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J. Robert Oppenheimer

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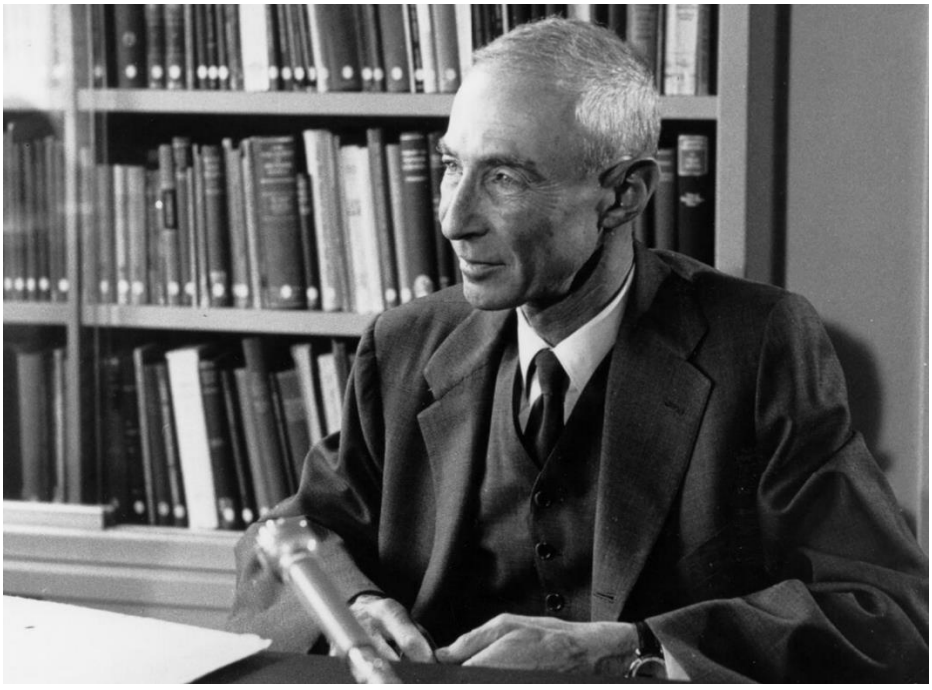

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Abstract:

Interview with J. Robert Oppenheimer for the *Saturday Evening Post*. The topic of discussion was the atomic bomb. The title of the article, "Behind the First A-Bomb." At the time of the A-Bomb, Dr. Oppenheimer was the director of the Los Alamos Scientific Laboratory. Work on the bomb had been going on since 1939 and Oppenheimer worked with the following scientists during that time: Enrico Fermi, Leo Szilard, Harold Urey, John Wheeler, Eugene Wigner, Arthur Compton, Ernest Lawrence, and Niels Bohr. Oppenheimer discusses what happened leading up and after the detonation.

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Oppenheimer:

So many points were clear that this was a very major chapter of human history and this _____ would not have to be marked out? It would not be in the light of hindsight. It was as it actually felt at the time. In other words, a great many things said in the emotion of that early morning of which I may not remember.

Cahn:

What were major problems?

Oppenheimer:

One of the difficulties of not having any secret information is you don't know what is secret. When we went to Los Alamos we had good reason to think that an explosion of high yield could be made with amounts of materials would be forthcoming in a couple of years—but we didn't know this. And there were in addition to imponderable doubts, there were substantial doubts on a couple of points. For instance, it had not been measured whether fission induced by neutrons there were enough neutrons released to sustain a chain reaction. It had not been determined that there was no time delay of any significance in the emission of neutrons. None of this would have prevented a pile from reacting. But it could have prevented a bomb from exploding. So the first physics experiments were directed toward these matters of principle which all were settled there rather soon and fairly affirmatively. We didn't have any reliable idea of how much material would be required. The critical masses were quite poorly known and the amount of material, the efficiency or yield, the function of the actual mass, was poorly understood. So these things were problems of theoretical calculations and of measurement—a great deal of measurement.

From the first we tried to devise a program in which as we gradually got more material we would check by more and more integral experiments the predictions which we had made on the basis of studying individual nuclear properties. So when we first had a chain reaction system there, which was just with a small amount of enriched uranium, called a water boiler, the amount we needed to make it react was within a few percent of the amount we calculated. And this gave us some assurance. There were a great mass of problems which were to get very much worse before we got through, on the chemical and metallurgical sides. With the gun assembly, the speed of assembly is finite and one must be sure that the background of neutrons is not so great that before the assembly is completed, the thing starts blowing up. Because that will frustrate the explosion. And

with plutonium, the alpha activity of plutonium is very great and we planned to get the plutonium pure enough so that the production of neutrons by these alpha particles would be compatible with the gun assembly.

