Oral History Interview: Duncan P. MacDougall

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Interviewer:
Lillian Hoddeson, University of Illinois at Champaign-Urbana, hoddeson@illinois.edu
Transcription:
Patrick Charbonneau, Duke University, patrick.charbonneau@duke.edu
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[TAPE 1]

LH: [0:00:00] MacDougall on April 9, 1986, at Los Alamos.

DM: Do you want me to put that on?

LH: You'll come out clearer if it's hooked on.

DM: [0:00:31] Of the many things that resulted from my accident is my hands are much less capable.

LH: This interview is an attempt to extend on the very short interview that Alison Kerr and I did on January 8, 1981. I have a number of questions to pose to you about explosives' work throughout the entire wartime Los Alamos period. I realize that you may not be in a position to answer some of the questions because you were not technically at Los Alamos, but I thought I would ask you anyway. If you don't know, you can tell me or refer me to somebody else, whatever.

DM: Ok.

LH: In the earlier interview, you mentioned that in January 1941, Kistiakowsky asked you to come to the National Defense Research Committee

Laboratory that was being set up on the grounds of the US Bureau of Mines, outside of Pittsburgh, in Bruceton.

DM: [0:01:45] It's highly unlikely that I told you that, because the facts are that Kisty asked me to take that job probably around September of 1940. A lot of things went on. I physically came to Pittsburgh in late January of '41. But a lot of things had happened before that.

LH: Perhaps, you could summarize those, to fill me in, because I am interested in the establishment of this laboratory. Why was it established? What was its mission? And so on. Because it was when the war started…

DM: [0:02:37] No. Before we got in the war. The war had not started for some time. There were many people who were convinced—certainly as early as early 1940—that it was only a question of time before the United States got into the war. I don't remember exactly. In fact, I'm not sure I ever knew precisely when the National Defense Research Committee was established. Probably in the summer of 1940, [but] I'm not sure. This was to conduct research and development on all sorts of things of interest to the national defense. In those days, we never spoke of war work. It was always defense work. I believe that both Kistiakowsky and Conant and some others went to England maybe around June or July of 1940 to begin making plans for various joint efforts with the British.

The big effort was one for which neither Kistiakowsky nor I had anything to do with, namely radar, in which this country had been doing quite a bit of work. But the British were way ahead of us in radar. That was the major area. As you may know, the so-called radiation laboratory at MIT was setup to work on problems with radar. And there was a lot of progress **with** the British in that area.

But there were a lot of other areas, one of which was explosives… That was the area that Kistiakowsky was concerned with. At the beginning, the head of the NDRC was Vannevar Bush, an engineering professor at MIT. One of the divisions—it was perhaps labeled chemistry, I think it was wider than explosives—was called Division B. It was headed by Conant, and Kistiakowsky was in charge of a section that had to do with explosive work. During the fall of 1940, one thing that I did was to spend a lot of time in the Widener library at Harvard, reading up about explosives. Because, while Kistiakowsky and I both had chemistry PhDs, neither of us knew particularly anything about explosives. We were sort of self-taught. I did spend many hours reading issues of a journal called the *Zeitschrift für das gesamte Schiess- und Sprengstoffwesen*. (Fancy title like that may be one reason

why the Germans lost the war.) This journal was continued after the war with the title *Explosivstoffe*. (That's explosive material.)

I remember that in December of 1940, there was meeting in President Conant's house, in Cambridge—he was of course president of Harvard, then—involving Kitstiakowsky, me, John Holtz, who was representing the Bureau of Mines.

LH: What was the meeting about?

DM: This was specifically about the establishment of an NDRC laboratory on the grounds of the Bureau of Mines.

LH: Devoted to explosives?

DM: [0:08:02] Devoted to explosives. In fact, at the beginning we were thinking specifically of high explosives. But very early, it was decided to have two branches of the laboratory: one for high explosives, and the other on propellants, especially rocket propellants. Because rockets in those days… Although rockets had been known for centuries, I think this was the first time that a determined effort had been made to use them in modern warfare. They were classified. The whole rocket fuel program in England the spoke about UP: unrotated projectile. That was just sort of a code word.

LH: Two questions. First of all, the two of you were invited because you were in the chemistry department. Is that correct?

DM: [0:09:37] I had been an instructor in chemistry at Harvard. Kistiakowsky came to Harvard from Princeton in 1930. He was at that time an associate professor. My limited-term appointment at Harvard expired, and I was actually teaching at Clark University in Wooster, Massachusetts. But Kistiakowsky and I knew each other because we had been for three years in the Harvard Chemistry department.

LH: So, he chose you because you were someone who would be appropriate for this.

DM: Yeah.

LH: I see. Another question. Just a little more about what it was felt in 1940, the explosives would be used for in addition to rockets.

DM: [0:10:35] Usually, when we talk about explosives, we usually mean high explosives. Although, I guess it's not wrong… Even propellants can be made to detonate. But the high explosives, this is the one I was concerned with all through the war, were to be, we hope, improved material for loading into projectiles, depth charges, all the sorts of things that use high explosives. There was a strong feeling—this probably does not need to go into the written record—that the research establishments in the Department of Defense. Of course, it wasn't that bad. It was the Army and the Navy. It was not even the Air Force then. It was just part of the Army. Their activities in explosives were way behind the times and so on. In fact, the explosive which we—by we, I mean a lot of different people in the NDRC—put into production and used was not invented by us. In fact, it was invented by some German almost 100 years ago, but nothing much was ever done with it.

LH: Which one are you talking about now? Are you talking about RDX?

DM: [0:12:23] RDX or cyclotrimethalene nitramine.

LH: I was gonna ask. That was invented in Germany?

DM: [0:12:30] The compound, cyclotrimethalene nitramine, was I think first produced and maybe even patented in the 1890s. But nothing much was done with it. However, in the period between World War I and World War II, there had been a lot of activity in England, and particularly…

LH: I was gonna ask. When you say state of the art, behind the times, you were talking about England?

DM: [0:13:12] I was talking about this country.

LH: There were people in this country also working on explosives?

DM: [0:13:20] Yes. The Army establishment on explosives and a lot of related items was [done] in the Arsenal, in Northern New Jersey, maybe 75 miles West of New York City. I've been there, but I forget exactly. We, perhaps, gave them less credit than they deserved. They gave us no credit at all. They said we were just long-haired professors that succeeded in killing ourselves.

LH: But there was explosive development going on at Picatinny[1]?

[1] Picatinny Arsenal: https://en.wikipedia.org/wiki/Picatinny_Arsenal

DM: [0:14:12] No. The chemical Cyclonite was first synthesized in Germany before 1900. The development of it was in England, not in this country. The Picatinny Arsenal knew about it, but I believe they thought it would be too hard to synthesize. I believe the explosive that was favored in this country was called PETN, which it pentaerythritol tetranitrate. This had been developed jointly, I think, between Dupont company, in this country, and the big chemical company in England, whose name I can't seem to remember, but it's a well-known… (All I can think of is EMI, but that's an electronics company.) Anyway, the point is that to the extent that any explosives were being pushed it was this pentaerythritol tetranitrate. RDX stands for Research Department Explosives and the Research Department means the Research Departments at Woolwich[2] in England this time, on the eastern edge of London.

LH: Do you know what context the development of explosives in England took place in this period? Why were they investing money in this area in England? If you happen to know.

DM: [0:16:16] I think there was a feeling—correct—that the old standby, TNT, something more powerful could be developed and they worked on it.

LH: In England, the major center was this company whose name you can't remember?

DM: [0:16:43] No. The major center for RDX development was the Research Department at Woolwich, which is part of… Of course, in England they changed things around a lot. I think it was perhaps in the War Department. Eventually, it was under the Ministry of Supply. During the war, you may or may not know, there were a lot of changes within the British government. But it was a government. That's the main point. Research Department at Woolwich was an instrument of the British Department.

LH: Was RDX the main development there or did they work on other explosives?

DM: [0:17:40] They probably worked on other ones too. I think they worked on titro… What is another explosive? But I think that their principal activity and principal success was RDX.

LH: Where was composition B –and composition A—developed?

