

CHEMICAL HERITAGE FOUNDATION

ANDREW S. GROVE

Transcript of Interviews
Conducted by

Arnold Thackray and David C. Brock

at

Los Altos, California

on

14 July 2004 and 1 September 2004

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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 Chemical Heritage Foundation with respect to my participation in the audio-recorded interview conducted by Arnold Thackray and David C. Brock on 14 July and 1 September 2004. I have read the transcript supplied by Chemical Heritage Foundation.

1. The audio recording, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by 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 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 audio recording(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.
4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

Please check one:

a. _____

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.

(Signature) _____

ASG
Andrew S. Grove

(Date) _____

5/7/07

This interview has been designated as **Free Access**.

One may view, quote from, cite, or reproduce the oral history with the permission of CHF.

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

Andrew S. Grove, interview by Arnold Thackray and David C. Brock at Los Altos, California, 14 July 2004 and 1 September 2004 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0293).



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



The Chemical Heritage Foundation (CHF) serves the community of the chemical and molecular sciences, and the wider public, by treasuring the past, educating the present, and inspiring the future. CHF maintains a world-class collection of materials that document the history and heritage of the chemical and molecular sciences, technologies, and industries; encourages research in CHF collections; and carries out a program of outreach and interpretation in order to advance an understanding of the role of the chemical and molecular sciences, technologies, and industries in shaping society.

ANDREW S. GROVE

1936 Born in Budapest, Hungary on 2 September

Education

1960 B.S., chemical engineering, City College of New York
1963 Ph.D., chemical engineering, University of California, Berkeley

Professional Experience

Fairchild Semiconductor, Palo Alto, California
1963-1966 Technical Staff, Semiconductor Research Laboratory
1966-1967 Section Head of Surface and Device Physics, Semiconductor Research Lab
1967-1968 Assistant Director, Research Laboratory

Intel Corporation, Santa Clara, California
1968-1975 Vice President and Director of Operations
1974-present Director
1975-1989 Executive Vice President
1976-1987 Chief Operating Officer
1979-1987 President
1987-1998 Chief Executive Officer
1997-present Chairman of the Board

University of California, Berkeley, California
1966-1972 Lecturer, Department of Electrical Engineering and Computer Science

Stanford University, Stanford, California
1991-present Lecturer, Stanford Graduate School of Business

Honors

1960 Best Student Paper Award, American Institute of Chemical Engineers
1966 Achievement Award, Institute of Electrical and Electronics Engineers
1974 J. J. Ebers Award, Institute of Electrical and Electronics Engineers
1975 Certificate of Merit, Franklin Institute
1979 Elected member, National Academy of Engineering

1980 Townsend Harris Medal, City College of New York
1984 Hall of Fame Award, Information Industries Association
1984 Council of 100 Members, Arizona State University
1985 Honorary Doctor of Science Degree, City College of New York
1987 Ernst Weber Engineering Leadership Recognition Award, Institute of
Electrical and Electronics Engineers
1989 Honorary Doctor of Engineering Degree, Worcester Polytechnic Institute
1990 George Washington Award, American-Hungarian Foundation
1991 Leadership in Technology Management Award, Portland International
Center for Management of Engineering and Technology
1993 Citizen of the Year Award, World Forum of Silicon Valley
1993 Executive of the Year Award, University of Arizona
1993 Medal of Achievement Award, American Engineering Association
1995 Technology Award, Heinz Foundation
1995 John von Neumann Medal, American Hungarian Association
1995 Steinman Medal, City College of New York
1996 Statesman of the Year Award, Harvard Business School
1996 International Achievement Award, World Trade Club
1997 Computer Entrepreneur Award, Institute of Electrical and Electronics
Engineers
1997 Technology Leader of the Year Award, *Industry Week*
1997 Man of the Year, *Time Magazine*
1998 Distinguished Executive of the Year, Academy of Management
2000 Honorary Doctor of Laws Degree, Harvard University
2000 Medal of Honor, Institute of Electrical and Electronics Engineers
2001 Lifetime Achievement Award, Strategic Management Society

ABSTRACT

Andrew S. Grove begins the first interview session with a description of his undergraduate life at the City College of New York, where exposure to influential professors shaped his professional outlook and personal demeanor. Grove studied fluid dynamics with Andreas Acrivos at the University of California, Berkeley, publishing four papers from a doctoral thesis. Grove also studied solid state physics and became employed by Fairchild Semiconductor. Grove cites Gordon E. Moore as a decisive factor in accepting the position. Grove had a close relationship to Moore at both Fairchild and Intel Corporation. In the second interview, Grove discusses the design of Fairchild Semiconductor offices and its effect on the accessibility of higher management, as well as the work ethic of the employees. Grove was attracted to an offer from National Semiconductor but remained with Fairchild Semiconductor after being promoted by Robert N. Noyce. The combination of personalities of Fairchild Semiconductor executives contributed to its success, a pattern which emerged in Intel Corporation as well after its founding by Grove, Gordon Moore, and Robert Noyce. Grove concludes his interview with a reflection on the contributions of Moore, Noyce, and himself to the semiconductor industry.

