaded and occupied with a desperate defensive war; to regard the whole enterprise as a common problem of cooperation for the Russians, the English, the Americans, and all the other allies, and to be quite prepared to offer full cooperation in scientific progress and in industrial exploitation in an open world. And he hoped that the situation in which the Russians would find themselves, the advantages that we might have to offer, the opportunity of associating themselves with a great, forward-looking change in the world, might alter the whole character of Soviet policy and of international relations, so that in an essential and major way force would cease to play a part, and nations would exert an influence by their example, their persuasion, and the extent to which they could contribute to the common welfare. He was looking at one of those examples of complementarity of which he wrote as a youth: the complementarity between love and justice. He spoke of all this, and surely more eloquently but perhaps not quite so precisely, to Anderson. Just a few months before his death, Waverly told me that he had never been reconciled to the fact that Bohr's view had not prevailed, nor his counsel followed.

Late in 1943 Bohr came to the United States, publicly to advance the cause of international collaboration after the war, officially to strengthen and help the enterprise, secretly asked by the British to strengthen their position, but above all, and most secretly of all, to advance, with Anderson's concurrence and blessing, his case and his cause. When he arrived late in '43, he saw the Ambassador of the United Kingdom, Lord Halifax, and his own Ambassador, de Kauffmann, who with incredible bravery and gallantry represented his nonexistent government in the conduct of the war. Through him he met Justice [Felix] Frankfurter again, who had heard of the atomic undertaking, and listened to Bohr with sympathy and deep respect. And then Bohr came with his son, Aage, who was his companion, his confidant, everything else all through these years, to Los Alamos.

I will only tell one short episode, which is not relevant, except to those of you who have tried to listen to Bohr. Groves, the commanding general of the Manhattan District, brought Bohr out by train. He thought, rather more than was true, that Bohr might know something of what the Germans were up to. And when they came out, Groves very considerately left Bohr and his son alone at our place for supper. The next morning, I saw Groves walking towards the laboratory, very stiff and limping; I asked him what had happened to him. And he said, *"I have been listening to Bohr."*

Bohr at Los Alamos was marvelous. He took a lively technical interest; we went back again over some things that the Germans might be doing, that would not have worked. We were all concerned about that. One time when there was a dispute about how to make the little neutron source that is supposed to start the chain reaction in an implosion bomb, Bohr explained that there were no difficulties in principle: you could not make it wrong. It would indeed have been very easy to make wrong, and it was Dr. [Richard] Dodson that managed to make it right; but in principle, as Bohr said, you could not make it wrong. But his real function was, at least for many of us, a very different one; he made the enterprise, which often looked so macabre, seem hopeful; he spoke with contempt of Hitler who with a few hundred tanks and planes had hoped to enslave Europe. He said nothing like that would ever happen again; and his own high hope that the outcome would be good, and that in this the role of objectivity, friendliness, cooperation,

incarnate in science, would play a helpful part; all this was something that we wished very much to believe.

Early in 1944 Justice Frankfurter talked to Roosevelt about Bohr's ideas, and the President listened with great interest and with a word of encouragement. And at the same time, John Anderson was talking to the Prime Minister, trying to see whether he would not open the subject up a little within the British Government so that they might look at what to do. Churchill was not very pleased with that. So, Bohr went back over in April, this is '44, with a word for Anderson of Roosevelt's interest. There he was given a letter from Kapitsa,[8] asking him whether he would not like to come to Russia. Bohr concluded that the Russians were interested in the nuclear prospects, and was cautious but very friendly and hopeful in his reply. He saw Anderson again, and Sir Henry Dale, who was the President of the Royal Society, and Cherwell,[9] who was Churchill's scientific adviser; at Churchill's suggestion they all talked to [South African Field Marshall Jan Christian] Smuts, and the group more or less agreed that when next Churchill and Roosevelt met, they had better talk a little about the future. Cherwell and Churchill met with Bohr, and this must have been a very unhappy affair. Cherwell did not prepare Churchill, for he did not have the reverence for Bohr which might have made it worth one great man's time to listen to another; and it was a very unhappy meeting in which Bohr could hardly talk, and that was not something to make him happy at any time, least of all this.

Bohr came back here, and came out to New Mexico again. And then late in August, after he had prepared a memorandum which Justice Frankfurter showed to Roosevelt, he met the President, and they had a long talk. I know that afterwards Bohr was enormously encouraged. He has never quoted anything that the President said, or anybody else; all he has ever quoted were his own words. I would like to read you just the last three paragraphs from this first memorandum; and you will see how Bohr put these things, and I hope see more in them than in any possible transcription I could make.