[2] Royal Arsenda, Woolwich: https://en.wikipedia.org/wiki/Royal_Arsenal

DM: [0:18:11] Composition B is basically RDX, TNT with a little wax put in which a little bit decreases its sensitivity. There's really not much of a development. RDX is very high [brisance] TNT is fairly low [brisance]. So, you make a slurry. RDX stays a solid. It's not particularly soluble in TNT. That's what composition B was. Composition A was a follow on to a development in England. In fact, one of our first tasks had to do with the fact that the British had developed as a shell filler—this was a sort of a semi plastic. We later called it composition A. They simply called it SPE—semi plastic explosive. It was 91% RDX and 9% beeswax. In retrospect, or in fact long before the end of the war, we were sort of aware that there was nothing special about beeswax. That was our first job. The bees were not turning out enough beeswax to make all of the stuff they wanted, so one of our jobs was to get a substitute for it, which we did. It turned out that most any mineral wax with a little polar material—as with hydroxyl groups or something like that—would be a completely adequate substitute. I didn't have very much, but it was.

LH: Am I right then that both composition A and B were developed in England?

DM: [0:20:28] Yes. Both composition B and composition A are American names. Composition B scarcely had development. It simply added RDX to maul TNT and put a little bit of wax, which had a detectable but pretty small effect on the sensitivity.

LH: Any other principal explosives that were developed? What about Baratol, torpex, some of the others that come up in this Los Alamos literature?

DM: [0:21:05] Torpex was an explosive developed in England, which was RDX, TNT and aluminum powder. As far as I know, the British were the first ones to realize that putting aluminum into explosives, which ties up… The aluminum reacts with any oxygen in some of the other materials there. So, you act to get less gas, but you get a lot more energy because aluminum reacts with any kind of oxygen and gives off a lot of heat. There was a limit on how much aluminum put in, but for a while the potency of the explosive [would] increase. This was somewhat of a surprise. I remember personally doing some experiments April and May of '41, I think, in a pond that was in Bruceton, investigating the effectiveness of various explosives as underwater things. In fact, I've got an ashtray in my house, which is… We had essentially a big pipe, a short pipe with a thin copper plate across it and put this some known distance from the explosive. Then, the shock wave in the water would dent this plate and by measuring it you got a good indication of the potency of the explosive. Also, a lot of these were given away as souvenirs to all sorts of people. They are all over the country now.

LH: What about, since we're discussing this, others that should be mentioned?

DM: [0:23:21] I guess the other area, what's called plastic explosives, which is very much in the news these days with terrorists… The British had developed… Again, this was an RDX composition. I forget exactly what it was. It was oil plus thickener. This was to be used in demolition work and so on. The point of it being plastic was that you could put it where you wanted it next to a target, rather than the way the engineers were doing in this story, simply blocks of explosives, which of course did their job, but it took more of them to do an effective job. One of the things we worked on a lot was again plastic explosives that had various properties we hoped at least improved over the British products.

LH: What about Baratol?

DM: [0:24:45] Baratol, that is something quite different. One of things, in fact the first experiment as far as I know at least in this country investigating the use of explosives lenses in an implosion type of device… You have a lot of detonation points around.

LH: We're going to get to that, but tell me…

DM: [0:25:18] Baratol[3] was first, as far as I know used… I forget whether we used that at Bruceton[4]. Since you've read all these reports, you may know more than I do about it.

LH: [0:25:34] I'm interested in your memories, because some things don't jibe, actually. For example, Kistiakowsky[5] mentions in one of his recollection pieces that Baratol was developed at Bruceton. However, I came across a document in the explosive lens patent file, which indicates that it was used earlier in England.

DM: [0:26:03] This may well be. I don't think we knew that. From the best of my knowledge, the explosive lens was actually the idea of a woman, Elizabeth Monroe Boggs, at Bruceton. The first experiment that I know… Now, we had a lot of interchange with the British, but change of information is never perfect or complete. That, I think, was independently. It probably had been done. Once you start thinking about it, it's not a very difficult idea to come up with. It's just as far as this country is concerned, the first explosive

[3] Baratol: https://en.wikipedia.org/wiki/Baratol
[4] Experimental Mine, US Bureau of Mines: https://en.wikipedia.org/wiki/Experimental_Mine,_U.S._Bureau_of_Mines
[5] George Kistiakowsky: https://en.wikipedia.org/wiki/George_Kistiakowsky

lenses with Baratol were at Bruceton, but there may well have been some in England before that. That I just don't know.

LH: One question about organization. What was the relationship of the Division 8 of the NDRC to the new Bruceton lab?

DM: [0:27:27] As I told you, the first organization was National Defense Research Committee, with Vannevar Bush at head and Conant was head of Division B and Kistiakovsky was head of a section of Division B. Fairly early in the game—I don't remember just when, I'm never I ever understood the details—NDRC itself apparently could not let contracts. So, the NDRC was converted to have a kind of advisory and OSRD, the Office of Scientific Research and Development was created under Vannevar Bush. Conant was in the NDRC and at least the section that had anything to do with explosives was converted to Division 8 of ORRD. Now, not all explosive work was done there, because there was also a Division 2, which had to do with things like air blast and some of the performances of explosives rather than with the explosives themselves. Anyways, Kisty was chairman. I'm not sure that was his exact title, but he was head of Division 8 and the main activity was Bruceton laboratory. Well, that's not quite fair, because there were a lot of particularly organic chemists in the country, especially Doctor Bachmann at Michigan, working on better ways of synthesizing RDX[6]. In fact, the contribution of people like **MiBAY?** Was probably in getting these things into a useful form and promoting their use. The really big development was that Dr. Bachmann and some of his people developed a method of making RDX from a compound called hexomethalene tetramene, which got twice as much RDX from the starting point as the so-called Woolich process, which was a direct titration. I'm not an organic chemist and I probably did know at one time the details of the Bachmann process, but I don't remember now about things like acetic anhydride and so on. But the main thing was that there was essentially a factor of two. In fact, for quite a long time the yields that he was getting were described as 160% and things like that, because it all went back to the process that the British had used. 100% would mean you got as much RDX as possible from the hexomethalene tetramene, while in the Bachmann process theoretically you got twice as much. You actually never got quite that much, but you did get 80% and 90%, and for a while you talked about yields of 160%, 170%. The first plant in this country was the Walbash River Plant, which was in Terre-Haute, Indiana, was built and operated by Dupont Company and simply used the British process, the direct titration of hexomathalene tetramene[7]. Later, but still fairly early in the war, as a result of NDRC work, the Tennessee-

[6] Werner E. Bachmann: https://en.wikipedia.org/wiki/Werner_Emmanuel_Bachmann
[7] Wabash River Ordnance Works: https://en.wikipedia.org/wiki/Newport_Chemical_Depot

Easton company, we contracted with them to build a plant in Kingsport, Tennessee, using the Bachmann process, which they did and very successfully. I remember that plant—I had been there many times during the war—used to have signs: "CBMQV". Composition B means quicker victory. Go ahead.

LH: I guess we'll move on now to early Los Alamos, which is what I am primarily interested in. It seems that in the early part of the program… This is before Kistiakowsky joined first as a part-time consultant in October '43 and a full-time staff member in February ['44], as Parsons[8]'s deputy on explosions. In this earlier period, explosives were not being looked at Los Alamos in a systematic way. I think they were only used in connection Neddermeyer[9]'s rather small and informal implosion effort on South Mesa.

DM: [0:33:46] You're talking about activities at Los Alamos?

LH: I think that there was almost nothing, except for this little effort around Neddermeyer. I don't know what they would use it for otherwise. Gun development didn't require explosives, did it?

DM: [0:34:03] Propellers, but not high explosives. Gun development was... A gun shooting… Actually, it shot the target to the projector rather than the other way around, but that was the sort of assembly. The situation with respect to implosion, as best I know, is somewhat as follows. The gun assembly looked very straightforward. In fact, as you know, the Trinity test was the first test of any kind of a nuclear detonation, but it was an implosion. It was told, I think, that that would probably work. And if it worked, then the gun assembly was sure to work, and they didn't even test it. As you probably know, the non-assembled bomb that was dropped on Hiroshima had never been tested as a nuclear assembly. There had been experiments, of course, using non-fissile material. Some people, I think G. I. Taylor was one of them, felt that the implosion technique was very risky, because instabilities would occur, and you would never get a decent implosion. The thing I believe which change this was the realization that because of the presence of plutonion-240 and 239, the neutron background was very high and therefore a gun with plutonium would always pre-initiate and you would get a kind of fizzle yield.

LH: That happened in the spring of '44, that plutonion-240 crisis. They began to suspect that around April, but the conclusive proof did not come until July because it was a very low counting rate experiment. However, the

[8] William Sterling Parsons: https://en.wikipedia.org/wiki/William_Sterling_Parsons
[9] Seth Neddermeyer: https://en.wikipedia.org/wiki/Seth_Neddermeyer

laboratory started work on implosion almost a year before. First, a very small effort around Neddermeyer, which was just a few guys blowing shells together.

