CHEMICAL HERITAGE FOUNDATION

R. VICTOR JONES

Transcript of an Interview
Conducted by

David C. Brock

Via Telephone

on

18 April 2006

(With Subsequent Corrections and Additions)

ACKNOWLEDGMENT

This oral history is part of a series supported by grants from the Gordon and Betty Moore Foundation. This series is an important resource for the history of semiconductor electronics, documenting the life and career of Gordon E. Moore, including his experiences and those of others in Shockley Semiconductor, Fairchild Semiconductor, Intel, as well as contexts beyond the semiconductor industry.

This oral history is made possible through the generosity of
the Gordon and Betty Moore Foundation.

CHEMICAL HERITAGE FOUNDATION
Oral History Program
FINAL RELEASE FORM

This document contains my understanding and agreement with the Chemical Heritage Foundation with respect to my participation in the audio- and/or video-recorded interview conducted by David C. Brock on 18 April 2006. I have read the transcript supplied by the Chemical Heritage Foundation.

1. The recordings, transcripts, photographs, research materials, and memorabilia (collectively called the "Work") will be maintained by the Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to the Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright, except that I shall retain the right to copy, use, and publish the Work in part or in full until my death.

3. The manuscript may be read and the recording(s) heard/viewed by scholars approved by the Chemical Heritage Foundation subject to the restrictions listed below. Regardless of the restrictions placed on the transcript of the interview, the Chemical Heritage Foundation retains the rights to all materials generated about my oral history interview, including the title page, abstract, table of contents, chronology, index, et cetera (collectively called the "Front Matter and Index"), all of which will be made available on the Chemical Heritage Foundation's website. Should the Chemical Heritage Foundation wish to post to the internet the content of the oral history interview, that is, direct quotations, audio clips, video clips, or other material from the oral history recordings or the transcription of the recordings, the Chemical Heritage Foundation will be bound by the restrictions for use placed on the Work as detailed below.

4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that the Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

Please check one:

a. ___X___ **No restrictions for access.** **NOTE:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, Pennsylvania.

b. ______ **Semi-restricted access.** (May view the Work. My permission required to quote, cite, or reproduce.)

c. ______ **Restricted access.** (My permission required to view the Work, quote, cite, or reproduce.)

This constitutes my entire and complete understanding.

Electronic Signature Approved (Signature) R. Victor Jones (electronic signature)
R. Victor Jones

David J. Caruso 1 Sept 2010
David J. Caruso Date (Date) 27 April 2010
Program Manager, Oral History
The Chemical Heritage Foundation

Revised 07/21/2009

This oral history is designated **Free Access**.

***Please note*:** Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation (CHF) Oral History Program to credit CHF using the format below:

> R. Victor Jones, interview by David C. Brock via telephone, 18 April 2006 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0336).

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

R. VICTOR JONES

1929	Born in Oakland, California on 8 June

Education

1951	A.B., physics, University of California, Berkeley
1956	Ph.D., physics, University of California, Berkeley

Professional Experience

	Shockley Semiconductor Laboratory
1956-1957	Senior Engineer
	Harvard University
1957-1961	Assistant Professor of Applied Physics
1961-1964	Associate Professor of Applied Physics
1964-1982	Gordon McKay Professor of Applied Physics
1969-1971	Associate Dean, Division of Engineering and Applied Physics
1971-1972	Dean, Graduate Schools of Arts and Sciences
1982-present	Robert L. Wallace Professor of Applied Physics

ABSTRACT

R. Victor Jones grew up in Oakland, California, son of Welsh immigrants. His father was a machinist, and Victor learned many useful mechanical skills from him. From a young age Jones was interested in physics, reading a great deal about the new fields of atomic and nuclear physics. He also liked to build things, his largest creation being an oscilloscope. He attended local public schools, where he says he was an indifferent student, except in physics and chemistry, which he loved.

Jones matriculated into the University of California, Berkeley, at which point he began to enjoy studying and to work hard. He entered the lab of Walter Knight, where he worked in the new field of nuclear magnetic resonance, and discovered the joy of doing research and of intellectual discourse.

Jones continued into graduate school at Berkeley in great part because there were few jobs. He worked in Carson Jeffries's lab, where his thesis work dealt with electron transport in a molecular afterglow. The Korean War again initiated a demand for sophisticated electronics, and Jones accepted a job with Bell [Telephone] Laboratories, where he would be able to pursue his work in gas discharge. He was finishing his thesis work when William Shockley walked into his lab and began asking questions. An intense afternoon and evening of discussion with Shockley led into aggressive recruitment until Jones finally accepted Shockley's offer of a job at the new Shockley Semiconductor Laboratory.

Shockley believed semiconductors were the wave of the future, and he espoused diffused-base technology. Shockley insisted on using only silicon, which at that time was extremely difficult to work with. He at once put Jones to work on the four-layer diode; he also used Jones to help him recruit other scientists. There were difficulties encountered in getting the lab started, though he was able to recruit a number of scientists. From the outset, lab work was compartmentalized and Shockley frequently changed the goals of the lab. From Jones's perspective, Shockley was a genius that was shortsighted because he had no use for magnetic resonance.

Uncomfortable in the high-stress atmosphere of the lab and wanting to work with his primary interest, electromagnetic theory, Jones decided after only two years to look for work in the academy, where he expected to be able to "decompress." Arnold Beckman, one of Shockley's financiers, tried to hire Jones as a liaison to Shockley, but Jones turned Beckman down. Instead, inspired by John Van Vleck's teaching, Jones accepted a position at Harvard University. He spent almost fifty years there, teaching electronics. His own work for his first twenty years there revolved around magnetism, but he then took up optics.

At the end of the interview Jones discusses his insights into William Shockley and the deterioration of their relationship; the development of semiconductor electronics; his own theory of a "planar metaphor" giving rise to a host of technological development; the importance of crystal growth and the lesson of semiconductors, viz. have good material; the underappreciated importance of a systematic, if time-consuming, approach in science as evidenced by Bell Labs' ten-year development of laser diodes. He describes how his reading of Leslie Berlin's recent book about Robert Noyce has led him to renew acquaintanceships with some of the other early scientists at Shockley Semiconductor Laboratory. He concludes by reiterating that Shockley, at least "one of the most complicated" scientists of the time, made invaluable contributions to physics.

INTERVIEWER

David C. Brock is a senior research fellow with the Center for Contemporary History and Policy of the Chemical Heritage Foundation. As an historian of science and technology, he specializes in oral history, the history of instrumentation, and the history of semiconductor science, technology, and industry. Brock has studied the philosophy, sociology, and history of science at Brown University, the University of Edinburgh, and Princeton University (respectively and chronologically). His most recent publication is *Understanding Moore's Law: Four Decades of Innovation* (Philadelphia: Chemical Heritage Press), 2006, which he edited and to which he contributed.

TABLE OF CONTENTS

INTERVIEWEE: **R. Victor Jones**

INTERVIEWER: **David C. Brock**

LOCATION: **Telephone Conference**

DATE: **18 April 2006**

BROCK: This is an interview with R. Victor Jones conducted by David Brock by telephone, on 18 April 2006. Professor Jones, let's begin with your personal background. I read that you got both your undergraduate degree and your doctoral degree in physics from UC Berkeley [University of California, Berkeley]. Are you a native Californian?

JONES: I grew up in Oakland [California] so Berkeley was a great opportunity for me.

BROCK: May I ask you a little bit about your family background such as your father's vocation and the development of your earliest interest in science?

JONES: My parents were Welsh immigrants. They came here individually in their early twenties, and met in Oakland's Welsh community. My father was a machinist, and I learned many mechanical skills from him which I have found useful over my lifetime. I have always considered myself an experimental physicist and I do feel there was some resonance in our interests. [In the year before his death, my father enjoyed visiting my lab at Berkeley a couple of times.]

BROCK: Was building your own equipment a large part of your experimental career?

JONES: Yes, I have always enjoyed that. However, my interest in building things figured in a disastrous way in the Shockley [Shockley Semiconductor Laboratory] experience.

BROCK: Did you go through the public education system in Oakland?

JONES: I went through schools in the now somewhat infamous Fruitvale District of Oakland [Hawthorne Elementary, Hamilton Junior High and Fremont High] before going on to Berkeley. I started there in 1947 when Berkeley was receiving a huge input of veterans. It was the most crowded year, I think, that Berkeley had ever experienced and, for example, first-year chemistry laboratories were held in the wee hours of the morning. It was an interesting and exciting experience.

BROCK: In your high school experience, tell me about the development of your interest in science in general, physics in particular, and how that may or may not have related to any of your hobbies outside of school?

JONES: I think it goes the other way. [laughter] When I was very young I read about physics extensively. I was very interested in stories about evolving atomic and nuclear physics. I read lots of popular books on the subject when I was a kid. When I got to high school I went on a very different route. I was a pretty poor student and became very active in student political affairs. Believe-it-or-not in my junior year I was head "cheer leader" and in my senior year I was student body president of quite a large high school. I was also very active in theater and dramatics, and viewed myself [as did several of my teachers] as a pretty good actor.

Physics and chemistry were surprisingly easy for me in high school. Given the school, the classes weren't very demanding, and I used to regularly cut science classes to pursue my other activities. For reasons that are not clear to me, the wonderful lady who taught physics gave me a lot of space. I was pretty undisciplined throughout high school, but that changed in college. I got very interested in physics in college and worked quite hard at it. In spite of my lack of science discipline before college, I had some earlier experience doing techie peculiar things. As a youngster I built a lot of electronics on my own and my biggest project was building an oscilloscope from the ground up. I did learn a fair bit of electronics in the process of building my oscilloscope…and I also got a lot of big electrical shocks. [laughter]

BROCK: Did amateur radio figure at all?

