CHEMICAL HERITAGE FOUNDATION

FRANK J. BIONDI

Transcript of an Interview
Conducted by

Dr. Arnold Thackray

in

Philadelphia, Pennsylvania

on

19 March 1996

(With Subsequent Corrections and Additions)

BIONDI

THE CHEMICAL HERITAGE FOUNDATION
Oral History Program

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Frank J. Biondi, interview by Arnold Thackray at Philadelphia, Pennsylvania, 19 March 1996 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0147).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

FRANK J. BIONDI

1914	Born in Bethlehem, Pennsylvania, on 22 September

Education

1936	B.S., Lehigh University, chemical engineering, with honors
1940	M.S., Columbia University, chemical engineering
1940-1942	Polytechnic Institute of New York, chemical engineering

Professional Experience

1940-1943	Manhattan Project
	Bell Telephone Laboratories, Inc.
1936-1948	Member of Technical Staff, Chemical Research and Development
1948-1958	Member of Technical Staff, Supervisor, Electronic Material and Processing
1958-1962	Department Head, Electronic Material and Processing
1962-1979	Laboratory Director, Electron Device Materials and Processes Laboratory
1979	Retired
1979-1989	President, Bond Engineering, Inc.

Honors

1945	Certificate, U.S. War Department, Army Service Forces/Corps of Engineers, Manhattan District, in appreciation for work essential to the production of the Atom Bomb
1946	Award for Chemical Engineering Achievement, Chemical & Metallurgical Engineering

ABSTRACT

This interview discusses Frank J. Biondi's education, career, and involvement in The Electrochemical Society, beginning with college experiences as a chemical engineering major at Lehigh University and initial work at Bell Telephone Laboratories [BTL]. Biondi describes his position within the structure of BTL in the 1930s and reasons for his pursuit of graduate education at Columbia University. After completing his master's degree in chemical engineering, he enrolled in the Ph.D. program and became involved in the Manhattan Project. Biondi worked on a gaseous diffusion program to separate uranium 235 from uranium ore, designing the diffusion barrier used for the atom bomb. Biondi describes the reasons for Union Carbide's appropriation of his barrier's design and related patent applications and process details, and the subsequent manufacture of large amounts of barrier. After making his contribution to the Manhattan Project, Biondi returned to BTL work and focused on electronics, initially developing long-life cathodes used by the British during the war. He continued cathode work, becoming involved with the ASTM to standardize three nickel alloys for electronics industry electron tube cathodes. Biondi describes his rise through various BTL departments, his entry into transistor work, and associations with The ECS, which began in an effort to assure BTL metallurgists designing semiconductor devices an outlet for publishing and presenting their work. After touching on solid-state activity and descriptions of new electrochemical processes in ECS publications, the interview returns to Biondi's BTL career progress, particularly his work on transistors. As Biondi reviews his later career, he discusses fuel cell work, relationships with N. Bruce Hannay and R. M. Burns, the electronics industry's first dust-free white room, semiconductor work for satellites, and improvements in battery manufacture and design. The interview closes with comments on the effects of changes related to AT&T and Lucent Technologies, the future of The ECS, and consulting work since retirement from BTL.

INTERVIEWER

Arnold Thackray is president of the Chemical Heritage Foundation. Educated in England, he was a fellow of Churchill College, Cambridge. He served on the faculty of the University of Pennsylvania for more than a quarter of a century. There, he was the founding chairman of the Department of History and Sociology of Science, of which he is the Joseph Priestley Professor Emeritus. In 1983, he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. His publications include *Gentlemen of Science* and *Chemistry in America.*

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LOCATION: West Palm Beach, Florida

DATE: 19 March 1996

THACKRAY: Dr. Biondi, let's begin with a little bit about your own background, your family, and your undergraduate career. How did you get into the chemical world?

BIONDI: I was born and raised in the Allentown-Bethlehem, Pennsylvania area. After graduating from high school, I worked for a year for Mack Trucks in order to earn some money for college. My father had come down with multiple sclerosis, so I had to shift for myself.

Anyway, I ended up in the Class of 1936 at Lehigh [University] in chemical engineering, which at Lehigh meant you took all the courses that a chemistry student took, plus the engineering courses—heat engines, and physics, and metallurgy, and things of this sort.

When I was ready to graduate, I had to make a choice whether I was to stay on and do some teaching and take a graduate degree there. While I was thinking about it, the professor I worked for, Harvey [A.] Neville, sent me down to New York to work for Bell Laboratories on a summer job for his former roommate from Princeton, R. [Robert] M. Burns, who was the assistant chemical director at Bell Telephone Laboratories. I took a job there for the summer, and before the summer ended—this is in 1936—someone whom I didn't know, a chemist, died. They decided to ask me to take the job. I was worried about my obligations to Lehigh. They said, "Don't worry, we'll take care of that. There are probably ten people standing in line for that job right now."

The Lehigh assignment was the sort of a job that didn't pay very much, but you could take all the courses you wanted free, towards an M.S. and a Ph.D. You took over for various professors, correcting papers, teaching some of their courses, things of this sort.

I ended up at Bell Laboratories, instead, in 1936. It happened that the Bell Laboratories hadn't hired any professional people since 1929. This was 1936—seven years. I didn't realize it at that moment, but I realized later what a wonderful opportunity that was, because there was no one ahead of us for promotion and things of this sort. If you wanted a promotion, sometimes that's not a good deal. It turned out later that I got promoted very early on. I became a supervisor instead of being a chemical engineer. In fact, Bell Labs didn't even realize I was a chemical engineer. They thought I was a chemist.

THACKRAY: Yes, that was a little unusual. They had been hiring chemists.

BIONDI: That's right. I told them that I was not trained to be a chemist; I didn't want to be a chemist. I was an engineer. They said, "We'll put you into the Western Electric Company, but stay with us for a couple of years." The Western Electric Company builds the equipment. So there I was.

The first thing I did was to register at Columbia University for graduate work. I turned down a job in Philadelphia to take the job in New York because I liked the idea of being in New York City. Their office at that time was down in the Greenwich Village section. I passed up a job with Sun Oil [Company] on Passyunk Avenue in Philadelphia. Maybe I'd have done better with them financially, but there I was. [laughter] The pay at Bell Telephone Laboratories was $28.50 a week. At Sun Oil it would have been $40 a week, but I think the Bell Telephone Laboratories route, from my present vantage point at 87 years of age, was the world class route.

I found myself among an interesting group of people, for two reasons. One, they hadn't added any new blood for seven years. Two, the Bell Laboratories was formed by the AT&T Company and the Western Electric Company, a manufacturing outfit, by combining forces—people who were working with materials and their processing into parts of communications gear. The AT&T people tended to be academic. The people they assigned to Bell Laboratories were titled Members of the Technical Staff of the Research & Development Department. The Western Electric Company just called their people engineers, and they put them in an old building down in New York City that Western Electric used for many years at 346 West Street on the Hudson River. You had two sets of people trying to solve the same problems. One was brand new; the other was an old organization that wasn't going to give any ground to these new academic types who were more interested in writing papers and attending meetings of The Electrochemical Society, among other things. The Western Electric people never wrote papers. All they did was, they knew about materials from raw experience. They compared three or four products—salesmen told them how good their materials were—and then they made some tests, but they were not an academic organization. Their aim was to produce reliable products for AT&T and the Bell Operating and Long Lines to affect reliable, long life communication systems.

I found myself in the academic part, but I was really interested in working for the other group. This caused all kinds of dislocations. I wasn't very happy about the spot I was in, so after I got the degree from Columbia—the master's degree in chemical engineering—I decided to take a Ph.D. program at Columbia. I was just starting on that when a couple of physicists, Nobel Prize winners, came over to what they thought was a chemical department at Columbia campus, but it was the chemical engineering department. They wanted to know if the department had a young man whom they could send over to the physics building, where they were working on a secret program.

THACKRAY: This was at Columbia.

BIONDI: At Columbia. This turned out to be the atom bomb project, the Manhattan District Project of the USA Corps of Engineers.

At that time, the Germans, the Japanese, and the English knew that if they took uranium ore, which is 7/10 percent uranium-235—which is what you want to pull out because it's unstable—the rest of it was 238, which was stable. That was the junk in the process. They all knew that if you dissolved this ore in hydrofluoric acid, you generated a liquid which when heated was a gas, uranium hexafluoride of both the isotopes.

One technique to separate out the uranium-235 was to pass it between sheets of metal that had tiny little holes in them. The lighter element had a longer mean-free pass. If you did this twenty thousand times, they figured you would get from 7/10 percent U-235 to 90 percent, which they thought might make a bomb. I was paid by Bell Laboratories; I was working at Columbia University on weekends and evenings on a Ph.D. program. I hadn't picked out a title for a thesis yet.

There were five Nobelists who were at the physics building who were trying to act as engineers. Now, this was a classical engineering problem, you see. This was like petroleum refinery engineering, except it was with a gas—a multiple-stage thing—a continuous process which had the potential of controllability and the production of sizable quantities of U-235. Some of the competing processes measured their product yield in grams which, even if successful, was too low a rate to meet the oncoming of our armed forces landing in Japan.

I said to them as I left, if they would work with flat sheets about a foot wide of nickel manganese alloy, and they first dissolved the manganese out, leaving small and visuous holes, and then these two sheets were put opposite one another in some kind of a holder which was made out of nickel so the hydrofluoric acid wouldn't attack it—you had to be very careful not to let tiny amounts of water in because that would make hydrofluoric acid and cause hole plugging—what would happen is, as the pumps varied in their speed or shut down and started up, a pulse went through this system, and the porous sheet shattered. Obviously it was weak and was all full of holes. They wanted me to do something about it so it would become strong, but not to change the dimension or the tortuosity of the holes. Offhand, it was not reasonable to even try it, but I spent two weeks at it. I tried various techniques—heat treating here, and borrowing a furnace from somebody, rolling it, and all the changes, all to no avail. The stronger the sheet, the poorer the throughput and the separation factor of U-235 in the U-235/U-238 mixture gas.

I told them, "You're doing it the wrong way. You're doing it the hard way. Why don't you take that sheet there and put a piece of cloth behind it, and roll the whole thing up into a tube. It's much stronger, even though it's a weak material." I went out to the men's room and got two pieces of bathroom tissue to show them how weak they were when flat. I rolled one sheet up with a couple pieces of scotch tape added along the length, and it had reasonable strength. They said, "Why don't you do that to produce the basic element of the gaseous diffusion project?" "Oh," I said, "I'm trying to get a Ph.D. here. I'm working for Bell Labs, not Columbia." They responded, "We'll talk to your people down there; we'll get a contract for Bell Laboratories to do this. You can work on it down there in one of their office buildings and bring us samples of your work as soon as you can."

There were about five different ways to separate it. As I say, one of them was a gaseous diffusion program; that's the one I was going to work with. Another one was a thermal diffusion program. There was another one that would ionize the material and separate it in a mass spectrograph kind of thing, and so on.

They had started to build a plant down at Oak Ridge, known as the K25 project—clear the ground, put in the power plant—without having the slightest idea where they were going to get this so-called gaseous diffusion barrier. This is all in the history books that are laying over there [indicates] (1). I finally decided that the way to do this was to take nickel powder, which I got from Clydach in Wales, where they take low quality nickel ores and they extract the nickel from the ore by exposing it, using a powdered ore at high temperatures, to carbon monoxide. That produced nickel carbonyl, a gas, which was later decomposed to produce a finely divided nickel powder and carbon monoxide.

THACKRAY: Yes, I understand.

BIONDI: Then they'd decompose it. You'd get a bunch of particles which looked like a bunch of grapes at high magnification. This is great because you put a bunch of grapes together, and you're getting a lot of tortuous paths. I mixed this all up in toluene with polystyrene, and then I rolled out a sheet. I went down to a secondhand machinery area of New York City and bought a roller that operated on DC, so I had to get a DC connection. Modern labs don't have DC. I found one lab where they were trying to come up with the first television system by mechanical means, a bunch of rotating screens. They had a circuit board that they could call on a generator down in the basement to give them anything up to a couple hundred volts DC. It was great. Maybe somebody was looking down from above, giving me all this handy stuff.

I rolled this sheet, and then I had to get the plastic material out whose only purpose was to hold the powder in a flat sheet. I built a furnace that would go up to about 1000^{o} C, by borrowing some platinum wire from the Bell Telephone Laboratories metallurgy department to make the heating elements for the furnace, which could easily get up to 1500^{o} C. I put this flat plastic sheet containing mostly nickel and heated it up very gently in nitrogen. As I heated it up, I depolymerized the polystyrene to styrene gas. Then I shifted over to hydrogen, which sintered together these nickel particles. One had to be very careful, in heating it up, to chase out the monomer organic material styrene. You couldn't overheat it because if you cracked the styrene, it formed carbon, and you can't remove that very easily unless you put a little water in the hydrogen, and we did not want to expose the hexafluoride gas of uranium to carbon.

I couldn't go out and buy the kind of equipment you can buy today to make all these determinations and controls; I just had to use common sense. Maybe that's what engineering was in those days.