In addition, plutonium is one of the darkest metals that was every studied by man. It has a great many different phases. It is one of the few metals which actually has a dense phase stable at higher temperatures than a less dense one, and for a long time even the elementary question, how dense is plutonium metal, could not be answered, until the metallurgists under Smith finally got it completely straight. And how to fabricate plutonium and work with it was. X before it is a very toxic substance. Our problem though was to produce metal chunks. We knew we couldn't touch them or breathe them. Nothing whatever had been thought about the weapon and engineering side of things. We hadn't even decided how high we wanted the thing to explode above the ground. And what sort of safety devices should protect it from exploding prematurely, and what sort of safety devices should insure that it would explode if it after all hit the ground. These were the things which we knew we had to do and calculations of blast damage, radiation damage, etc.

Very early in the game we became somewhat dissatisfied with the primitiveness of the just sticking one piece into another and Neddermeyer came to Los Alamos very interested in assembling from all sides. This had obvious geometric advantages, and that's what he was thinking of. But he was still thinking of explosives as the propellant of a solid body. Von Neumann then pointed out that if one didn't use too much plutonium and if one used well focused spherical explosives, one could make the assembly time short enough so that one wouldn't have to worry about the neutrons. Or wouldn't have to worry much about the neutrons setting it off prematurely. It would do it so quickly. As soon as that began to be looked at it was clear that if one got into this regime where we were essentially dealing with a flowing metal, plastic flow, and not a solid body to be pushed, that the pressures would get very high and density changes would occur, which were very favorable. And then we had a design which we thought would accommodate a rather small amount of plutonium, which would give relatively modest explosives, maybe in the kiloton range, but which we could get perhaps earlier because the amount of material was small. The whole effort was to get something sensible and typical quickly.

Then two things happened. First was that these rather radical designs for implosion gave every sign of not working. I believe that in '52 or '53 they were tried and were successful, but it was almost a 10-year delay. And so we lost our hope of using very small amounts and perhaps the whole program would have been discouraged out of existence had it not been for another development. Which is that we suddenly discovered we couldn't use gun type assembly with plutonium at all and as Fermi used to say, "I don't like the idea of just seeing it pile up on the shelves." That was because in the higher flux reactors at Hanford which were producing plutonium, an isotope which produced neutrons spontaneously by fission is very abundant. And there was no use

purifying it, the neutrons came from plutonium, and only a further isotope separation, of very great difficulty, could have got rid of this stuff. So if we weren't going to use it, we had to implode it. That, then, made it necessary to find a practical solution.

The reorganization reflected in the first place that the great chemical job, which many talented chemists had come to do, we no longer had a need. It didn't matter if plutonium was pure. And on the other hand very many new problems were created in explosive technology, in neutron physics, in metallurgy, and with regard to the device which we called the initiator which provided the neutrons in large numbers just when you wanted them, but not beforehand. These were high priority problems, so the lab was very largely redeployed, so the chemists, having had their problem shot out from under them were simply marvelous in turning in on the other problems which needed solution. Then, we did get on with a more conservative design, which Christy probably holds a patent, and which was the basis for the first several of many generations of improvements. This remained the standard way of doing it for a long time, it was the one the Russians used in their first bomb.

We still had incomplete evidence of the actual flows of matter, the space of time in an implosion. We thought that the densities rose, we thought that the spherical geometry was well preserved, but this was based on the interpretation of pretty subtle and difficult experiments which one called Ra-La for radiolanthanum was the most spectacular—we put a strong, many hundred thousand curies source of radiolanthanum in the middle of this small model of an implosion device, imploded it and from the increased density, the transmission of the X-rays from radiolanthanum was reduced—this happened in a microsecond or so—studying these traces we tried to conclude how much compression had been, how long it had lasted. But you don't have overwhelming confidence that you haven't missed something.

