

An Interview with  
ROGER B. LAZARUS

OH 390

Conducted by Anne Fitzpatrick

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### Abstract

Lazarus worked in Los Alamos Scientific Laboratory's Theoretical (T) Division beginning in 1951. Trained in Physics, Lazarus eventually became an expert in computing for nuclear weapons research and development. In this interview, Lazarus discusses early card-programmed calculators at Los Alamos, and the delivery and use of the IBM 701, 704, and STRETCH systems and the science and politics within the efforts to build and use these machines. Lazarus eventually served as director of the Laboratory's Computing Division.

Fitzpatrick: Today is October 10, 1995. We're at the home of Roger Lazarus in Los Alamos, New Mexico. Today we are going to talk about some history of computing at Los Alamos Laboratory, and some of the work that Dr. Lazarus did in this area.

Fitzpatrick: I would like to begin by asking you to talk a little about yourself; for example, your background, and when you started here.

Lazarus: My doctoral work was in quantum electrodynamics. I knew virtually nothing about computing. In retrospect, I recall that I attended one lecture which was about mercury delay lines. When I came here my first piece of work was in estimating yields of what was then going to be a somewhat novel design of a purely atomic weapon with no thermonuclear influence. The machine available at that time was the CPC.

Fitzpatrick: Where you using the 602 then?

Lazarus: There were several models. Some could do sines and cosines. These machines were very simple. The total data memory depended on how you hooked things up but it was approximately 24 words. The cards contained the program and you could punch out some numbers to put in on the next cycle like the time-step. We did integration of simultaneous ordinary differential equations. When I came here, I got very interested in calculating yields of purely atomic bombs.

Fitzpatrick: You came here in '51?

Lazarus: This was in January of '51. Hans Bethe was the one who proposed the method of approach – the simplifying assumptions that could be calculated on a CPC. At this time, the first MANIAC was under construction, under Nick Metropolis's guidance. The MANIAC was going to have 1000 words for program and data. There was also the SEAC in Washington, at the Bureau of Standards.

Fitzpatrick: Los Alamos was using that machine also, correct?

Lazarus: Yes, it had 512 words of memory delay line, and 512 words of electrostatic memory, which was so-so. We went there in the summer of '51. This group was under Bob Richtmyer's guidance, and the project was a thermonuclear design called Alarm Clock.

Lazarus: This was thrilling work. We had the machine from midnight until 8 in the morning. The code was a full 1024. In fact, my contribution was that I was asked to look over the code, and it was small enough that I could do a cycle or two by hand. I found three errors. Fixing them made us require 1026 words which wouldn't fit on the machine. But it was a 4-address machine: A and B as argument, C as result, and D as where to take your next command. Thus, you could jump around. I managed to get some of the instructions to have a numerical value of some of the constants so that we could double up the constants with the instructions and fit it into 1024. We worked for a month with only one good run, and that run was just long enough to let the experts back here deduce

that there was a factor of ten error in one constant. The error was equivalent to having the wrong speed of light, so the explosion front was moving too slowly by the square root of ten. We never could get the SEAC to go again.

Lazarus: Later in the summer, I and some others including Seymour Parter, coded it for 512 words plus magnetic tape to add to the SEAC. It had no spools, just free-flowing tape, falling into a vacuum suction chamber that would pull, but two glass plates kept it from tangling. That worked, and we calculated a sufficient number of cycles for whoever was really interested in this device, which never got anywhere because of its weight.

Lazarus: Let's see, when did MANIAC I come on line?

Fitzpatrick: Early '52.

Lazarus: It was just before the 701. It beat the 701 which was essentially of the same strength.

Fitzpatrick: There were both similar to the IAS machine.

Lazarus: Yes, the technology was like that of the IAS. Our main deviation from the Princeton machine was to use smaller cathode ray tubes. This was Jim Richardson's idea.

Fitzpatrick: A 2 inch instead of a 5 inch tube?

Lazarus: Exactly. It was a little easier to control the beam. We actually succeeded first, before the IAS machine was finished. I noticed that for some reason this notebook [looks at his personal notes] starts 8 August 1952. The first sentence is: “Up to this time we have the following MANIAC routines relevant to T-4.” They were running. “The spill that evidently occurred on 7 August should be looked into...” Spill was the overflow; this was a fixed-point machine. So, you were right, by August of '52 it was running.