I worked out a program where I could heat it up and produce this sheet about two feet long, about eight inches wide, of pure nickel sintered together that had considerably more strength than the nickel sheet resulting from the removal of manganese from a manganese-nickel alloy that they were using up at Columbia. I would take little samples out, and we would measure the gaseous throughput and ability to separate a mixture of helium and carbon dioxide. From that we could calculate what the heavier elements might do. We were not allowed to work with radioactive materials in a commercial building, and I didn't want to get involved with that part of it. I would send samples up to Columbia twice a month, and they would measure it with the radioactive materials and give me a number on the separation factor and the throughput. A good separation factor was no good unless there was volume going through. They figured they needed a couple of hundred pounds of uranium-235—not milligrams. All my competitors were making milligrams and grams of this stuff, and my sheet could stand the stress of pumps starting and stopping without shattering.

Mind you, I was 26 years old at that time, and I had nobody with muscle behind me to represent me on the Columbia campus. Professor Harold [C.] Urey, who was my boss up there, didn't think the whole thing was going to work in the first place—the whole business of designing the bomb and all that, which I was not involved in. Professor Urey had a few years earlier become a Nobelist by his separation of heavy water, deuterium oxide, which he produced by the milligram.

Anyway, one after another, these programs fell apart. They were actually thinking of scrapping the gaseous diffusion program because nobody could produce a separating barrier. We, the English, the Germans, or the Japanese—we were all working on it. Then somebody remembered, "Who's that young kid from Bell Labs who's up here occasionally? He sends us samples; they look pretty good."

They asked me up and put me in the physics building at Columbia. They told me to send all my equipment up to Columbia, which I did, because it was supposed to be owned by the government—including the forty thousand dollars worth of platinum; it was used as the heater element in my furnace. It was in a junk pile up there on the campus. They called a junk contractor in, and for so much he picked it all up and threw it away somewhere in a dump. I tried to find out how to get that forty thousand dollars worth of platinum out of there, but there was no way I could figure out how to do it. It didn't belong to me anyway; it belonged to the metallurgists who loaned it to me. We had to finally pay them on the government contract we had for that forty thousand dollars worth of platinum wire in a dump.

The reason I used platinum is that it's a very good heating element—better than chrome alloys. In those days the war was on, and I couldn't get the standard heating alloys—like the ribbons you have in your toaster, except in larger shapes—because I didn't have the right priorities.

The Columbia people tried to reproduce what I was doing. In the meantime, before I left Bell Laboratories, I took three of my flat porous sheets, each about two feet long—spot-welded them together, and made a tube out of it five-eighths of an inch in diameter and about six feet long, which I lowered out of the window of the six-floor building downtown to a colleague of mine who was waiting in a taxicab on the sidewalk down below with the roof open. I couldn't take it out in the elevator because it wouldn't fit. People would ask questions, "What is all this?" I took it up to a meeting at Columbia, and they said, "By God, it works!" I had a piece of canvas, the sheet and a steel rod, and I rolled it up—I had watched some people who were rolling their own cigarettes to see how they'd do it with their flimsy paper—and backed it up with a piece of cloth and rolled up the sheet and welded it where the overlap was. They thought, "This youngster can produce a material that has the right kind of properties; it's rugged and has the potential of producing pounds of stuff, not grams." They were trying to figure out where they were going to do this when the chief engineer, who was Union Carbide, came down and said, "You academics are going to foul this whole thing up. We're taking it over."

They took us over, so then I went to Niagara Falls where they had an installation where they were going to do this process. I taught them everything I knew. What I didn't know in the meantime was that my management at Bell Laboratories didn't want to be involved in this program. They didn't want anything relating to a bomb that might kill people to be a product of the Bell Laboratories, so they stopped pursuing seven patents that I had applied for in the name of the government; the government owned the contract, but the Bell Laboratories patent department had agreed to apply for these patents as a support for my work at Bell Telephone Laboratories. As a matter of fact, the man who was doing it turned out to be the best man at my wedding a few years later, Fred Samerdyke.

To make a long story short, the Bell Laboratories transferred all my patent applications and all process details to Union Carbide. Union Carbide was almost a natural to take over the task of producing large amounts of barrier. They had a gas division, Linde Products; a metallurgy division at Buffalo; they had a plastics division, Bakelite at Bound Brook, New Jersey. Frasier Groff visited Bell Telephone Laboratories several times and I showed him my technique. He took it the next step into a factory that produced the barrier needed, or he did one part of that process.

I'll tell you the history of the development of the only successful gaseous diffusion barrier which I executed essentially single handed. The management technique at Bell Labs had a young man report to a supervisor, or when he became a supervisor of three to five MTSs, he became a department head who had anywhere between five and ten supervisory groups, like fifty MTSs. Four department heads with two hundred persons reported to a director; four or five directors reported to an executive director; four or five executive directors reported to a vice president—all of whom reported to the president.

The history books talk of my invention as "the Nix Barrier" which he produced at Bell Telephone Laboratories. I reported to [Foster C.] Nix; I ran my own affairs and talked to him very rarely. Before my arrival, Nix had two technical aides, skilled in doing experiments outlined by Nix and Shockley in order/disorder phenomena in alloys. It was that very work which brought Nix to the physics department at Columbia, seeking to devise techniques using neutrons to achieve an enhanced understanding of the order/disorder studies at Bell Labs. It was in this atmosphere at Columbia that Nix was encouraged to seek an OSRD contract to study the pressed powder metal discs as possible diffusion barriers. It was shortly thereafter, in 1942, that I joined the group and quickly realized that small discs would never produce large quantities of U-235. I proceeded to work on producing sheets that could be made as large as desirable with currently available production facilities. Nix did not contribute to my choice of experiments and only showed interest when the small half disc approach was shelved. He then asked me every few weeks what I was doing, so he could run up to Columbia to report the developing progress "as the result of his ideas;" eventually the barrier producing process and the barrier structure became known around Bell Telephone Laboratories as the Nix Barrier.

All my work in developing my version of the gaseous diffusion barrier is in a notebook of mine that Bell Telephone Laboratories has. I am no longer cleared to see my own notebook. I know from the Bell Telephone Laboratories patent department that Nix had not filed any patent application on my work, which the patent department saw me about weekly.

Nix was not very successful in trying to sell his barrier, and the head of the Manhattan District began to focus on my barrier as the only one which had physical strength, and as good separation and volume throughput as the fragile barrier had, and the promise of producing pounds rather than grams from a continuous process which would meet the amount of U-235 in time to close out the war with Japan. We, of course, never knew these numbers; we only were told they could be met—so let's go into production. At this point the Biondi, then Nix Barrier, became the Union Carbide Barrier, in which work Frasier Groff was knighted as the inventor.

If you look in the history book, which is the black one, you'll find that a chemist, Frasier Groff, at the Bakelite Division of Union Carbide in Bound Brook, New Jersey, was the guy who invented this system I'm talking about and originally applied for patents on. I was told to cut my ties with this atom bomb project and get back to work at Bell Laboratories.

THACKRAY: When was that?

BIONDI: In the mid part of 1943. Let me see. I decided to write a history of all the things I'd done. This is in code. The code is my handwriting. [laughter]

I once won a five million dollar case for AT&T because no one could read my handwriting in my notebooks. If I were dishonest, I could say what I wanted to about it, but actually I was not dishonest; I read it. The company that was violating our patents at that time was National Cash Register [NCR Corporation], which AT&T bought five years later. [laughter]

THACKRAY: I didn't know that.

BIONDI: Early Work. I went up there in August of 1941, so it must have been 1942 or 1943 that I got back to Bell Laboratories. Here it is, 1942. I was 28 years old at that time.

Before I left it, I went down to Washington. Secretary of War [Henry L.] Stimson was in retirement, but he met me in the White House. He shook my hand; he gave me this piece of paper and this little tiny medal. It says here, "Participation in work essential to the production of the atom bomb, thereby contributing to the successful conclusion of World War II. This certificate is awarded in appreciation, August of 1945."

Nix suffered the same ignominy that I endured when Bell Labs ordered both of us back to Bell Telephone Laboratories work. The MDP for our part was over. Nix left Bell Labs shortly thereafter. He envisioned fame as the inventor of the only successful gaseous diffusion barrier—fame, even the Nobel Prize, which he often told his physics friends might result, and which he hoped for; his associate Bill Shockley made the Nobel goal for the invention of the transistor in 1956. As fate would have it, Walter [H.] Brattain, Shockley's co-Nobelist, did much of the transistor reduction work in one of my laboratories, since much chemistry and electrochemistry was needed and not workable in the physics labs.

But as the French say, "Se la guerre." All of us, big and small, real and fictitious, were able finally to get enough U-235 to test a bomb at Trinity , Los Alamos, and drop one on Hiroshima, and while we did know it at the time, the rejects of the gaseous diffusion separation of U-235 from 238, the 238, was sent to another MDP in the state of Washington where the number of neutrons in the nucleus was changed to produce the first man-made element, plutonium, which is perhaps even better than U-235 for bomb purposes. It was first tried out on Nagasaki.

All together there were of the order of twelve ways to separate U-235 from U-238, none of which could continuously produce pounds of U-235 rather than grams or milligrams. At one point in history before the Bell Telephone Laboratories Biondi Barrier was taken from my hands and taken by Union Carbide into their "let's do it" factory, the whole program for an atom bomb might have been canceled. You can imagine the consequences in subduing Japan, et cetera. Manhattan District to August 1, 1945 cost $1,665,142,423.70. I personally received one dollar of that amount and sunk back into history—some of which was far from real. Nowadays, movie and TV companies buy and sell each other for ten times that amount.

Coincidentally, two weeks ago, a representative of Columbia University called me and said he would be in Florida the first week in December, 1996, and would like to drop in to see me. He wanted to invite Ginny and I to meet the new president of Columbia at a lunch on December 12. I told him about the Manhattan District work at Columbia—all a big surprise to him—and told him the chemical engineering department would not accept my development of the only barrier that worked as my Ph.D. thesis. He listened carefully and after he returned to New York City, wrote me a letter that he was bringing down the chairman of the chemical engineering department to the president's lunch, where I could meet and talk to him.

The first gaseous diffusion began to operate in the K25 plant at Oak Ridge in April, 1945. Four months later, August 1945, the first atom bomb was dropped on Japan. The diffusion plant was the last item filled in the process jigsaw puzzle. If it had not worked, the whole program would have not produced on time, if ever. Boy, did it produce!

As the *History of Engineering and Science in the Bell System—National Service in War and Peace 1925-1970* (2) says on pages 348 and 349, the barrier development at Bell Labs "was due primarily to F. J. Biondi," and "more than thirty years later, the Bell Telephone Laboratories method is still the process chosen for fabricating the long life, reliable material, not only in the Oak Ridge plant, but also in the Kentucky, Ohio, and Georgia plants later.

THACKRAY: That's quite fine.

BIONDI: He gave me a one dollar bill and made me sign a receipt for it, because that was all the government was going to pay for it. While all this was going on, the Bell Laboratories was paying my salary. They decided it would be too complicated to have me paid by the Manhattan District directly.

That's hanging up there on the wall of my office (3).

THACKRAY: Well, that's very interesting complex chemical engineering, but it's a long way from electrochemistry, isn't it?

BIONDI: Yes. In the meantime, do you see this ceremonial saber from when I was commissioned a first lieutenant? I was an ordnance officer, and they wanted to call me up to duty. I'd been made a second lieutenant and a gentleman by an act of Congress. Did you know they do that?

THACKRAY: No.

BIONDI: I didn't know it either. They not only make you an officer, but they make you a gentleman.

THACKRAY: That's nice.

BIONDI: I keep that here in case some burglar comes in or something. [laughter]

Here I was. Disconnected from this thing, what I didn't know was that the discards from the process of separation—U-238—was sent out to the state of Washington, where there was an installation, the Homford Engineering Works. They'd bombarded it in a carbon pile and made plutonium, which is even better than uranium-235.

The gossip I had—to finish this thing—was that using my barrier at Oak Ridge, which they fitted in after everything was put there, they made enough material for two and a half bombs. I didn't know about plutonium at that time. They used the first one in the first test in Trinity, the code name for the first test at Los Alamos. They didn't know if it was going to work. In fact, they were afraid it might overwork, that it would keep on going and destroy the whole state. They actually talked about these concerns. Then they worked at the University of Chicago, under the athletic stadium, where they had their laboratory. They were afraid it was going to take off and they couldn't control it. [Enrico] Fermi was there. He developed how to control it with the carbon rods. He died after the war at a young age due to extreme radiation exposure.

Well, the Trinity thing worked. That was about June 1945, I think, but I wasn't quite sure. I wasn't involved in that phase. While I was not supposed to know about it, in any big organization like that, there's always underground information going on. The spies can steal everything that way, and they did it. The Russians knew about it, because we were living with a bunch of guys in New York who seemed to be more interested in Russia than they were in the USA. *The Daily Worker*, the daily newspaper of the United States Communist Party, was the paper they read. I read the *Herald Tribune,* a Republican paper which went belly-up after the war.

In the meantime they were designing the bomb and, as you know, they dropped number one on Hiroshima. Then number two, which I didn't know about, was a plutonium bomb. They never tested that. They dropped it on Nagasaki for the first time; that was the test. They'd actually had another conventional bomb built in the event that bomb didn't work, so it would explode the whole thing. The Japanese at that time were still enemies. Why give them the advantage of where we were?