So that it became essentially clear when you knew that we had to use an implosion method, and it began to look like it might be our first thing, because the two were sort of neck and neck, and it became clear that an organic test, an integral test, was very desirable. To have one of these things that had been paper tried, and components proved in, but never really shot, would be very difficult and might involve us in serious trouble in a military campaign. And how true this is would be apparent from the fact that the best guesses of the yield were really quite far off. It could have been off the other way too—and this would have been very, very bad. As soon as it was clear that we had to make a test, we made one precaution, which was to procure a large steel container, Jumbo, so that if the explosion went off but the fission didn't occur, we could save the plutonium. We never used it because by that time our degree of confidence had become pretty high, the production of plutonium was such that we would get enough for another bomb before we could clear up Jumbo, etc. And it inhibited the diagnostic tests, to see what had worked, how, if it had failed, why it had failed. Decision not made though until the explosive system, the very complicated

detonator system had been proven and proven again and we knew that these things had a chance to succeed.

I bet with Kistiakowsky that his detonators wouldn't work—but I thought they would.

I remember that we sometimes tried to say, suppose it doesn't work, what will we do. And we could never answer that because we felt that from where we stood we didn't know enough to imagine what we would do unless we also found out why it didn't work. We didn't have the material to make other uranium bombs. We could, of course, have tried to make implosion uranium bombs, and I proposed to Groves we make something that would be a little more efficient use of the material. But he, and I think probably rightly, felt that it was a time to be sure and to be fast, and that delay might be more serious. The pressures to get out of the invasion of the Island of Japan was very great. I got my information (on when [we] had to fire—before Potsdam) in one of two ways. The only formal channel to the laboratory was through Groves. There was another channel which involved me personally, not as the director, was the Stimson committee, the Interim Committee. We were always asked to do it as quickly as possible. I tended to think that there would be more trouble if I painted a glowing picture to Groves and then we welshed on it than if I made a fairly conservative estimate and then we did a little better. I thought that if he presented a fairly optimistic picture and then things started slipping, the whole confidence in the operation might get lost. Perhaps I was wrong.

Cahn:

Things that had to be ready—the firing unit—the detonators—

Oppenheimer:

—which were fussy; we had frightful delays in getting explosive lens molds.

Cahn:

—timing device; and the plutonium—

Oppenheimer:

Indeed there was, and very fussy things with the plutonium because it had to be treated in a quite complicated way in order to be suitable. I think if you counted the shelves that this object had, going from the outside in, you got something like 30—it was a very complicated… much of it no doubt, superfluous, but all of it intended to decrease the chance of something going wrong and increase the efficiency. (core itself had a large number of shelves).

It was a tense time. But we had one thing in that laboratory, the only thing that seemed to me of absolutely vital importance, apart from very good people, we had good communications. You could always talk to each other. And no one ever had a problem

alone. And since we were in actual fact a little overstaffed from the point of view of very bright people, if you could identify the trouble early enough and talk about it, you could usually find something to do about it. Every group had meetings, and all the leaders of the groups had both laboratory wide and divisional discussions. And policy committees of the lab handled different parts of this job. So it was a well watched and well discussed operation.

Cahn:

Example of good from non-compartmentalization—Hornig example.

Oppenheimer:

An example, I am very moved by it—there was a physical chemist called Hirschfelder (now at Wisconsin), he helped with explosives. But he wasn't playing a very central part. But we came up with one very big thing. He understood there would be fallout and predicted it, explained what one could and couldn't do to reduce it, and prepared us for it so that we could at least measure it, take some precautions. He was doing something like studying the effects of the explosion on the atmosphere (there were all sorts of complicated things), but this one point for which no one had any responsibility, which of course has had quick a history since then.

Cahn:

Delay if compartmentalization.

Oppenheimer:

I think if the ordnance people had been given an assignment rather than told the problem, and the initiator people had been given an assignment rather than told the problem, etc., it would have delayed us—re: the gun barrel weapon. But the implosion problem might well have never been solved. And certainly not quickly.

Cowpunchers Committee—to make sure that there were not any little elements which had to be in the bomb but were not in the schedule. I for a long time felt that maybe we were not taking the initiator problem seriously enough, but that was cleared up and Bohr played a large part in giving a general argument that it was much harder to misdesign than to design an initiator, that is that almost anything you put together would work. We did overdesign it. But the notion that this thing would fly together and fly apart without there being a neutron to start the chain reaction was not very attractive. We designed it so that the chance of not having a neutron in its relevant time was less than one in ten to the fourth (1 in 10,000). And it was better than that, but that was the requirement.