> *Indeed, it would appear that only when the question is taken up among the united nations of what concessions the various powers are prepared to make as their contribution to an adequate control arrangement, it will be possible for anyone of the partners to assure themselves of the sincerity of the intentions of the others.*
>
> *Of course, the responsible statesmen alone can have the insight in the actual political possibilities. It would, however, seem most fortunate that the expectations for a future harmonious international co-operation which have found unanimous expression from all sides within the united nations, so remarkably correspond to the unique opportunities which, unknown to the public, have been created by the advancement of science.*

[8] Pyotr Kapitsa: Nobel in Physics, 1978.

[9] Frederick Lindemann, 1st Viscount Cherwell.

Many reasons, indeed, would seem to justify the conviction that an approach with the object of establishing common security from ominous menaces without excluding any nation from participating in the promising industrial development which the accomplishment of the project entails will be welcomed, and will be responded to by a loyal co-operation on the enforcement of the necessary far reaching control measures.

Then Bohr wrote, after his visit with Roosevelt, a supplementary note, which may have had unhappy consequences, pointing out how close the relations between members of the scientific community had been, and saying that although a statesman must decide, and make proposals, and act, perhaps scientists who had known each other could help prepare the ground. In September, Churchill came over, and he and Roosevelt met at Quebec; they saved discussion of atomic problems until they met at Hyde Park. Of this discussion there is an aide memoire, initialed by both men. They reached three conclusions. They were based apparently on a substantial if not total misunderstanding of what Bohr was after; the first conclusion was that Bohr's suggestion that the world be told about the development be rejected. Bohr did not want to tell the world about anything; he thought it was important for someone bearing Roosevelt's authority to talk to Stalin, or - if he existed - someone bearing Stalin's authority, about the future, and the need for common responsibility and an open world. Only if everyone could agree, and the whole thing worked out, and there was, in fact, such a thing as an atomic bomb, would one then think that one would tell the world what could come of it. But they harshly rejected this approach and said the highest secrecy should be maintained. It was of course being tried, but it was not working too well. They said in the second place, that when the bombs were ready, then after mature deliberation, they might be used against Japan. And they said third, that they would like to have a very careful watch on Bohr; they did not trust him.

This outcome was not funny; it was terrible. For one thing it shows how very wise men dealing with very great men can be very wrong. It worked itself out; it was rectified because the English were sure this was all nonsense, and Bohr was, as you might say, cleared. But the fact is that it stopped, it ended his communication with the President, and it very seriously impeded his communication with our government.

In March of 1945, many months later, Bohr wrote another memorandum. By then the dates for the coming of the bombs, which were at last almost entirely determined by production schedules, were known; and the United Nations were to have their first meeting in San Francisco. Bohr had a great sense of urgency that the question of the atom and the bomb not be let go too long. I shall read you one more passage, which is the end of that memorandum of March '45. It is a little long, and I do not apologize for it.

It would seem most fortunate that the measures demanded for coping with the new situation, brought about by the advance of science and confronting mankind at a crucial moment of world affairs, fit in so well with the expectations for at future intimate international co-operation which have found unanimous expression from all sides within the nations united against aggression.

Moreover, the very novelty of the situation should offer a unique opportunity of appealing to an unprejudiced attitude, and it would even appear that an understanding about this vital matter might contribute most favourably towards the settlement of other problems where history and traditions have fostered divergent viewpoints.

With regard to such wider prospects, it would in particular seem that the free access to information, necessary for common security, should have far-reaching effects in removing obstacles barring mutual knowledge about spiritual and material aspects of life in the various countries, without which respect and goodwill between nations can hardly endure.

Participation in a development, largely initiated by international scientific collaboration and involving immense potentialities as regards human welfare, would also reinforce the intimate bonds which were created in the years before the war between scientists of different nations. In the present situation these bonds may prove especially helpful in connection with the deliberations of the respective governments and the establishment of the control.

In preliminary consultations between the governments with the primary purpose of inspiring confidence and relieving disquietude, it should be necessary only to bring up the problem of what the attitude of each partner would be if the prospects opened up by the progress of physical science, which in outline are common knowledge, should be realized to an extent which would necessitate exceptional action.

In all the circumstances it would seem that an understanding could hardly fail to result, when the partners have had a respite for considering the consequences of a refusal to accept the invitation to co-operate and convincing themselves of the advantages of an arrangement guaranteeing common security without excluding anyone from participation in the promising utilization of the new sources of material prosperity.