We had enough then. We tested one bomb; we dropped one. We had a half a bomb's worth left over. If the Japanese had held tight, we'd have had to wait about another three to six months before we could get enough to drop another one on them, so that was all over.

Just to show you how that thing works, my wife had worked in Atlanta, Georgia, for an aircraft company, Bell Aircraft, on the B-29, which dropped the bomb; how's that? She left Atlanta and came north to marry me in October, 1943. On August 6, 1945, she called me at Bell Labs and said, "President [Harry] Truman has just announced he dropped something called an atomic bomb on Japan. Would you know anything about it?" I said to her, "Why, what makes you think that I know something about it?" "Well," she said, "you came down to visit me in Atlanta once, and you took me to a movie about Madame Curie, and you explained a lot of things. You seemed to know more about radioactivity than the average person." This culminated what we were doing. I said, "I was in West Virginia at the International Nickel Company plant making elements of the program to produce U-235, and it was from West Virginia I came down to Atlanta to see you."

So much for that history. I went back to Bell Labs and got into the electronics business. The group I had worked with, who were all chemists, had moved to New Jersey. I didn't want to go to New Jersey in a new lab at Murray Hill. Housing was in short supply so I opted to stay in New York City. Greenwich Village in downtown Manhattan was a great place to live. I was deep into classical music—many good symphony concerts, the opera, ballet, et cetera.

THACKRAY: When was that move?

BIONDI: That was about 1943.

Across the street from the main Bell Telephone Laboratories building at 463 West Street, New York City, Greenwich Village area of Manhattan, there was a building where we designed and made electron or vacuum tubes. At that time we were up to our hips in trying to help the English with magnetrons for the defense of Britain. The big problem was, these magnetrons were built in England and shipped out with a Ph.D. with each one of them, because each one of them was different. It wasn't like building cars that are all alike. They had a poor short life, like twenty or thirty hours, which maybe was all right if you were fighting a battle and you're constantly getting in new people, whole new magnetrons, but it was no long-term solution to the radar problem. The Germans apparently didn't have radar at that time.

The first job I was put on was to see what you can do about this cathode which produces the flow of electrons but which is destroyed by high-voltage back bombardment from the anodes. It was a sleeve of nickel about like a thin pipe, about an inch in diameter, this big. Inside was a tungsten heater. The outside is coated with a mixture of barium and strontium carbonates, which you break down to the oxides before the tube is sealed. When you heat the cathode in a vacuum, you get little patches of barium and strontium metal which reacts with some of the carbon coming out of the nickel sleeve to produce patches of barium and strontium and gives you a flow of electrons with the proper voltages on them to the cathode. High voltage back bombardment would blast that stuff off, so they had short lives. [interruption]

I quickly concluded that if we coated the cathode with a sponge of the same nickel powder I used in the uranium-235 diffusion barrier, filled the sponge with the carbonates, reduced them to their oxides, then inside the sponge pores on applying anode voltages, the barrier oxides would not be bombarded off, to end life, but would continue to generate electrons. It was just that simple. In a short time we had a two hundred-hour life and counting, rather than the older short twenty-hour life. The Battle of Britain—shooting down German fighters and bombers improved from the British viewpoint, and I became a cathode expert overnight in a new turn of my career.

The people designing electronic tubes were mostly electrical engineers and a few physicists who took the wrong road somewhere and became practical people rather than theoretical people. I made an analysis of their cathode problem. There was something like twenty different analyses of nickel used to make a variety of vacuum tubes. Each one who designed a new vacuum tube said, "That's my private composition." They were even secretive about the damned thing.

I looked at it and decided what they really wanted was, some wanted a highly active nickel that had a lot of carbon in it which came out and reduced the strontium oxide to strontium, for example. If a less active performer was wanted, a very low amount of carbon or tungsten was used. However, if you wanted a tube to go into the broadcast business, or into the business of magnetrons, you wanted something that gave you high activity. High activity is generally associated with low life; low activity is long life, such as 20,000 hours—833 days—2.28 years, which was considered a long life in pre-semiconductor days. We really wanted a minimum of 40 years life. That is why we went after the semiconductors.

Now in the telephone business, the whole emphasis was generally for long life and reliability. In those days, if tubes lasted twenty thousand hours, they thought that was great, like two or three years. I thought that was pretty bad. The interesting thing about it was that I went to ASTM [American Society for Testing and Materials] at that point. Do you know them in Philadelphia? I went to see if I could not help write industry-wide specs for the materials used in tubes, reduce the types and varieties of materials used, resulting in lower costs and a larger centralized supply, reducing costs.

THACKRAY: Yes.

BIONDI: I joined their Committee B-4, which specifies heating alloys. It was in that committee that I made my suggestions that we standardized three alloys of nickel for the electron tube cathodes. If I had first consulted all of my own tube designers and then the hundreds of tube industry designers, I would never have succeeded. I stuck to my three alloys, 110, 220, 330, and it was accepted by the industry. The Bell System bought a small amount of the total needed in the country. It was just before the days of television, but the radio business was a big business, and they bought that stuff by the ton. We bought it by the pound. If we could get what we wanted to be used by everybody else, it would be in our interest, a more reliable supply at lower prices of three categories of cathode nickel. I talked to the International Nickel Company—old Manhattan District contacts—and they said they would be delighted with three grades rather than hundreds of grades.

I sold my managers on this and they said, "Go pursue it." Then I thought about it. The problem is ASTM worked by committees. I had shoved through the cathode specs in Committee B-4, which was responsible for heating alloys, not cathode or glass or steel or tungsten for use in tubes. There was a glass committee. If we wanted to be sure to have a specification describing what the glass we used in our electron tubes was, we had to go to the glass committee, which was interested in windows primarily and glass bottles. They had to write these specifications, so we didn't get very far. When we wanted steel wire, we had to go to the steel committee. They were interested in railroad tracks.

I went to the board of ASTM and said, "Why don't you start a new set of committees? Instead of a committee for each type of material, how about a committee for all the materials for an industry?" They agreed to it quite quickly, and we organized F Committees; F-1 covered all the materials in the electronics industry. I remember F-2, I think, was all the materials for the surgical industry. Now that's a big deal. They had never thought of that idea. If we wanted to write a standard on steel to be used, or tungsten to be used, or glass to be used, we did it—we, the people in the electronics industry, not a bunch of other people who regarded us as a nuisance.

You can get to see the pattern. I was the organizer. As soon as I got that organized, I dropped out of ASTM. They wanted me on their board and I said, "No, I've got other fish to fry." In the meantime I'd gotten married, and children were coming along, and I got involved in community affairs. I was mayor of the town, and I was chairman of the board of a four hundred and fifty-bed hospital. I organized baseball teams, Little League, Intermediate League, American Legion team which took the state title the first year it was in the League, with my wife naturally being involved with our three boys.

THACKRAY: [laughter] You lived a quiet life.

BIONDI: My wife complained about it because she had to take care of the family. Let's see if she's in here. Ginny?

[VIRGINIA] BIONDI: Yes?

BIONDI: Come on in. I want to introduce you. This is Arnold Thackray.

THACKRAY: We're having a good conversation here.

[VIRGINIA] BIONDI: Yes.

THACKRAY: He seems to have been a busy man.

[VIRGINIA] BIONDI: In those days he was, very. He spent a great deal of time at the ASTM.

BIONDI: In the early days, yes.

THACKRAY: He ran the hospital in the evenings, then?

[VIRGINIA] BIONDI: That started in 1956. That was the year our third son was born.

BIONDI: I told him we were both involved in community affairs. She was on the planning board and things of this sort.

[VIRGINIA] BIONDI: Yes, plus we had three boys, and Little League, and basketball, and football—you know the story.

THACKRAY: I was one of three boys myself, so I know how that works from a mother's point of view.

BIONDI: As a matter of fact, there's a picture [pointing to the wall] of the Columbia campus. The building I was working in is right over here outside the picture. They're now trying to collect a billion dollars. They suddenly discovered I went to school there. You know how that is.

Well, you ask the questions.

THACKRAY: What was your position in Bell Labs then?

BIONDI: As soon as I got over into this building with the people who designed electron tubes, they remembered they had a chemist somewhere in the building. They got him, and I became a supervisor. Before I knew it, I had four or five people. I was concerned about them, seeing they were on the good programs, and I was starting to let go of doing things with my own hands, which I didn't like very much because I was just out of school a few years. Before I knew it, I was doing things that I wasn't trained to do. I wasn't trained to be a manager, and I didn't like it very much—merit reviews and rate reviews and hiring people. In my 43 years with Bell Labs, I hired ninety Ph.D.-trained people, mostly chemists and metallurgists and electrochemists and this sort. I fired two of them out of ninety. Engineers generally went to the Western Electric Company, our manufacturing arm. Since they and I were birds of a feather, I was soon getting introduced with the Western Electric Company. I did an unheard of thing. I put Bell Telephone Laboratories people in residence at the factories to better understand and deal with the plants. It was great.

To get those ninety people, I had to look at nine hundred of them; ten to one was the ratio. We had a system. Our people, graduates of most of the universities in the country, and even overseas, looked at graduate students from their first year and kept track of them for two or three years. By the time they were ready to graduate, we had all kinds of information on these people. It was a real good system. Other people, like my department head, would interview them, and they'd finally get down to the point where they thought we ought to make an offer. I would then interview the person and help make the decision. I was proud of them.

The last couple of years I was there, it was really odd. I didn't see a single Caucasian boy. They were all Orientals. Most of them didn't even speak English, but they were perfectly bright people. I hired two or three of them—especially one electrochemist I hired, Yukata Okinaka, a Japanese young man, very bright. He did very well for us. He was given awards by The Electrochemical Society. I also was involved with Dennis [R.] Turner, whom you talked to. I pulled him out of the chemistry department and brought him over to my area as a supervisor of electrochemical activity. By that time I had risen to the title of director of the electron device process development laboratory.

I was no longer in the chemistry department. I'm now over in the electronic device applied groups. By that time I was in the transistor business. Then, about 1948, my office was next to Bill [William] Shockley who got the Nobel Prize—he and two others. You will remember he was a companion of Foster Nix, who made contacts at Columbia University which eventually got me involved in the development of the gaseous diffusion barrier.

I lived among the physicists and electrical engineers. They regarded me as a different person because I wasn't a chemist. There was a lack of ability for physicists and chemists to talk to one another, but I had sort of carved a spot amongst the physicists, so they would talk to me. In fact they did most of their work—other than the theoretical work—in my laboratories, with my people helping them or vice versa. By that time I probably had about forty people working for me.

We dropped the tube work and got involved in the early days of germanium. Of course germanium isn't used anymore.

By the way, I edited a couple of volumes of *Transistor Technology*. Those were the first publications. Actually, the first one was a secret manual. The Army came in and decided that they didn't want anybody to know about this thing that we called a transistor.

[END OF TAPE, SIDE 1]

BIONDI: Believe it or not, the Army thought we could keep this thing secret, so they put a secrecy label on everything, including a volume that I and others had helped put together. It wasn't until about five years went by that we persuaded them to make this thing public. This way we could get patents on the various ideas involved.

That's where publication comes in, in The Electrochemical Society, for example. At that time I had a bunch of metallurgists working for me. They were working with physicists generally, occasionally an electrical engineer, designing semiconductor devices. Every time they had any kind of a new idea, a new fabrication process that they tried out in small scale, they were publishing it in The American Physical Society publications because the principal team member was generally a physicist. It was a journal that was not read by other chemists and other metallurgists, and they were unhappy about publishing in this journal. Of course in *Physical*

Reviews, a publication of the Physical Society designed for this quick review in a fast moving technology—and the first publication which helped in getting patents—new ideas would come out quickly. The patent department was very interested in having us do this to beat other people to the punch. That's where people were all over the country, and the new Japanese who came in at the time and bought licenses from the Western Electric Company to use our patents. I had to do something about this. That's where I met with the president of the American Institute of Electrical Engineers and the president of The Electrochemical Society.

THACKRAY: Who was that?

BIONDI: R. M. Burns ran this almost as his private publication for years. I don't know who took his place, but whoever it was, I met with him. I was going to propose The Electrochemical Society as probably the best place for my people to publish in. We were thinking of starting an electrochemical division for the society. That developed. The American Chemical Society, a respected old society with many divisions—tremendous—was not interested. They were in the sixty thousand range or so, whatever it is. I don't know what it is today.

THACKRAY: They have one hundred fifty thousand members.

BIONDI: Is that what it is? I have all their insurance policies, by the way. [laughter] I've been a member for fifty-six years. Their *C&E News* [*Chemical & Engineering News*]—I look at it religiously.

Anyway, one of the principal people I had involved was in the Chemical and Metallurgical Engineering Society. He tried to get their publication to be the one that would be the flagship for the process activity of semiconductors. There's a lot of processing in semiconductors, much of it electrochemical in nature—a lot of etching, a lot of plating, a lot of depositions of oxides, for example, from gaseous mixtures of various sorts. Most of the aging problem, early life problems were electrochemical in nature. The ratio of designers of solid state devices to people who make them—the chemists, organic, electrochemical and the metallurgists—is about 8-1. One designer could keep these eight process people busy building all the various pieces of equipment that had to be built, things of that sort, from new ideas.