Then of course we didn't really know how well this detonator, lens, tamper system would work in actually making a high degree of compression. We had estimates which didn't differ terribly much but enormously influenced the expected yield. And it was

perfectly rational to have a wide variation in your estimate of what would happen. But we were not absolutely sure that the thing would remain reasonably spherical. Because it is an unstable situation when you have a light medium pushing a dense one. You couldn't get away from it. Plutonium is dense and explosive is light, and although we tried to reduce the effect, we couldn't be sure we succeeded.

I probably started the pool. Rabi and DuBridge knew very little about it, they were visitors. Rabi as little as a year before had said the implosion thing will never work, why don't you forget it.

I believe that Bethe's estimate, they put in everything, and it was optimistic, about 5,000 tons. This was before the coordinating council meeting.

Before I left for the site, I had worked out a code with my wife. This had to do with the question of whether on Saturday following the test we would have a supper—a big one, much bigger than we could get in our house—there were a lot of visitors—to celebrate. And the code on that was I was to say to Anne Wilson (his secretary) that she (Mrs. D) could change the sheets. Also, my wife picked a four-leaf clover (and they were hard to find those days at Los Alamo, took her a long time to find it), she gave it to me the day before I left, we were walking around in the garden. I was there (Trinity) not more than three days before the test. I had already been there some time when the plutonium core arrived. (core might have been turned over to security people). Before we fired it, when we hoisted it into the tower, I asked for a receipt from Gen. Farrell from U.S. Government for the relative number of pounds of plutonium. I had signed for every milligram of the stuff, I was responsible for it. I wasn't going to be able to account for it. This was a terrible ritual. The guards would come, three or four of them, and they had bags and pistols, and receipts in quintuplicate, and everything came to the lab that way. All that childish stuff that Boy Scouts dream about.

We had [a] storage vault to make sure [we] wouldn't blow up or burn up. If the plutonium had ever caught fire, there would not have been anyone left alive in Los Alamos and probably in much of New Mexico, it is so terribly toxic. It burns in oxygen. It is not an inert metal like gold. If it caught fire, it would have been very dangerous.

Cahn:

Thing not fitting?

Oppenheimer:

I don't know who was the first to remember that plutonium had a power plant, it had been hot, it had been in past times always kept in contact with metal where it would be cool. Here it had been out in the desert air.

Cahn:

Creutz electromagnetic experiment?

Oppenheimer:

He phoned to give me this awful report. Bethe found out experiment was correct but didn't mean anything. For a while it looked very bad.

When we left Los Alamos I was pretty clear that nothing that happened at Los Alamos in the next days was going to affect us. We had the best judgement that a year of hard work was going to give us and we just had to go ahead. I was disturbed by the Creutz experiment. I didn't actually know until after the test firing what the mistake in magnetic measurements was.

There was a dramatic small point. When I got back to Los Alamos there was a report on my desk giving an account of the enormously high reaction cross-section – tritium deuterium reaction. It was very relevant for the thermonuclear. Bacher had been working on it. It was very favorable—or ominous.

Decision to postpone made about 10 or 11. I moved around during the night. I might have cat-napped for an hour. I went out to the firing site once or twice before I went out for the actual firing. I was just checking to see that everything was ready, looking at the sky, trying to find out what was happening at Kirtland, because that whole thing went to pieces. Decision to fire made about 3 in the morning. About 4 or 5 hours when we didn't know. And we thought we would like to have it when we could see if a squall was coming, in spite of fact that this would obscure some of the instrumentation, because we thought we could see the weather a little better. And if there were squalls coming we could notice them. And we didn't want stuff just to rain right down back on us. I didn't like having it up there during a storm anyway. We never knew what a sudden surge of power such as by lightning would do—the detonators would do—it wouldn't give a very good bomb but it might do enough to wreck everything.

I remember talking with Bacher and that if we had to cancel—the main argument for going ahead was that we really had no assurance of just when we could do it if we had to cancel. The weather was uncertain and the amount of last-minute checking and coordination, you couldn't drive people. I had in the back of my mind the pressure of getting it done before Potsdam. One of the things which was discussed, and is recorded to some extent by Feis in book he is writing now and in the Stimson diary, is the question of what, in what way, with what motive, what detail to discuss with the Russians the development. Bohr had always felt that it was very important to talk early with the Russians about it, not discussing technical things, but with the point of view that this is going to alter the whole problem of living in the world, and it was in a certain sense a joint achievement—but it is very doubtful that anything we said to Stalin would have made any difference. Bush and Conant in a somewhat more pedestrian way held the same view. I believe that as soon as I saw Bush, probably after the test, he said that he had been to a discussion in Washington to which it had been agreed that there would