JONES: No, it didn't.

BROCK: Did war surplus vacuum tubes aid you in any of your experiments?

JONES: That was a great help. [laughter] It helped me to become familiar with the whole arsenal of emerging electronics. I was basically growing up during World War II and just missed it, but I was aware of evolving electronic technology. However, as previously noted, I was definitely not a serious science student in high school.

BROCK: What reoriented you back to physics in your undergraduate days?

JONES: That's actually an interesting question and I'm not even sure I know the answer to it. I went to college on a lark because I was thinking about doing other things, and I think that it was taking some of the early physics courses in college and doing pretty well at them that catalyzed my interests. I certainly didn't go to college expecting that I was going to be very successful at it. It was a peculiar time for physics. Physics had, obviously, been important in wartime, but by the time I was nearing graduation it was very difficult to find technical jobs. In fact in my senior year, I actually went through a program in elementary education. I went through the teacher training program just to make sure I could get a job when I got out. That situation changed dramatically by the time I finished my Ph.D.

BROCK: Did you have an opportunity to do research as an undergraduate?

JONES: Yes, I did. I worked with Walter Knight [Walter D. Knight]. He was doing nuclear magnetic resonance in metals [thus, the origin of the famous designation "Knight Shift" in metals]. I built some apparatus for him and worked with his graduate students. It was a great experience.

BROCK: Was that quite early in the nuclear magnetic resonance game?

JONES: It was early. The fellow I did my Ph.D. with was one of Felix Bloch's students at Stanford [Stanford University], so it was the early days of nuclear magnetic resonance—or the resonance business in general.

BROCK: I see.

JONES: Walter Knight was a good person and a very good experimental physicist. He eventually became provost at Berkeley.

BROCK: Was he a mentor to you as an undergraduate?

JONES: I would say somewhat. [laughter] He certainly helped me to define myself as an experimentalist. But I wouldn't say it was a strong mentorship. The important aspect of my undergraduate experience at Berkeley was my immersion in a very hot research environment and exposure to many outstanding researchers. During that time, I picked up the excitement of doing research.

BROCK: The excitement in the process?

JONES: Yes.

BROCK: Tell me about your decision to pursue your Ph.D. at Berkeley?

JONES: I never intended to go on for graduate work, but as I said this was in 1951 and 1952, the economy was in pretty bad shape. Top graduates from Berkeley in electrical engineering were installing TV sets. It was not a time of great opportunity. I enjoyed physics a lot and it seemed like a good holding pattern to go on to do some research. With the Korean War, the interest in electronics and physics picked up very considerably. I think it was a fairly passive decision to go on. It was easy. I had done fairly well as an undergraduate and certainly Berkeley was perfectly happy to have me, so it was somewhat a passive decision.

BROCK: Describe the process that you went through to pick someone to work with?

JONES: Berkeley physics was dominated by the cyclotron lab on campus and the Lawrence Laboratory on the "Hill"—"big physics." I think I have always gravitated away from big physics, in the sense that I really liked—and this goes back to the Walter Knight experience—I enjoyed doing experiments but I always enjoyed doing experiments where I could do the theory to interpret my own experiments. "Small physics" has always been attractive to me since I've have always enjoyed doing theory as well. Experimental big physicists tend not to do much theory and, so, I did not follow the dominant pattern at Berkeley.

BROCK: Who was it that you eventually—

JONES: I worked for Carson Jeffries [Carson Dunning Jeffries], who was quite a remarkable man. Carson Jeffries was an extraordinarily good experimental physicist and also an artistic figure of some prominence. I was his second student. He was young—he wasn't tenured when I started with him—and he was putting together a small group of people to work in experimental physics. His first student was a Japanese-American student who had just gotten back from fighting in Italy, and he did a project in nuclear magnetic resonance. Bill Dobrowolski and I started Jeffries' work in electron magnetic resonance. We built a lot of equipment from scratch and that was a good experience. I've always enjoyed that. Looking back on it, we were building things which now people buy routinely. We took a long time doing that, but it was fun.

BROCK: Did you construct your own spectrometer?

JONES: Oh, absolutely. We built our own. At that point Varian [Varian Incorporated] had just started in the magnetic resonance business, and began to sell high-resolution electromagnets but we didn't have the money to buy such a magnet so I built from scratch a hard-tube highly regulated power supply for an electromagnet built in our shops. We also built highly regulated klystron power supplies and state-of-the-art lockin amplifiers. I learned a lot about electronics, but it took a lot of time. [laughter] It was fun and something I enjoyed very much lots of mechanical and electronic work. I learned a whole lot about the design and control of a variety of electromagnet systems. Perhaps not surprisingly, I have spent much of my subsequent career on magnetism and magnetic technology.

BROCK: Did you feel that in the research topic that you finally struck upon that you were able to get that blend of theory and experiment that suited your inclination?

JONES: Absolutely. I think the thing I was most proud of in my thesis work was actually a theory of electron transport in a molecular afterglow. The nitrogen afterglow is an old subject going back to Lord Rayleigh that eventually became an important new subject. I did a thing called cyclotron resonance in molecular afterglows. It was a very interesting subject and I learned a lot about gas discharges and I really found this an interesting juxtaposition of experiment and theory. The experiments were hard, but they eventually worked, and the theory was not even there when I started.

BROCK: Interesting.

JONES: I really enjoyed what I put together for my thesis.

BROCK: As your research came together and you saw that there was an endpoint in sight for your thesis, what were your career thoughts at that time?

JONES: I wasn't interested at all in academia, and I'm not sure why [I had been doing a lot of teaching as a graduate student]. I think there were some very naïve notions that I was working on. I had an officemate, George Feher, who was just beginning some early interviews with Bell Labs [Bell Laboratories Incorporated] and they were offering him a good deal of freedom to develop a program in magnetic resonance. I was beginning to see that there were opportunities for doing good research in an industrial environment and that seemed to me to be exactly what I wanted to do.

BROCK: Was it the question of application or social impact that was coloring your attitude toward academia at that point?

JONES: I'm not sure I ever really pieced that out. It seemed to me that I was enjoying what I was doing as a graduate student, and, as I've observed in my later career, graduate study is a very good time for most people, even though it seemed like drudgery at the time. For me it was a very good time in the sense of being able to do almost anything I wanted. It seemed that the industrial career path would allow me to continue that. I think for most people industrial science—in the good old days of Bell Laboratories and alike—was certainly a very rich experience.

BROCK: Yes. Did you embark on a round of interviews with industrial laboratories?

JONES: I went through the usual rounds and I had offers from some good, but highly directed research industrial labs. For instance, I interviewed with several chemical laboratories associated with the petroleum industry. A lot of these companies were interested in exploring various aspects of magnetic resonance at that point for a variety of chemical applications. I also got a lot of good offers from places like the GE Research Lab and Westinghouse, but I was particularly attracted by a very good offer from Bell Labs, to continue my work in gas discharge. At that point, remember, vacuum tubes were still an important part of the electronics and communication technology.

BROCK: Of course.

JONES: There was a fellow by the name of Homer Hagstrum who ran a very good research group at Bell Labs—this was part of the really core fundamental research part of Bell Labs where the research was pretty free. He was an outstanding scientist and had a very active group working on a variety of problems in vacuum and gaseous electronics. They were very interested in the work I had done in my thesis. If I did go there, I would have probably started working on things that I'd done in my thesis and then worked off from that, which was a fairly typical Bell Labs pattern at that point. They gave people an opportunity to get a few papers out when they first arrived and then encouraged them to go out and do something new.

BROCK: Was that in the 1956 time period?

JONES: 1955 or 1956. I have a little uncertainty on some of these dates but I think the job offer from Bell Labs was very late in 1955.

BROCK: Okay. When and how did William Shockley [William Bradford Shockley] emerge in the mix?

JONES: That was an odd experience. I had already accepted a job at Bell Labs, but was finishing up my thesis research. My research room at Berkeley was in LeConte Hall (the physics building), just off the front hall. One day this little man walked into my lab and, without introducing himself, started asking me searching questions about my experiment. I was used to getting lots of madmen visiting my lab because it was right across from the front entrance to LeConte Hall (just opposite the Campanile) and often crazy people would just walk in and start asking questions about all kinds of crazy science fiction stuff.

BROCK: Okay. [laughter]

JONES: So I was guarded when this little guy walked into my lab and started asking questions. But, the questions were reasonably perceptive and I started to pay more attention to him. Then, he confessed that he had sought me out following a conversation with Charlie Kittel. Do you know that name, Charlie Kittel?

BROCK: I do. Yes.