I conceived the idea and sold it with a little foot-dragging on the part of some of the other societies. IEEE [Institute of Electrical and Electronics Engineering] wanted to handle it. I said, "They have no history in this business." They were compartmentalized, too. They had people interested in batteries, but only because of power. They weren't interested in low-power batteries and things like that, or even solar cells.

Some of our public relations guys inadvertently called them batteries. I used to go to a town nearby to get my hair cut at noon. The barber said, "How's your battery doing?" I thought, "What's he going on about my battery for?" "Oh, it's doing fine." Next time I went down, he asked me again. I said, "Look, you asked me that a month ago. Why are you interested in my battery in my car?" "Oh, I'm not interested in the battery in your car—but you're at Bell Laboratories, aren't you?" "Yes." "They've invented a new battery." "Oh," I said, "that isn't a battery. The public relations guys call it that. It's a photocell. It's a positive/negative junction which when exposed to sunlight generates electrons. Sure, you might call that a battery, but it's not." The barber was trying to be nice, talking to people, to show that he knew about Bell Laboratories.

THACKRAY: Where did your own connection to The Electrochemical Society begin? Was that through R. M. Burns?

BIONDI: No, it wasn't, surprisingly. I should have done that, perhaps. I went to one of their meetings. Some of my people were publishing papers in it because they were members.

THACKRAY: Papers on what sort of subjects?

BIONDI: On solutions to selectively etch, for example, trenches of a certain size and width and things of this sort; and electroplating, contact zones, things of this sort.

I met some of the leaders—I don't know who they were at the time—had lunch with them, and said, "I'm proposing to our people that they publish all their papers in The Electrochemical Society journals and present them at The Electrochemical Society meetings. How do you react to that? Would you welcome it?" They'd have to think about it for a little while, so in the afternoon they collared me somewhere out in the hall. They said they'd talked about it and decided that was a pretty good idea, and would I help organize it? I said I would. "I just want to tell you one thing. I'm not a member of this society." "Oh, you're not? We thought you were. We'll take care of that right away." I wrote a check out for whatever the fee was, and I became a member of the society.

THACKRAY: This was in about what year?

BIONDI: Oh, I would say this was in the early 1950s. For a while I attended a lot of their meetings, and I chaired some of the papers section.

THACKRAY: They hadn't got a division at this time for solid state.

BIONDI: No, that's right. Then I found I was spending more of my time in the halls talking to people and worrying, "Is the projector for that meeting there or not? Did so-and-so arrive, or not?" I wasn't attending any of the papers; I was in the hallway. I said, "This isn't for me. It's a waste of my time," so I left the society at that point. I got involved in other things.

THACKRAY: When was that?

BIONDI: I would say this was in the early 1960s. I kept the journals coming for years. I hated to throw them out. In fact, I had such a collection of them that I tried to give them away to small universities, and they didn't want them. Finally, I found out that the Chinese would take them for their universities. The only catch was, I'd have to pay to ship them there—box them up and apply for an import license. [laughter] Can you imagine that? Giving me a hard time. I told them to run up a tree. I finally threw them away. Nobody wanted them. They were old journals, but this would be great for people who are starved for material.

For example, I have a "Paris" tube story. Before I left the tube business, and when the Germans left Paris, their military took the switching tubes out of the telephone exchanges. Not all of them, but enough of them so they were inoperable. Our military flew samples over to the U.S., believe it or not. I was called in one morning to talk to someone from the U.S. Army Signal Department, I guess it's called. They wanted us to make a couple of hundred tubes, all that were necessary, and they had three samples of the three different kinds of tubes. We looked them over, and we decided, "I think we can do this, but it will take us at least thirty-six hours." "Can't you do it faster? We want to get these exchanges working." "All right." We brought some cots in, had the cafeteria across the street bring over stuff to eat, and we worked right through, around the clock, for about twenty-four hours.

First of all, the electronics guys had to test the tubes to find out what their performance was like. Then the mechanical guys took them apart and measured the thickness of everything. By God, we sent those tubes back in less than thirty hours. We never heard from them, so they must be working. [laughter] You think they'd let us know. They never told us.

We thought they'd let us know if they were not working. The Germans expected to be back. That's why they took the tubes out; they didn't want to destroy the exchange. They really thought they were going to get back, I suppose.

THACKRAY: Let's talk about the creation of solid state activity within The ECS.

BIONDI: As I recall for the time I was with them, they were concerned mostly with process problems. We had a lot of etching to do. The way you etch a design into silicon or germanium beforehand is, you coat it with an oxide of the silicon or the germanium, or you sometimes put silicon dioxide on germanium. Then through a photo process, you put a photoresist on it, and you optically impress a design. Then you put it in acids, and you etch out where the photoresist has not been activated by the light.

In those early days we made our own photoresists. I got Eastman Kodak [Company] involved in the thing, and they became the principal supplier, but it took them five years to crank up speed. In the meantime, we had photoresists that were better than other people's photoresists. Other people had techniques for etching that were better than our techniques. After they'd patented these things, it would be nice to share these things among one another; but The Electrochemical Society, as I recall, was not very much involved in growing single-crystal silicon or single-crystal germanium before then.

That was all done in the Bell Laboratories. I don't know where they published that work because I wasn't involved in that aspect of it. The metallurgists had that equipment across the street in another building. Once the silicon was grown, I was more involved in how you sliced it, how you oxidized and how you impressed a pattern on it, and how you etched that pattern, how you made contacts on that pattern. Then how did you encapsulate it, and how did you wash it. How do you make sure the finished transistor or circuit is free of conducting ion or particulate matter which may be a spring for atmospheric acidic gases? Here is where electrochemistry comes into play and where deleterious reactions should be understood and presented to all.

We had to install the first clean room in the industry, because tiny amounts of sodium had a tremendous effect on leakage between very tiny lines that were tenths of mils apart, and we had to develop pure water to wash out the conducting ions. I had to work with the water people. The deionized water wasn't good enough. The particular young Ph.D. who did this wasn't in his room one day when I brought two visitors in. I had to explain what was going on in there. The next day I called him, and before I could get very far, he said, "Yes, I can tell you and two other people were in my laboratory yesterday." "How could you tell?" "Well, you were all breathing out carbon dioxide. Carbonic acid was formed in my water, and it registered on my meters." [laughter] That's how pure that water was; he could tell me how many people were there by the magnitude of the change in conductivity of the water. This is the guy who invented that duck that dips in the water. He invented that. Never made a nickel on it. He sold it to some toy company who made a mint. Miles [V.] Sullivan—I still remember him.

THACKRAY: Miles Sullivan, yes.

The Electrochemical Society would be where all these sorts of processes were then published.

BIONDI: That's right, the work that was mostly electrochemical in nature.

I really didn't appreciate it, and the older I get, I think about it. Electrochemistry is a part of chemistry that the usual inorganic chemical group has over in some sidebar; but if you think about it today, with the tremendous plant we have around the country in bridges and highways, with a little bit of electrochemistry—with a little bit of electronics engineering to generate DC currents and feed them in back—you could prevent corrosion of all these facilities that rust and have to be scraped and painted and then finally replaced. I tried to sell this idea to a man who was on the Federal Commission on Bridges, and he was polite about the thing, but he wasn't going to recommend any large effort in spending money to develop these techniques.

The small electrochemical circuits are generated, are called corrosion. If you can buck it by putting another circuit to buck the corrosion potentials, you can do that with photocells and some circuitry, no moving parts. But somebody has to develop this industry. It's still lying there undeveloped.

That's just this one example. I'm pretty sure there are tremendous examples of electrochemistry in the human body—microcurrents human and man made. I'm sure of that. Fifty years from now we're going to have replacement parts in the way of chips in the human body. Somebody has to do this.

I didn't wave a flag saying, "I'm the guy who put this in The Electrochemical Society, and then organized a division, and it's going very well." I dropped out because I had solved my problem. I had found an outlet for my people; they were in capable hands.

THACKRAY: Were you actually engaged in the organization of the divisions in The Electrochemical Society?

BIONDI: No. After I found myself wandering around the halls and not getting involved, I figured I'd done my job, just as I did with ASTM.

I'd done my job, and we got on to other things. I had more problems to solve than I had time, so I had no time for the luxuries of getting associated with the society and getting an officer slot, vice president or president, and going to meetings and giving talks. That wasn't my job. Back home at the Bell Laboratories, I had things to solve. I was being promoted. I became a department head, and I became a director. By the time I retired I had four hundred people working in, let's see, Murray Hill, Holmdel, Whippany, Allentown, Reading, Boston, Cincinnati, and Kansas City. In the last five plants I had Bell Telephone Laboratories people in residence in a Western Electric plant. This is where I had people. I spent my last five or ten years in an airplane, running around talking to people, listening to what they were doing, and coming back home and trying to sell my management for money, support, and things of this sort. I was a technical manager—hiring people, firing people where necessary, and all that sort of thing.

Dennis, for example, Dennis [R.] Turner. He was more or less of a fundamental chemist when I met him. I brought him over from Research and put him into the applied chemical group, the electronic materials and processes laboratory. I gave him the job to design an electroplating line to gold plate the wires in the ubiquitous telephone connector, these little tiny plugs that go into the back of telephones—you've seen them?

THACKRAY: Yes.

BIONDI: They're little gold-plated wires. The Western Electric Company decided they didn't know anything about gold plating. This was being made in someplace out in Idaho. They had seven subcontractors they had gone through who couldn't produce a plated spring wire that didn't have holes in the plating, so it corroded. It didn't meet the Bell Labs specification. We did it in a lab. We produced gold without pores; without holes in it, the substrate metal, which was a spring wire, didn't corrode and come through and oxidize and become an insulator instead of a conductor in the surface of the connector wire.

I told Dennis, "Why don't you build a machine? I'll get you the money. If you can make this to meet all our requirements and they don't have to go out for six subcontractors, they can do it all themselves." The last thing I did before I retired was, Dennis delivered this machine to the Western Electric plants in Idaho. The manager called me up. He said, "I know you're retired, but I want to say this is the first machine we ever bought from anybody that worked after we plugged it in." [laughter] As far as I know, that machine is still running.

Dennis had never done anything like this before. He had to build all the facility, and he applied his mind as a scientist to it. He not only built the machinery in its physical form, but he also built the techniques for analyzing all the solutions, adding to it whatever it needed. It had a whole bunch of circuits in a cabinet that stood six feet high. It was a great job. It never would have been done by a chemist all alone in a laboratory. He had to have circuit designers help him. I had all those people, you see, in various parts of my organization.

THACKRAY: As your own career went on, how central was the transistor to <u>your</u> career?

BIONDI: It was everything, as a matter of fact. It sort of pushed everything aside, especially in its early days when the reliability was at a very low ebb. I had to persuade these hard-headed electrical engineers that this was because contamination was getting onto the surfaces of the transistors. You needed to wash these things in very pure water as a last step, and you had to put them together in rooms that didn't have a million particles per cubic foot, like this room here. I'll bet there's two million particles right here you don't see.

I had a young engineer working for me whom I borrowed from the Sandia [National] Laboratories. We had to run the Sandia Labs for the government and had Bell Telephone Laboratories people in management. I brought him into Murray Hill and supported him. He's the fellow who developed the hepafilters that allowed us to filter particles of less than one micron size out of air as we never did before. I'll tell you what happened. Sandia was subcontracting to build navigational gear used by the Air Force and the Navy, in someplace up in Syracuse, New York. They had learned up there that they had to scrub all these particles out of the air, and people had to suit up in the clothes, seal them in, because the dirtiest thing in a room was the person. This young fellow's idea was, we would outfit a couple of large moving trucks with benches and filters and fans to bring air in and clean it, and to bring water up from the tanks that we had and purify them. When we got contractor A, who might only last for a year, we'd take this moving van up to his place and move it up back of his plant, open up a door. He ran it. When the contract was over, he shut the door, got a truck up. We hauled it away and took that van to the succeeding contractor. We didn't have to wait for them to build new rooms all the time and teach them all the experience. We moved it with the job. I thought it was a good idea, and it worked.

I used that in the hospital later. In deep-hip surgery, it's about a six-hour job in these dirty rooms with these surgeons spitting in the wounds. They have masks on, but they're sodden. I went over to England where one of our licensees was. I had talked to them five years earlier about hepafilters. One of the people I talked to had a brother who was a surgeon in hip surgery; he had installed these in England. By the way, the surgeons in England are addressed as "Mister," not as "Doctor." [laughter]

He had reduced the rate of having to reopen many hips because of problems where the wrong materials had gotten in from the air, from 7 percent down to 2-1/2 percent. I sent a surgeon over from New Jersey to find out what he was doing and paid his way. He came back, and I said to him, "We're going to put a room like that in here." He said, "No. I'm the head man in this department. No." I said, "You don't realize I'm the chief executive officer of this hospital. You're going to do what I tell you." I put in a hepafilter room in Orange, New Jersey. I put them in gowns with the umbilical cord on the back so we brought air-conditioned air in and took it out. They didn't sweat, didn't have to spit with masks on. We put Xs on the floor where they were allowed to stand, because they couldn't have the air washing over them, because they were the dirtiest things in the room, even with all this equipment on. We got the same—something down to 3 percent; it was from 12 percent to 3 percent.