Fitzpatrick: I read that MANIAC was used, along with SEAC and the card punches here, to predict the results for Mike, the enormous device in the Ivy series.

Lazarus: In January '51 when I arrived here they were working on George.

Fitzpatrick: Yes, for the Greenhouse shot. I suppose it was something of a proof-of-principle shot?

Lazarus: Right. I remember one of the first things here for me was an explanation of how radiation flow could implode something. That was the big excitement then, and George was the proof-of-principle, as you said. At the same time there was Item, which I guess was the proof of fusion in liquid. The tests were alphabetical then. I also worked on How. My code could handle the very large How no-fusion device, to see how far we could go with that.

Fitzpatrick: And your code was run solely on the card punches?

Lazarus: Yes, but then Nick Metropolis made me an offer: If I would join his group, and be the physicist-consultant to help people bring their problems to the computer, then in exchange he would give me preferred access. I used this to expand on the CPC work, starting with some methods developed by Conrad Longmire. Do you know of his role?

Fitzpatrick: I know his name but not his work.

Lazarus: He was here through the 60's. He was very good, and one of his specialties was the electromagnetic pulse. But early on, in '51 or so, he developed the 11 mass-point method, which was a crude beginning to discrete zoning for hydrodynamics, and for diffusion and radiation flow. Now I think 11 would just fit on the CPC. He urged me to try that. So I went from this Bethe method, which was essentially to assume that the density in each material would stay approximately constant as a function of radius, not the time of course. You could use that assumption to get rid of all the partial differential equations. With the 11 mass-point you needed to do finite differencing with partial differential equations, and indeed it seemed better. The MANIAC attracted me because I could go up to 14 zones instead of 11. But I started on the MANIAC with the Serber-Wilson method, which was a way that was great for the CPC for calculating neutronics, because you had these fast calculators of the circular functions and exponential functions. But, it was terrible for fixed point work. It involved no physical bound on the sizes of the numbers. So, another thing that I think Longmire started was something called modified

diffusion, which was at least as good and which fit in with ordinary finite differencing and therefore with fixed point. So that's how I developed the techniques on the MANIAC.

Fitzpatrick: Did you help build the MANIAC?

Lazarus: Not really, I was a theoretician. [Laughter]. I was around but not involved in its construction.

Fitzpatrick: What about the MANIAC and IBM 701 relationship? There is something in Cuthbert Hurd's papers that suggests that the IBM folks here were sort-of spying on Nick Metropolis. Perhaps "spying" is too strong a word but they were afraid Metropolis would finish the MANIAC before IBM finished the 701. They feared that if the MANIAC were completed, IBM wouldn't be able to rent Los Alamos a 701.

Lazarus: I don't think that's true. I don't remember where Hurd and Metropolis had their first association but they knew each other. For my work, every little bit in advance of computer power for single-stage weaponry, just to calculate the yield, was marvelous. We could get a valuable improvement. I moved from CPC to the MANIAC to the 701. Perhaps not the very first one delivered but they quickly got a 2000 word memory on the 701 and they hoped to get faster magnetic tapes.

Fitzpatrick: I believe the first 701 ever delivered was delivered here.

Lazarus: We never got UNIVACs here, but we worked on them at NYU in the summer of '53.

Fitzpatrick: That was Eckert and Mauchly's company. Correct?

Lazarus: Yes, then Remington-Rand bought them out.

Fitzpatrick: Then they built LARC, which I might like to talk about a little later.

Lazarus: I know very little about LARC, but we passed on LARC I think because IBM came to us first with the STRETCH proposal. It may have had something to do with Metropolis or Bengt Carlson knowing Hurd. He steered them here, having established the principle that we would buy anything that would give us more capacity. If we didn't buy it, we would go use it wherever it was. Livermore did the same, because there was so much physics that was known, and applied math that was relevant, but there just wasn't enough machine-capacity to do it. So IBM came to us in '55 with the proposal for STRETCH which was expected to be 100 times faster than the 704. They naively thought this might be "it," in that you wouldn't need any more new machines. It was to be the first all solid-state, although the 7090 came along while they were building STRETCH. Originally this was the 709 built with vacuum tubes, and when it was transistorized they renamed it the 709-T. Then someone suggested the 7090 since it sounded exactly the same.