JONES: Charlie Kittel was an outstanding solid state theorist at Berkeley and former associate of Shockley at Bell Labs. Shockley told Kittel about his attempts to recruit people and Kittel said, "Why don't you go and have a try at this guy Jones?" Thus, Shockley's visit to my lab. I was in my working togs in the lab, so Shockley said, "Why don't you go get cleaned up and we'll go out for lunch," and, flash forward in Shockley's Jaguar, we were soon at the Fairmont Hotel in San Francisco for lunch. [laughter] For a starving graduate student the luxury was impressive and a bit of a shock, but there followed what it was probably the most intense afternoon of physics I have ever enjoyed. Shockley and I spent, I would say, the next six or seven hours in very intense discussions and, as it happened, he asked me about subjects about which I knew quite a lot. I guess I showed up pretty well. He thought he was asking very hard questions, but they were in areas of comfort for me. One of the greatest and most interesting subjects, to me, has always been electromagnetic theory. I had some very good courses in the subject at Berkeley that had strongly triggered my interest. Shockley had just come from a visit with Feynman [Richard Feynman] at Caltech [California Institute of Technology], and he (Shockley) had learned of a number of electromagnetic paradoxes that he was interested in quizzing me about. It turned out that I was—not because of my brilliance but just because I happened to know the subject pretty well—able to cope with the paradoxes that he posed. [laughter] I don't mean this to sound immodest since my supposed brilliance was essentially accidental. However, Shockley was impressed by me and he made a real pitch to get me to work for him. I said, “No, I've already taken a job at Bell Labs and it's a good one.” Of course, that was even a greater incentive for him because he wanted to win a victory over Bell Labs. [laughter]

This debate went on for several days. I don't know if you've picked up on this or not, but one of the things that Shockley was enamored with was having psychological profiles of his people. He said, “Well, you don't have to come with me, but I'd like you to submit to a psychological profile because I'd like to calibrate the guys that are doing it. As a personal favor would you do this?” I did it and that was an interesting part of the whole recruitment. At that point, I really wasn't inclined to go with him. I had already established my interest with Bell Labs. But, I liked the West and would have preferred to stay in California. I had family in California, and my mother was getting along into her eighties, (although she did eventually live to a hundred). I also had a semiserious girlfriend who lived in California. The idea of going to East was not attractive for me. However, there weren't many opportunities in California at that point. Certainly nothing like Bell Labs existed in California. So, by hook or crook, I ended up going to with Shockley. As result, I became a *persona non grata* at Bell Labs for the rest of my career. [laughter] Whenever I met somebody from Bell Labs in subsequent years they would say, “Oh, you're that guy.” In those days, you just didn't turn down a Bell Labs job.

BROCK: It must have captured their attention, certainly.

JONES: That's how I got to the Shockley Semiconductor Laboratory.

BROCK: Were you aware of Shockley's reputation?

JONES: Not to the degree that I became aware of it later. [laughter] In retrospect it was a very naïve decision process, but I was young.

BROCK: What about his technical or scientific reputation?

JONES: I knew it was high. I was fairly aware of what was going on in contemporary solid-state physics since I had audited Kittel’s course and one of my closest friends at Berkeley, Gene Dresselhaus, was one of Kittel's outstanding students. Kittel brought solid-state physics to Berkeley. I was aware of a lot of the work in his group, and I knew Arthur F. Kip who was an experimentalist working with Kittel. I was aware of the personas in solid-state physics, and one of the reasons I did cyclotron resonance in gases was the fact that cyclotron resonance in semiconductor materials was a very hot subject. Gene Dresselhaus did an extensive theoretical analysis of cyclotron resonance in semiconductors. So I was inclined to have a crack at it in gases. After a lot of effort, it actually worked. My thesis had a couple of pieces but that was the dominant piece. I was well aware of who Shockley was, but I didn't know about all the crazy stories. I'm not sure that if I knew them it would have changed my mind. [laughter]

BROCK: At the time he recruited you, how did Shockley describe the nature of his new operation and his intent?

JONES: I think his view was that interesting devices were invented at Bell Labs using all the wondrous opportunities that it offered for doing pure research, or at least directed research of a certain kind. Yet, Bell Labs wasn't able to capitalize on these by moving forward solid-state technology. His feeling was that the opportunity was there to do things that weren't being done, and that the technology should move forward. In particular he and Morrie Tanenbaum [Morris Tanenbaum] invented and developed the so-called mesa transistor. The mesa transistor, to me, may be characterized as a major metaphorical development in modern technology—I can expand on this contention if it would be useful. Shockley put forward, in our discussions, the fact that there were new opportunities in semiconductor device development which were not being exploited. Based on my conversations with Shockley, I came to believe his contention.

I believe to some extent he was triggered by some of the Hewlett Packard concepts of getting together a group of people who were research-level scientists but who would be motivated to get something out the door. As you probably know Hewlett Packard worked for many years on the philosophy of people taking a device from the basics to the market. I knew Shockley was very familiar with Hewlett Packard's operation, whether he picked that up from them or whether it was something that came from him, I don't really know. Shockley's idea was that he would recruit people who could make their living as research scientists but had the willingness to bring stuff to utility. To me, that was an attractive thought. I've always viewed myself as an applied physicist in some sense. I'm not sure I could have articulated it at that point, but as life has gone along that is what I enjoy.

BROCK: Let's talk about what you mean when referring to the mesa transistor, the diffused-junction transistor, as a metaphorical development?

JONES: I have not done the historic research to know whether Shockley is the unique figure in this regard, but I suspect he is. What Shockley articulated at that time in the development of transistors was the need to make things extremely small. The specific big issue was that in the physics of junction transistors, the thickness of the base region is the crucial parameter. The base region has to be kept thin, for a whole variety of reasons. If it's too thick then the electrons injected from the emitter into the base won't get to the collector junction and transistor gain will be impaired. The base region has to be physically thin. The technology available at that point was developed around various mechanical or semi-mechanical means for slimming down the devices into very small thin structures. Let me note parenthetically that Bob Noyce [Robert N.

Noyce], before he came to join the Shockley Lab had been working at Philco [Philco Corporation] on a methodology for making very thin base regions on germanium transistors, which was a mechanical marvel in some sense. It was obvious that they we weren't going to be able to keep making things smaller and smaller by basically mechanical means. Shockley said, "We must use controlled natural processes to arrive at small transistor structures."

What became the technology for the diffused base transistor was to start with a plain semiconductor surface and then to use processes of sequential diffusion controlled by time, pressure and temperature to differentially build structures in the vertical direction from the plane down. I think that was the advent of one of the key metaphors of our time because if you look at any technology now—acoustics, biotechnology, optoelectronics—the idea of building structures with planar-oriented processes – diffusion, ion implantation, lithography, selective etching—is a dominant metaphor. I have not come across any examples of the use of this metaphor before the advent of the mesa transistor. I could be wrong, but when I posed my view to Jay Last [Jay T. Last] at our recent luncheon, he thought it was an interesting thought, but he wasn't quite sure he believed it. [laughter] But, it's been a view that I've have bandied about for some time. Photolithography and a whole arsenal of planar-oriented techniques now permeate every facet of technology. In fact one of my sons has worked on interesting biological applications of planar structures of this sort. In my view, this powerful metaphor first emerged in the context of the diffused-based, mesa transistor.

BROCK: Right. And certainly it sounds as if Shockley was putting that foremost, to getting—

JONES: Oh yes. The diffused-base technology was one of the fundamental design ideas that he put forth. He felt that—I don't know what the history at BTL was—but he felt it was insufficiently appreciated. I had conversations with Morrie Tanenbaum in later years about this, and he recognized that it was a new metaphor that had been developed with Shockley. He didn't deny that it probably wasn't made useful by Bell Labs in the way that Shockley had hoped. But I really don't have any other insight into the history.

BROCK: Did Shockley discuss a strategy for his new operation with respect to crystal material at all?

JONES: Not at that point, but it was quite clear when we got in the business that it was a serious issue. I think the diffusion technology, as a basis of device development, was the key matter as well as the idea of taking research-level people to do the development. I think the success of Intel [Intel Corporation] demonstrates the validity of the assumption. The success of Fairchild [Fairchild Semiconductor Corporation] and Intel certainly bore that point out. Most of the people that were active in those early developments would certainly be of the caliber who could operate successfully in a research laboratory.

BROCK: Right. When was your first day at Shockley Semiconductor?

JONES: [laughter] I'm not sure of the first day. It was early 1956. I hadn't submitted my thesis yet but I started working there. My thesis was done but I was putting together a few final things. I think I appeared sometime in early 1956. I don't really have a precise recollection of the date, but I remember having a very depressing impression. With all of Shockley's high-minded rhetoric, putting together the laboratory was something less than an exciting process—Jay Last once characterized the early days in an old garage as life in the "putty knife factory." [laughter] In fact, when I started with Shockley there was nothing. We were sitting around in a storefront somewhere in Palo Alto. We were a strange group of people. Most of the later people had not yet been recruited. There was a fellow by the name of Bill Happ [William Happ], there was a fellow by the name of Leo Valdes, and there was someone by the name of Smoot Horsley. Those were the people. I was taken aback. This was not Berkeley, but of course Shockley was there. Shockley put me to work on a device which had great complications, and that was the four-layer diode. He immediately said, "You're a good theorist, so why don't you work on understanding the four-layer diode because we're going to make them." I have got to say that the four-layer diode is still, to me, one of the most complicated devices in semiconductor technology. [laughter] It is a remarkable device and it obviously resembles some gaseous discharge devices that I knew about, but the theory was wild and very nonlinear. For other reasons I spent time looking at that theory in subsequent years and I still believe it was a overwhelming task to put on a guy who knew nothing about semiconductor physics, but that's what Shockley did.

I spent most of my time reading and trying to learn about four-layer diodes. The reasons for Shockley's interest in the four-layer diode, which triggered some of the later problems with the staff, was that he saw that it could be used for telephone crossbar switching. Indeed, it could have been a very interesting possibility for that application. I think Shockley saw the four-layer diode as a unique but fairly simple use of his diffusion-based planar technology. Namely, what had to be done was to diffuse in four layers, and then contact could be easily made at the top and the bottom, of the four-layer stack. I think what he saw—I'm somewhat projecting here—was the fact that one could put a whole bunch of these on one chip and then have a crossbar. Whether he really saw it that way or not, I don't know, but it would not have been a stretch to imagine that. I think even though it seemed crazy at that time the idea of building a chip of crossbar switches was not too big a stretch.

BROCK: I've heard varying things about the four-layer diode. One of which was that the real issue with it, from a manufacturing point of view, was that it was hard to get uniformity from diode to diode because the diffusions were so delicate?