be some conversation with the Russians. Actually, this agreement was more tentative than he thought it was, and the actual discussion as recorded by Truman was very trivial and also very uncontrollable. Because Bohlen did not go over with Truman, so there was no American who could understand Russian. Also the Russian interpreter was not even the standard fellow—I think called Pavlov—but someone who was standing in for him who no one had heard of since. And nobody knows, first, what Truman said, because that is only what he remembers. Second, what Stalin heard, because only the interpreter knows that. And third, what Stalin said. But I had the feeling that increased confidence in this undertaking would make it easier for them to do what they (the US gov) wanted, and this was important, very important. Perhaps as important as having it ready for military use. So I was very anxious not to delay.

[End Reel 1]

[Begin Reel 2]

Cahn:

What happened last few minutes?

Oppenheimer:

I remember I didn't know whether I could remain conscious. I leaned on those pillars inside. I remember thinking: "now I must remain conscious." I think we had really unusually good reason to believe that the elementary physics and fission which had taken a little to work out—what happens in an exploding bomb—disassembles it—that this was basically sound in the sense that these bombs were possible. I think if we had had a rational chance of wishing whether they were possible or not, people might have taken very different views of what they wished. It is a small point but the one man who on grounds of principle begged not to come to us, but who, because he was in uniform was brought there anyway, was the man ever since who has been the director. But he felt this was a thing to be avoided—that he wanted no association. But that was no longer the point. The point was whether we in our lack of thoughtfulness or stupidity or bad luck had missed something and were going to obscure the realities which were military, political—by our own fumbling. And this I thought could only be bad—that is, if the failure of the test were to establish that no one could do it, it would be very all right. But if the failure of the test was only going to prove that we had muffed something—that after all the detonators were not well enough synchronized or if after all the lenses were not cast properly, then—or if it had come out so terribly piddling that doubt had been cast on its making any difference in military circles, if these things had happened, we would have given our Government a bum steer as to how to be thinking and acting. And this was what seemed to me to be avoided in our thinking. It would have wasted money because I think in war one always does that.

Cahn:

Passage which came to mind from Bhagavad-Gita?

Oppenheimer:

Krishna? … Vishnu was trying to convince Argina who started out as a pacifist that a man must do his duty and that his soul is good, it is affected by whether he does his duty but not by whether he does harm. Real harm cannot be done by man. And this goes on for 30 chapters. And at one point he assumes his most awful form and says to Argina "I am become death the destroyer of worlds." And this is the line that came to mind. And when he is all thought with that, he then goes back and says, "Now I have resumed my normal fore-armed [sic] form." And I remember it always because I read it with a student at Berkeley who said to me the first time, "I suppose that's how a young man feels when he is behind the wheel of a powerful car." This came to my mind (the line from Bhagavad-Gita) when I saw the light of the bomb. I was turned away from it, at the left of the door, looking out toward Mockingbird Gap. It was much brighter than sunlight, it had a golden quality. The first evidence that I had that something had happened was that the whole desert lighted up in the most brilliant golden light, and that's all. I was located between the door and the automatic timer, my back was to the tower.

I was outside and perhaps a little protected by the building when the shock caught up with us. Immediately after that hell broke loose, everybody started talking, and Kisti put his arm around my shoulder and said, etc. (What Kisti said is almost certainly right). I remember talking to Farrell. I remember a kind of solemn quality to it. I think there may have been a little jubilation, but it was a very solemn thing perhaps punctuated by someone saying, "that wasn't so bad," or guessing the yield. I rode back to base camp with Farrell, and he started talking of his emotions, he said that he could remember what it felt like when he was a Lieutenant in the first world war waiting, looking at his watch when he had to start out of the trenches with his men. He said up to that night he thought this was the most dreadful tension which anyone could experience, but it was nothing like what he had just been through. The kind of emotional material which he wrote for Groves was clearly taking shape in his mind all during that drive.

Cahn:

Any evidence that Farrell was correct about religious feeling of those there?

Oppenheimer:

Well, I think he said a sense of awe, and I would go along with that, but I would not in that company equate it with could be properly called a religious experience. Farrell is a practicing Catholic and I have no doubt that this as a true account and it may have been true of some of the others. But I don't think that's how Kistiakowsky or Bacher or Conant would have reacted. There was a sense of great emotion.