DM: [0:36:55] The point is that Neddermeyer never had a good idea. And I think that some of the theoretical people were right to some extent. It was a riskier, more difficult approach. Until this problem with 240, it was not the first priority. Once that was realized, you're certainly right… You've been studying all these reports and I haven't. What I know is that my first visit to Los Alamos was May 1 or '44 and by that time work on the implosion was well under way.

LH: By that time, although the crisis had not reached its climax, people were certainly worrying, because by April the Clinton plutonium was being put into the ionization chamber—it's the Grays group—and they were seeing some counts already right away. I don't remember which date, but the first four counts, which came in April, they suspected it.

DM: [0:38:27] Of course, since you've been reviewing ancient history, you're much better informed. In my case, the laboratory was not nearly as compartmentalized as general Gowles would have liked, but it still was to a certain extent. I believe that the specific behavior of plutonium 240 and the effect that that had on the gun program was mostly not know to me. I was aware of the implosion program, and that was why Kisty was brought out here, why soon after that I was made a consultant and we started a program at Bruceton, but a lot of the details were not known to me.

LH: What I'm really trying to get at—I'm being very long-winded about it—is that the laboratory started taking implosion very seriously long before the plutonium 240 crisis. What I've learned from the documents and the people I've talked to is the following. Neddermeyer got this good idea and presented it in April of '43 at conferences that were held here. And not too many people were interested in the Neddermeyer concept. The novel element in that concept is that you would bring in continuously a plastic deformation, the shell of the material and make it supercritical. There had been some earlier implosion-like concepts around. For example, Tolman had one, in which pieces of material were put around in a spherical configuration and were expected to be blown together. But Neddermeyer suggested actually using a shell.

DM: [0:40:44] That was, of course, not what was really done.

LH: No.

DM: [0:40:50] The basic design… I think Neddermeyer was responsible for the basic idea of using…

LH: A shell.

DM: [0:41:04] But that was not used for years after the war.

LH: The idea was not suggested by **Christie(?)** until September of ’44.

DM: [0:41:14] My point is that… I think to the extent that Neddermeyer had a useful suggestion it was in using high explosives rather than a gun. Because actually we did not design a weapon using active material in the shell until into the 1950s.

LH: Yes. Because it was dropped. Because of the **Christie(?)** idea. But the question I had about Neddermeyer’s work, which just consisted—from the document—of him and **John Stride(?)** and Bradner[10] and maybe McMillan[11] helped a little and maybe Pritchfield a little bit. But that was it. Do you know what explosives he was using? At that point, the high explosives art was very primitive compared to what happened over the course of the war. There is a reference to his visit to Bruceton in that early period, together with MacMillan.

DM: [0:42:35] It’s taking into Bruceton, I would guess—again, you have some records—about November of ’42.

LH: ’43.

DM: [0:42:49] No, ’42.

LH: ’42?

DM: [0:42:53] Yes, McMillan and Neddermeyer were at Bruceton[12]. They were…

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[10] Hugh Bradner: https://en.wikipedia.org/wiki/Hugh_Bradner

[11] Edwin McMillan: https://en.wikipedia.org/wiki/Edwin_McMillan

[12] Both years 1942 and 1943 seem plausible. See, *e.g.*, “Edwin McMillan’s Lecture,” *Los Alamos Laboratory* (2016). https://ahf.nuclearmuseum.org/voices/oral-histories/edwin-mcmillans-lecture/ (Accessed July 24, 2023.) Kistiakowsky favors 1943: “The man who deserves full credit for developing the concept of implosion, necessary to explode a plutonium weapon, is S. Neddenneyer. He and his assistant visited our NDRC Explosives Research Laboratory in Bruceton, near Pittsburgh, in the summer of 1943. We made the first implosion charges for them, fired them off, and the visitors went away rather pleased with themselves and with us.” G. B. Kistiakowsky, “Reminiscences of Wartime Los Alamos,” In: *Reminiscence: of Los Alamos 1943-1945* L. Badash, J. O. Hirschfelder and H. P. Broida, eds (Boston: D. Reidel Publishing Company, 1980): 49-65.

LH: If that's true, then…

DM: [0:43:05] If you said: "No. I know they were there on the 28th of October." I would say "Okay".

LH: But '42 is long before the Los Alamos project.

DM: [0:43:14] Right! That's what they were there for. They were trying to get my advice—partly a matter of terrain. In fact, they had a very vague statement. "Supposed you were trying to do experiments with high explosive and moving metal and so on, is this the kind of thing you could do here?" I'm this was before not only Los Alamos was started, but before the final decision had been made to make it at Los Alamos. Some of these things, of course, I realized later. They didn't come here and say: "Development of the nuclear weapon is…"

LH: If that's true, I wonder if there would be any way of document, because that would establish Neddermeyer's interest in this sort of things well before the idea of the implosion was suggested by him at Los Alamos. He didn't make the suggestion at Los Alamos until April '43.

DM: [0:44:29] I don't know what the record shows. I am virtually certain it was before Los Alamos was started and most of the questions had to do with generalities. That is: "We're thinking of a laboratory that has to do the following things…"

LH: So, he was asking general questions about high explosives. See, there is a visit that they did make. I don't have the dates here; I could look it up. I think it's something like the summer of '43, where they went and they actually blowed in some shells and things. I think we've got some documentation—it's not very good documentation—of a visit then. But they may have…

DM: [0:45:29] Of course, work had been going on at Bruceton long before Los Alamos had started. The specific program there that we called project Q started only after my visit here May 1st of '44, which followed on Kisty coming out here full time. Not completely cutting his ties with NDRC, but obviously with a full-time job here, he was… In fact, I guess maybe Louis Hammett perhaps took over as the head of the division. That, I'm a little vague about.

LH: But do you remember a visit… This is of Neddermeyer and McMillan, very early, before Los Alamos and asking very general questions about what could be done with high explosives.

DM: [0:46:28] Yes. This had more to do with terrain and so on.

LH: Do you remember any actual experiments during that visit?

DM: [0:46:41] No.

LH: Just discussions.

DM: [0:46:44] They of course did not say what their objective was, but I think they clearly indicated that there were some people involved in selecting a good place to do some experiments, and they wanted my comments on what sort of place might be useful. At that time, as far as I know, I had never heard of Los Alamos.

LH: Nobody else did either at that point except maybe Oppenheimer.

DM: [0:47:29] Oppenheimer certainly had. **[28 sec of CUT MATERIAL]** Not true, but he did have a ranch over in the Pecos[13]. In that way, he was familiar with this part of the country.

LH: Right. Do you remember another visit a bit later from Neddermeyer and MacMillan, after Los Alamos had started, after the experiment.

DM: [0:48:23] I don't specifically. Of course, if they came during the summer of '43… The only visit that I clearly remember is one in late '42.

LH: Do you know which explosive Neddermeyer began using when he started? I mean there's no reason why you should.

DM: [0:49:03] One thing I can say is that when I was out here in May of '44, quite a lot of experimentation was being done with Pentalate, which is the PETN analogue of comp. B, as it's PETN and TNT.

LH: I see. So, likely, he was using that.

DM: [0:49:26] It may well have been. Although I do remember that S8 I think first got into action while I was here in May of '44. And I remember that a cylindrical charge was made, a pretty charge, I think it was 500 pounds or

[13] Oppenheimer had a ranch in the upper Pecos since 1928. See, *e.g.*, R. F. Bacher, "Robert Oppenheimer (1904-1967)," *Proc. Am. Philos. Soc.* **116**, 279-293 (1972). https://www.jstor.org/stable/985898

more than that and it was fired at a firing point that was called Anchor Fire Point[14]. You perhaps know roughly where Anchor Ranch is, or don't you?

LH: No, I don't. Where is it? I've seen it in the literature a lot. Is it on the way to Waco?

DM: [0:50:14] No. Do you know where the back gate is?

LH: Yes.

DM: Do you know where S side is?

LH: No. Maybe you ought to give me a little bit of education where the various sites are.

DM: [0:50:30] There is a road now, completely open to the public, which goes from Los Alamos proper and basically south to the back gate, where you can go on to route 4. In fact, if you turn right there, you go up the hills and into the via Grande and finally to Jemez Springs. Anyway, Anchor Ranch is roughly speaking at least between the town side and the S side, the town side and the back gate.

LH: I see.

DM: [0:51:20] Earlier, you mentioned South Mesa. That was an explosive site. In fact, it was one of my sites for detonator work. But it was taken over. In fact, it's where the main administration building is now, that's TA-43.