Fitzpatrick: If it's all right, I'd like to get to STRETCH a little later. I was going through EdVoorhee's papers. He kept copies of almost all the STRETCH Math Planning meetings and I would like to discuss those particularly a little later on. If we could go back a bit, though, that would be good. The first 701 arrived in Los Alamos in 1954 if I recall correctly, although it was announced publicly in '53. What I have in front of me here is one of the 701 logs. I saw your name in here [Hands log to Lazarus] associated with one of the codes.

Fitzpatrick: At the back of this document are a series of code-names, and numbers with them.

Lazarus: Yes. T-7 was the MANIAC group. T-1 was the IBM group – that was the main computing group. It remained the main computing group under Bengt Carlson, although at first with Max Goldstein as his associate, until Max went to NYU to run their computer center.

Fitzpatrick: What about Preston Hammer?

Lazarus: Oh yes, there was a big split.

Fitzpatrick: I thought that Carson Mark relieved Preston Hammer from his post as head of the computing group since they had a personal conflict.

Lazarus: I'm not sure, but there was a split in T-1 and then T-5 was established and became Hammer's group. Each member was allowed to choose where they wanted to go, and Hammer's group carried on hydrodynamics. Bengt and T-1 stayed with computing and Bengt's personal research was neutron transport. There were no professional managers in those days. Everybody was a scientist or mathematician and had his own research. Administration was so simple that you could do real work.

Lazarus: Preston's real interest was convex bodies, but his group, T-5, did hydrodynamics. Harwood Kolsky, whom I knew at Harvard, came here about the same time I did. Kolsky was actually an experimentalist. He wanted to work for Jerry Kellogg. Kolsky had done a molecular beam doctorate at Harvard. But somehow Kellogg didn't have room and Kolsky switched and did theoretical work. He was also on the STRETCH development committee. Here I'll backtrack. First there was a STRETCH proposal to Los Alamos, and it even had a ridiculously low price in the very first proposal, but we were to help them. It was a cooperative project. There was a cocktail party and Johnny von Newmann was asked what he thought about it, and he responded that the price was so low that of course we should grab it. There was also the guy who was head of the division of military applications, General Starbird. He was here at that time at some social event and we told him what IBM was saying and he said "go" also. The rules required that we put out a request for proposals, however.

Fitzpatrick: Yes, I saw a draft of that. It was a request for a 10 megacycle machine. I believe you got proposals from IBM, RCA, Remington-Rand, and Reeves Instrument Corporation. But was it defacto that IBM would be chosen, even though you went on plant tours of these other companies?

Lazarus: No, it was not. I had a very embarrassing personal problem that helps me remember that Remington-Rand was a substantial competitor. At some point IBM seemed to be stalling us. There was terrible trouble in those days about patents. It wasn't until quite recently that the federal government woke up to the fact that when they wouldn't let any commercial agency have patent rights than the agency had no interest in collaborating with the government. So at that time the government was saying that IBM wouldn't have patent rights to anything and IBM balked. I got the feeling that IBM was stalling, and made some speech at the committee meeting where I said that I thought we should take the Remington-Rand proposal.

Fitzpatrick: But going back to the planning. Now who did first propose the machine?

Lazarus: IBM proposed it "out of the blue."

Fitzpatrick: But Los Alamos solicited to several vendors to build such a machine?

Lazarus: Yes, for a machine to be 100 times faster than the 704. Speed was important.

Fitzpatrick: Was it true that IBM at this time wanted to get out of renting machines and into selling them?

Lazarus: Yes, I believe that was true. You could not buy a 701 or a 704.

Fitzpatrick: I see, and apparently when Livermore wanted to build a similar machine, John von Neumann suggested to Teller that they acquire a fast computer. IBM bid to build the LARC but Remington-Rand got the contract.

Lazarus: Well, IBM did not bid to build the LARC, but a similar machine. LARC was solely Remington-Rand's. We were worried that Livermore might have made the right decision and were a little glad when it turned out they hadn't. [Laughter]

Fitzpatrick: LARC wasn't as fast as it was supposed to be.