I think Farrell and I said to each other driving back what became the slogan at Los Alamos for the next month or two, and which we always drank to, "That there would be no more wars."

At some point Bush very solemnly said, "The fellows of science have done something for mankind." And this notion which was increasing in everybody's mind that this would make a re-enactment of the first and second world wars unlikely. Something was afoot now which couldn't be downed. How much people thought about the Japanese war that night I don't know. Farrell says something about it. It wasn't in my mind at the time. I was, perhaps quite wrongly, thinking beyond that.

Cahn:

About evacuating people?

Oppenheimer:

That was not decided for quite a while. Radiation levels did develop at some communities to the Northeast which didn't do any damage but we were concerned.

Cahn:

Groves had power to declare martial law.

Oppenheimer:

Yes, and he did for two or three hours—over all the cacti. The evacuating of people was left open until about 10:30 or so when we decided it was safe. Then we left. I tried to help Groves draft his cable. We went over the evidence about yield and Groves said I think it's safe to say 20,000 tons. I said that was all right.

Fermi's experiment at base camp gave an estimate of 10,000 tons.

Cahn:

How did it work?

Oppenheimer:

The push of the shock wave is imparted to the falling paper. If the paper were infinitely high and the shock wave also infinitely high, that push would continue and the paper would be picked up by the shock wave. But it was exposed only a finite time and so the distance that the paper is displaced is a measure of the push in the shock. And that in turn, when you know the distance, is a measure of the power of the explosion. The shock wave is a wind, and the displacement of the paper measures the product of mass and velocity of this wind. It is clear that by watching the paper you learn something. Well Fermi had done a little calibration, so that he came within a factor of two of being right. That was a lot better than he did before.

At base camp Farrell and I killed an almost dead bottle of brandy. We got our figure of 20 from the size of the fireball and from the radiation detectors and from the blast. The instantaneous radiation gives a measure of the reaction—not the delayed radiation. They all agreed, and it was very close to 20. I think it was probably 22 ½.

Just after we had gotten the reports on the yield, maybe about 6:30 or so, I walked out alone into the desert near base camp. And I saw a turtle, about a foot wide, on his back, helpless, probably knocked over by the blast wave. Sort of helpless. And I turned him over. It just seemed we had done enough harm for one night. It was a land turtle, fairly big, with a major axis of under a foot.

Later Frank (my brother) and I took a walk into the desert. I think we probably said, we hoped it was for the good. And I think Frank was eager to go back to Berkeley. I may have said to him, I wonder whether it made any difference that I came here.

When the laboratory opened, I phoned Anne. We figured everyone would see it. You know about the Security Officer (T.O. Jones) holed up in Albuquerque who had orders to keep the test secret. He said, "They want the Mississippi River secret next."

I called Anne as soon as I thought she'd be at the desk. I gave her the code, "Tell Kitty she can change the sheets."

We left about 10:30. I had some things to arrange with Groves. We still had a directive to get the weapons in the Pacific theater. The "Little Boy" assembly and all of the nuclear materials that we had were on the Indianapolis. The rest of it wouldn't be ready until the first of August. What we didn't have, which was probably the projectile, was flown over and left Los Alamos about the 27th of July. There were several copies of the Fat Man on the way, and the core had to go later. It didn't exist at the time of Trinity.

A third core (for Fat Man) was on its way and when we heard the General Order of Admiral King to stop shipping munitions of war into the Pacific, we recalled it. It was still in the States, but on the way. I recalled it. I called Groves afterwards and said I had interpreted it this way, I hoped I was right. The Japanese had essentially accepted. This was about the 9th. When they first indicated they would surrender. The plane was in California ready to go.

Ramsey and Rabi drove back with me. In a Plymouth with a speed control which wouldn't let it go very fast. It was an Army car. I drove. We went back over the mountains, over the Valle Grande, the road was open then. You go up through Cañada to Cochiti and past Bland, up into the Valle Grande, and cut across the corner of it, then come down over Pajarito mountain and hit the back Frijoles Road and into the back of Los Alamos canyon. It is just about as quick and it is very beautiful, and Rabi and Ramsey had never seen it. We talked and talked and talked. We were talking about the changes in the world. My guess is also that we probably had long periods of silence.

I think I drove home and then went back to the lab very briefly. Kitty smiled and I remember Charley Lauritson coming up to the fence and when I asked him in for a drink, he smiled and said, "Not today." Then I went over to the office and saw the papers on my desk and decided nothing more could be done that night.