JONES: That's correct. But to build a CMOS [complementary metal oxide semiconductor] device, I think, had similar challenges and the challenges have been met. I think the three-layer junction transistor device was simpler, but on the other hand contact had to be made with the intermediate layer. That made it harder. [Laugher] At this distance, I don't think one could make a good parse of which device is the most desirable. I think that probably what was the more pressing point was that the four-layer diode was a fantastic device for telephone switching, but if one was trying to sell a general purpose device which would go into many different products, I don't think it was a very good choice. After all, the bipolar transistor is a little bit like a general computer. It's a device that can be used for many different purposes, whereas this four-layer diode has only a very unique function.

BROCK: I see.

JONES: I once played with the idea of semiconductor optoelectronic devices which mimic the functions of the retina and the visual system.[1] In such an application the four-layer diode could well play the crucial role of a synaptic-like element—it has the response of an axion. It is a very interesting device and I don't think it should be pooh-poohed out of existence, but it certainly was not a good way to build a market, which I think was the real criticism of the people that left Shockley.

BROCK: Okay. Just to make sure that I understand you exactly, this was still in the days when Shockley Semiconductor Laboratory was a handful of people in a storefront when Shockley gave you the tall order of getting your head around the four-layer diode theory?

JONES: That's correct. I think it was basically a smart thing. He knew that I was bridled by the less than inspirational people I was dealing with and I think he was probably afraid that I would take off because I did not have any particularly strong bonds there. He was trying to give me something challenging. I also spent a lot of time on things like helping to find lab space we eventually ended up with an old garage that became the putty knife factory, in Jay Last’s term—and recruiting people.

I traveled a lot with Shockley. He was trying to recruit people in various places, and again, he had recruited me away from Bell Labs so he used that as an argument. He would say, “This guy Jones has made a commitment. Why won't you make a commitment?” I remember going to visit Eugene Kleiner in New York, and meeting Jay Last in Cambridge [Massachusetts], and so forth. Also Shockley was trying to pick up funding from various places. He had a big connection with the Army and he kept trying to sell them on four-layer devices for, I believe, proximity fuses. There was a lot of traveling around to get the word out, to do recruiting, to pick up some money and to give papers, because Shockley never gave up on his desire to be a famous scientist.

---

[1] R. Victor Jones, “Photoneural Systems: An Introduction,” *Applied Optics* 26(10) (15 May 1987): 1948-58.

In fact, just to jump ahead in the story a little bit, I think that one of the key problems that occurred—most people would agree—was that Shockley was never clear whether he wanted to be the greatest scientific or technical entrepreneur, or whether he still wanted to be the Nobel Prize winner. People would come in one day and everyone was to produce papers that could be published. The next day they'd come in and he'd be disturbed that we had not taken over the market with new devices. I think that the ambivalence underlies a tremendous number of the problems that plagued the Shockley enterprise.

BROCK: From the first group of people, who you met in the storefront, were Happ and Valdes from Bell Lab or the Bell System?

JONES: Bill Happ was from Raytheon [Raytheon Company] where he worked on semiconductor devices. Leo Valdes had been a real actor in Bell Labs device development so he knew a lot about device physics. In fact, he was targeted to be the guy who was going to do materials for Shockley. He was spending his time planning to build conventional crystal growers and was ordering parts. He was the most experienced person. He was the only person with some autonomy until Bob Noyce arrived. Bob Noyce was obviously very experienced in semiconductors. He had been working on germanium transistors at Philco for three years. I would say of the early group of people it was only Leo Valdes and Bob Noyce, who came somewhat later, who knew anything about semiconductors. Jay Last, Gordon Moore [Gordon E. Moore], and I, knew bupkis about semiconductors. We used to get up an hour earlier every morning and give each other seminars. We used to go through the classic papers on semiconductors because we felt that we were tremendously disadvantaged.

BROCK: The three of you?

JONES: I think it was only the three of us most of the time. Sheldon Roberts showed up upon occasion, but I think as a materials person he was less concerned about not knowing much about device physics. Roberts did come to these seminars on occasion but the three of us were desperately trying to get ourselves into the position where we knew something because none of us had any academic experience in that regard.

BROCK: When did the inklings of the crystal grower project first become visible?

JONES: In talking earlier about the organizing principles of the laboratory, I left out the third element: namely, the dictum that we would only work with silicon. At that point silicon was a monster; the lifetimes in silicon were just terrible. Because of the higher melting temperature it was a very much more difficult material to work with than germanium and, while silicon dioxide now contributes to making silicon the greatest material in the universe, initially it made working with silicon a horror since the dioxide formed quickly on fresh surfaces and had to be removed with hydrofluoric acid—ugh. For many of the folks working in semiconductor technology, silicon was an absolute monster so these folks were more comfortable making germanium devices. In fact, there were many good germanium devices on the market at that point. But Shockley wanted silicon. We were going to make it devices with silicon. We were going to solve the problems of silicon. One of the key problems was the notion that defects were being incorporated from the quartz crucibles in which the crystals were being grown. The view was that one had to get away from graphite-heated quartz crucibles because the quartz-silicon interface supposedly generated oxygen defects in the crystals.

The defects had not as yet been identified, but Shockley always had in his mind the history of deathnium in development of germanium technology. [laughter] He always had in mind the deathnium story while he was trying to solve the lifetime issue in silicon. In fact, that was another thing I got deeply involved in; calculations on how to how to do floating zone crystal growth of silicon, which is, because of the higher melting temperature, much more difficult that floating zone growth of germanium. We did work up some pretty good designs for silicon floating zone techniques. These were vertical column setups very much like what later evolved in other silicon floating zone systems. The problem was that the rf (radio frequency) heating had to be controlled to a very high degree. The control problem is very nonlinear and it was difficult to build adequate control circuits in those days. I spent a lot of time on that issue. Shockley and I were on one of our trips and we started talking about the whole issue of how to grow silicon without a crucible. Somehow in our conversation—I may have first posed it—but we got into this idea, "Why not use silicon as the crucible?" "How would you do that? What are the different ways you could do that?" I think I first put the idea forth because it had resonance with something else I had once done. But, I really can't put that history together and I don't want to make any unwarranted claims. In the process I said, "If there was a heater that brought the whole silicon boule to a temperature close to the melting temperature, we could then use a small resistance heater above the surface to just heat a puddle of molten silicon from which a crystal could be pulled." We spent the rest of the night talking about the idea. It was a transcontinental trip, and in those days that meant a long night. By the time we got to Washington, [DC] which was where we were headed, we had pretty well scoped out what we were thinking about. It seemed like a promising idea.

When we got back, I took the next step. As mentioned before, I really enjoy down and dirty experimental work, so I took a simple tabletop chemical oven that went to a hundred and fifty degrees or something like that, and I built a little surface heater and got a pool of Wood's metal within a boule of Wood's metal. Wood's metal is a good experimentalist's trick since it melts at something like eighty degrees centigrade; I was obviously trying to impress Shockley. I think in about a day of fooling around I was able to use a little drill motor to pull little cylindrical ingots of Wood's metal up through the surface heater. Voila! There it was— of course, it wasn't a single crystal because that may not even be possible—but it was a nicely shaped polycrystalline cylindrical ingot grown from a boule of Wood's metal. I took these ingots in and showed them to Shockley and of course he was ecstatic. As I look back, this was one of the greatest mistakes I ever made in my life. Shockley thought, "It's so easy to do this. We did this in one day. We just go from here now to silicon." I said, "Well, no. We really should work out some of these other issues." He said, "No. You haven't got the big picture here. In order to move in this modern technology, one has to really be bold." With that he pulled in Knapic [Dean Knapic], Kleiner, and Julie Blank [Julius Blank] who were at that point underemployed, I would say. Shockley viewed them as people who were going to be doing production lines and things like that. He asked Dean Knapic, "Do you think you could do this?" Of course, Dean Knapic said, "Yes," no matter what the job was. I cautioned them—this was a big shift from Woods metal's melting point to silicon's, and there were some major problems. But, Shockley was absolutely convinced that we were going to do this, so that began the adventure of building "the crystal grower." Of course, we rapidly started to work up some really difficult technologies. We ended up with things like rolled molybdenum heat shields and titanium heater wire, some incredibly expensive stuff. I spent a lot of time going to various kinds of vendors to see who could do this and who could do that. I did a lot of traveling. Of course, it all had to be secret because, of course, Shockley was a very secretive guy and he thought this was going to revolutionize the world.

About this time Valdes left. Valdes and Shockley didn't get along because Valdes thought he knew more about semiconductors than Shockley. That was not a healthy working relationship with Shockley. After Valdes left all crystal growing for the lab fell into my lap which was amazing because I had no previous experience with that kind of thing. But, I had read articles on conventional techniques and so we started pulling crystals—not great quality crystal, but silicon that was reasonable and useful for early development work.

While I was doing conventional Czochralski crystal growth as a service function for the rest of the lab, I modified one the conventional growers to test further the "puddle growth" idea. Of course the problem was that the surface heater suspended above the silicon in the conventional configuration was heated both by a current source and uncontrollably by the rf generator. Somehow, I roughly shielded the surface heater from the rf. I've forgotten exactly how I did this, but it was a kluge for sure. I could make a puddle in this modified grower, and I was able to reach down and pull up a crystal. It showed the feasibility that something like this could be done. The real grower couldn't be built with an rf heater because there was no way to control what was being heated. The crystals from the modified grower looked really funny because funny things were going on. But, I was able to make them slower and then hotter, and so forth. So, we did grow these little crystals. Of course, that again made Shockley ecstatic. I was so deeply mired in this project at this point that I needed this as some kind of reassurance that it wasn't total nonsense. So I grew those early, or only, puddle-grown crystals (In fact, I actually grew these puddle-grown crystals before I had the convention crystal growing program underway.). A number of years ago one of my kids found one these feasibility crystal in a drawer and he said, "What's the heck is that?" A picture of that crazy crystal is now on one of my web sites.[2]

Meanwhile, the full-scale experimental crystal grower just grew like topsy. I don't know if this was a punishment or something, but it was mounted next to my desk in the old garage. I

[2] See, http://people.seas.harvard.edu/~jones/shockley/first_crystal.html.

almost lost my hearing because a worker putting up a steel structure popped off a powder actuated gun near my ear—I lost my hearing for about seven months.