When I went out of office four years later, they closed that room. This guy was still there. [laughter] He wasn't going to have some non-physician telling him what to do. Would you believe that? Terrible! It's hard to believe that. I had sent people from the Bell Labs down there to help them design this room and to advise them what to buy and things of this sort.

I was always doing five things at one time. I was running a baseball team then, and being a managing mayor of the town. [laughter] I had sort of a fractured existence running from one thing to the next and not getting any understanding of things except what people told me. I had to depend on them; I couldn't do it with my own hands. These weren't my own numbers; they were their numbers. I had to depend on people, so I was careful to hire what I thought were good people. Some of them are still there.

You know that AT&T no longer owns Bell Labs, which has been spun off as an independent company called Lucent. The operating telephone companies and independent telephone companies were reluctant to buy telephone equipment from a company, Bell Labs, which was owned by a competing telephone company, the AT&T.

THACKRAY: Yes.

BIONDI: Let's see if I've picked out the right one. Yes, here he is. Joe Abys, technical manager of electroplating development, advanced light-weight products research, Murray Hill. He was awarded the D. Gardner Foulke Memorial Award by the Metropolitan New York Section of The Electrochemical Society. [laughter] He was the last person I hired, before retiring.

THACKRAY: When you look at transistor development over time, how important was The Electrochemical Society as a forum for discussion of developments? Was it the most important place, or just one of several?

BIONDI: As I understand it, first of all, it depends. Important to whom? It was certainly important to the people doing the work. They were published in a journal that their peers would read, not in a physics society journal that their peers didn't even know about. That was an important thing to them. Obviously these were key techniques, and they're still used today. As a matter of fact, these techniques are what limits the absolute growth to finer and finer dimensions. They're thinking of getting down into the angstrom area.

I'll admit to you that I was preaching for the last five years there, "We're going to try to get all wet chemistry out of the transistor processing; we'll do it all in ionized gases." The people who made the transistors, especially in the factory, they loved that because of the cost of these chemicals. Now, for example, I was on the committee at Bell Labs on the environment.

The last thing I did before I retired was, I shoved across a requirement that manufacturing engineers have to include in their cost of product the cost of protecting the environment from the outfall of their products, processing waste. It was amazing how quickly large decreases in contaminating chemicals, acids, bases, solvents of all sorts occurred.

For example, hydrochloric acid used in the Allentown plant by the ton was bought by overhead like they buy water. It wasn't assigned to any particular product. This time, when a fellow put in an etching process that required hydrochloric acid, he not only paid for the acid, but he paid to neutralize the acid. They weren't very happy with it, but it's the only honest way to do it. You'd be surprised how much the hydrochloric acid dropped down in use. They found other things that worked just as well, things that didn't have the problem of reclaiming them or neutralizing them, things of this sort.

That's when gases began to make their presence known. Take a gas and ionize it; obsolesce the liquid using groups and get on with another technology, ionized gases. You're going to have new people looking in to find out what's wrong with what you're doing today, what will be a better way to do it. That was my constant source of preaching, to stimulate them. Don't sit back and say you know how to do it—not good enough. Learn how to do it better and cheaper and more reliable, more effective. You can see I was an ion bull to start with in this organization, but I flitted around among these things, and I did very well for myself and the people involved.

Batteries—I was involved in fuel cells. Are you familiar with fuel cells?

THACKRAY: Yes.

BIONDI: In the landing on the moon, they were going to rely on fuel cells. We, the Bell Laboratories, had a bunch of people down in Washington consulting with NASA [National Aeronautics and Space Administration]. Ian [M.] Ross was down there; he was president of this subgroup. He finally came back to Bell Labs and became my boss. He called me from Washington during mid-1963 and said, "We don't know whether we can do the moon job because we're not sure the fuel cells are going to work. We have three subcontractors, and we get all kinds of mixed signals. We may have to scrub it. Would you get a committee together of senior people, go out and talk to these three suppliers and let us know what you think?" I did this. Of course we had a very short fuse on this; we had to do this all in two to three weeks. We made a recommendation, and I'll show you in the history what they said about that. I think I have it here. This is a book on *History of Engineering and Science in the Bell System, National Service in War and Peace (1925-1975)* (3). This is it. "Bell Labs on occasion also assisted in evaluating the status of technical development. For example, both the Gemini and Apollo programs' internal power of the spacecraft was supplied by fuel cells utilizing the gases hydrogen and oxygen, producing water and a flow of electrons, a relatively new and unproven technology. By mid-1963 there was concern that none of the three commercial contractors carrying out the development would come through in time, thus seriously delaying flight schedules. Three specialists from Bell Labs: F. J. Biondi; [N.] Bruce Hannay, who later became a vice president; and Upton [B.] Thomas, who was an old Electrochemical Society guy from day one—I think he was born in the society; he was always an electrochemist—were in it. It says here, ". . . agreed to survey the different developments relating to fuel cells. In late 1963 they reported favorably on the prospect for success with one of the three contractors. They were, as it turned out, correct." How's that? That means they didn't believe us. [laughter] Apollo 8 was the only one in the Apollo series which actually landed on the moon and then successfully returned to Earth. Apollo 13, the subject of a thriller movie in 1996, almost did, but a leak in one of the fuel tanks leaked one of the gases. They very luckily got back to Earth.

Oh, by the way. Here is the thing on why they disclaimed any credit for the Manhattan Project here. It says here, "Samples of the Bell Labs material were being checked weekly by test labs at Columbia . . . The process was due primarily to F. J. Biondi with contribution later by Ed [Edward M.] Tolman." Now it says here, "More than 36 years later, the Bell Lab process is still the process chosen for fabricating the long life, reliable barrier material for the K25 plant at Oak Ridge and also at Paducah, Kentucky and Portsmouth, Ohio plants and also at the Savannah River plant." Of course there are newer ways of doing it now—centrifuges and things of this sort that we couldn't use because we didn't have alloys strong enough to take to Ohio. Yes, here it is. It told how I got involved here. "Bell Labs' experimental work with Columbia and Bell Labs began in an oblique way early in 1939 . . . Shockley and Nix were studying order/disorder phenomena in alloys earlier, while many Bell Labs staff members were pursuing graduate degrees under Bell Labs sponsorship. A fertile team of physicists, including Fermi, Dr. [John R.] Dunning and Dr. [George B.] Pegram were involved in experiments in neutron collisions. In the Columbia chemistry department, Dr. H. C. Urey had concentrated deuterium." He was a heavy water guy. "Shockley, later one of the transistor developers and Nobel Laureates and Nix visited Columbia to seek advice in the use of neutrons in their order/disorder studies. At that time, 1939, they learned of varied experiments with U-235." On it goes. It says here that Nix formed a small group, including Don [Donald] MacNair, George Clement and Elmer Larson. Elmer was the only member of technical staff. Then I came in, and I didn't want to go to New Jersey, and he left. Then it goes on; this all talks about the mixtures of helium and carbon dioxide. This is the only history there is that I can find anywhere where Bell Labs did anything.

THACKRAY: Yes. That's the *History of Engineering and Science in the Bell System* that you're reading from, pages 348 and 349. That has a pretty good account.

BIONDI: Yes. I don't know whether you can get this in a public library.

THACKRAY: Yes. I think we have a copy of that which was published in 1978.

BIONDI: You probably have something.

So there it is.

THACKRAY: Very good. You mentioned Bruce Hannay. Did your paths overlap a lot? I guess they must have.

BIONDI: Yes. Bruce, when he first came to Bell Labs, was in a group that was studying the corrosivity of the gaseous diffusion barrier that I had developed. He reported to G. T. Kohman whose group of six had laboratories in the Murray Hill, New Jersey lab. G. T. Kohman was an electrochemist of the old classical school.

BIONDI: We were worried about, would the barrier plug. Nickel is not supposed to plug, but you don't take "not supposed to." You actually make tests. He was running these tests out at Murray Hill. Bruce Hannay earlier brought the first mass spectrometer into the Bell Laboratories. Guess where he put it? He put it in a room next to where I had an office where I was doing this secret work in the New York City lab. He had to bring people through it.

After getting the mass spectrometer up and running and training a technician to run it, Bruce went to Murray Hill in New Jersey. In a room on this side there were three women with geodetic survey maps. They were laying out a horizon-to-horizon hilltop survey from Manhattan to Boston for the first microwave telephone relay system in the world. Later on, the first electron tube they used in that system was this one here [pointing to a display board], the 416-microwave amplifier. This is the first transistor here, the first thousand-bit memory, utilizing solid state positive/negative junctions.

As I said, I had five or six groups. This exhibit is from my Allentown group, the other from my Murray Hill people—the two principle groups.

THACKRAY: [laughter] I like your weak points.

BIONDI: Well, this is the 416-microwave amplifier. This is a traveling wave tube, another more powerful microwave amplifier used exclusively in cold war detection of Russian rockets. Now this is a nickel-cadmium battery, which has a life of ten years compared to the normal life, which is three years; they used it in the moon project. You notice they had ceramic seals rather than plastic seals. That's hermetically sealed. This is the first transistor, the 1A, which is a point contact on germanium. This is the first amplifier tube buried in the trans-Atlantic cable. This is some of my early activity in creosoting wood. I only lasted a year in that job, discovered penicillin.

I discovered penicillin on that job a year before it was announced by a physician in Canada. He discovered it the same way I did. I was trying to keep successive generations of various fungi alive, and I was losing twenty percent of my samples. I looked in the petri dishes, and I could see little water like drops surrounding the wood cube, impregnated with a test fungicide. I went to my manager and said, "Hey, this is something that is so powerful that a fungus that would eat up a cubic centimeter of wood in a week is killed. He said, "Look, we're in the telephone business. Never mind that medical stuff." A year later Sir Alexander Fleming was knighted by the queen. Was it the queen or the king at that time, 1937? Twenty years later in the semiconductor effort and the airfilter effort to reduce contamination of semiconductor surfaces, I realized what it was that produced the penicillin—it was from the organic material in the air of suburban Summit, New Jersey which was not filtered out; it was the source of the fungus that generated the penicillin. Nice to have an old problem solved at last in my mind.

THACKRAY: It must have been the king.

BIONDI: Yes. You know him [Fleming], do you?

THACKRAY: No, but I know about his work.

BIONDI: He was a Nobelist. I wrote him a letter and told him how I'd discovered my penicillin. He wrote back, and he was very nice. He wrote back and said he discovered it the very same way. [laughter] "Too bad your people didn't pay any attention to you."

I didn't last very long in that job because people in this lab were on the metric system. They used cubic centimeters of pine wood, and they measured how many grams of impregnant they put in. Then they had to transfer all this knowledge to an engineering group down in Louisiana where they filled telephone poles with the impregnant under study. They were engineers, and they wanted to know pounds per cubic foot, not grams per cubic centimeter.

The first job I had was to sit down at a machine with a handle and multiply these numbers by some other number to get it from grams per cubic centimeter to pounds per cubic foot. I worked the thing backwards and found out something was wrong. [interruption] I carefully checked my calculations, screwed up my courage, and went in to see my boss of six months duration to tell him we were using an incorrect number to convert from grams per cubic centimeter to pounds per cubic foot.

[END OF TAPE, SIDE 2]

BIONDI: My boss said, "Show me your calculations," so I did. He finally had to agree I was right; so I figured, "I'm not going to last very long in this department." But I upstaged the boss. He was the person who had, years before I arrived, developed the incorrect multiplier. [laughter]

He was R. [Robert] R. Williams' son-in-law. R. R. Williams was the person who synthesized Vitamin B_1. Those were great days, I tell you. But can you imagine turning something down and it turned out to be penicillin?

THACKRAY: Yes, that's quite something.

BIONDI: There were a bunch of biochemists at Rutgers University at that time whom we could have gone to. They did much of the early development work on antibiotics. In the summer of 1936, Professor Neville sent me in to New York to spend the summer at Bell Labs in New York City with R. M. Burns' group. He was assistant chemical director. While I was there an older chemist died and R. M. Burns suggested to Neville that he would like to hire me as the replacement.

THACKRAY: Tell me about R. M. Burns, if you would.

BIONDI: Harvey A. Neville and R. M. Burns were roommates when they were graduate students at Princeton. I was Neville's right-hand man—set up lectures, corrected papers, was his baby sitter, serviced his car, et cetera. I had very little contact with him except in the hiring stage. Lehigh Professor Harvey Neville said, "Why don't you hire this young man? If you want to use him as a replacement, we'll find somebody else to take his place out here on the Lehigh faculty." Then Dr. Burns assigned me to go out and work in Summit, New Jersey, which was thirty miles out.

I lost touch with R. M. Burns. The next time I got involved with him was, I met him at a dinner. This was a dinner in 1946 at The Waldorf Astoria [Hotel] where the Chemical Engineering Achievement Award dinner went to the atom project. He was at table 32 with me. In fact, we met at his house ahead of time. He's at table 32, and here was I at table 32. [looking at the table assignment booklet]

THACKRAY: This is the Chemical Engineering Achievement Award dinner table at the Atom Bomb project in 1946.