LH: Anyway, Neddermeyer worked for a little while and then a bit priority increase for the implosion work came in late September of '43 or October ['43]. Fall of '43. John von Neumann visited in that late September-early October period, and he looked at what Neddermeyer was doing, and he said that that wasn't the way to do the implosion. He said that in the Neddermeyer concept, it would just blow together until it would become a supercritical mass. [von Neumann] suggested a faster way of bringing it together, so that there would actually be compression of the material and that would lead to much greater efficiency.

DM: [0:53:22] Of course, John von Neumann had been a consultant to us at Bruceton long before the project was started.

[14] "Gun Site," *Manhattan Project National Historical Park* (2022). https://www.nps.gov/places/000/gun-site.htm (Accessed July 24, 2023.)

LH: That's what I want to ask about. Because this is a very key turning point in the program. It led suddenly to a great deal of interest all over the laboratory. In particular, Oppenheimer, Teller, Bethe, among others, got very much interested in the implosion now that von Neumann pointed out this faster way involving compression. This then led the laboratory to put a lot more people into the program, and they began to design various diagnostics and flash X-ray and so on. You know, photographic methods. The other more esoteric ones came later, RaLa, magnetic pin and so on. At this time, October of '43, Oppenheimer wrote a letter to Kistiakowsky encouraging him to come. Apparently, he was reluctant at first, but Tolmann said he should do it. So, at first, he came as a consultant and later as a… So, there was kind of a turning point. It still didn't become the first priority, and it became a much greater effort here.

DM: [0:54:55] This is my understanding. Of course, I was not connected with the program at that time.

LH: I'm just trying to fill you in on some of the things that I've looked so far. In one of the documents that Kistiakowsky wrote, LA-1043, he mentions that at that time late September '43-early October '43, the time he was invited, he said: "Several men experienced in explosives research were brought to the project from Division 8 of NDRC." Do you know anything about people from Division 8 coming to the lab and possibly who these people were? I haven't been able to track that down yet.

DM: [0:55:45] I am not aware—as far as I know, but there were things that I didn't know…

LH: It could be wrong too.

DM: [0:55:59] The first Division 8 person who came out to the lab physically was G. B. Kay. The second one, I think, was me. There were people that came after me, and came here and stay, like Wayne Campbell, for example.

LH: When did Wayne Campbell come?

DM: [0:56:27] I'm not sure. Sometime during '44, I think.

LH: Ok. I haven't spoken with him yet.

DM: [0:56:39] See, I was never an employee here. I simply came out. I guess my first visit was the longest when I was here about 10 days in May of ['43]. I guess from then on, for the next year or two, I came about three of four times a year for a few days. I can't remember now. One of the people who

was involved in the **HA** experiments here was Henry Linschitz[15], whether he was at Bruceton for a while. I know him very well, but I can't remember for sure whether he came here from Bruceton or not. Wayne Campbell, I know did.

LH: Tell me a little more about both of these. If I had infinite amount of money, I'd interview everybody who I could, but I'm going to have to be a little selective. Although I'd like to interview both of them, I'm not absolutely sure I'm going to get to them. If you could just give me a little bit more information about them. Also, your opinion on whether or not I ought to make an effort to interview these people.

DM: [0:57:54] I would think. I don't know where Henry Linschitz is now. Do you? He's probably around somewhere. Wayne Campbell is here. He was the last time I looked.

LH: What was his role?

DM: [0:58:17] He was… I guess I'm not familiar in detail enough with how the organization at the lab changed as time went on. I know that at the time I came here permanently—of course, that was in 1948—Wayne Campbell was the group leader of what was called X8—and shortly after I got here was called GMX-8 and was particularly the group that studied at close to **non-** using photographic techniques. So, I would think that especially you wouldn't have to spend any travel money… In fact, the last I heard, which was I think a few weeks ago, is that while Wayne is retired, I guess he still goes to work quite regularly. In fact, some of the [his successors] say: "The only difference is that he now gets to work at 9 o'clock instead of 8 o'clock." He was definitely at Bruceton, and he came out here before Bruceton closed up. But, obviously, he's a much better source of when exactly he came than I came. Harry Lynches, I remember very well, but I can't remember for sure whether he was at Bruceton.

LH: What did he do?

DM: [1:00:15] There were quite a lot of work done in plane mockups. There is a **bell lens** and main charge but say an inch or so thick and plane instead of spherical waves. A lot of these, I think, the diagnostic was the effect on a heavy metal plate, as you could tell something about the interaction of the detonation wave and so on. Again, Henry would be the… I think at that time

[15] See, e.g., Interview of Henry Linshitz by Steven Heims on 1988 February 23,Niels Bohr Library & Archives, American Institute of Physics,College Park, MD USA, https://www.aip.org/history-programs/niels-bohr-library/oral-histories/5039

they had not yet actually made any spherical implosion. I think Walerkovsky perhaps was the first one who did some diagnostics on actual spherical charges or partial spheres.

LH: Let's go back to von Neumann and his connection. Since he's not available and since his visit to Los Alamos was so pivotal in this implosion story, it would be interesting to know a little bit about his background. What gave him the know-how at that point to make this very pivotal suggestion.

DM: [1:01:45] You got me there, because I don't know what his suggestion was that was so pivotal.

LH: Just to use shape charges somehow to make the implosion go much faster and then cause compression which would then make a much more efficient device.

DM: [1:02:10] Since I was not in on the program at that time, I don't know.

LH: It's background that I'm asking about. You said he was at Bruceton.

DM: [1:02:23] He was a consultant.

LH: He was consultant, I see. What did he consult on? Was there any special thing?

DM: [1:02:32] It particularly had to do with theoretical background with respect to detonation waves and shock waves.

LH: Do you know anything about shape charge work that he had done?

DM: [1:02:47] It depends on what you mean. Every charge, of course, has some kind of shape. What is usually meant by shape charges is the use of hollow cones to get a lot of… I don't think that had much to do with development at Los Alamos. We did have a long program on shape charge development at Bruceton and discovered and copper was a considerably better material for the cone and so on.

LH: I see. Anyway, then Kistiakowsky was brought in. Was he the natural person for Oppenheimer to bring into the project? Or was there some special reason why he was chosen?

DM: [1:03:52] Well, he was a very smart guy, and he was head of the division in NDRC (or OSRD) involved in high explosive research, both the chemistry and physics. I'd say it was fairly natural.

LH: Were there other people in the country who might have been selected?

DM: [1:04:26] We were all really kind of self-taught. As I said, the Picatinny Arsenal said we were just a bunch of long-haired professors who were going to kill themselves.

LH: Conant was involved with Los Alamos; did it perhaps have something to do with Kistiakowsky's relations with the Conants?

DM: [1:04:50] It could. It did, or course. The Conants and Kisty were quite close. Conant was the one who got George to come to Harvard from Princeton. In fact, I remember George telling me one time that he was talking to Conant about the possibility of coming to Harvard. Kisty said: "Harvard is kind of a snooty place. I don't know that I would sort of fit in." According to George, Conant said: "Well, if you don't try commit rape on Cambridge Common, you'll probably be alright." That was. All of those things fit together. None of us were explosive experts at the beginning of 1940. We were all sort of self-taught.

LH: Do you know who introduced the x-ray method of examining charges? I gather that had a history before its use in Los Alamos.

DM: [1:06:10] Oh yeah! The first person that I'm aware of doing experiments like that was Jack Clark at Aberdeen.

LH: About when?

DM: [1:06:25] '42, maybe. It was before Los Alamos. He is a PhD physicist. (I don't know if he's alive now. Some are over the line, I think.) I don't remember when he came out here, but I know that the flash x-ray machine he had, which I guess was… The machine itself was I think a General Electric development. I'm not sure about that. But I do know that Jack H**aydn(?)** was at Aberdeen a number of times on visits before Los Alamos. We were sort of working for the Army and the Navy. There were not specifically our bosses, they were our sort of customers. At least, we hoped they were customers for what we could provide to them.

LH: The early methods that were suggested immediately by Kistiakowsky in the fall were: x-ray, flash photography and terminal observations. It was only after a while that the other methods were suggested. That made me think that those three must have been around before. So, X-ray certainly was around. What about flash photography?

DM: [1:08:02] We had been doing that at Bruceton. I forget now just… We developed there, I think, these argon flash bulbs certainly before Los Alamos was started. I don't remember the details. This could be found out from Bruceton reports. I just don't remember off-hand.

LH: I just wanted to establish that it had a history before. Terminal observations: what are terminal observations?