BROCK: Oh my gosh.

JONES: This machine just grew and grew. Of course, it was very complicated to build things in Palo Alto at that point, because of the limited technological resources in the Bay Area. The engineers (Knapic, Blank, and Kleiner) had come from the East Coast where they knew that there was always a guy to make a vacuum system over there and another guy to do complex machining over here. It was pretty frustrating for them since there just wasn't that kind of technological base in California at that time. A lot had to be built and imported from the East. However, I think the grower’s huge vacuum tank was fabricated at a shipyard in South San Francisco. The most difficult problem was the design and fabrication of the heat shielding for the resistance heated core. Resistance heating was used in the system as the only way to solve difficult temperature control issues and the resistance heater had to be shielded to minimize heat loss. But, of course, all these structures had to be stable at silicon’s melting temperature (1410° C). Nothing was simple. Everything was complex. In conventional Czochralski crystal growth, rf heating makes things pretty simple since only the crucible needs to be at the silicon melting point. The shielding issues are relatively simple and it is very easy to get power in and out.

Resistance heating was a much more complicated business. The resistance wire available when we first started the heater core had to be used in a vacuum. As we designed the system there, it was just one problem after another. These difficulties certainly contributed to my wanting to leave since I was stuck with this monster. I spent a lot of time with patent attorneys developing the patent that I saw for the first time yesterday after you sent it to me.[3] [laughter] The patent people were very good—and I've always enjoyed working with patent attorneys because they can make you expand your thinking to increase the coverage. I really haven't read the patent yet but I would suspect that, from the guys we had, the coverage was pretty good on it. [laughter] They always said, “Could you use it for this? Why can't you use it for that?”

That is the whole history of the grower as far as I was concerned. It did go on after I left, but at that point I wasn’t really central to it. It became an engineering project. It was certainly a lesson that I learned for future work; namely that one can’t have a half-cooked idea and then take it immediately to the endpoint. New ideas have to be worked up slowly. Again, it was Shockley’s mistake – I had little autonomy at that point. He was out to beat the world in the shortest possible time. He just wasn't patient.

BROCK: I have a question about the floating zone work that you were doing prior to the flight where the two of you came up with this idea for the crystal grower.

[3] William Shockley and R.V. Jones, “Crystal growing apparatus,” U.S. Patent 2,979,386, issued on 11 April 1961.

JONES: We did not build any floating zone apparatus. Those designs were all on paper.

BROCK: Okay. Was the idea to use a floating zone method to further refine single crystals?

JONES: No, for crystal growing.

BROCK: To do crystal growing through a floating zone method?

JONES: The idea was to start with a vertical boule of polycrystalline silicon. An rf heater coil could be used to create a small molten zone which, under favorable circumstance, could be constrained by surface tension. By first moving the rf heater coil to, say, the bottom of the boule column the molten zone could be made to contact a silicon crystal seed and then the boule could crystallized by moving the rf heater coil up the column. Of course, the first semiconductor floating zone applications were on germanium and there it was mainly used for the segregation of impurities. I think designing a workable silicon floating zone grower would now be a relatively simple problem, but in those days control electronics were not very sophisticated. It was a sizable control problem to stabilize such a floating zone. We did set down on paper what should be done: we could use an optical method to track the size of the zone and then we could use this data as input to a feedback magnetic amplifier for control of the rf generator. A doable, but nontrivial, control problem.

BROCK: I know that you had an electro-optical control system, at least in the patent on the crystal grower. Was that a transfer?

JONES: Sure it was.

BROCK: Okay.

JONES: I never saw the patent before. It was interesting to see that hexagonal shape of the heat shields because I remember drawing those. [laughter] As I said, most of this was worked out with the patent attorney. They would ask, “How do you control this?” I would say, “Well, you could do this or you could do that.” I am sure that some of grower control work was coming from the floating zone design.

BROCK: I see.

JONES: But, that's a long time ago. [laughter]

BROCK: Let's talk about the difference between—in terms of control—an rf heating system and a resistance heating system. I'm not quite sure that I quite understand why resistance heating would give you greater control than rf heating.

JONES: No, that is not issue; I wanted to separately control the heating of the puddle and of the surrounding silicon. If I had used the conventional way of heating the large mass of silicon by coupling rf energy into the graphite crucible, then it would be difficult to separately control a small surface heater since the rf would couple into the heater as I mentioned earlier.

BROCK: Oh, I see now.

JONES: Those are not separately controllable elements. The idea here was to bring the silicon block within twenty or thirty degrees of the melting point, hold that, and then come down with the surface heater and add the additional heat to get the surface puddle. Now, to control those two heaters separately with a radio frequency heater would have been a nightmare. There's no way I could think of to do that.

BROCK: Because it would be really hard to control what temperature the surface was?

JONES: As I mentioned earlier, I did a feasibility experiment by which I used the concept to make little crystals. Basically what I did was to modify a conventional Czochralski grower configuration by pushing up the graphite crucible as high as I could in the rf heater coil to heat the bottom of the crucible. When the silicon was near the melting point, I came in from the top with a small resistance heater coil and I controlled its output manually. In this way I was able to grow a couple of crazy-looking crystals. There was certainly some rf coupling to the surface heater, but the basic idea worked and crystals were grown.[4]

BROCK: I see.

---

[4] Ibid.

JONES: Whether it could have been done with rf heating by some more clever way if we had thought about it more, I don't know. At the time I couldn't think of any clever way. [laughter]

BROCK: When you mentioned secrecy, keeping the grower essentially a trade secret, certainly that was keeping it secret from the outside, from the various vendors you were talking to. Was there any internal secrecy about the crystal grower?

JONES: That's a very interesting question. I don't know the answer to that. It was not something—certainly it wasn't secret in regards to Julie Blank, and Knapic, and Kleiner. It was never discussed that I remember. I remember having some discussions with Victor Grinich who had just came onboard, but he was the only person around who was an electrical engineer. I do remember having some discussions with him about control circuitry. I don't remember ever having a conversation with Shockley in which he said, "Don't tell the other guys about this." There was no attempt to have a brainstorming session with other smart people, which I think could have been very useful. I think, in general, things were very compartmentalized in the Shockley lab. For example, there was work on lithography going on which I was not privy to. I think it was only later in life that I learned what was going on there. There was a tendency to keep things very separate.

BROCK: And that—

JONES: Shockley didn't trust people. There's no question about that.

BROCK: I've heard at one point there was a tremendous electrical short that happened with the grower?

JONES: Yes?

BROCK: While talking to Julius Blank. The way he recalls it was that it was at Leo Valdes' insistence that the crystal grower have sapphire rods as a non-contaminating support for some of the resistance windings, and that they—

JONES: That's got to be wrong because Valdes wasn't there at that point.

BROCK: He left prior to that?

JONES: Yes. Whatever problem there was, it was after I left. The crystal grower was never fired up in anger while I was there. [laughter] I am sorry to hear that the sapphire rods didn't work out since I thought that was one of my best ideas to avoid contamination of the heater wire. Valdes had absolutely nothing to do with the design of any part of the puddle crystal grower, because by that time Shockley had gotten rid of him.

BROCK: Did he work on the project at all?

JONES: No. Not at all.

BROCK: Okay.

JONES: My memory is clouded by lots of things these days, but the Valdes episode was a very trying time for me personally. Jay Last and I were unencumbered because we were both single. We had no family commitments or anything of the sort. But a lot of the people like Valdes that had moved their families were under great stress. Shockley soon soured in his relationship to Valdes, and wanted to get rid of him early on and I felt I had to make the case for keeping Valdes. I got in a situation where I was being viewed as Valdes' advocate and Shockley would then direct all his venom at me to get with the drill. I was very familiar with the whole dynamic of Shockley moving Valdes out the door. Leo was never involved in the experimental puddle crystal grower.

BROCK: That's interesting. Because—

JONES: In fact, Shockley never trusted him.

BROCK: It's interesting because in the perceptions of others, several people have linked his departure with Shockley's frustration about the large crystal grower.

JONES: No.

BROCK: Okay.

JONES: No. As I mentioned before, Leo bought a lot of equipment for the conventional crystal growers, and a lot of the early equipment we had there for conventional Czochralski crystal growth was associated with Valdes. Valdes had nothing to do with puddle growth—what gives me confidence in asserting this is that Shockley viewed the experimental grower as one of our great secrets, and given the fact that almost from day one he and Valdes were at each others' throats. Shockley knew that Leo would go off to some other semiconductor company. Valdes would never have helped with this. I think Shockley worried less about me because I was a neophyte in the semiconductor industry. [laughter] I was a virgin mind whereas he certainly viewed Valdes as a competitor.

BROCK: Perhaps people are conflating Leo Valdes' efforts with conventional crystal growing with your crystal grower?

JONES: I don't know whether Leo actually ever grew a crystal with the grower he bought but he bought most of the machinery. I got saddled with crystal growing after Valdes left and I grew a lot of crystals. I guess it was appropriate for me to take over crystal growing since I was doing this other experimental project.

BROCK: Using a conventional crystal grower?