INTERVIEWERS

Arnold Thackray is President of the Chemical Heritage Foundation. He majored in the physical sciences before turning to the history of science, receiving a Ph.D. from Cambridge University in 1966. He has held appointments at Oxford, Cambridge, Harvard, the Institute for Advanced Study, the Center for Advanced Study in the Behavioral Sciences, and the Hebrew University of Jerusalem. In 1983 he received the Dexter Award from the American Chemical Society for outstanding contributions to the history of chemistry. He served on the faculty of the University of Pennsylvania for more than a quarter of a century. There, he was the founding chairman of the Department of History and Sociology of Science, where he is the Joseph Priestley Professor Emeritus.

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

- 1 Education
City College of New York. Fluid dynamics. Undergraduate thesis. Admission to the University of California, Berkeley. Andreas Acrivos. University of California, Berkeley. Paul L. Chambré. Doctoral research. Solid state physics.
- 9 Professional Formation
Fairchild Semiconductor. Bell Telephone Laboratories. Role of Gordon E. Moore in job selection. Facilitating Gordon Moore's input at Intel Corporation. Value of fluid dynamics education to semiconductor work at Fairchild.
- 16 Career at Fairchild Semiconductor
Analysis of silicon dioxide surface for MOS devices. Value of previous experiences as background to the work at Fairchild Semiconductor. *Physics and Technology of Semiconductor Devices*. Promotion. Development of management style to avoid Fairchild Semiconductor difficulties.
- 28 Career at Intel Corporation
Concept of Intel. Robert N. Noyce's personality. Executive strategy. Gordon Moore's ideals. Gordon Moore's management style. Importance of Gordon Moore's innovation in the technology industry.
- 35 Notes
- 38 Index

INTERVIEWEE: Andrew S. Grove

INTERVIEWERS: Arnold Thackray and David C. Brock

LOCATION: Los Altos, California

DATE: 14 July 2004

THACKRAY: We'd like to begin at City College [of New York], because you've written extensively about the time before that. We're also curious about what you were doing as a student from the chemical engineering point of view. You won the AIChE [American Institute of Chemical Engineers] award for the best student paper. Is that what helped cement your interest in fluid dynamics? How did that work?

GROVE: It was amassed along the way. My interest in fluid dynamics started by my becoming acquainted with, and being employed by, professor Harvey L. List, who happened to be interested in fluidized beds. I worked part time for the [chemical engineering] department as an assistant, and some of my work had to do with Harvey List. I was fascinated by the behavior of fluidized beds. I wrote an undergraduate thesis, which was an unusual thing to do.

THACKRAY: At City College, how many of the students had those sorts of positions?

GROVE: Just me. The chemical engineering department had essentially only one job. It was a well-paying job—either \$1.69 or \$1.79 an hour, and all the other student jobs were \$1.00 an hour. That made a difference. I didn't realize going into it, however, that exposure to people like professor Schmidt [Alois X. Schmidt] and Harvey List would actually have a long-term impact on, in the first case, who I became, and in the second case, what I became. I liked unit operations, if that phrase is meaningful. It is the least chemical of chemical engineering. I liked fluidized beds and their behavior. All of this suggests fluid dynamics. Yet I'm ahead of the timeline here. I wanted to go to California and through the influence of another gentleman, Morris Kolodney, professor Kolodney, who was my faculty advisor—that part is in the book. They are both still alive. I saw [List] a number of years ago and he looked disgustingly the same. All of us show our age; but Harvey, who has got to be ten or fifteen years older than I, looked younger than me. They each had a bit of momentum imparting into the pattern. Kolodney was sending me to California; Harvey was sending me to fluid dynamics; Schmidt was turning me into an asshole. [laughter]

THACKRAY: Extend that one.

GROVE: Toughness. He legitimized a kind of brusque, no-nonsense behavior which I had no trouble adopting. Polite company frowned on those traits, but Schmidt practiced them: I thought if he can do it, I could do it too. That comes later in my career. The problem is that the interests in California and in fluid dynamics were not obviously converging. I did not realize for a while that Berkeley [University of California, Berkeley] had a fluid dynamics program in the form of Andy [Andreas] Acrivos. I wanted to go to Berkeley and I wanted to do fluid dynamics, and it was only some time into the process that I realized that Acrivos was somebody to reckon with. The family of chemical engineering and fluid dynamics is [the University of] Pittsburgh, Wisconsin [University, Madison]—I didn't want to go there—Michigan [State University], but not Berkeley. I first verified that I liked California by getting a summer job at a Quonset Hut research lab associated with Tidewater [Associated] Oil [Company] refineries in Martinez, California.