Lazarus: Neither was STRETCH, but generally STRETCH was a success. Livermore was doing far-out things. They had to in order to compete and catch up. Weapon design was thrilling math and physics; there had never been anything so wonderful. And, the growth of the computing industry fit so well that it too was thrilling. Most of us in the early 50's had gone into physics despite the thought that we'd never get more than a living wage. There was no money yet in it, and all of a sudden money was available and Washington was saying – "you guys do what you think is best for the country." Some of weapons design was like a sport, there was a competitiveness between us and Livermore, even

though there was also joint work. We laughed at them if their devices didn't work. Also, there would be joint meetings once or twice a year of all the AEC computer installations to compare notes and share techniques.

Lazarus: After STRETCH won the proposal and we started negotiating in '56, I led the negotiations for the technical side with AEC lawyers. Then we started having meetings, here and in Poughkeepsie. One of our jobs was to develop test codes for all aspects of the machine. This is pre-operating system; machines of course did not come with software then. Bob Frank, for example, worked on code that would test the magnetic tapes. Whoever could get on this committee and develop codes would get lots of machine-time once the computer showed up.

Lazarus: Would you want to know about STRETCH and its design?

Fitzpatrick: Yes, I've seen only a little documentation on that, and mostly from the perspective of Steve Dunwell.

Lazarus: People like Dunwell suffered because some of the IBM accountants said STRETCH was a flop. Do you know about HARVEST which was being done in parallel to STRETCH?

Fitzpatrick: Heard of it. Wasn't it supposedly sold to the Navy?

Lazarus: As a front. The cover story was that it was sold to the Bureau of Ships. But it really went to the National Security Agency, and it was so secret that nobody could even say where it went. We deduced it because when people wouldn't tell you anything, then NSA was involved. That was a Super-STRETCH, meant to break codes. They wanted incredible speeds for a few simple jobs. So they and Los Alamos were the 2 external money sources. IBM made plenty of money out of this deal, even if the machines didn't become good products. Only a few STRETCH machines were sold, and there was good reason. It was the end of no-operating system machines. It was a dream for hand-coding. But STRETCH was the first machine I think that had synchronous I/O interrupt. The whole idea of interrupts began with STRETCH. I'm not sure about the golf ball typewriter from which IBM made a fortune. That was at the same time because we thought it was a joke. Just before IBM delivered STRETCH they invited some news people to see the machine, and there was one thing in the room that remained covered and people were forbidden to talk about. The real money in that company was in those typewriters.

Lazarus: We spent a lot of time in Poughkeepsie, and the checkout of the machine was thrilling. There was so much new stuff. A lot of us were very good at debugging, because we had been working with the MANIAC and 701 with their vacuum tubes and electrostatic storage – thus failures were normal. You never believed the result automatically. You had to repeat everything to see if you got the same answer twice. STRETCH made all these strides in error detection and correction. There were people under Bengt Carlson's section who were developing an operating system including

something they called spooling, which was for the printer output. But, the HENRE code was going to use the entire STRETCH, and we had essentially our own operating system in it and we knew that hand-written code so well and had so many tests within it that we could sort out computer errors from our own errors. Unfortunately it was too slow.

STRETCH itself was too slow in everything except floating point arithmetic, so all these lovely things that let you write complicated codes by hand were slow and furthermore hard for compilers to use.

Fitzpatrick: What about STRETCH's memory? I read that this was STRETCH's biggest asset and one could put a whole code in memory at one time, making it possible to do the calculations for Dominic and later tests?

Lazarus: Of course a lot of codes went on STRETCH. Some like HENRE had their own management – input/output. Some were under the operating system which we developed during '61 and '62. HENRE was spherically symmetric, so it couldn't be used for radiation implosion or yield calculations for most secondaries. Although it was used in a special way for some of the high-altitude shots. By then everything was spherical and we could calculate the late time fluxes of neutrons.

Fitzpatrick: What do you recall as your very general impression of STRETCH?