JONES: We had a number of famous accidents. [laughter] We once had a show and tell for local industry (including several local CEOs), and, as part of the display, I was growing crystals with arsenic which produces arsine gas within the grower's quartz envelope. The quartz envelope fractured and I had to delicately get everyone out of the old garage (of course, there was no safety ventilation facilities). [laughter] I didn't tell them they were all about to get arsine poisoning. [laughter] My technician and I were sick for a few days, but no one else complained of any problem.

BROCK: Who was your technician?

JONES: I've actually forgotten his name. He was a nice kid who had come from one of the community colleges. I've forgotten his name.

BROCK: Was the Blank-Kleiner combination involved in helping to make not only the large crystal grower but the conventional crystal growers as well?

JONES: I think later on they did. They took over when I left—but I don't remember that transition. Until I left, I was pretty much doing all the crystals.

BROCK: But the large crystal grower was not operated until after you left?

JONES: That's correct.

BROCK: Okay. One thing that I have personally puzzled over is Sheldon Roberts in this period. Here's a metallurgist, somebody who grew crystals, or had experience with crystals, but he seemed to not to be in the center of the crystal growing activity.

JONES: That's correct.

BROCK: Do you have any thoughts about why?

JONES: He was in the middle of the diffusion technology.

BROCK: Yeah.

JONES: Again I think this was a Shockley separation of things, because I'm pretty sure that Sheldon knew what we were doing. I've never had that conversation with him, but I think he thought it was a crackpot idea. He was not working on crystal growing though.

BROCK: Right. In talking with him he said that he really didn't know what was going on.

JONES: Yeah.

BROCK: Apparently he had no real opinion about it because he was never briefed about it.

JONES: I'm sure we had many conversations about the conventional growers, because that was a well-known process. As I said this garage was a funny place because it was this gigantic space and whenever you grew a crystal there was a gigantic light over in the corner and people would come over and say, “Hey.” It was very communal. [laughter] There was no secrecy about the conventional crystal growing, and it got to a point where it became standardized and the technician ran it. By the time I left, it was a growing business (to make a poor pun). There was nothing mysterious because it was such a well-known technique. After Valdes left I stepped in, and within a day or so I was able to master the techniques. I'm pretty sure I put together most of the apparatus. Leo arrived from Bell Labs via a diode fabrication facility in Southern California and brought a lot of ordering information. Bill Happ also brought ordering specs from previous employments. In particular, he arrived with ordering information on some of the first diffusion furnaces from his employment at Raytheon.

BROCK: Their prior vendors?

JONES: Right. I remember Valdes had set up some crystal growing facilities at a diode maker in Southern California. When we were in the store front he was ordering up materials, rf generators and things like that. All that stuff was in place when I took over. I really don't remember whether Valdes had grown crystals or not. He certainly bought all the equipment and raw materials. It was not a great stretch for me to make things work.

BROCK: Could we talk then about—

JONES: I had a lot of experience in gas-handling systems when I was a graduate student because I had done gaseous research. An important part of the conventional grower was a gas-handling system.

BROCK: When did you start to think "This is not where I want to be"? Was it before the crystal grower was complete?

JONES: No. The crystal grower certainly was worrisome, but by that time I was moving on to other things. I was actually beginning to do some device work. The crystal grower was taken over by Knapic, Julie Blank, and Kleiner. I was certainly involved in it but, it did become this technical monster.

Why did I want to go? Well, Shockley had made a promise to me when I came onboard. I said, "I've got this wonderful job at Bell Labs and you're asking me to come into this uncertain lab." Shockley promised me, "If you come on board I will make every effort to see that you go on to do what you want to do if it doesn't work out." I must say he was absolutely true to his commitment. I went to him after much soul searching. I realized that I was not doing things that I wanted to do and I wasn't enjoying my job. I certainly didn't feel I was moving forward scientifically. Part of it was the fact that this kluge was taking an enormous amount of time, and another part of it was the fact that Shockley involved me in nearly every management flip-flop. There's a dynamic that Shockley used throughout his career. He identified "promising" young people as sort of surrogate sons. I've shared my perception of this dynamic with some earlier surrogate sons of Shockley. Both Morrie Tanenbaum and Galt [John Galt] at Bell Labs seem to have been such surrogate sons. Shockley identified young people that he made confidants. I certainly was a confidant on certain aspects of the early Shockley management problems. I was fairly frank with him when I said, "I don't feel I am moving scientifically and I feel bogged down." Of course, the thing he was very concerned about was that I would go off to another job in the semiconductor industry. I had already anticipated that and I felt that the best thing for me to do was to take an academic job as a sabbatical to depressurize. Shockley lab was very high pressure and I felt that I needed to go to, what I then perceived, as a quieter environment. [laughter]

I went to Shockley and when he raised this issue about, "Are you going to become a competitor?" I made it very clear to him that I preferred to go into an academic place to decompress. With that, he wrote a letter to a number of people extolling my virtues. It was a peculiar letter because it said, for example, I ran the Safety Committee, which most academic places wouldn't be very interested in. But at any rate, he wrote to quite a number of his National Academy-type associates and I was well-received in the job search. I went off and talked to a lot of people. Shockley viewed my leaving as an organizational problem, so he convened a general meeting of everybody and told them what I was doing and he read the letter that he was sending out to people. That was an attempt to get over it. Meanwhile, I was approached by Beckman [Arnold Beckman], because Shockley had sold me big to him.

BROCK: By Arnold Beckman?

JONES: Yeah. Beckman was concerned that I was leaving and that the lab was falling apart. Beckman approached me to go on his staff as a liaison.

BROCK: Interesting.

JONES: Thankfully I did not bite on that because that would have been—I can't imagine anything closer to a living hell. [laughter] That would have been a living hell. [laughter] I politely declined. But, I think it was a measure of the fact that Beckman was getting a sudden panic attack.

BROCK: The panic was about—

JONES: He had told his backers that Shockley was this great investment —that Shockley was going to do a lot of good for Beckman Industries, and mine wasn't the only story he was hearing about people leaving. Shockley told him that I was leaving and that certainly triggered his whole response.

BROCK: Had Beckman heard, for example, that Valdes had been fired?

JONES: I don't know about that. Probably. I would be surprised if he hadn't.

BROCK: A person leaving to return to academia does not necessarily seem a cause for panic for somebody like Arnold Beckman or an indication that the place is falling apart. I'm thinking about what else is in the context, if that's the period of time when some of the other folks were calling up Arnold Beckman?

JONES: No, I am pretty sure that my leaving was several months before others contacted Beckman. First of all, I'm sure you do realize that Shockley had this reputation elsewhere of being a madman. Certainly Beckman had to cope with that. He certainly was aware of that from the very beginning. To whatever people he had to account to, he had to be defensive about this because, as I later learned, a lot of people in the financial community felt that Shockley was an unstable personality. I'm sure that Beckman, as it came out in Leslie Berlin's book [*The Man Behind the Microchip: Robert Noyce and the Invention of Silicon Valley*], Beckman went through two or three gyrations about backing Shockley, because it was a complex issue for him. Here's a tremendously successful businessman and this marked probably his greatest failure.

The reason I figured into this was the fact that there were two or three people that Shockley viewed as prizes and whom he talked up all the time. Again, this sounds terribly self-serving, but he had used me as an example that, "Here was a guy from Berkeley, an outstanding place, and he was going to Bell Labs and he made a pact to come work for me." Bob Noyce was also somebody he talked about in this context. I'm sure there were others. In terms of his success, at that point, it was about recruitment because there was no device coming out the door. Recruitment was a measure of how successful he was. If one of the guys that was sold big to your financier was taking off, Shockley wanted Beckman to hear that I was leaving from him, not from somewhere else.

BROCK: Okay. I see.

JONES: I hate these statements because they sound like I'm saying that I was an important cog in the whole enterprise, which is certainly not true, but in terms of the game that Shockley was playing, I was valuable.

BROCK: It's very interesting, your comment about Shockley's prior reputation at Bell Labs and Arnold Beckman having to contend with that fact in establishing Shockley Semiconductor. I'm wondering if that informed Beckman's actions later on when he was facing the larger rebellion, and his eventual decision to back Shockley? It's almost like a face-saving technique.

JONES: I think that's definitely plausible —I'm sure you know this history better than I do. I think Beckman did appoint some people in the role that he had discussed with me?

BROCK: Yes. He tried. [laugher]

JONES: Later, I think he took somebody from the Berkeley Division, and put him into that role, which I don't think was a very comfortable role to be in. The point was that he had backed Shockley based on—Shockley was a charismatic personality and a Nobel Prize winner, and there aren't very many guys as smart as Shockley trying to set up a business. I think Beckman had a hunch. He believed his hunch and the hunch turned out to be only semi-wrong. I mentioned before this group [the founders of Fairchild Semiconductor] did turn out to revolutionize the silicon industry. But Shockley couldn't do it. Beckman's decision was not that bad of a decision, it was just that he didn't understand the instability of Shockley. I think there were many factors. Terman was backing this to the hilt. Terman always had this thing about building up industries near Stanford. I'm sure, to some extent, he was a very strong inertial force in keeping everybody straight with this. But again, this is way over my pay grade. [laughter] I'm just hypothesizing here.

BROCK: Another issue that I wanted to talk about was during your tenure at Shockley Semiconductor. A number of people that I've talked to have described a dynamic that was difficult to contend with—Shockley advocating to get quickly going on new ideas. But this quickness tended to entail a constantly changing agenda or priority list, because he would be seized with a new idea or a new direction, and a lot of energy would go toward that, and then there would be a new priority. Was that something you encountered?

JONES: I think I alluded to that earlier. It wasn't just product instability in that regard, but it was goals. One minute it was to publish a really good paper on silicon lifetimes and the next minute it was to get a device out the door. Again, given the sort of compartmentalization of the lab, I think it was also a particular problem because individuals would hear that Shockley was writing a paper with somebody and if that individual was slogging away trying to get the operation closer to business goals there was a bit of jealousy.