THACKRAY: I thought we'd somehow find you with a refinery, with catalytic cracking, thinking of your interest in fluidized beds.

GROVE: No, it wasn't much to do with that. It was doing whatever the job needed at ten o'clock on Wednesday morning. They told me at 9:45 on Wednesday morning what I would do, but it was in California.

THACKRAY: Whereabouts?

GROVE: Martinez. It's a sleepy little town near Concord. It's a little north—it was actually the first capital of California a hundred years ago. Concord became big and prosperous, and Martinez remained a refinery town. Tidewater was eventually acquired, but I didn't follow them. We moved out here in the summer before I graduated, between my third and fourth years at City College. My wife [Eva Kaston] and I drove fourteen thousand miles, including three thousand out, three thousand back, and eight thousand during the summer discovering California and that I wanted to live here. The next thing was getting admitted to Berkeley. Getting acquainted with Acrivos was the task of the next year. The first one happened and then I met Acrivos and he scared the shit out of me. He was very soft spoken and very—he intimidated me—esoteric. He frightened all the other students. When I said to somebody that I wanted to work with Acrivos, their response was, "My God, you're not very good in math," which I wasn't. That's how it started.

THACKRAY: Let's go back to City College for a minute. How large was the graduating class of chemical engineers?

GROVE: The total engineering class was four hundred sixty. Out of that the chemical engineering graduates must have been forty or fifty.

THACKRAY: What were the other chemical engineers doing typically?

GROVE: They went to work for DuPont [E. I. DuPont de Nemours and Company, Inc.]—unless they were Jewish, in which case they went to work for Allied Chemical [Company] or Pfizer [International]. DuPont was still reputed to be—

THACKRAY: “White shoe,” or something.

GROVE: Right. Some years later when a guy called Shapiro [Irving S. Shapiro] became the CEO [chief executive officer] I couldn’t believe my eyes.

THACKRAY: Were many going to graduate school?

GROVE: Not many, but some. It’s a very interesting question. It’s now forty-four years after the fact so that my memory may not be precise. As departmental assistants we kept references and records and statistics so I knew the files and knew the history of previous graduates pretty well. The interesting thing was that the best students didn’t necessarily go on to graduate school because the best students weren’t necessarily able to afford to go to graduate school. The City College economic experience did not extend once you moved. For most people, if you lived at home and commuted, school was practically free as compared to going to MIT [Massachusetts Institute of Technology], which was the school of choice for City College chemical engineering graduates. If you had to move someplace, you had to pay for residence and tuition. There was no correspondence to the best students going on to graduate school. If I take a stratum—in my recollection—of the best five to ten students, I’m pretty sure that half of them went on to graduate school.

In my class, after about ten years or so when I got an award from City College, a number of them turned up. One of them got a Ph.D. in chemical engineering and remained in classical petrochemical engineering. One of them did not go to graduate school and became a CIA [Central Intelligence Agency] analyst. It was a radical thing to do at the time! [laughter] I tried to move one of them out to Fairchild [Semiconductor] after I started working there, but he had family obligations. He was the second or third best student. I went on to graduate school and the next two didn’t. That’s how I remember it in my class.

THACKRAY: Was it a “no-brainer” that you would do that?

GROVE: Graduate school? No. Let me quote my witness on this one: I met my wife after my first semester at City College, and she still remembers that my original intent was to get out of school as fast as I could, go to work, make some money, bring my parents over here, and never did graduate school enter the picture. That was probably 1957. The picture changed as a result of my experience as a departmental assistant and my unit operations course.

I really liked distillation, column designs, combinations of mechanical design with fluid flow, and separation. I really warmed up to that, remember, I went into chemistry by default. I actually got interested in it but I didn't become that much of an aficionado. Then I got distracted from it by life. A couple of years later I reignited with unit operations and fluid dynamics. It was then that I realized that I'm a nothing unless I get a graduate degree. I learned that I knew very little. When did that happen? Probably the year before my last year, what you would classically call the junior year but most people, including myself, took more than four calendar years, so it doesn't quite fit.

THACKRAY: What about the financial hurdle?

GROVE: Graduate school was conditional on my getting a scholarship. My wife, who I was then married to, went through graduate school while I was finishing City College. She graduated from Columbia [University] with a master's in social work at the same time that I graduated from City College with my bachelor's degree. Her earning power was all right. She went to school while I went to City College, then she was going to work and it was with the combination of her and my scholarship that I hoped would be sufficient to live on. As verified by the summer experience, California was actually a much cheaper place than New York. We rented a cottage during that summer for a period of time—forty-five dollars a month. I mean, it wasn't much of a cottage but forty-five dollars was not a lot of money then. I thought we were going to manage. We had some history with the economics of a frugal lifestyle in California, and it turned out that way. I had a summer job the year before I graduated. I also had a summer job the year after.