Lazarus: It was bigger, and faster by a substantial factor. It had single-bit error correction and double error detection. It had a good fast disk so if you detected an uncorrectable

error you could pick up the restart dump and go again. The machine was very productive for a long time, although eventually it was surpassed by the 6600. Moreover, hand-coding fell by the wayside.

Fitzpatrick: The test moratorium started in 1958 and ended in 1961. This is the time in which STRETCH was being built. How was this period important to the STRETCH project?

Lazarus: The test moratorium was very important for STRETCH. We all had lots of time on our hands. There was a lot more time to devote to hardware and code development. There was this consciousness even then that if there was less testing, we would need better computing. I remember in the 50's as a newcomer and not really understanding how you built these weapons. I would get so mad that they would put things into these experiments that they shot in Nevada and in the Pacific which I thought were to test my codes. But they put things in to satisfy the military, which seemed to me ridiculous.

Fitzpatrick: In regards to STRETCH's planning. I've looked at some of the Mathematical Planning Committee Meetings of which you were a part. In the first meeting in 1956 Lazarus discusses a large problem with the assembly and disassembly of bombs. But, there is no further mention of this problem in the meeting minutes. Was this how you justified to IBM your "needs" for STRETCH? This didn't make much sense.

Lazarus: Assembly and disassembly means implosion and explosion, of course.

Implosion was still a secret word at this time. Although I can't quite remember the timing of the work on the two stage devices but there was so much waiting to be done and useful to do.

Fitzpatrick: It seems that during the 50's and 60's the AEC became a huge patron of computing and allotted much money for these endeavors. Do you have any comments on this?

Lazarus: One excuse for getting infinite money for computers was when they were building stockpiles and by some method of accounting there were costs that were associated with  $^{235}\text{U}$  and Pu and Tritium that were enormous. If it was decided that you would have so many of these devices, and every gram of tritium or kilogram of plutonium that you could save, with everything else being equal, would be worth millions of dollars. Thus, you could argue "get us another computer" in computer procurement justifications.

Fitzpatrick: Speaking of computer procurement, some Los Alamos folks went before the Joint Committee on Atomic Energy to ask for money for computers. In particular, Metropolis and Carson Mark testified before the JCAE in the mid 1950's to get funding for STRETCH.

Lazarus: That's interesting because it was always my opinion that STRETCH was a shoe-in. I think the original proposal made was around \$2 million. I think the contract, which we made about a year later, was about \$3.2 million. Probably this was a fair price.

Fitzpatrick: Could we talk a little about weapons codes and their evolution?

Lazarus: On the 701 I came up with an acronym for a code that would calculate one-stage device yields. This was HENRE, which was an acronym for Hydrodynamics, Energy production, Neutronics, Radiation, Et Cetera. The et cetera was because I was tired of naming new codes every time we got a bigger machine and some new physics. HENRE went all the way to the CDC 6600 and 7600.

Fitzpatrick: Is this what you were referring to, here in the 704 log? [Shows log to Lazarus].

Lazarus: Yes. HENRE was very important in operating system terms. It started on the 701. Dave Woods joined me and we sort of divided it up. He would take care of the hydrodynamics, and I would take care of the radiation and we shared the neutronics, which went from modified diffusion. Are you familiar with Bengt Carlson's Sn method?

Fitzpatrick: I know of it.

Lazarus: In '59 a group of men from the French AEC came here to visit. One of them didn't know any English and I was assigned to him since I was planning to go to France and I wanted to brush up on my French. When Bengt gave a talk this man said, "This is like meeting the Pope." So was that much of a step forward at that time.

Fitzpatrick: These codes, then, most of them had to do with specific hydrodynamics problems? [Shows him 704 log again].

Lazarus: Yes, HENRE was the yield calculation, which grew into other kinds of output – neutron fluxes and radiation fluxes. HYDRO was an implosion code. Harwood Kolsky was working on that.

Lazarus: In T Division, T-4 was the main group for one-stage devices and yields. T-2 was the two-stage devices. T-5 was the high explosive group and implosion. T-1 worked on neutron transport. I was in T-7 but worked with T-4. W-4 was more of the engineering side of things, and their group leader didn't trust anyone else's code so they had their own.