I think there was a very complicated dynamics going on with the ambiguity of our mission. That was a very central part of it. The four-layer diode versus the bipolar transistor was certainly one element of that. Different ideas on crystal growth were another area of contention. Shockley was working with Sheldon Roberts on other possibilities of growing from other kinds of melts, but I didn't know anything about it. It was very compartmentalized. Was it a business issue or was it a scientific issue that we were dealing with? I think Shockley pursued compartmentalization in the extreme. One of the first things I remember when I arrived at Harvard was another peculiar Shockley incident. I had decided to go to Harvard because of John Van Vleck, who was always one of my scientific heroes. Shockley had written a particularly nice letter to Van Vleck on my behalf and so that was a great inducement for me. Van Vleck had refereed a Shockley paper soon after I got there in which Shockley had made some mistakes on fundamental magnetism. [laughter] It became really a nasty affair because Shockley didn't like the fact that his paper was criticized. Here was after all a Nobel Prize winner and *Physical Review* wasn't going to publish the paper. Van Vleck got very nervous because Shockley took such an angry tone about it. The question is, why was Shockley writing a paper on subtle aspect of magnetism when he was in the middle of all this other stuff? It was just crazy. Shockley knew a lot about magnetism, but that was not what he should have been doing when his whole empire was falling apart.

BROCK: In terms of that compartmentalization, was the reporting structure of the laboratory, and the communication structure, a hub with Shockley at the center and everyone reporting right into him? Was the communication largely individual between Shockley and whomever?

JONES: Yes, to a very large extent. In later times I think it changed because Smoot Horsley was put nominally into the second in command position, but that was only in title. The two senior officers, if you will, were Smoot Horsley and Knapic. Knapic was an administrative CEO [chief executive officer]—he was in charge of administration. Horsley was nominally everybody's boss, but that was not the fact.

BROCK: Horsley is somewhat of an enigma to me. [laughter] In talking to any number of people, his name comes up but very few people have a recollection of the nature of his contribution, what he was doing, where he came from and where he went. Do you have any insight on it?

JONES: I once did a Web search and I found absolutely nothing —of course you get the Smoot-Hawley Tariff and Horsley was a relative of the Smoot in the Smoot-Hawley Tariff. More recently, I have found Smoot Horsley's name on a number of papers on explosive technology. No, it was a great mystery why he was there in the first place. As I mentioned to you, when I first showed up in Palo Alto he was nominally the senior guy, and I would go to him and say, "What do you think about this?" and there was no feedback. [laughter] He would smile and look rather important, but that was no response. I think your comment is a safe one, that he's very mysterious. I don't think anybody knew anything about him, and I don't know where he went. I think he may have come from a military testing laboratory—and, thus, he may have had some connection with Shockley's involvement in proximity fuse development. Smoot may have come through that path. I don't really know.

BROCK: Right. Let's talk about your impressions of and experiences of what some of the other people in the laboratory were doing during your tenure there. We talked about the informal seminar that you, Jay Last and Gordon Moore held. Perhaps we could just talk about some of the other figures and your impressions of what they were working on and the contributions that they were making.

JONES: Yeah. Certainly they're all people that I have—well not all of the people—but the ones you've mentioned are people I have considerable respect for. I really was not very aware of what they were doing. Again it was compartmentalized. Even Jay Last, we were very close, but on the other hand I was not really sure what he was doing. I don't think he knew what I was doing. In a way Shockley was very successful in making us isolated. Part of this also was the fact that we were all under tremendous stress. It was a very stressful environment, even before all the breakup stuff. I was only vaguely aware of what Noyce was working on. The one interesting thing, which I think came out in Berlin's book, was the fact that Noyce did invent the tunnel diode in this time frame, and I think I was the guy who signed his lab book on that one. Noyce was a very smart guy but he was also one of the great enigmas. I think Leslie Berlin said that "Everybody who was interviewed about Bob Noyce said they didn't know him." We all knew him as a very smart guy, but I'm not even sure I knew what he was doing. I knew he was working on some kind of lithography but I didn't know what that was about. It was a very remote environment in that regard.

In some respects that was one of the reasons that I felt I had to get out of there because it was not interactive—other than with Shockley, who was of course extremely smart and a very provocative guy to deal with, that was a very rich experience. For me, the most valuable period was early days before things got complicated. In the earlier time, when I was dealing with Shockley all the time it was tense but very challenging, in a good scientific way. As he got mired deeper and deeper into various things, it became clear that I didn't have that scientific stimulation. One of the people you haven't mentioned was Jean Hoerni. You must have talked to Jay Last about him, because they were actually quite close later on. When I was with Shockley he was beating Jean Hoerni apart. Hoerni was a brilliant guy but Shockley was beating him up. Jean Hoerni was a very smart guy and certainly has had a tremendous impact on semiconductor technology. I can talk about people's personalities but I really can't talk about their work.

BROCK: We talked about how the diffusion technology was really a centerpiece of Shockley's conception for the future of the technology and for his operation.

JONES: Correct.

BROCK: Is it correct that Gordon Moore was focused on diffusion work during the time that you were there?

JONES: A lot of people were.

BROCK: Yeah.

JONES: Certainly Sheldon Roberts was working on that extensively. We are talking about a time before computers and Jean Hoerni was an extremely useful theorist. [laughter] He went off someplace and would calculate diffusion profiles for multiple elements. Jean Hoerni really worked through all of the theoretical models of diffusion. Then there were a variety of people who were doing the diffusions, building diffusion furnaces, and, then, there was all the business of characterizing the diffused materials. The standard characterization of those days was to grind the wafers into a wedge-shaped element and then to do four-point probe scanning of the resistance profiles to evaluate the diffusion models. There was a lot of core development of diffusion technology. I was aware that Sheldon was doing it. I was aware that Gordon Moore was doing it, but above all I was most conscious of the fact that Jean Hoerni was doing all the theoretical work. It was a core central element of the laboratories work. In fact, even in the close proximity of the putty knife factory, the diffusion stuff was in the middle of the room, as I recall.

BROCK: Okay. I was wondering if there was any connection, later in this story, after your departure, in the coalescence of the group of dissatisfied folks, Gordon Moore took on the spokesman role for that group in the discussions with Arnold Beckman. Was there any connection between him working on diffusion, which was at the center of things, and that role?

JONES: No. I don't think it's, I think it's—

BROCK: Just personality?

JONES: As you probably know he's a very—I think of the modern word “centered.” He's a centered personality and I am sure people trusted him. I can’t imagine him doing anything dishonorable or compromising his integrity, and I think that comes through when people talk to him. He is a centered individual so I think that if people were going to trust somebody I think their first choice would be him.

BROCK: Did you follow, at all, the fate of your crystal grower or of Shockley Semiconductor closely after you had gone to Harvard?

JONES: Not much. No. Jay Last and I corresponded and I was aware of various famous incidents, the thumbtack story for instance. I was aware of an ongoing story of problems. I was aware when rebels left and I had some thoughts about joining them, but I was where I wanted to be at that point. After Fairchild [Fairchild Semiconductor Corporation] was in operation I remember taking one trip out there to renew ties with old friends, but I had really gone in a different direction. I really didn't have much contact with any of these people over the years. Noyce once was on the visiting committee to of my department at Harvard, but it didn't work out very well for a variety of complicated reasons. [laughter] I was, of course, hoping he would give some money to Harvard but that didn't work out. [laughter] I really did lose track. In fact, it's only through Leslie Berlin's book that I've renewed contact with some of these people.

BROCK: Well, that's good.

JONES: Particularly Jay Last.

BROCK: Let's discuss your experience of participating in this early unfolding of semiconductor technology, the semiconductor industry, and what effects that had on the shape of your subsequent research career at Harvard? Is there a connection between the two?

JONES: Not really. Not in any substantive way. I have viewed the Shockley experience as more of a laboratory in human behavior. I mean that quite seriously. It was a hyper-stress environment and I think I learned a lot of lessons about how people perform in hyper-stress environments. I certainly learned how not to manage people. As I mentioned before I think that Shockley was a reservoir of wrong lessons in management. [laughter] I would say the lessons that I took away tended to be things about human behavior—I think I grew up there. I was a kid from East Oakland and I didn't have much experience with life, except for life on the streets, and Shockley was a very mind-expanding experience in a bunch of dimensions.

I really went back to resonance and magnetic-oriented problems when I came to Harvard. Partly that was because I was truly inspired by Van Vleck. Van Vleck was a towering presence at Harvard. He founded and led for several years the division within which I have found a comfortable haven for nearly fifty years. From the beginning, this division has been committed to the importance of teaching applied physics and, more generally, applied science within the context of a liberal arts college. I first encountered Van Vleck's brilliance when as a graduate student I did a whole series of seminars on his work on magnetic resonance oriented physics, and he certainly was the most brilliant and humane man I have ever known. I feel very fortunate to have had the experience of working, within the span of a few years, with two of the most brilliant scientists in American history, Van Vleck and Shockley were a contrast in personalities for sure. [laughter] I prefer the Van Vleck model. [laughter]

My life got very busy at Harvard so I really didn't look back much at the Shockley experience. One of my close cronies at Harvard always teases me about the Shockley experience. For a number of years we have jointly taught a course in design within which we have tried to talk about how the real technological world works and how to function in that world as a scientist and engineer. One of my colleagues' major routines was to make fun of me as the only guy in the Shockley enterprise who didn't become a multimillionaire, and that the students shouldn't listen to me because in my life I taught the wrong lesson to learn. [laughter]

One of the disturbing things as time went along was how bad my relationship with Shockley deteriorated. Since I had not gone back on my word, I left Shockley on a pretty positive note and for a year or so afterwards we'd have dinner together when he came through Cambridge. But, he eventually came to view me as a traitor too. That was quite disturbing.