BIONDI: One of the other persons at table 32 was Bill Shockley. At that time, he hadn't invented the transistor. This dinner brochure is very well done, by the way, for a historian-type person here.

I've dragged that around from one house to another in a little old box. It gives a good review of the program, the whole thing, with all the people who were involved in it (5).

THACKRAY: Yes. I must say, I don't know if you've given any thought to the eventual disposition of chemical mementos like that, but we would certainly covet them.

BIONDI: Okay. Well, that's a good thought. Otherwise, they get thrown out.

THACKRAY: Yes. Well, our business is, among other things, to physically conserve these evocative pieces of the chemical heritage.

BIONDI: I tell you. It's such a throbbing organization, and it keeps changing. It's like the hydra; you cut an arm off and it grows another one somewhere else.

THACKRAY: Oh, yes.

BIONDI: People get stuck on the arms that get cut off, by staying with them instead of moving with the whole thing. I was in a privileged position in that I didn't have any established history in any one area. I could move around and do what I wanted to do, and then move again to another area. That was the thing that was my thing.

THACKRAY: You exploited your liberty very wonderfully.

BIONDI: In Europe, my father's family is divided into two groups. One group is in Siena. They're all physicians. In fact I went down a country road there one time, and I saw the back of a bust. I go around and look in the front—my name, spelled in Italian of course. He was a chief surgeon at the local hospital, a member of the faculty of the school. He was looking down at his valley. He was buried on top of a mountain where you could see for forty miles on a clear day, and there are lots of clear days there.

The other part of the family is up around Pisa and Lucca, and they're in the electrical business, electrical engineering. My father was the oddball there. He wanted to study music. He graduated from the Conservatory of Music of the University of Pisa. Incidentally, Enrico Fermi graduated from the same school's physics department. He came over here, because he found out there was more sheet music sold in the Lehigh Valley of Pennsylvania than any other part of the U.S.A. That didn't last very long, because he found out that music teachers in this country aren't paid real well. So he went to Penn State and got a degree in mechanical engineering. He brought his brother over, and he got an electrical engineering degree.

This is a wine down around Siena, Biondi Santi—Brunello di Montalcino. This sells for one hundred and fifty dollars a liter. Ten years old before it's bottled. When the president of Italy went to England, he had the queen over to the embassy for dinner, and this is the wine he used. I have that menu. Would you believe that?

THACKRAY: Yes, yes.

BIONDI: We had five or six bottles of this. This is from the *New York Times* here.

THACKRAY: This was in 21 February 1985. We're talking about, "While Biondi Santi is unquestionably the greatest name in Brunellos." Boy! You really missed your vocation. [laughter]

BIONDI: Our number three son, Michael, is a wine expert. He has two wine cellars in his home in Greenwich, Connecticut. When he was taking a law degree at Penn simultaneously with an MBA from Wharton, he helped edit a wine column in a Philadelphia newspaper. Michael earns his living as the CEO of Wasserstein and Perella—a New York City merchant bank. Here are the prices in the May 1, 1984 New York Times. Brunello di Montalcino Biondi Santi—1997, $99/720 cc; 1964, $200/720cc—both in limited supply and good up to fifty years in the bottle.

THACKRAY: "Biondi Santi, $99 for—" Is that per bottle?

BIONDI: That's for 720 cc.

THACKRAY: For 720 cc!

BIONDI: Yes.

THACKRAY: Okay. So that's really where the family comes from in its deeper origins.

BIONDI: Yes, yes. There are twelve bottles. In 1972, we had four of the 1972 bottles and opened each one in turn as our four grandsons were born.

THACKRAY: This is just fine.

If you gaze into the crystal ball of the future, will much of the solid state type activity continue to be included under the electrochemical heading?

BIONDI: It's very difficult to predict. As I say, they're getting rid of aqueous processing—acids, alkalies—because first of all, there are problems of people; there are problems with the atmosphere. They're going to go more and more towards working inside of glass chambers with ionized gases where you could make a pound do where a ton would do with the aqueous based process. That's the way they're going to go.

You can clean it. You can check it out before you use it. Clean air. As a matter of fact, one simple thing that I did to one factory that includes a clean room, a separate building in the factory, was to put one wall up so that everything that was delivered to that building was on the outside. That's where they brought in the carboys and connected them up to pipes, and all that, and not bring them into the room where they made the transistors or the circuits. They put all that outside. They brought it in a pipe. Even the furnaces, when the furnaces would fail and you had to take them out with all the insulation, I backed them up against the wall and had doors in the wall with tracks. You'd just roll them out, close the door, and it's repaired outside. When it's fixed, open the door and bring it back in again. It makes just common sense.

Automobiles are done the same way, I think, working on an automobile. You take it to where the equipment is. You don't repair an automobile in the parlor. That's what we were doing. We were repairing ovens in the parlor.

It takes money to do this, I'll admit. This new building at Murray Hill that they put up, I remember they hadn't quite approved all the plans, but I got word of the plan that we were going to be on the fifth floor. I went to the vice president in charge of the work that I was doing, a guy named Jack Morton, who was a character, I'll tell you. He fired me three times—but called me up two days later to apologize. We didn't agree on certain things. Anyway, I had called Jack up one day and said, "Jack, you're the guy committing space. I'm going to be on the fifth floor with all my labs?" "Yes." "I'd like to come up and talk to you about it." "Okay. How about right now." That's the kind of guy he was—right now. I said, "Jack, the average height of the floor-to-ceiling here is ten feet. I'd like to have the fifth floor be fourteen feet high." "What in the hell do you want that for?" I told him, "I want to build clean rooms up there, and I want them to have a floor that comes up two to three feet with all the wiring underneath. I want special walls. On the outside wall the dirty stuff comes in. I want a special ceiling, and I want to put in hepafiltered air." It didn't take him five minutes. "Good idea!"

I knew what a competitive guy he was. He wasn't going to agree just that fast. I said, "Now you're going to have the cleanest room in the Bell System or in the semiconductor industry." "Oh. This is better than Allentown has?" The Western Electric plant in Allentown, Pennsylvania, did our manufacturing. Allentown was my old home town. I was instrumental in bringing this plant into Allentown rather than in Vermont. His competitors had the manufacturing. "Yes." He bought it right then and there.

We finally did that. Later on they were complaining that this was a gold-plated outfit; nobody could get in it. We didn't allow visitors. This is not a place for a visitor to go in. They could look through the glass. If they wanted to go inside, they had to get my personal signature, and they had to get dressed for it. They thought we had a gold-plated outfit, so I said, "We have to have gold-plated treatment in order to make platinum and gold containing parts." That's how we got reliability. We didn't change anything in the solid; we changed it in the atmosphere around it. We got rid of the sodium ions by washing in pure water, getting rid of the dust particles, which was the last need we had. Remember my penicillin story of 1936. It was dust in the air, particularly organic dust from trees and bushes in a suburban lab with no air conditioning, hence open windows. The dust brought in the fungi which developed the penicillin I observed. Problem solved after 35 years.

That particular little tube was used in the towers, microwave towers—they had a flat cathode like a pill, and they had a grid over it, tungsten wires, seven tenths of a mil, above it. That wire was so thin that it broke at 14 grams, so we had to wind that on a machine on a flat sheet of tungsten with a hole in it. The wire was up there one time, and then it was down flat. We had to do it without breaking it at 14 milligrams. Very tiny. This grid was seven tenths of a mil above the cathode. The whole thing could be plugged in; we built a microwave plug on it. That made possible coast-to-coast television, too. We originally did it for coast-to-coast communications, not television. The same Jack Morton designed a microwave tube and I put a clean room in for him, a complete new white room in a vacant coal storage room—the only space available in the building—to assemble that tube: first such white room in the electronics industry, in 1946. Kept this program going despite the fact that World War II was going on, and this was a non-war program. Now everyone in the transistor industry has a dust-free white room.

Then we put up the first satellite before the U.S. government got one up. Telstar I, it was named, went up—nobody in it. Three o'clock exactly it went up. The Army exploded an atom bomb right in the path of our Telstar satellite. Not that they were trying to destroy it; they had planned that, but they didn't tell anybody. Would you believe that, after all that work? Private satellite. We lost it.

What it did was, the radiation creamed all the semiconductors, so that we said, "We've got a new challenge. We've got to design semiconductors that will stand that," and we did. The present orbiting satellites come from that early start and take advantage of that information now. The early work was all classified, the Army satellite people were not identified at first. They in fact did us a favor, since the Russians would hear about it, and they could destroy the usefulness of orbiting information-seeking satellites of ours, the same way our army did.

What happened to it, I'm sorry to say, I can't tell, because I didn't stay with it—not because I wasn't interested, because I had other things to do. I went around and patched here and patched here and patched somewhere else. The whole thing worked, you see.

THACKRAY: You were a key benefactor of the results from it.

BIONDI: I didn't drop that atom bomb. In fact, I'm surprised they dropped the damned thing. I thought they would test it in a more humane way, but we had Harry [S.] Truman down there who was a tough old bird, and we had a bunch of tough guys in the Army Air Corps who wanted it. They said, "A million of ours versus this atom bomb, a million of theirs instead." They wouldn't understand it. They didn't understand even after it was dropped. They thought they could sweat it out. Tremendous business.

Boy, that business about the atom bomb. Norway was occupied by the Germans, and Sweden was not. Norway, and what's the other?

THACKRAY: Denmark.

BIONDI: Denmark. We wanted to bring in a professor from Denmark. Our OSS [Office of Strategic Services] people went in, took him out, put him on a plane, brought him over to New York. We talked to him for four hours, put him back on a plane and sent him back and reinserted him. They would do that all the time. They said, "No problem. [laughter] You want to see him? Just let us know." I still remember that meeting.

THACKRAY: That was in connection with the atom bomb?

BIONDI: That's right, yes. We could only talk to him in one room that our Secret Service people had checked out that wasn't bugged. This experience was useful after the war with the Japanese coming over here as transistor patent licensees. They were always taking pictures of everything. We had a room we'd take them through as they exited, that would fog all the film in their cameras, and they never complained. They even understood why and said, "Oh, okay. It makes sense. We're trying to steal your ideas, and you know how to protect yourself."

THACKRAY: Yes. How have the recent changes with AT&T, since 1984 and now this new thing, affected the Bell Labs' role and capability in these areas?

BIONDI: They're back to where they were in 1926 when they put it all together. That's when they had AT&T running mostly the telephone companies, all the operating companies. They were the headmaster of all those. Then they owned the Western Electric Company, which was the manufacturer. They decided to put them together. I told you that was some of the hard times that we had. We had to wait for a generation of people to retire before it became smooth, because the old flags were still flying. Now they're back to where they were again. AT&T doesn't run the local telephone companies any more because they haven't got them. They run the long distance telephone system. They're now going to be allowed by law to get into the same local service. That's going to take ten years, but they'll get into it. They're also going to get into cable TV—anything they can get into now.

They're going to start an AT&T Laboratory. To do that, they took about thirty percent of the old Bell Laboratories. The other seventy percent they left behind. They still call it the Bell Laboratories, and it works now for the organization which used to be Western Electric, which makes switching equipment and all kinds of things, called Lucent [Technologies, Inc.].

You see, the Baby Bells wouldn't buy switching equipment from the old Western Electric Company after they were separated, because they knew that AT&T owned Western Electric Company and was going to be their competitor. Why should they buy and make a profit for their competitor? Now, by even changing the name to Lucent, there's no hint of AT&T involved in Lucent.

I think they were first. All these other companies started buying switching equipment from the Canadians or from the Germans, because they didn't want to give a profit to AT&T. AT&T's profits from switching equipment are going downhill. That's what these things were in the wall exhibits [indicates glass case on wall]—removable circuit boards that can easily be pulled out of sockets and replaced by spare boards. The big problem was not the board contact itself, but the gold-plated connector at its forward end which was slid into receiving gold contacts. These contacts were a key problem. If they developed highly resistive areas, the whole board was useless. Then ESS, Electronic Switching System, was AT&T's big gun for telephone circuit switching, very expensive and very reliable. I had the problem of the gold plating and proving it would remain reliable for greater than forty years.

There are gold-plated contacts on the ends of that. That's very important. There were a lot of problems with that. That whole switching system, which can handle something like half a million calls during the busy hour, works on the basis of all the circuitry being plugged in. Everything goes through those gold-plated prongs at the end, here and here. The system checks itself out every three minutes, and tells the operators if there is a problem, what's wrong and what to do about it—self-diagnosed, part of the system. All that equipment is all based on circuit boards, glass circuit boards, or copper circuit boards with something with seven layers where you go down to layer two to get power and up to layer three to get conductivity this way or that way. That's called the Electronic Switching System, ESS. That was developed twenty years ago now, maybe twenty-five. I was involved in the gold-plating end of it because they were saying, "God, all these beautiful circuits we have. If the connectors don't work, we've lost the whole thing."

You've got to be able to pull them out and shove another one in. It's working. It was the premier switch anywhere in the world. They licensed people to make them. They've licensed the Chinese to make them. I presume they're going to get some money from the license.

I think The Electrochemical Society is going to continue to have a role in the electronics business. I think, like any society or any organization, it ought to be smart enough to look out into new areas. I mentioned some of them very briefly. I don't know if anybody's doing anything about it; I don't read anything about it anymore.