DM: [1:08:38] That was what I was referring to, particularly in these two-dimensional things. Instead of a sphere in two dimensions, you have a piece of Baratol. Now this could be a cross section of a spherical thing, but let's just say it's two dimensional in the sense that in this direction it's perhaps an inch or two inches thick. Then, you have it on a heavy metal plate, so you can deduce from what happened the behavior of the detonation wave: where it interacted and where its focus was. By picking up the plate afterwards, that's what's meant by terminal observations.

LH: You do something and then you examine what remains.

DM: [1:10:00] You examine it over the next minute, hour, days, weeks. It's just there.

LH: It's not something where you do something while it's happening.

DM: No. Terminal observation means that's something that you pick up the piece and examine and deduce things from it. Not to be confused with terminal illness.

LH: [1:10:40] In one of the Kistiakowsky documents, he says that the Bruceton Explosives Research Laboratory Division 8 at the NDRC was drawn into the work on implosion early in 1944, and then from July '44 to June '45 participated formally as Project Q in charge of D. P. MacDougall, G. Messerly[16] and E. Eyster[17].

DM: [1:11:14] What initials did you give Messerly?

LH: G. Is that wrong?

DM: [1:11:19] No. I thought you said R. G. [It's] G. H. Messerly. I probably misheard.

[16] George H. Messerly (1911-1981)
[17] Eugene Henderson Eyster (1914-2013).

LH: [1:11:38] I gather Kistiakowky was the person who was instrumental in setting up this Project Q.

DM: [1:11:45] Yeah. He and I together.

LH: [1:11:51] I wonder if you could expand on how this Project Q was setup and how it functioned.

DM: [1:12:00] Well. Eh.

LH: [1:12:04] We have some of the technical reports here. I don't know if that that would help you to trigger your memory.

DM: [1:12:12] No. I remember this alright. A lot of it was by separate discussions between GBK and me. At that time, we had a good deal more capability in what you might call explosive or detonation physics, detonation phenomena than they had out here. So, we agreed to put—I don't remember really what fraction of the effort—maybe it was 20% of the Bruceton effort on explosives that was devoted to things of particular interest. As far as I know, there was never any financial… Money, of course, was not any problem in those days.

LH: [1:13:20] But how did it function this collaboration?

DM: [1:13:26] In two ways. People, of course, could guess what they wanted to, but officially some of my people at Bruceton were working… I guess in a lot of cases the same people were doing some experiments of specific interest to Los Alamos and some other interests.

LH: [1:14:01] What were these, some of the things that were [done]?

DM: [1:14:05] Well, the first log spiral lens was designed by Elizabeth Boggs [at Bruceton] and fired at Los Alamos.

LH: [1:14:15] Do you know who thought of the log[arithmic] design? Who thought of the design of the logarithmic spiral?

DM: [1:14:22] There may have been other people, [but] in this country it was Elizabeth Boggs. Once you set your mind to it, it's no great thing. I believe that simultaneously in England they also thought of a logarithmic spiral. But the first design and the first experiments were done at Bruceton, and the design was from Elizabeth Boggs. She was basically, I think, a theorist rather than an experimentalist, although basically she was in charge of Messerly's group there.

LH: [1:15:09] I see. I'm always happy when I see a woman involved in something like this. Could you tell me just a bit more about her? There are so few women in this whole story.

DM: [1:15:25] She was very, very capable, I think. Her husband was also a PhD. I think she was probably smarter than he was, but that's probably unfair. I didn't really know him very well. She had been… I don't think she was a Rhodes scholar, but she had studied in England, I know. I forget where her PhD was from. I remember her telling me of some occurrence in England, and what she used to be introduced as Miss Monroe. So, obviously… Her full name was Elizabeth Monroe Boggs, and exactly… But her life story, I would guess, is in American Men of Science—or, sorry, American Men and Women of Science[18]. (For many years it doesn't have, although it had women in it for quite a long time.)

LH: [1:16:30] Right. I want to take a two-minute break. Kistiakowsky says in the same report that I was quoting from before that already in the late fall and winter of '43 certain parameters were selected, such the approximate weight, the maximum diameter and length, so that the design of the bomb bays of the B-29 planes could proceed, and so that the engineering staff could begin on case of tail design, release mechanism, and bomb ballistics. I was wondering whether these early choices might have led to any problems vis-à-vis the explosive development. They fixed certain things concerning the size, whether that would have limited the explosives work.

DM: [1:17:55] I don't I don't. I was picked pretty big. In fact, all of the future development involved getting smaller and smaller things. So, this thing was about five feet in diameter, and I don't think that caused any particular trouble.

LH: Well, the next thing that happens then it in the Los Alamos story is that… Neddermeyer was working in the earlier Ordinance and Engineering division, E-5 was his section. In the early spring, that began to expand and get more structure in it. It was divided into a number of sections. Bradner was put in charge of flash x-ray work. Kosky was put in charge of flash photography and Patapoff in charge of rotating prison camera; Strive in charge of data analysis; and Henry Lynches is in charge of terminal observations.

[18] American Men and Women of Science: https://en.wikipedia.org/wiki/American_Men_and_Women_of_Science **PC:** She was not!

DM: [1:19:41] That's what I was telling you about, these plane waves experiments.

LH: D. Busby in charge of S side and Jay Fitzpatrick in charge of maintenance and service.

DM: [1:20:07] The last two mean nothing to me.

LH: Then, Bainbridge[19] was pulled in, and he was put in charge of a division which eventually grew into the Trinity group.

DM: [1:20:20] Ken Bainbridge, I remember the name.

LH: His group was divided into sections under R. W. Henderson, W. Shaffer and L. Puffle. Furthermore, Lyme and Parrots Group, which was an instrumentation group, was brought into this this work. Then, eventually this effort after the reorganization that took place here in the summer became Expedition. Now, we're at the time that you started coming to Los Alamos, when these changes were we're being made in the E division that that eventually were mapped over into X division. I'm just wondering whether, thinking back on your early visits to Los Alamos, whether there's any more that you can remember about this effort, because this is probably the effort that you were exposed to when you came here.

DM: [1:21:49] I was certainly completely exposed to the experiments they were doing. I don't believe I was particularly aware of organizational details.

LH: Maybe you can just give me a very quick overview of this work. I mean flash photography is simply just like when ordinary flash bulb shot people. This is technique I gather where they just took many flash shots and then tried to deduce from this what [was going on]?

DM: [1:22:38] Yes. That, of course, was one of the problems, to get meaningful results. That allow the shots had to be as nearly identical as possible, because you would take pictures at different times after you started but the only way you could interpret them was that they were the same thing at different times. Although, of course, they weren't exactly at the same time.

LH: And Flash x-rays is the same thing except using x-ray?

DM: Hm.

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[19] Kenneth Bainbridge: https://en.wikipedia.org/wiki/Kenneth_Bainbridge

LH: And rotating prism camera do you know what that?

DM: [1:23:25] I don't think I ever completely understood why they did that. A rotating prism was sort of a triangular thing with three mirrors coming out to a point. That's why they call it a prism. This thing would spin, and so this was different from either flash x-ray or flash photography. It would take pictures as a function of time, but they were all else of me the devil, because it was a peculiar sort of motion. As these three-sided prisms, and we fairly soon went to simpler rotating mirror of just a plain mirror. I guess there was a time maybe when they believed that they could get higher speed. Of course, this whole thing only took a couple of years. Nowadays, you can't do much of anything in the time that serves as two years. It takes forever. Those days, things were done pretty fast. The rotating prism camera was a predecessor of the rotating mirror, which was simply a plane mirror which made a very easily understandable record.

LH: Do you know if that had a history of before Los Alamos, the rotating prism?

DM: [1:25:33] Certainly rotating mirror was fairly old stuff. We had rotating drum camera.

LH: And that's different?

DM: [1:25:45] That's different, yes.

LH: Tell me what's different. That's something else that comes up in the literature too.

DM: [1:25:59] Imagine that you have a cross sections somewhat like that, but it's really a thing of revolution. This is the axis on which it spins, and it has it has a little track in here, and 35 mm film is put in there. In this case, the film moves. There's a whole loop of film and with a suitable array of lenses and mirrors the image of the detonation wave is focused on the film, so that you get a curved bright line running long like this, where this is, say, distance and time.

LH: Physically, where does the implosion take place?