BROCK: After?

JONES: After a year or so had gone by. I'm only mentioned once in Leslie Berlin's book, but her words have a peculiar import when she writes, "…and then Jones left" just before she describes how things fell apart. In some ways it did start going downhill rather rapidly after I left. Obviously I had nothing to do with that, but in some ways I think in Shockley's mind it may have played some role. The last time I saw him he was very cool and I heard from many people that he said some very unflattering things about my leaving. I found that disturbing because I had tried to leave the lab on good terms and not to have him fearing that I was going to reveal secrets to anybody. I was approached by quite a number of people at that time to come to work for them. There was a sense that Shockley was doing wondrous things and the idea of hiring somebody who had been on the inside was attractive to many.

BROCK: Other semiconductor manufacturers?

JONES: Exactly. I would never have done so. As I said, Shockley was very fair to me and that would be the last thing in the world I would ever have thought about doing.

BROCK: Would you care to mention who they were?

JONES: No.

BROCK: [laughter] Fair enough.

JONES: I think there was a general feeling that either Shockley was doing something very special or it was crazy. There was an aura about Shockley in the semiconductor industry. The opinions varied between those two extremes. Certainly, there were people who had less in-house talent who would be interested in building on what Shockley was doing. That, in fact, would have been smart move—again, I go back to the fact that the Fairchild success is positive proof that Shockley was on the right track.

BROCK: That's exactly right.

JONES: There was value in what was going on at Shockley's lab. It's just that his ambivalence of goal killed it.

BROCK: I know that across your work in applied physics at Harvard, electronics has been a recurring presence.

JONES: I have taught electronics for 47 years.

BROCK: Have you been surprised by the unfolding of semiconductor electronics?

JONES: Oh yes. [laughter]

BROCK: Would you care to talk about that a little bit?

JONES: The point was that here I was at one of the birthing places, and in some ways I have not built that into anything. I could say, "Gee, you know, too bad. I should have continued working in semiconductors," but I can honestly say that when I got out of the Shockley experience it was such a relief, in personal terms. I've tried to convey that it was a very hot house. It was a very high-intensity experience and I most certainly did not want to get myself involved in new stressful experiences—I think the guys who went through the Fairchild transition were very courageous. I don't think I would have had the courage to do that. I'm a much different person. I'm a passive person, and to go through that kind of singeing situation is just not for me. I certainly would be richer. [laughter] But, I'm perfectly happy the way that it went.

But, I do enjoy teaching electronics very much. However, over the years, I have spent most of my time teaching and doing research in optics and magnetism. I worked probably twenty years in various aspects of magnetism, but I did make a positive effort to switch the emphasis of my work to optics a few years ago because I found that I was able to guess the content of most new papers in magnetism. [laughter] Now that's not true. In more recent times, magnetism has boomed again and there have been some more recent exciting developments in magnetism. But, there was a point where magnetism was a little slow and optics was much more exciting. I really view my primary love in this world as electromagnetic theory which, of course, encompasses both optics and magnetism. In some ways the history and development of electromagnetism is almost orthogonal to that of semiconductors, While Shockley was director of physical research at Bell Labs, there was no work in magnetic resonance, because Shockley felt that magnetic resonance had no real world value. [laughter] The first person hired in magnetic resonance at Bell Labs was an officemate of mine at Berkeley, George Feher, and he went to Bell Labs after Shockley left there because subsequent managers said, "Gee, Bell Labs really ought to get into magnetic resonance." [laughter] Of course, if that had not happened Bell Labs would have had no role in masers and lasers. Shockley was a genius but he was a short-sighted genius. [laughter] Electromagnetic theory is the field that I've enjoyed more than semiconductors. But semiconductors are an incredible story.

BROCK: Can you think of anything that I've overlooked or is there anything that you would like to add that I could give you the opportunity to?

JONES: No. Obviously crystal growth has played a huge role in the advancement of semiconductor technology. But I've certainly addressed my view that it was a tremendous mistake to have invested so much effort in the surface-puddle crystal grower. In the rest of the semiconductor industry, crystal growth improved in small increments. People kept making larger and larger single crystal boules and life times improved as the technology built up slowly. The proposed surface-puddle technology was just too complicated. I should have been much more resistant when Shockley first proposed building this machine. But, I certainly didn't have the standing to do so—Shockley was a pretty strong personality. I really didn't stand up to him on this. Most people didn't stand up to Shockley on much. One can only makes advances by tiny steps in complex technologies.

BROCK: I see.

JONES: That's was probably the most enduring scientific lesson I learned out of the Shockley experience. I would also like to reiterate my belief that the Shockley-Tannenbaum diffused-based technology was a starting point of what I have called the "planar metaphor"—the starting point for a whole host of technical developments. If Shockley's remembered for anything and if it's true that he and Morrie Tannenbaum were the first to articulate this approach, I think they have been under appreciated. I have not researched this and I would have a hard time extending this thesis, but as a question in the history of science and technology it's an important one.

BROCK: Certainly one hears people talking about the lithography approach, in that manner. Something that's become more of a generally applicable approach for creating structure. But, it's a very intriguing idea about diffusion, in—

JONES: I think it's more than that. Shockley's dictum was that natural processes had to be the guidance as people sought means to build tiny structures. In that context starting from a plane is really what governs the whole thinking because it is the plane which gives a standard reference. Photolithography without a plane is dead in the water. Diffusion is dead in the water. Ion implantation is dead in the water. Etching is dead in the water. Everything depends upon having a well-established plane to build the structure to be formed by natural processes. All of the things we just mentioned are all controlled by the parameters of natural processes—time, pressure and, temperature.

BROCK: It's a very intriguing concept, so thank you for sharing it with me.

JONES: When I gave this same speech to Jay Last he looked at me and said, "Well, that probably is true, but…." I don't think he was impressed.

BROCK: It's simply the case that I had not thought about it in those terms before. I do understand that it's much more than talking about a technology that's being a general purpose technology but more of a technological mode if you will?

JONES: Yes. In more recent years it relates to integrated optics. I don't think anybody would even have a second thought about the fact that that's where to go. This is a well-developed notion, coming out of semiconductor technology and it can be carried over, but where did that come from? I think the start is in the diffused-base structures. The only way one could get at that is to probe more into the invention of the diffused-based device. I don't know that history. That's deep in Bell Labs and one would have to look there to see if they had a prior model to call upon. I think that once the metaphor was in the technical repertory the rest of integrated circuitry follows. As I'm sure you are aware, there was a lot of controversy about how much Noyce had to do with the development of the integrated circuit.

I can remember luncheons at Shockley Semiconductor with people sitting around over hamburgers and beer talking about making transistors and someone would say, "Gee, you know we could actually put a resistor and capacitor on this thing." Of course in these remarks we were thinking about building classic common emitter junction transistor circuits that had to have a coupling capacitor and a collector resistor. In these informal sessions, the integrated circuit was evolving. Once there was the notion of a planar surface sitting there and the techniques to build structures on that surface, it's almost obvious to put down multiple components and devices. None of this works without a good material. The whole lesson of semiconductors is to have good material. [laughter]

BROCK: That's true. To just connect this back once again to the crystal grower episode at Shockley, it seems like Shockley's willingness, or his push, to start this project immediately, to hold it as a very closely guarded trade secret, to invest the space, the expense, the staff effort, that really does seem to express the centrality of the materials issue?

JONES: Absolutely. Shockley was so keenly aware of how the purification of germanium had figured in the start of the whole business. Nothing was possible until we had good germanium. He was acutely aware of that and at the time that his lab started, silicon was not a great material. I don't think I would criticize the fact that Shockley wanted to solve this problem. What I would criticize is his approach to solving a very complex problem. I would contrast his approach to another important example in materials development. In the early days of semiconductor laser research, people would be able to make various three-five semiconductor combinations laze, but these lasing diodes would burn out in a few microseconds. When one observed these lasing devices under a microscope one would suddenly see a black spot appearing at the diode junction, which would migrate about for a bit and then the device would die. These early semiconductor diodes were an interesting curiosity but they were by no means anything anybody could bank on. But, in their commitment to understand the materials issues the folks at Bell Labs developed a major effort in epitaxial crystal growth—layer by layer growth of materials, until they understood the cause of the problems in terms of impurities, and eventually built devices that we now take for granted. Since the techniques of beam epitaxy are very expensive and complex once one understood what the defects were, people could go back to simpler methods and refine them. But it really took a ten-year effort to get the materials right. Now we take it for granted, but it was a triumph of materials science to be able to build reliable laser diodes. It is again, I think, an unappreciated achievement. But of course it has revolutionized technology.

BROCK: Sure. You would say the contrast approach there was very much—

JONES: To realize that number one it was a hard and difficult problem, and one had to find careful means to climb the mountain. I think eventually molecular epitaxy became that technique for three-five semiconductors, which is not a simple technique by any stretch, but it was directed at solving problems, in a systematic way, whereas our puddle-based crystal grower project was certainly not a systematic approach.

BROCK: Okay. Thank you so very much for spending so much time with me on the telephone today.

JONES: As I say, the Shockley story is a complicated one. He was probably the most complicated scientist—well I won't say "the most complicated," but certainly one of the more complicated of our time. [laughter]

BROCK: Very much so.

JONES: But he did make some contributions.

BROCK: I think that's unquestionable. Well, thanks again so very much.

JONES: It was a pleasure. Bye.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INDEX

M

N

O

P

R

S

T

U

V

W