Lazarus: Bob Frank and I developed a mixed Eulerian-Lagrange code. Frank coded it for 704. I coded it for MANIAC II. But we already had STRETCH by then.

Lazarus: [Looks at 704 log]. SWORDTAIL was a radiation implosion code. Bob Frank worked on this. THICKET was in the high explosives field I believe. Ivan Cherry was

into rational functions of polynomial fits to analytic functions that were no longer efficient to calculate by power series.

Lazarus: CDC eventually took over here with the 6600. Bengt asked me to run a committee – he was getting tired of this sort of thing – and there was a competition between IBM and CDC. By then Bengt was pushing me to form a computing division. He refused to run it, and so I ran it. In the mid 1960's IBM was proposing new designs and pulling some stuff that wasn't right. That led to the big suit between them and CDC and anti-trust difficulties. It partly involved us. We had first chosen the IBM design over the 6600, and we had sent somebody to check on their production. He thought it was OK, but in fact they weren't coming through. At some point it was the last straw, and we went with the 6600. Everything that Seymour Cray did for a long time was marvelous. So we became a CDC lab with the 6600 and 7600.

Fitzpatrick: I'm not as familiar with the CDC equipment. Perhaps we could go back and talk about some of Los Alamos's own machines. I think you wrote the MANIAC II report. I recall from the introduction to the report that the idea behind building MANIAC II was because commercial machines were not built with scientific applications in mind. This was in the text, but was this the true reason?

Lazarus: Not really. There were a few reasons. One was a rationalization that had some merit: All the commercial people were going to switch to magnetic cores. Jim Richardson was really excited about barrier-grid cathode ray tubes. He convinced Nick and me that

since we needed the engineering staff to maintain MANIAC I, and they were getting bored, it wouldn't cost that much to build a new machine, and we might as well go with the barrier-grids as a form of insurance.

Fitzpatrick: Did MANIAC II have floating point arithmetic?

Lazarus: I think Jim Richardson did not want to do floating point at first, and the early design did not have this. I wanted floating point because I was aware of the terrible programming cost of checking the ranges of your variables. There was a thing called B-boxes that were invented by the British, which became index registers. These were the greatest as comparable to floating point in the computer development. I was vacationing at Lake George with the family and I got a call where we bargained about multiple index registers and floating point. The machine ended up with floating point but a very strange floating point. It had a base 2 to the 16<sup>th</sup> instead of base 2. This meant the most of the time operations would be effectively at fixed points. Exponents would match. If they didn't we could do fast shifts of 16 places. We had a fairly long word and you could afford to have plenty of zeros, binary, in the fractional part. The exponent was just a few bits.

Lazarus: Indeed it was an unusual design, and the barrier grid memory wasn't really successful. However, we did not manage to beat the 704 the way we had beaten the 701.

Fitzpatrick: Was the intention really to "beat" the 704?

Lazarus: Well, no, just joking in a sense.

Fitzpatrick: Of course, when comparing the MANIAC II and 704 machines they seemed alike; they both had core memories and used floating point.

Lazarus: And index registers.

Fitzpatrick: But which was the more useful machine for designing thermonuclear devices?

Lazarus: The 704. It was in the main group with a large operating staff. They also quickly got several 704s and moreover, its memory was more reliable. We added some core memory. We also acquired a drum for one which increased the memory.

Lazarus: Another thing was that Nick Metropolis was against punched cards. He liked paper tape. There is some argument why paper tape is superior, but it's not from the user's viewpoint. Cards were great, in part because the reproduction, sorting and searching facilities, and the verifier had almost zero error rate, except for your handwriting in the keypunching. Even with the handwriting, they had persons at that time – all women – type the cards and a different one re-read the text and go through the same finger motions in the verifier, which would stop if there was a disagreement.

Fitzpatrick: The part about women in such roles is interesting in itself. But, more generally, who trained these people?

Lazarus: There was a woman named Phylis Heyman who was head of this group. I didn't know her terribly well until the end of her career, but I don't think she got enough credit. She had an early insight of how much rest or break time was necessary if you wanted first class performance, and boy did we want first class performance, because a key-punch error cost so much machine time. So they had a very classy group, considering that they were "mere women" [laughter] in that unfortunate era. They had great morale.