THACKRAY: Areas such as what?

BIONDI: Corrosion. You know there's utterly billions and billions of rusted-away bridges, sidewalks. Down in Philadelphia there was a bridge put in on the Schuylkill River Expressway. It only lasted two years because the steel rods in them began to corrode; somehow or another, water got in cracks or something, and salt from the ice which was salted.

Now, a little generator, it's used on pipelines. In fact, some of the gas pipelines take tiny amounts of gas out of the pipelines and burn it to generate power with some thermocouples. Warm up the thermocouples a little bit, and you generate an electric current to offset the corrosion currents. That would be a tremendous area to work in. I don't know why there isn't more attention paid to that.

THACKRAY: The Electrochemical Society seems to have been unusual, perhaps, in having been very open to all these applied areas and issues.

BIONDI: How about cross-blending with the American Chemical Society? Does The Electrochemical Society get involved with them? No joint meetings?

THACKRAY: No, they've really kept themselves pretty distinct, although some of the same people turn up wearing a different hat.

BIONDI: Oh, sure. As you know, there are all kinds. There are societies that deal with a phenomenon only, or with a material only, or a process only; way down at the bottom you have specialists in each area. Then at the other end you have these monoliths like IEEE and American Chemical Society that deal with everything. If you go to a meeting, you get lost in the mush. Tremendous. In between, there are all kinds of gradations. Some of these societies maybe no longer have any real reason to take the spot they had twenty years ago. They no longer have it; they're not entitled to it. Techniques move along, something moves along in response.

That's the danger when you work on something that is helpful to the electronics industry, which moves all the time. That's the price you have to pay. You'll be left behind them, either because you're obsolete or maybe you lost interest, or maybe the new activities involve techniques or science that is way away from your province.

THACKRAY: Well, The Electrochemical Society seems to have stayed with it so far.

BIONDI: How is its membership showing? Is it increasing? Holding its own?

THACKRAY: It's holding its own. It's been increasing, I think, with overseas membership, and that globalization phenomenon is what's happening, I think. [laughter]

However, I think the society thinks of you as one of its benefactors for bringing in the solid state.

BIONDI: Well, let me say honestly, I didn't do this with the idea in mind that I like this society, I'm going to help them. They were in a position where they could help me. I needed an outlet for my people to publish their work, and I couldn't find it. It was going to be scattered. It was going to be published in eight different places, which is even worse. I thought if I could centralize it in one place—for most of the people, you can't guarantee this, see. After all, I was not a dictator; I couldn't say, "You have to publish here." I got most of them to do that, but I didn't have to take much pressure to do that.

There must be dozens of guys around like me who worked on these other societies. I don't know who they are; they don't know who I am. You see? We meet in the middle.

THACKRAY: Well, you were the one who made this stick somehow. Thank you for that.

BIONDI: Oh, no! I crystallized it by throwing something into it, and then went away. Whoever took it over and kept it running has done the job.

THACKRAY: Well, you scratched the test tube. [laughter]

BIONDI: Yes. The ASTM is still in existence.

THACKRAY: Oh, yes.

BIONDI: There are other societies around that are doing the same sorts of things.

THACKRAY: The same thing, yes.

BIONDI: Yes. They had such an antiquated structure. They were worried about glass windows, and railroad tracks and big industry items like that, and pipes. Every time I pick up a polyvinyl chloride pipe around here in my irrigation system, it says ASTM on it, committee so-and-so. Do you believe it? I picked a piece up yesterday and just looked at it. Right here, it says, the thing is still operating.

THACKRAY: They're still very much in business.

BIONDI: I used to go to some of the meetings of their board on Saturdays, so it wouldn't cut into my work week, especially in the wintertime when I didn't have baseball considerations and things like that. I didn't even know what they were talking about. Little problems come up that are different in each area of specialization, and you had to have a history of that to understand what the problem was. Nobody stopped to say, "Look, there's a new man in the room. We'd better say, 'X years ago we were doing thus and so, and this and this happened, and now we're doing it this way, and we're having a problem here. That's where we are today.'" Then they'd go on, and I'd understand. They never did that; I didn't expect them to do it. I was sort of hanging on trying to understand it.

In fact, I did put a man on their board. I found a man who was interested in that kind of thing, got him on their board. He turned out later to be a vice president of AT&T! I lost track of the guy. [laughter]

THACKRAY: Well, you've had a good talent for selecting winners.

BIONDI: I'll tell you. He was a guy who, when he became the vice president of Western Electric Company, he saw a tremendous growth about to occur. We had to go out and buy property in eight or ten states—Alabama, Wisconsin, Washington—for future plants, in fact, start the work on the one in Los Angeles. Then the bottom fell out of the market. We were stuck with all these properties; in some cases, we had half-finished buildings. Boy, was he wrong! We didn't have that growth. They were talking seven or eight percent a year, which is fantastic. We're at 1 1/2 percent now.

Then he also did some smart things. Anytime we wanted to start anything new, we had to do an analysis of the break-even points. One was what it cost us to get into this, and then there's how long would it take, and what would we get out of it. For example, when I got involved in batteries—the Bell System, the operating companies, work on DC. I don't know if you know that.

THACKRAY: Yes.

BIONDI: They buy AC, rectify it, and then trickle-charge batteries, and then operate the plant off batteries. In case there's a drop-off of the AC, they can operate for—depending how many hours the place is designed for—anywhere from one to fourteen, fifteen hours. Some plants put in a diesel engine in addition to running an alternator, which then is rectified.

We were having trouble buying batteries that we could depend on. Vintage things. Batteries made in a certain year in a certain plant on the second shift but not on the third shift were better than those made on the first shift. If one battery went bad, you couldn't put a new battery in on the whole string. I thought, "Terrible." We found out all we knew about big batteries was what the battery companies told us. I said to my boss, "Why are we believing these guys? They don't have any evidence that they understand this thing. Why don't we take that understanding over? We'll run our own show. We'll design our own batteries." He said, "Come on! They've been doing this for a hundred years. You're going to beat them out?" I said, "I think we can get a better battery out if we design it ourselves. Forget about these guys."

We sat down and closed the door one day. Up to the board; problem number one. We decided we'd put modern science into every aspect of it. "All right, we're going to use lead, too much history to discard, but we are going to use pure lead rather than Pb/Sb alloy used by the entire industry."

Why didn't we want to use Pb/Sb alloy? All battery plates, positive and negative, whether held vertically or laid almost flat, as our pure Pb plates are held, have a thick frame around the edge and internally are filled with blank small Pb/Sb or Pb thin metal areas, like a screen but much larger in open areas. We call them picture frames. We then paste into the empty frames a mix of fine lead powder in the negative plate and fine PbO, lead peroxide, in the positive plate. After continual charging and discharging in a sulfuric acid electrolyte, the Pb/Sb frame expands more than the pure Pb frame and the paste falls out, reducing the capacity of the battery. This is why we do not want to use Pb/Sb frames. Pure Pb frames are essentially free of expansion for these reasons.

Further, as the Pb/Sb frames oxidize, they electrolyze the water in the sulfuric acid electrolyte to form O_2 and H_2, creating an explosive mixture but also reducing the water level in the battery, requiring frequent addition of new water, an expensive maintenance procedure, and the step itself is dangerous. The O_2 and H_2 mix to form an explosive mixture, if a spark is provided. Spilled electrolyte on the outer surface of the battery near the positive external electrode will cause corrosion and poor contact corrosion.

So we decided to use pure lead, and to avoid the cold flow problem, we would make them like a stack of flat pancakes—higher in the center, lower at the edges, 10^{o} slope. Then by having a hole in the center, the succeeding plates from bottom to top formed a Pb pipe, so we had the advantage of sulfuric acid electrolyte circulation, due to differences in gravity and temperature from bottom to top, to flow through this central pipe to homogenize the gravity of the electrolyte, and thereby the electrolyte activity from bottom to top. Without this flow, the electrolyte stratifies, heavier at the bottom, less heavy at the top, resulting in a variety of electrolytic activity which is not desirable.

Another improvement was to avoid industry practice of using flames to melt the metal connect the various lead parts. Flames caused formation of lead oxides in the stacks, providing a locus for corrosion and poorer contacts. We decided to use heat in a non-oxidizing gas atmosphere to connect the lead parts, with the pasted + and - plates separated by insulating separators all held together in a large jig. This stack was three feet high and heated by radio frequency coils around the jig, moving from bottom to top while flowing pure nitrogen gas through the stack. It only took minutes to go through the whole stack and you could then insert the stack into a plastic cell, add the top, and you were ready to add the electrolyte. Incidentally, the cell was made of fire resistant polyvinyl chloride. In the past, the cells we bought were in flammable, usually hard rubber, cases. As we were doing this design, we lost, due to a battery fire, an important West coast telephone office which was carrying a large load of military traffic to the Western Pacific action with the Japanese, so a fireproof battery was welcomed.

It looked like we had a potential winner and I had ten MTSs and fifteen tech aides on this job. Our first step was to design a tester cell. We studied the longest-life commercial cells and shortly developed a test cell. In two years we were able to take a new idea and put it in this test battery and get twenty years of experience in two years, so we had a handle. Such a life-testing cell predicting long-term life was new in the industry, but not at Bell Labs. We could then make changes, put it through this test. We were able to redesign our cells using new processes or materials. They're called round cells now.

Then our managers said, "We're not in the battery business. We're going to take this out and have the battery people make them." I said, "You mean these old guys who gave us these lousy batteries, you're going to let them make our new design batteries? Come on! I don't believe you!" They said, "You can't make that battery that you're talking about unless you have guys in white coats with Ph.D.s." I said, "I'll tell you what I'll do. I'll rent some space from our junkyard." We had a junkyard in Staten Island, south of Manhattan.

I went down to Staten Island and rented some space from them. They had earthen floors in this building; that shows you how old it is. I put a building up inside the plant for our offices, and we made the batteries outside on the earthen floors, and we put them into this test that we had. Then we put them in a telephone exchange. Finally, he was convinced. "Hell, if you can build them down there in the junkyard with an earthen floor, these battery guys can't be so bad." He was still wrong. We had to nourish these battery guys. We had to pay them to make our batteries because it was our idea. As it turned out, twenty years later that battery that we're selling all over the world is making more money for AT&T, now Lucent, than the transistor business! Would you believe that? [laughter] Beautiful! I had an exhibit on it before they threw them all out, because there were big plates. In fact, the guys made me a lamp with an old battery. My wife said, "That's out." Couldn't bring it in the house, joined the junk pile with mementos and parts from the atom bomb job.

In the meantime, we had a fallout of this thing. W. C. Campbell, a Bell Labs supervisor in the electrochemical department, this fellow who was sitting at the table with us at the atom bomb award, and whom I chose to sit on my committee on fuel cells—instead of a lead antimony, which causes all this water to be gassed into hydrogen and oxygen—you have to add water all the time—he used another Pb alloy. That was put in there to try to make the lead stronger and more brittle than even Pb/Sb, not to flow, but it was built on a vertical plate design that we were making obsolete. He added another item to it, calcium, which resulted in the no-maintenance battery, because it didn't break down water into hydrogen and oxygen. He used a lead/calcium alloy which hardened the lead almost as well as Sb, without posing the water electrolysis and additions that Sb entailed, but he was never able to sell his ideas and data to the telephone and Bell Telephone Laboratories telephone power engineers.

Did you know that not a single company came to us to buy a license to use that patent? The day the patent expired, J. C. Penney called us. Somewhere around the country they sell batteries for cars in their stores. They wanted to buy know-how—not a license to use the patent because the patent had expired, but how much was the know-how going to cost, so they could use this lead for the vertical plate but with a new element in it to make it strong, but which did not hydrolyze water—in short, a maintenance-free battery. At the same time, on the one end, this guy was working to make it a better battery using the old technique. We had to add additional features to the low-maintenance feature, so we were working on the other end with the flat plates—relatively flat, ten degrees—using the same Pb/Ca alloy that W. C. Campbell used and Bell Telephone Laboratories uses to enclose wire in cables.

THACKRAY: Yes. Very good.

BIONDI: The battery business. Always going to be with us, the battery business.

THACKRAY: Oh, yes.

BIONDI: The guys who were working on the battery business got involved with a committee of the IEEE, which was standardizing batteries. That's where their outlet was. I got involved in that thing. It's still involved. Then we decided to start a new society called the Power Society of IE^3, and it deals with power of all sorts—microwatts to megawatts. That's still running, and one of my guys is still involved in that. He calls me up every now and then. He sends me programs, and I say, "No, I'm not going to your meetings. I wouldn't understand what you're talking about. It's a waste of my time." I've been retired now twelve years or so.

When I left Bell Labs, I became a consultant. I found I was solving the same problems I'd solved ten years earlier at Bell Telephone Laboratories, in the outside industry. I was amazed at the lack of honesty in most American corporations. They were thieves. They were buying information. They were hiring people only if they brought with them all the information from their former job, including blueprints, specifications, et cetera. Those working for the government were worse.