Cahn:

Incident of someone X-ing out the word "after" on the cartoon.

Oppenheimer:

I remember someone doing it.

Cahn:

What did Kitty say?

Oppenheimer:

Probably very little. We had been living with it a long time. She had not seen it. She probably said, because she felt it so strongly, that I must think about getting out of there. Would it be all right if we had bacon and eggs for supper. (Boy was 3, born in May of '42). Little girl was a tiny baby.

Name Trinity: I invented it. It doesn't really mean anything. It has no technical bearing. I was thinking very much in those days of this line of John Donne. "As East and West in all flat maps and I am one are one, so death shall touch the resurrection." That is in a set of poems, another of which is, "batter my heart three personed God." And it was this completely accidental, personal constellation of things that made the word Trinity come to mind. These are from two poems, the first one from "Ode to my God on my sick bed (John Donne)", and the other is one of the Holy sonnets—they appear on contiguous pages in most collections of Donne.

Cahn:

Fuchs there?

Oppenheimer:

Probably.

Cahn:

Meeting in office few nights later, where Fermi arguing that should not use it because there always would be wars.

Oppenheimer:

There is disagreement on that date. I am doubtful that it was on the 20th, and it has been also ascribed to the 10th (by Feis). Such discussions did take place. They were the panel of the interim committee which had met with the Interim Committee on the 31st of May—on general orientation, although the problem that I remember as having been discussed was what should we look forward to, should we tell the Russians about this in general terms, is there any such thing as international control. But there were other things too. But they then asked us to put our thoughts to the question, "What do you do with this stuff?" What is the future, for peaceful, for military uses? We convened at Los Alamos. My impression is that this was the week before Hiroshima. We also convened earlier and it might have been the 10th of July. In response to a request from Mr. Stimson and Mr. Harrison. That first time we gave a more or less formal answer, saying the future is unpredictable, the thing shouldn't be legislated in detail. We should have a commission which has the responsibility for a rather wide area. We perhaps sketched it out a little. Compton, I believe, brought a verbal request for our views, the scientific community, did we oppose or not oppose the military use of the bomb. And as to that we wrote a memorandum which has never been declassified although excerpts from it have. Saying, primarily, that there were two big points, one was the saving of lives in this war, and one was the future. And we felt that both were important, perhaps that the second was the more important. …. I don't believe we came back to this after Trinity, this meeting was before. The meeting afterwards was to talk—Lawrence was there, and Compton and I think that was the talk about the future of atomic energy—such things as raw materials, power, research, thermonuclear bombs, alternate methods of making the stuff. We wrote a big report which was finished in September in Washington. The meeting of which you speak most certainly could have happened and it would have been typical of Fermi to say, "I think people fight because they get bored, and I am worried about what we are doing here. There will always be wars." It may have been that I said, "I don't think that's the only way to formulate why people will fight."

By then everyone was thinking and talking and wondering and worrying about what the hell could happen. I had mostly confined my earlier talks to Bohr. We talked endlessly. But the rest, I thought we had something to do and we'd better do it. Toward the end of July or early August it was impossible for people not to wonder what was obvious. I believe I am right that there were two meetings. We may have discussed it later, but not with an idea of doing anything about it. And the primary purpose of the later discussion was to get some humanly manageable account of what this was all about down on paper so that the Executive branch of the Government could begin thinking of what to do.

Cahn:

About advice given to use it?

Oppenheimer:

We were prisoners of the view that the Japanese would not surrender without an invasion or something like this. It is hard to reconstruct, but it was believed by Stimson, and we had been told this and we have no contrary evidence, and I didn't know until

much, much later that the Japanese had tried to negotiate with us through the Russian government and that the Russians blocked that. We didn't know of the delicately poised debates in the Japanese cabinet between the war party and the peace party. (It never was our decision, anyway). The evidence is that the Interim Committee made its recommendations to the Secretary of War and the President prior to talking it over [and] asking us about it. It was taken for granted. They took it for granted that we would go along. And then when the protests came in from the University of Chicago they wanted to know, is there anything in this. At that point, they turned to us for paper. But that was not when they were making their decision.

Cahn:

If you had been apprised of these factors, might you have suggested doing something different?