DM: [1:27:20] Oh. Off here somewhere. This is not something you blow up each time. This is a substantial thing. I forget now, but it was maybe a foot in diameter. You couldn't get at high speeds going and the like. The rolling mirror camera, the film stayed stationary, and the mirror rotated, so you got effectively the same effect. That is, the image of the detonation or the

shock, whichever it was you were studying moved because of mirror turn. The film was stationary and this way you could get… It seems to me with the rotating drum camera, time intervals much less than a microsecond were pretty hard. This would be cm in length and quite a few microseconds. With the mirror camera, the mirror be a lot smaller, so it was easier to get it to rotate faster. The arm, then, was just an arm of light, rather than in this case the arm sort of was this whole aluminum casting.

LH: And the rotating drum was used at Bruceton?

DM: That was used at Bruceton, yes.

LH: And probably before Los Alamos.

DM: [1:29:08é I think so.

LH: While we're in the diagnostics, I was wondering if you could tell me just a little bit about the other the other four methods which then had large developments associated with them at Los Alamos: namely, the electric pin, the magnetic, the Rolla and the betatron[20]. The electric pin method, I gather, was one in which the metal itself moved in and contacted electrical pins, little posts that were set up. I don't know how many, but a lot of them.

DM: [1:29:52] A lot. I'm not sure myself.

LH: And then they would complete circuits.

DM: [1:29:57] Yeah, and when the pin was contacted, you got a blip on the scope.

LH: I see. But the disadvantage there, I gather, is that one could not use complete spheres there.

DM: [1:30:11] Obviously not, because you had to… I forget now what. Maybe we would have perhaps three-quarters of a sphere and then and there is a heavy pipe and so on. In this case, with the pin method, of course everything was destroyed. But by having the cables protected by a good deal of metal, you keep them intact long enough to get the blips on the scope.

LH: The reason that it was impossible to use spheres was that you had to have a place to leave.

[20] Betatron: https://en.wikipedia.org/wiki/John_von_Neumann

DM: [1:31:06] Right. You not only needed to have a place, but also protection so they would at least stay for a good many microseconds before being destroyed.

LH: The magnetic methods, which I don't understand yet, somehow caused the motion of the metal caused…

DM: [1:31:28] I never had much to do with that.

LH: The Rolla, I understand. That had a source of gamma rays in the center and chambers outside to capture the gamma rays, so one could tell something about the amount of metal in between and the density. Then, the betatron method, I don't know anything about it except, I guess it has something like the X-ray.

DM: [1:32:00] It's essentially a flash x-ray.

LH: They used the betatron to produce higher frequency x-rays. Is that what was done there? I have some reading to do here.

DM: [1:32:19] I guess so. I'm not… This was going on, but I think had pretty well subsided by the time I came. See, I came out here full-time only in 1948. From then on, I presume I knew what was going on. I think it was basically flash x-ray. I think it was easier to get it short. I know that in the original flash x-ray machine that Jack Clark had at Aberdeen, the length of a pulse was short in comparison to something, it seems to me it was a microsecond or so. You have things that are moving though a centimeter per microsecond, a microsecond then is pretty long. I think that in the betatron you could get shorter pulses. I think that was the main plot. But as I say, both this and the magnetic had pretty well disappeared by the time I really came out here and took charge.

LH: At a certain point, I don't remember when, they discontinued the x-ray method. But I don't know the story yet there. I haven't read all the relevant documents.

DM: [1:33:55] There was another technique, which was used and is still used, which we call PHERMEX[21].

[21] See, *e.g.*, D. Venable, "PHERMEX," *Physics Today* **17**(12), 19–22 (1964). https://doi.org/10.1063/1.3051266

LH: In what ways, did the PHERMEX machine supersede the other methods and also when did it come in? It was after the war.

DM: [1:34:25] Yes. We started on PHERMEX, I think, it was close to 1960. I have forgotten, but it was long after I had come out here. That was simply a way of getting a very short pulse of very intense x-rays.

LH: So, it was a development of the x-ray datapoint technique.

DM: [1:35:06] In fact, it works specially well, again, I don't remember just when this was. sometime in the '60s, I think, that we simply closed down the Rolla program.

LH: Okay, well, let's turn for a minute to the entry of the explosive lens into the Los Alamos program.

[TAPE 2]

LH: The patent that we have—you are welcome to look at it—says the invention was made at Los Alamos around May 5, 1944, by Chuck Neddermeyer and von Neumann[22]. I wonder if you know anything about this or about earlier developments on explosive lenses.

DM: [0:00:30] No. See, actually, that was why I was here. You said May 5, '44?

LH: Here's the record of invention, which leads to the big document. That says that this was conceived when you were here. I think this may be Tuck[23], I'm not sure. Apparently, he told Chadwick, Kistiakowsky and Oppenheimer about it and then developed it together.

DM: [0:01:39] I just don't have no particular comment. As far as I know, we were making explosive lenses at Bruceton before they were here.

LH: That's what I want to know. I don't know to what extent this was really an innovation.

DM: [0:01:58] I guess I don't know. I'm a little puzzled.

[22] John von Neumann: https://en.wikipedia.org/wiki/John_von_Neumann
[23] James L. Tuck: https://en.wikipedia.org/wiki/James_L._Tuck

LH: There's two interesting documents in this file. They are both British. This one is by Dr. M. J. Poole[24]. Have you heard of him?

DM: [0:02:30] Sure. I used to know him.

LH: Tell me something about him. [Silence.] What's interesting in here is that this lens—I guess it's not a spherical lens, but it's definitely a lens. It seems to use Composition B and Baratol, which is what was used here. Or maybe it's RDX.

DM: [0:03:25] I'm not sure what those things mean.

LH: I don't know it either, but there certainly does seem to be…

DM: [0:03:33] That's why I was fairly careful that to the best of my knowledge Elizabeth Boggs was the first person in this country. I wasn't aware of this, I think, until after the war, when we did see British documents. But I remember one time… Have you talked to Max Roy[25]?

LH: Yeah.

DM: [0:03:58] I think Max was the one who one time said he thought this was probably the earliest thing that had to do with the lens.

LH: Yes. I discussed this with him a little. I was trying to see whether you knew something about this as well.

DM: [0:04:16] Max knew Poole better than I. Max, during the war, was stationed in Washington, whereas I was at Bruceton, or I was all over the place, but Bruceton was my home base.

LH: He didn't tell me he knew Poole, but he did.

DM: [0:04:35] Oh, yeah, he did. In fact, Max is the one who said—this was fairly recent, the last 5-10 years—that he thought Poole was the inventor of the log spiral lens.

LH: I see. Is this log spiral? I can't tell from here.

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[24] Michael J. Poole. See, *e.g.*, Ferenc Morton Szasz, *British Scientists and the Manhattan Project: The Los Alamos Years* (London: MacMillan, 1992)

[25] "Max Roy", *Atomic Heritage Foundation* (n.d.). https://ahf.nuclearmuseum.org/ahf/profile/max-roy/ (Accessed October 26, 2023.)

DM: [0:04:56] I don't know.

LH: It may be. It doesn't look like a log spiral from the drawing.

DM: [0:05:20] No, it doesn't. Does it? Well, it may not have been this. It may be at some later time. I remember Max remarking that one time. I say this was moderately recently, like in the last five to ten years. Of course, Max Lives only a few doors from me. I see him occasionally.

LH: The other interesting document in here—I don't know what to make of this at all—is something on stationary of the British Supply Council in North America. This one does look like it might be a log spiral.

DM: [0:06:13] This is dated may of '44.

LH: This is about the same time as the Invention at Los Alamos was supposed to have taken place. Well, anyway. It's an interesting document. I don't what it means.

DM: [0:06:36] I don't either.

LH: I don't know why they are in that file. This is something that has come up in a number of interviews, but it may be a red herring. Do you know if there was a direct line from the idea of the shape charge to the idea of the explosive lens? Does it sound logical?

DM: [0:07:22] I don't think so. I mean they both involved… The simple shape is not much of a shape. Normally, it's a cylindrical charge that has a conical minor. I know that at some time—I don't remember when this was—we did things like putting maybe some inert material in here to, we thought maybe, shaping the wave somewhat would improve the severe… They're all sort of related, but I don't think very directly.

LH: It's a different concept completely, no? This is a way of increasing penetration.

DM: [0:08:33] Yeah. Well, this thing moves in and behaves just like a fluid and then you get a thin jet squirting out here. Some of the material, because of conservation of momentum and so on, relative to this moving axis stays behind, called the slug. But yeah, you're right, this is not directly related in any way to what the project here was trying to do, which was to bring in metal smoothly from some sphere. This was almost the reverse, trying to get a jet formed. In fact, while people knowing about this work thought we would have lot of trouble avoiding that in an implosion system.