I was asked to consult on some jobs where they were developing connectors. I became an expert in the connector business because I knew something about gold plating and spring material Be/Cu. I couldn't inspire an American company. I inspired a Japanese company to build a piece of equipment you put at the end of a plating line which measured the plating rate in each cell of a plating line. If the plating rate had dropped for some reason, it increased it in that cell, or if it was too high, it decreased it. Then it tested 100 percent of the product as it came out. Here they could tell a customer, "One hundred percent of what you're buying has been tested, and we know it hasn't got holes in it to corrode to the base Be/Cu material. It has low-contact resistance because you've got the amount of gold there that you specified you want to be there." I couldn't interest any American company. I went over to Japan, spent three days over there representing an American company. The only way I could get to these Japanese companies is, I had to become a consultant to these Japanese companies for three days. They gave me Japanese name cards. They took me around to all their places. They had an interpreter with me. I had a chauffeur who couldn't speak English. They hauled me all around, mostly around Tokyo, and it was a very successful operation. The company who bought it, with whom I was consulting, fired the guy I was working with because he was the guy who for all these years had been telling them how good the product was, and when they bought this equipment from the Japanese, they found out how bad the problem was. He was shipping out two or three times the thickness of gold that he needed to.

THACKRAY: Why?

BIONDI: He was always afraid it was too thin, could not make in-process thickness tests. He'd had reports on it. This machine measured it, you see, to a micron as the parts were being plated. You're supposed to put on so many microns of gold. He was putting on double or triple. When they found out about this machine, the cost of the gold each month dropped to a third. [laughter] They fired him.

That was one of my problems. One company I worked for, three vice presidents were discharged because I was put on the job to find out sequentially what was wrong with their program, and I found out. [laughter] Do you believe that? I won't tell you the company name because it's a well respected company. People were afraid to talk to me. I decided to cut my ties with this company.

THACKRAY: I could imagine.

BIONDI: Yes, because I garnered a reputation, "Don't talk to that guy, he'll get you fired." That didn't happen in Bell Labs. This is a very respected company, still in business today, making a lot of money.

One of the companies out on Long Island was building a connector for a circuit that was going into an Air Corps fighter, where these young kids go up and they're going to crash half of them. I could understand, if they depended on this firm's product. The stuff they put in was thirty years old, these connectors. What they did was, thirty years earlier in some airplane they were making, they proved that that connector was working properly. That's the way the Air Force works. They certify you as a supplier of the parts; you're certified. Every now and then they'll send an inspector around to see if you haven't changed anything. That doesn't happen very often. What's happening is, the thirty-year-old product is getting worse and worse because the guy who was manufacturing them is no longer interested in them. He's got five new products, but he can't change, because if you do that, they've got to recertify that, and that takes two years. For two years you can't sell this product to the government, so they don't want to recertify it. Each part was supplied to the plane manufacturer from several sources, so they did not have to depend on one source.

They called me out to this place on Long Island and wanted to know whether I would work with them to solve their problem so they didn't have to recertify. They wanted to make some changes. I said, "You know you can't do that. That's not legal. You shouldn't be asking me to do something illegal." I said, "Just for that, my bill is going to be three times my normal rate." They paid it. I said, "Look, if anything comes up here, I have my own records. You can't sue me, because I didn't help you avoid the law." Terrible, the way these people operate.

I had a guy who was plating—this is in electrochemistry—he was plating platinum on a contact that again was going to a government agency. The chief engineer was the "bookkeeper." He called me up one day because I had helped him. He ran out of gas on a highway in New Jersey, and I stopped and helped him get some gas. He introduced himself. I told him I was working for Bell Labs and all that. He called me up one day after I'd retired. They said, "Oh, he isn't here anymore," but they gave him my number. He called me up and said, "We're having a problem. The platinum on our connectors is corroding." I thought, "Wait a minute, platinum doesn't corrode. I'd better keep quiet about this until I find out what's going on."

I went out to visit this plant in suburban New Jersey. I found out, by God, he was getting corrosion. Couldn't believe it. I said, "Look, you've got to give me carte blanche to go anywhere I want around here. I've got to find out what the hell is going on or I'm not going to help you." "Okay." I got carte blanche.

[END OF TAPE, SIDE 3]

BIONDI: I said, "Something's going on here," so I went down to the local library and got the local paper. I found out that this company had been pouring carbon tetrachloride and cleaning solutions out of the window onto the earth, and they had contaminated the wells of about forty adjacent home owners. They had been sued, had to put in a water system for the homes, and they had been prohibited by the court to do any more plating or use of carbon tetrachloride at this plant. I had trouble from the guy I was working for about this. Then I went over to someplace up in Hackensack. I went up and visited it and, boy, terrible place. Bad plating practice. The guys were eating their lunch and smoking cigars, putting ashes in the plating bath. I couldn't believe it.

Anyway, I got back to the plater and said, "What happens now?" "Well, we send the platinum-plated product over to the customer's machine shop and he'll check them out to be sure they're thick enough and that the male/female connection would match." There was a kid on a lathe. He checks them out with a gauge. Can't go in—it's too thick—so he puts that particular contact in a lathe and spins it around and runs a tool against it. Guess what he was doing? He was taking all the platinum off that they had put on unevenly, and he wasn't doing it uniformly; the thing wasn't spinning perfectly. It was wobbling a little bit.

He took it out of one place and exposed the stainless steel underneath, and that was what was rusting when exposed to salt water spray. That's why the bookkeeper thought the platinum was corroding. I had to go up and get the guy. I said, "Come on down and don't say anything after I show you and until we leave. I don't want the kid that's doing this work to be embarrassed—not his fault." Well, he saw it. He walked around it and he said, "What was wrong with that?" I said, "You didn't notice? He took off quite a bit of the platinum. Some spots have a different color than the platinum. He's taking it off and he's leaving a spot there. That's where it corrodes, the stainless steel." I said, "By the way, I don't want to be involved with you anymore. You're the third guy I've run into in a month who's trying to be illegal, who's going to say that I'm the guy who authorized you to ship the product. I don't want to be involved with you anymore. Here's my bill, and goodbye." I cut off doing business with him.

I tried to get liability insurance. Couldn't buy it. The American Chemical Society was selling this for a while, but they dropped it because it got too expensive. I tried to work on a contract for Georgia Power [Electric Company] on batteries. They wanted liability insurance and I didn't have it, so I couldn't take that assignment. The larger the company, the more lawyers they had, and they brought up the liability insurance issue.

I worked on one problem for Commonwealth Edison Power Company in Manhattan, in New York. They had a big power plant to operate a circuit breaker solenoid. This big solenoid is a switch that operates when there's an overload on the line, before the whole line goes down. A large number of batteries operate that switch which is on the roof. It's operated only by a union guy. He wouldn't let me see the damned thing, would you believe it? They took his place. I couldn't see the thing. I was supposed to take a look at it and open it up; I didn't know what I was going to find. I had only to look at the battery plant that supplies power to the solenoid.

They had contracted with a company to run two little wires to every battery, and they were connected with the telephone system to the boss's home at night. The battery, about thirty feet long, was a bunch of individual cells, each about 2 feet by 2 feet by 4 feet high. This was a useless kind of thing. Measuring the voltage on each cell did not tell them how the whole battery was doing. I said, "Look, there's only one thing you can do here. Batteries are like a wine. You have to taste the wine to see if it's any good. All the other tests are worthless.

On batteries, testing the potential of one cell doesn't tell you a damned thing. What you have to do is deep-discharge this entire battery like it's an actual case. See how they behave. Measure each one, but as you deep-discharge them, not as you trickle-charge them. It's ridiculous."

They said, "How are you going to do this?" "Well," I said, "the thing you do is, you set up a battery central somewhere where you keep batteries charging, and you have a bunch of trucks, maybe four or five trucks, depending on how many you need, with batteries on their backs. You run them up to a place that's having trouble and you run the cables in the windows. They're all on the first floor, since they're very heavy. You disconnect the big cells that are in there, put your cells on an empty companion truck back to central. Then you operate. You take these old cells out of there and you get new cells. You test them by deep discharge of each cell and find out how many were weak, then replace them in the string before you put them in, back in battery central. They didn't tell me why they were doing all this, but I found out. They were checking whether that solenoid was working or not, once every three months, by taking a big copper cable—about two inches in diameter, strapped to a two-by-four to hold it—over to a steam radiator that would make a big arc. That would draw a lot of current and the solenoid was supposed to open, powered by the battery plant to operate the motor of the solenoid. That solenoid was eight feet tall; a big thing, oil-filled. I never saw it, because the union guy wouldn't let me in there—like I was going to take his job away or something.

They had been ordered by the courts to hire somebody who knew anything about batteries to solve this problem for them. They knocked out all of Manhattan from 14th Street south, at 3:30 p.m. in the afternoon, including the stock markets! I didn't even know about this. They were very quiet about that. They finally said to me, "Would you be willing to go up to Albany as a witness and tell them what you propose here? All the other companies we hired never thought of this thing." I said, "This is what we do in the telephone company. We deep discharge them." Look for weak cells by measuring current output while deep discharging. If they're too old, you only get half the capacity, maybe a quarter or even less. I said, "I'll tell you, I don't like this idea, because I don't like to deal with a bunch of politicians talking about technical problems." I said, "If necessary, I'll do it." They never called on me. I said, "You tell them who I am, where I came from. I'm an independent consultant now, no longer with Bell Labs. That's the way you solve your problem." I never heard back from them. Either they abandoned it, or they're using it. That's the worst part about being a consultant. The clients are chickens.

Here I am in West Palm Beach, bored. I'll tell you, I've thought about getting involved with local universities, but boy, they're politically dominated. All they're interested in is the salaries of the teachers and the salary of the chairman of the board. We have a junior college here. By God, the CEO is retired and he's taken something like seven hundred thousand dollars out—just came out in the newspaper, another two hundred thousand. These are all things that he arranged while he was chairman of the board. It was sick pay, vacation pay. He named two colleges after himself. [laughter] Oh, I can't believe this. No place for me.

THACKRAY: Good messages. [laughter] Oh, dear. Let me encourage you to write your memoirs more extensively.

BIONDI: I'm not finished. I'll tell you, another reason I quit is that my memory was going bad on me. Sometimes things or names that I ought to know, I have difficulty recalling them. I understand that's not unusual. I'm eighty-one and a half, and I still do my own income taxes, things like that.

THACKRAY: Well, I'm delighted to have this on tape.

BIONDI: Everybody says that to me. I sounded off to a back surgeon at the Cleveland Clinic [Foundation], because the laser was invented at Bell Laboratories and he was still not using it. When it was invented, I was always on the lookout for what could we use this for other than communications. I suggested, since I was in the hospital business, that we try hospitals, maybe they could use it. Since Alexander Graham Bell was deaf and he was interested in medical problems, we'll just continue that tradition. At the time there were more than sixty problems Bell Telephone Laboratories people were working on to benefit the medical profession.

The only reputable place in the country that responded to our invitation to discuss lasers was the Cleveland Clinic in Cleveland, Ohio, so I went out there. They showed me around; I was impressed. They said, "We'll work with dogs. You supply the lasers." I said, "I'll do better than that. I'll put two men in residence, not only to watch what you're doing and find out what you need, but to find out what you're doing, so we can gain experience by it." "We're only interested in medicine here, we're not going to try to sell it." We had these two fellows out there for four years.

When my wife and I moved here, she had trouble with her gall bladder. Nobody in New Jersey would operate on her because she didn't have the classical symptoms, so I went down to the Cleveland Clinic branch in Fort Lauderdale, Florida, forty miles south. They said the same thing, "You don't have the classical symptoms." I said, "Let me be a doctor, will you? Why don't you feed her some ice cream, a high-in-fat diet." They said, "Okay, we'll get the girl." They have a girl who figures out menus to recommend. She did that. Two days later, Ginny woke me up at two o'clock in the morning. She said, "I think I'm going to die." Terrible pains; better call the Cleveland Clinic right away.

I called them. I got the guy who was in charge. He was a surgeon from New Jersey, by the way. He said, "Bring her right down." The minute she got there, she felt a little bit better. He wanted to keep her overnight. I said, "Just stabilize her. We'll see what we do tomorrow."

The next day, that original surgeon who was reporting to the on-call-at-night surgeon who was his boss and all that night, called up here and said, "Boy oh boy, that idea of yours worked," he said, "That's classical gall bladder." He said, "My boss was in here; he reamed me out for giving her that ice cream. He said, 'You could have killed her.'" I said, "Here, write a paper on it, right away. We'll call it the Sesto test." The original surgeon's name was Sesto. "Go down in medical history." [laughter] I was joking with him.

They took that bladder out and the problems she had for years disappeared. When they took it out they said it was like an old boot. It was full of stones and it was like rubber. The ice cream test did it.

By the way, what is your exit time here or there?

THACKRAY: I think we should be wrapping this up. Thank you very much for spending this time with me.

[END OF TAPE, SIDE 4]

[END OF INTERVIEW]

NOTES

1. Richard G. Hewlett and Oscar E. Anderson Jr., *A History of the United States Atomic Energy Commission*, Vol. I, The New World—1939-1946 (University Park: Penn State University Press, 1962).

2. *History of Engineering and Science in the Bell System*, *National Service in War and Peace 1925-1970* (New York: Bell Tellepone Laboratories, 1978).

3. For copy of medal and citation, see F. J. Biondi research file, Chemical Heritage Foundation Oral History File # 0147.

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