Oppenheimer:

Well, I'm sure that if I had then had the view of the state of the Japanese government that I have now, which may be wrong too, I would have been at the very least for a much more explicit warning. I would have tried to reduce the loss of life. And I think that I would have tried to get a negotiated peace without the use of the bomb. But I would then have wanted to do something better than blowing it up at Trinity, where the visible after effects were zero—the tower was gone, that was all. You could have vacated an industrial area and had a demonstration. After Trinity it occurred to me, but not vividly, and not so we could do anything about it, that the pyrotechnic effects are not so trivial. This enormous light—that might have been enough to impress an enemy who was ready to be impressed. But none of this happened at the time. We thought we were dealing with an enemy who was prepared to repeat Okinawa on a hundred times larger scale. And that we were dealing with an American Government that knew what it was up to. But I think that even if you accept everything that was then wrongly believed, or grounds which have since been subjected to doubt, the warning which was supposed to go with the Potsdam declaration, should not merely have said that they would have been subjected to further bombardment. Because they had had that and I'm sure they didn't understand that something new was coming up. And the warning shouldn't have been so vague that no evacuations from the target areas were undertaken. And having only the two planes, they sounded an all clear within 10 minutes of the original warning. They had seen the places, so they assumed it was just observation plane. Immediately before the bomb exploded an all-clear sounded. Their radars picked up something, and they gave the warning, but when they got visual sighting or closer radars, they decided to give the all-clear.

(more)

I think the sense which a surprising number of people had—I don't want to attribute this to Fermi because I'm not sure, and he was a very hard-headed customer, but which Bush had, and which Farrell had, Bacher had, which I had, was that it wasn't going to

be long before wars were really a thing of the past, or at least wars in the modern sense, and that we had a certain obligation to help and explain what we knew, and that this was a big change in the ultimate situation. Eisenhower himself once said that this will blackmail mankind into peace. He said at that time. I think he said it when he came back, when he was Chief of Staff.

Cahn:

In the light of today, how do you look at this, is it too late.

Oppenheimer:

I think one can only say that it has been a pretty rough road, but that very few of us have given up hope. And even if you take the divergences between Dyson (who wrote piece in For. Affairs) and Bethe, you find that both speak of a peaceful world, differing on what immediate steps to take. Teller (like Dyson) would be another good example of why it is important to continue testing. You don't find them arguing that it is all right to go ahead and have an all-out war. You find they are only arguing about what are the best steps to take in a tangled and tough time to make sure that this doesn't happen.

Cahn:

Do you feel, that as history looks back on us, the event or the atomic bomb being successful would have forced us…?

Oppenheimer:

I don't know. I would say that we don't know. The bottom may fall out of everything at any time. But it seems to me one thing to say that we don't know and another thing to say we don't hope. I would like that the countless instances of really quite great dedication on the part of men who otherwise would be involved in quite purely scientific things, to try to see that the political realities conformed to the technical realities—a very hard program—so that this is not just an individual quirk but something the whole community was touched by. I'm pretty sure that that was the dominant sense in the morning of July 16, 1945, with differences of emphasis as to whether it was this rather marvelous and unexpected scientific development putting an end to this dreadful war, all the horrors that everybody had committed, or looking way beyond that to the notion that of a world in which war wouldn't occur. I don't want to affix(?) that because I can only do it for myself. [handwritten] *I am afraid I was doing the latter.*

Cahn:

If there had been no bomb, if it hadn't worked…?

Oppenheimer:

If it hadn't worked because of a matter of principle. Well, I think the history of the world would have been different in the past fifteen years. How different, in what way, I

don't think that I'm the right guy to say. I'm not a historian, and I'm not very good at thinking alternate history.

Cahn:

But not from the standpoint of whether we should have used it, but from the viewpoint of whether we would have had to work these things out anyway, that it basically wouldn't have made the change that people… hoped for?

Oppenheimer:

Well, I'm not sure. You had some strong arguments for it in the revulsion from the second World War. You had some strong arguments against it in the character of the Soviet regime and in the character of the new states which have developed since and are developing now. And the gross disproportion between prosperity and welfare in one part of the world and in another, I don't really know what would have happened in Europe in the fall of 1945. Over there, history would have been very different. The counterpoise of Russian power, which was very high, they not only had won the land war in Russia but had organized the peoples of Europe—if the counterpoise of that offered by some signs of power and intelligence on the part of America hadn't existed, how this, how this would have gone, I don't want to speculate on that. But this was the beginning of a new age for man. The problems that have bedeviled him in the past were not going to stop bedeviling him. That this was a new one which would alter the light in which they were looked on was the true sense of what we breathed that morning after the first test.