All such opportunities may, however, be forfeited if an initiative is not taken while the matter can be raised in a spirit of friendly advice. In fact, a postponement to await further developments might, especially if preparations for competitive efforts in the meantime have reached an advanced stage, give the approach the appearance of an attempt at coercion in which no great nation can be expected to acquiesce.

Indeed, it need hardly be stressed how fortunate in every respect it would be if, at the same time as the world will know of the formidable destructive power which has come into human hands, it could be told

> *that the great scientific and technical advance has been helpful in creating a solid foundation for a future peaceful co-operation between nations*.

I do not know whether Roosevelt ever read that memorandum. He died very shortly thereafter. When he died, he was writing a speech, since published but never delivered on the new powers of science in war, and the need to take thought; indeed, he said, the need for *a "new science of human relations."* The hour that Roosevelt died, Lord Halifax and Justice Frankfurter were walking in Lafayette Park, by the White House, talking of the bomb and of Bohr's ideas.

With Roosevelt's death, Bohr's memoranda were given to [Henry] Stimson, the Secretary of War. Shortly thereafter Stimson appointed a committee of which Karl Compton, [Vannevar] Bush, and [James] Conant were the technical members, in which State, War, Navy, and the Office of the President were represented, called the Interim Committee, to think about the future.

In a sense, of course, Bohr was not alone at all. Bush and Compton and Conant were clear that the only future they could envisage was one in which this affair would be internationally controlled. Stimson understood this, and that the central problem lay in the relations with Russia. The authors of the Frank Report were clear that this was the only course of hope. The scientists who banded together after the war, and who were saved from God-knows what, and saved for good things, by Willy Higginbotham, they also understood that the international control of atomic energy was the only right thing for atomic energy. And many of you and many others were clear about this. But there was a difference: Bohr was for action, and for timely action. He realized that it had to be taken with those who had the power to commit and to act; he wanted to change the framework in which this problem would appear early enough so that the problem itself would be altered. He was for statesmen; he used the word over and over again; he was not for committees, and the Interim Committee was a committee. It appointed a scientific panel, or rather Stimson did; we were Arthur Compton, Fermi, Lawrence, and me. We met with them on the 31st of May, we talked just about the question of relations with Russia and the future of the bomb and of atomic energy and of science. I hear that other people talked about the use of the bomb at that time, but this was not in committee sessions. I was deeply impressed with the wisdom of General [George] Marshall, and of Secretary Stimson; I went over to the British Mission and tried to comfort Bohr; but he was much too wise and he would not be comforted; and he shortly left for England uncertain of what would happen.

The scientific panel had another occasion to answer a question other than that that was put to it. We recommend that before the use of the bomb, or a firm decision on its use, our government talk of the future to our allies. On the 21st of June the Interim Committee agreed that this was the right thing to do, plausibly at Potsdam.

We were planning, and managed it, to set off our first test, which was technically something that had to be done, in time so that there would be a little indication of how things were when the President and the Secretary of War reached Potsdam. The bomb worked, but there was no talk with the Russians. Some of you may have read the history of it. Stimson was horrified when he saw what sort of thing the Red Army was; perhaps, as he says, he lost his nerve; [Secretary of State James] Byrnes was against it entirely. Churchill was against talking about it in any serious

way; but they all agreed that if the President said something to Stalin about it, and used the Trinity explosion as the occasion, it would at least relieve us of the worst reproaches for secrecy and double-dealing. When the news came in from New Mexico, rather more lurid then than it would seem now, the President dismissed his interpreter, Charles Bohlen, in order to keep things casual, went over to Stalin, who did not have his interpreter Pavlov, but another, and remarked that we had a new weapon which was quite powerful and which we were thinking of using against Japan. According to Truman, and there is no one else to give an account of it, Stalin said he wished us luck, and hoped it would work.

With the use of the bombs, which raised other and not entirely separate, but rather separate, questions, there were vague and slow pronouncements about international control; but it was not until very late in 1945, when the English, Canadians, and Americans had agreed to seek some action, and when the debate on legislation was in full course in the United States, that Secretary Byrnes agreed to bring the matter up with the Russians. He had in mind simply asking them whether they would approve the creation of a commission in the United Nations to talk about it. He was rather afraid that they would ask him how to make a bomb. But they were much less eager to talk about it than he was, and nothing whatever was said. They agreed to make a commission.