LH: I didn't ask about the history of the shape charges. Does that go very far back or did that just start at Bruceton.

DM: [0:10:01] No, a fellow named Munroe—I don't really remember but maybe before 1900—observed that if you cut out a piece… I'm not sure he ever did any experiments with liners, but if you just took a charge and, say, cut out something like that, rather than having the effect on a metal plate less because there's less explosive it was more. This was called the Munroe effect[26]. I think at the beginning, he did things like carve initials in the explosive. And surely, if you put this on a plate, you could form these initials on the metal plate. But I think at that time this was it was just kind of fun and games.

LH: Where was this work done?

DM: [0:11:12] I think Munroe was American[27], but I've forgotten just where he worked.

LH: Los Alamos, to go back, it seems that the lens entered in May. By June, it seems that people were taking it very seriously already. I gather this only from the following that when I briefly spoke with Bradbury Baptist, he said that when he came, which was in late June—he was brought to Los Alamos, he'd been working with Parsons at Dahlgren[28]—and at that time he was brought in it was the feeling of both Parsons and Kistiakowsky was that the work on lenses was not going fast enough. That was only a month, but already then it was felt that that was the only way that they could make the implosion work.

DM: [0:12:17] There's one thing, which they had had tried. They had done some experiments. I guess they were mostly, again, terminal observations even with spheres, and initiated with what's called Primacord. Have you come across Primacord?

LH: That was the first detonator.

DM: [0:12:46] Yeah. This led really to the first explosive detonator program because it was felt that Primacord wasn't precise enough in setting off a spherical implosion.

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[26] Shaped charge: https://en.wikipedia.org/wiki/Shaped_charge
[27] Charles Edward Munroe: https://en.wikipedia.org/wiki/Charles_Edward_Munroe
[28] Naval Proving Ground in Dahlgren, Virginia: https://en.wikipedia.org/wiki/Naval_Surface_Warfare_Center_Dahlgren_Division

LH: Well, actually since we're on the electric detonator. Whatever you know about that story. I haven't looked into that in detail yet. If you have even an overview of that development, it would help me in orienting me. All I know is that they started with Primacord and then at a certain point. I know Alvarez plays some role here. He suggested, or perhaps it was others, …

DM: [0:13:50] The one who was put in charge of the detonator group, but I don't know just when was Hugh Bradner... But I don't think that… For example, at Bruceton we did do a lot of things related to lenses and so on, and various diagnostic techniques. I don't think we did anything. I was aware of this detonator activity, but I don't think we did anything about it at Bruceton.

LH: I'll try track down Bradner and talk to him about that.

DM: [0:14:32] See, I'm pretty sure that for some time he was the head of the detonator group.

LH: At a certain point, they went over to this spark gap method. I don't know when that happened. I think it was the summer of '44.

DM: [0:14:55] That's just the triggering, you mean.

LH: That's right. Well, I don't know much about…

DM: [0:15:05] Well, the detonator that was developed was not a spark gap detonator; it was exploding bridgewire.

LH: Ok. It was an exploding bridgewire. And a spark had to start the whole thing, all the bridgewires to explose, of which there were 32. And they each had two bridgewires, the connections?

DM: [0:15:48] I think so.

LH: Anyway, I'll look it up. Well, I think we're up to reorganization in August. August E-5, which is Neddermeyer's group, but Kistiakowsky was by then pretty much the main person, turned into X1, which was given to Bradbury. E-9, high explosive development under Ken Bainbridge became X-2 and E-10, which was S side under major W. A. Stevens, devoted to maintenance and construction, turns into X-3, which is the factory group.

DM: [0:16:54] Well, I am aware of those groups. The details, E became X, that was not known to me at the time. In fact, X-1 ended up something quite different, namely it was the radiographic inspection group by 1948.

LH: Anyway, just becomes the big group leader now. I wonder if you could comment a little bit about his role as a group leader, as a person who intuitively understood the right direction and how to organize people. I don't know if there's anything special that comes to mind there. You had known him as an academic chemist and then as a person involved with explosive development connected to Bruceton, and here he's turning into a scientific administrator.

DM: [0:18:10] That happened to all of us. He was well he was quite a combination, I think, of very high level of competence, very attractive personality, and like lot of people quite egotistical. In addition to scientific ability, I think he did have quite an act for inspiring people to do things.

LH: Was he an optimistic person? A pessimistic person?

DM: [0:19:06] I would say he was generally optimistic.

LH: He said so many positive things, for example, about the explosive lenses, at the very early stage, when most of the development was still ahead. It's a question of how—40 years later reading this stuff—I'm supposed to interpret his statements. Was it his personality to see things in a very positive way, or was he realistic?

DM: [0:19:42] It's a little hard for me to say. I, of course, knew him personally very well.

LH: A lot of reports are written by him, you see. So, if I knew a little more about him. It's always that's why interviews are so wonderful, because then I know a little bit more about the people. I know how to read a little better. How to read.

DM: [0:20:13] He was, I would say in general quite enthusiastic.

LH: Some other names: Ackerman. Did you know personally?

DM: [0:20:35] I knew him casually personally. I don't seem to remember just what… I remember the name and I remember what he looked like, but exactly what his role was I guess I don't really know.

LH: What about Major Stevens?

DM: [0:20:53] I don't think I even knew him. The name, again, rings a faint bell, but that's all.

LH: Going back to lenses for a second, do you know anything about the lens versus non lens debate? I gather Parsons was rather against lenses for a while.

DM: [0:21:25] That is not known to me.

LH: Just a word that comes up in one of the documents. It says that the two-dimensional lenses were studied with the tracer technique. Do you know what that mean?

DM: [0:21:45] I guess I don't unless it's just another word for it the terminal observation.

LH: It might be. I think it's also connected with **winches?**

DM: [0:21:55] I think that must be. I mean they did leave traces on this this plate that was next to the explosive, so I think maybe that's just another word for terminal observation.

LH: Three nasty words come up a lot in the reports: asymmetry jets and spauling. These are the reports on [it] that tends to…

DM: [0:22:23] It was because of concerns like that some of the people, I believe G. I. Taylor, for example said he believed the implosion technique would never work because of jets. I'm not sure he was so much worried about spalling, but asymmetries which caused jets and so on.

LH: Spalling is when a little piece breaks off, the detonation hits the metal and bounces back. Is that it?

DM: [0:23:05] Yes. It's really a shock. See, a detonation wave in the explosive turns into a shock wave in the metal. If the shock is a rather sharp one, it can be reflected as a tension wave and the part that flies off is called small spall.

LH: And that carries energy away, is that the problem?

DM: [0:23:41] Well, it isn't so much that it carries energy away, it carries part of the stuff away, and spoils what you're trying to accomplish, I mean the assembly of material into presumably a sphere.

LH: Jets are due to the interaction of two detonation waves?

DM: [0:24:06] Well, that's one way of putting it. Actually, it's one detonation, its interaction in two pieces of either moving metal or two shock waves in the metal. Either of those can cause a jet.

LH: A jet is always a piece of metal shooting out, and the problem is that they get there too early, so you don't have what you want, which is everything getting there at the same time. And symmetry is just a more general term, which consists of… It doesn't all get there to the center at the same time.

DM: [0:24:52] A perfect symmetry, of course, is a perfect circle shell at all times. Any deviation from that is general term… Any asymmetry simply means not symmetric.

LH: Not spherical.

DM: [0:25:06] Actually, it has a more general meaning. It's any deviation from symmetry. It could be axial symmetry, but in this thing, it is spherical symmetry you're trying to achieve.

LH: The problem there is that you lose efficiency if all parts of the metal don't all arrive at the same time. Ok, the choice of explosives. It seems that in composition B, the fast component was chosen almost immediately, but that it took quite a while to select the slow component. They started looking at Beranol, and they studied some other things aerated Beranol, Torpex and finally Beratol. Do you know anything about that? Was that some of the work that was done at Bruceton?

DM: [0:26:04] I don't remember whether we did anything with aluminized explosives. What we're trying to do of course is to get a slow explosive. That was what the barium nitrate was supposed to do. The barium nitrate plus the TNT, technically the barium big a heavy metal made the detonation wave considerably slower. Torpex itself was developed by the British as an underwater explosive. It had nothing to do with this. It may have been looked at.

LH: It was looked at at some point around October. It was looked at very seriously by X division as a candidate for the slow explosive, but I don't know why it was [dropped].

DM: [0:27:16] I don't remember.

LH: But then, certainly by February of '45 they had settled on Beratol. It may have happened earlier, but I know that by then it was chosen. Could you comment on the factory effort that was going on to make the lenses, to cast the explosives, and so on at Los Alamos during the war, at S side, I guess, is the main place. I mean is it correct to call it a factory?