Fitzpatrick: That is interesting too since there was a similar situation during the war, where women were also running the hand calculators and Feynman was in charge of them. Another thing was that when the punched card machines finally came, Feynman and Nelson assembled the machines and trained people without IBM representatives, because there was no IBM person here yet to do this. But what about later, when the 701 came, were there enough IBM reps here to train people how to use the machines properly?

Lazarus: Yes, there were IBM people even with the punched card machines.

Lazarus: I remember that first code on the CPC had gotten up to a 12 hour run, modified, to take care of boosting, you know, in particular the lithium-shell boosting.

Fitzpatrick: Was HENRE used to calculate this lithium-shell booster?

Lazarus: It wasn't HENRE yet. It was this CPC code for boosted Thor – a lithium deuteride type.

Fitzpatrick: The use of lithium deuteride is intriguing because one of the things I'm trying to explore is the pursuit of workable thermonuclear devices.

Lazarus: In relation to that, one of the first big things I did by myself was called Buzzard, on MANIAC I. It was during the time of atmospheric testing. It was a one-staged device with a free-run solid core, etc. I think it was Ted Taylor's idea to take a free-run device and fill it with deuterium gas to see what would happen. If it remained one-dimensional this stuff would be compressed, the fission bomb would go off, and the deuterium would get very hot, and make fast neutrons. It would be a glorified boosting, it wouldn't have enough deuterium gas to have substantial thermonuclear yield. I worked out a good approximation to estimate how the products of the DD reactions would deposit as a function of the density, and calculate a yield. I got some enormous number like 100 kilotons, then I learned about Taylor instability. I looked at this graph I had made of this little thin layer of gas stopping the incoming uranium and turning it around, and I realized there was no way this could happen. It's like air trying to stop the flow out of an upside down glass of water. I made a simple assumption called free-falling mixing, which is, for all intents and purposes, where the metal would ignore the gas, and keep right on going. On that assumption I estimated the burn would stop as soon as the free fall of the outer

metal met the exploding inner metal. That gave a prediction of 26 kilotons, which was still nice. So they decided to shoot it. I went out to Nevada to watch this thing. We had a very complicated pool, in which you could bid on the yield. I bid 26, and it was 26!

Lazarus: Mixing was always the “talisman” of what you couldn’t touch – like turbulence, etc., to remind you that you would never be able to calculate exactly from the basic principles. There were some nuclear explosions that were primarily for the designers and the code-writers. A fair fraction of the instrumentation was to check on things. You knew at the gut level that you had a long way to go.

Lazarus: Nothing was better for boosting than free-flow mixing. It’s very difficult to calculate, and essentially 3 dimensional. Livermore went in to 2 space dimensions before we did. We sort of took the view that rather than have these codes put deliberately on machines that could not do the job right, you would do better to try and design things so that they were symmetrical either true 1-D spherically symmetric, or sufficiently thin cylinders that were immune to end effects, that you could do in 1-D cylindrical. Our radiation implosion was really done with 1-D codes.

Fitzpatrick: It sounds like there was not only competition between the weapons programs, but in the computing programs as well, between Los Alamos and Livermore.

Lazarus: As far as competition in computing between the 2 laboratories, Sidney Fernbach was more aggressive than Bengt or me, so they spent more on computing and on high-

speed output. We jokingly claimed at one point that they were getting an online shredder, so their printouts could go directly to the shredder and no one would have to bother to read them.

Lazarus: But for the country, it came out all right because the computing industry was being supported. The lab budgets were not nearly as big as they are today. And, computing, unbeknownst to us was going to play a much bigger role in the country than just scientific computing.

Fitzpatrick: In trying to sum up the relationship between nuclear weapons work and computers, do you have any other comments?

Lazarus: The single-stage atomic bomb had helped influence computing because of the safety question of one-point detonation. You wanted it to be relatively harmless. The Sherwood Project also drove computing – the plasma physics calculations. Then, time-sharing, which is why I resigned as head of the computing division to go back to research – I came out of the batch era and this is what I understood – the efficiency of the machines were batch processing was the way to do this. Time sharing was people efficient, not machine efficient, and the way of the future. When I realized this I felt it was time to get back to physics. This was in '73.