Thus, in January 1946, the Secretary appointed a committee of five, under the chairmanship of the Under-Secretary, Mr. [Dean] Acheson, to devise controls. A few days later, the Under-Secretary appointed a panel under the chairmanship of [David] Lilienthal to devise what was to be controlled. We sat for two months - it should not have taken that long - thinking of what things really needed to be worried about in atomic energy, what could be left free, how they could be related to each other, how one would deal with new discoveries, how international effort would be required to explore things, and so on. It was a committee document, but for a committee document, and for the times, not too bad. But Bohr was not happy with it; it was not centered enough on the absolutely central theme of openness. I may say that when this became part of the United States position at the United Nations, our military staff officers said that if this were to be open, then everything would be open; there would be no secret military installations, or it would not work. So, in a way Bohr would have had his openness if it had come into being. But Bohr said further, and this true, and a very true reproach, *"It calls for action. It was an action to make the bomb."*

Bohr did not quite abandon hope, although it was clear that the idea of involving the Russians *ab initio* as collaborators, allies, and guarantors of the peace, had been lost. But he still felt that it was a great cause to do away with barriers to information, a great cause to model the cognitive state of the world, whether it had to do with technical things or economic, or political, or cultural on the world of science. He talked in '46 to the Acting Secretary of State, and then in 1948, through [Assistant Secretary of War John] McCloy, he had a long, thoughtful, grave interview with General Marshall. [10] The Secretary of State [General Marshall] would go to Paris to present the American position at the U.N. Assembly. Bohr hoped that Marshall would say, *"We are for doing away with secrets, and we are prepared to do it."* The Secretary did not say that.

[10] Now Secretary of State.

By 1950, after the first Soviet explosion, after the decision for hydrogen bombs, it was clear that this country proposed to intensify its armaments, and just before the Korean War, Bohr wrote an Open Letter to the United Nations. He gave a very discreet account of history, of what he had been up to. I will read you, and it is the last thing I will, how he had changed. He is not now talking to governments; he was talking to you and me.

> *The efforts of all supporters of international co-operation, individuals as well as nations, will be needed to create in all countries an opinion to voice, with ever increasing clarity and strength, the demand for an open world*.

I cannot tell, and I think that no one can tell, whether early actions along the lines suggested by Bohr would have changed the course of history. There is not anything that I know of Stalin's behavior that gives one any shred of hope on that score. But Bohr understood that this action was to create a change in the situation. He did not say, except once in jest, *"another experimental arrangement,"* but this is the model he had in mind. I think that if we had acted in accordance, wisely and clearly and discreetly in accordance with his views, we might have been freed of our rather sleazy sense of omnipotence, and our delusions about the effectiveness of secrecy, and turned our society toward a healthier vision of a future worth living for.

With the development of the arms race and its intensification, and the bitterness of the cold war, and the multimegaton warheads and the rockets, Bohr concentrated more and more on what he knew he could do, on international cooperation in science, on good communication, on proper institutions, on goodwill. His own Institute of Theoretical Physics, the little Scandinavian institute called Nordita, were early examples. He spoke at the first Atoms for Peace Conference, a modest thing, but a true beginning in the erosion of certain barriers to communication. He took pride in the fact that the only Danish contribution to the second Atoms for Peace Conference was a joint paper by an American and a Russian. He played a most helpful part, not only in the establishment of your great friends and competitors, CERN, but in keeping CERN free of the provincialism of The Six, of Euratom, and free of the military preoccupation of NATO. He thought, as he had during the war, of going to Russia, and went there not long before his death. He traveled extensively in this country, in England, in Israel; he had an exhausting trip to India. In October of 1961, he spoke retrospectively at the 50th anniversary of the Solvay Congress.

In June of '62, at Lindau [Germany] with other Nobel Laureates, he had a light stroke. He appeared to be recovering. In October and November, he recorded the first five interviews of what were to be a history of quantum physics, and thus largely also a history of Bohr. He died on the 18th of November, the retrospect incomplete.

Bohr often spoke with deep appreciation of mortality: mortality that screens out the mistakes, the failures, and follies that would otherwise encumber our future, and that makes it possible that what we have learned and what has proved itself is transmitted for the next generation. On November 18th, as Bohr died, his son Aage was returning with his wife from a month in China, where he had lectured on nuclear structure.

It was in late September in 1945 that Colonel Stimson left Washington for good. He was an old man, and not well. On that day he had a cabinet meeting where he would advocate, very belatedly, an open approach on the atom, and an open and friendly approach to Russia. Later in the day General Marshall would have every general officer in Washington out on the runway to salute him and say goodbye to their chief. For all this, he had to have his hair trimmed, and he asked me sit with him when he was in the barber's chair. When it was time to go, he said, *"Now it is in your hands."* Bohr never said anything like that to us. He did not need to.