DM: [0:28:11] Yeah, I think, in a general sense. In fact, it was. I mean the 80 components of all of the first bombs, those components were made at S side. It has remained—I forget just when, it was some time before I came out here that that work was transferred to Inyokern, at the salt water pilot plant[29]. You may run across that terminology. And there was quite a period, I think of a small number years, in which the bomb's stockpile had components made at a Inyokern. In fact, they used to come into Albuquerque and one of my fellows, Johnny Russell, used to go down and inspect them to make sure that they were made properly and so on.

LH: It's okay to call it a factory, but then this is a factory that was in moved out during the Bradbury era, when one Bradbury's aims was to turn this more into a research [facility].

DM: [0:29:42] That's right. When I came out here in May of '48, I believe we had stopped making things for stockpile. We made a lot of assemblies for non-nuclear experiments; you know, pin shots and all that. All that, of course, has continued ever since, but it was first transferred to in Inyokern, which is a Navy establishment. Then, sometime during '49 a part of a high ordnance plant that worked for the Army was put under the AC and then with Silas Mason[30] as the contractor they became the principal fabricator of stockpile components.

LH: Some other names that came up are Donald Hornig[31].

DM: [0:31:00] Yes. He had a very pretty wife. He was young PhD physicist by then. I seem to associate him with Brown, whether he came from Brown or went to Brown when he left here. He was involved, I think, with Henry Lynches in one of the H experiments.

LH: What about Jay Hoffman?

DM: [0:31:35] Jay Hoffman, I'm afraid I don't.

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[29] Salt Wells Pilot Plant: https://en.wikipedia.org/wiki/Salt_Wells_Pilot_Plant
[30] Silas Mason Company: https://en.wikipedia.org/wiki/Silas_B._Mason#Silas_Mason_Company
[31] Donald Hornig: https://en.wikipedia.org/wiki/Donald_Hornig

LH: W. G. Marley.

DM: [0:31:42] Marley. Of course, aside from being the famous Marley in Dickens. He was English, and he I think had invented a camera, which was used here. Whether later cameras were related to that, I don't remember. But that was his role, I think, high speed camera.

LH: Kauzmann[32]?

DM: Walter Kauzmann. he was a young PhD chemist, I believe. I think he worked with Hugh Bradner and I think later went to Princeton.

LH: Gerald Tenney.

DM: [0:32:42] Well, he got his training in Europe. He was an electrical engineer, I think. At the time I came out here to stay, he was the group leader of X-1, then became GMX-1, was the non-destructive testing outfit. He died. Edith is is as far as I know still healthy, Jerry died two or three years ago, I think.

LH: Lieutenant Copper.

DM: [0:33:25] Oh gosh. The name rings a bell, I can't quite…

LH: J. B. Price?

DM: What initials? That doesn't. There was a J. S. Price later, but not that one.

LH: Do you know what the role of Yorktown naval mine depot was? That comes up in some of the documents. They apparently collaborated on the manufacture of lens explosives on a large scale.

DM: [0:34:18] They may have. I was down at the Mine Depot at times. I remember one time after they've had an accident, some Torpex[33] blew up and I went down to testify in the investigation. There was quite a lot of contact between NDRC and Bruceton and the Mine Depot. Just exactly what role they played, if any, in components for weapons I'm afraid I just don't know.

LH: The Christie suggestion came in September to go to the solid core. That came out of the theoretical division, T-1. And it took a number of months

[32] Walter Kauzmann: https://en.wikipedia.org/wiki/Walter_Kauzmann
[33] Torpex: https://en.wikipedia.org/wiki/Torpex

to figure out whether or not it was better. Apparently, some of the first experiments didn't show that it was any better than the hollow core, but then with the addition of lenses and especially the electric detonator, it was finally decided that that was the best design.

DM: [0:35:44] Well, I don't quite understand what you're saying, because the all-hollow design theoretically was always better. The question was: could you get a good implosion. So, I guess I don't know quite what you're saying when you say it was decided the Christie design was better. It was safer and good enough. I don't think that if you made theoretical calculations, I don't believe you'd ever… Because we went to all-hollow designs later and they were fine.

LH: It was decided in February of ’45 to use the Christie gadget.

DM: [0:36:32] That was because of concern about all these things like mixing and asymmetries and jets and so on. But I don't think a theoretical calculation ever showed the Christie gadget better. It was…

LH: The big problem of T division at that time was the problem of asymmetry and so on.

DM: [0:37:00] But T division couldn't solve that problem. It had to be the people working with the explosive system. I think it was never perfect and there was just a lot of worry. As far as I know, the Christie gadget was picked because it would work not better than the all-hollow—the all-hollow on paper looked better. We made it years later.

LH: In what way, did it look better?

DM: [0:37:41] Get more yield, higher efficiency.

LH: Higher efficiency if it stays spherical.

DM: [0:37:55] Yeah. In those days, we had no capability for anything but one-dimensional calculations, so the calculation all was made the thing look a lot better than it would and it was sort of, as far as I know, it was some degree of conservatism. Even with the Christie design, I think some people thought it would not work because of these problems with one-dimensional calculations weren’t considered.

LH: I didn’t understand the last thing you said. The one-dimensional…

DM: [0:38:37] A one-dimensional calculation sort of by definition doesn't take into account the effects of things that are not one-dimensional, such as asymmetries, jets and all this kind of things. I'm just saying that as far as I know, the all-hollow design does give you higher compressions than the Christie design, but everybody felt it was much more susceptible to problems and of asymmetries, jets etc. See, during that time, while I knew what was going on, I was not in on discussions about comparing the hollow design and the Christie design. As I said, there was some of degree of compartmentalization, particularly people like me who weren't full-time employees here and were just a consultant. I don't think I was ever in in a discussion of whether the all-hollow design would work or not.

LH: My question is: once they decided to go to the Christie design, what were the implications for explosives development.

DM: [0:40:17] I think it was the same as they were before. To get as good an implosion as you could: starting off simultaneously and getting a good spherical detonation wave. It was simply the realization, I think, that any degree of deviation from perfection, the Christie design would be less susceptible to difficulty than the all-hollow.

LH: But it didn't change the lens design or anything like that. One thing did change. It was much more important to get a modulating initiator once they went to the Christie design for reasons I haven't worked out yet. But it's clear that that played a role.

DM: [0:41:15] I think you have to have that for any of the designs.

LH: Apparently, a simpler initiator would have worked, but I shouldn't talk yet because I haven't studied this piece yet. All I know is that suddenly the initiator becomes a big issue, when they switched to the Christie design.

DM: [0:41:38] There again, that was a nuclear physics part I was not in on discussions. But of course, I know how the thing is built and as far as I know for any implosion design—even the gun design—there was a question whether you could assemble it and it would just sit there essentially forever and eventually to go off, or whether you better do something more than just patience. But certainly, for any implosive system, it was obvious eventually the thing was going to fly apart. This is true either of the Christie design or the Neddermeyer design.

LH: It's a question of efficiency. Anyway, once they settle on the Christy design, settle on using lenses, settle on the electric detonator and so on, all this happens around February, although the initiator has to be developed, the

effort at Los Alamos, turned much more into a development effort with deadlines and so on than it had been earlier, when research was with the larger components. In describing this development effort, what do you think I should put in about the problems of explosive development? I mean casting, bubbles, cracks. Could you comment on some of that?

DM: [0:43:32] I suspect that what we know now is that it probably would work pretty well without so much worrying about perfection, although we did do our side experiments, that is pin shots and so on, which we didn't have any lenses and just had an all points of initiation. The action, importantly was not too bad, but I think that particularly at a time when no bomb had ever been built and fired… Of course, there was one at Trinity. But up until that time, for these deviations there was no way of telling just good or how bad they could be, and so the general direction was in every respect do the best you can. I think sometimes probably if it hadn't been quite that good, it still would work pretty well.

LH: I'll wind up with a very general. I wonder if you could comment on the extent to which the war opened up the field of precision explosives work?

DM: [0:45:23] I guess this had never been attempted before. It has been sort of ever since. We've gone on different kinds of explosives. Plastic explosives rather than cast. I still suspect we don't know just how good as we may have them really lot better than they have to be, but certainly it is true that that for whatever it's worth there were big advances in using explosives as kind of a precision affair, rather than just something you put a detonator somewhere and let it go bang and didn't really care about the details, which was certainly the situation in, say, 1940. Okay?

LH: Yeah! I really appreciate this.