THACKRAY: Where was that?

GROVE: At Standard Oil [Company] in Richmond, California.

THACKRAY: These were classic chemical engineering jobs.

GROVE: Very classic chemical engineering jobs. In fact, I designed the nuts and bolts of a distillation column. It was a feasibility study, so I didn't think it was ever going to get built. Nobody paid much attention to it, and I was bored stiff! My other summer job was in a classical chemistry lab of Stauffer Chemical [Company], also in Richmond. It was a very depressing run-down lab. One incident stuck in my mind and had a real impact on me as a future manager. People came into work on Saturday—the permanent workers not the summer students like me—and I discovered that they sat around and bull-shitted and had an eye on the window until the most senior person's car pulled out. Minutes later, next senior person pulled out; and minutes later the next one, and within ten minutes after the senior guy left the whole place was empty. [laughter]

I did not like that place, or the work either as you can tell, but that was chemistry. It was boiling liquids, glassware, and Bunsen burners. At Standard Oil, the whistle went off at five o'clock and people ran to their cars. The same thing happened at Tidewater [Associated Oil Company]. At Stauffer [Chemical Company], there was no whistle.

It was not a good scene. I had three work experiences, one worse than the other. I fought, in later years, against summer jobs at Intel [Corporation]. I was against having people come in for summer jobs because they were miserable. I never would have gone to work during the summer, and I did not want people to form a negative experience which they inevitably would have done. It's interesting to connect the dots between some of my personal experiences and the obstinate beliefs with which I operated later as a manager.

THACKRAY: How did you get summer jobs in California while you were in City College?

GROVE: By two numbers that I remember: one of them was obtained as a single offer after either seventy or seventy-six applications.

THACKRAY: Letters?

GROVE: Letters. The other one, having experience and having moved to California, I think only required seventy.

THACKRAY: Progress!

GROVE: It was a very discouraging event. Since we did not know California geography—you probably don't know it well enough to appreciate what I'm about to say—we looked at the map. My job was in Martinez; and my wife got a summer job at Sonoma State Mental Hospital. That was our first summer in California. They offered free housing for the staff, so we were going to

live in Sonoma and I would commute to Martinez. It didn't look that far on the map, maybe half an hour to forty minutes. It turned out to be an hour and a half! Before the end of the first week, we found that forty-five-dollar cottage. She quit the job that she hadn't yet started, and found a clerical job. We only had one car, and we made stupid mistakes that nobody from California would have made, so it required a fair amount of persistence.

THACKRAY: Like what?

GROVE: Like thinking that driving forty miles would take half an hour in California. Everybody knows that you can drive sixty-five miles an hour so, mathematically, forty miles is a half an hour.

THACKRAY: Thinking of the appeal of California, was it mostly Berkeley?

GROVE: It was San Francisco. Berkeley was the closest we could get to San Francisco. A friend of mine—a guy I escaped from Hungary with—went on to Stanford [University], and I came down to visit Stanford. Actually it was pretty bad. I think it made me appreciate how nice Berkeley is! [laughter] San Francisco was special. I was trying to come closer to my recollection of Budapest, and New York City was not close. That's how I began in California. I hated New York. Kolodney said, "Why don't you go to California?" I asked, "Where in California?" and he replied, "San Francisco is sort of a European city." One comment—a life changed.

THACKRAY: Did Berkeley admit you just on the basis of recommendations from your professors?

GROVE: Yes. What else could they have done?

THACKRAY: Interviewed you, perhaps?

GROVE: I visited Berkeley during the summer of 1959, but that was before I applied. Somebody showed me around, and it was wonderful, but even more from the standpoint of formally taking stock of what it would be like to go there. There was no interview. I think I applied to MIT, Berkeley, Wisconsin—some classical places. I got accepted with a scholarship to all the places I applied, yet nobody interviewed me. That's a kind of "Harvardish" thing.

THACKRAY: Where do you live? What does your wife do?

GROVE: She was a social worker, and over the period of three years that I lived in Berkeley, she worked at two counties' welfare departments. She started out in the Marin County Welfare Department and then the Solano County Adoptions Department. Later, when I graduated and we moved down to Fairchild, she worked with the Santa Clara County adoption agency. She worked until our second child was born.

THACKRAY: How was Berkeley?

GROVE: It was very interesting. Physically it was gorgeous. The courses and the students were a strange mixture. Courses were hard—it took me a while to discover that they were not inherently hard. They assumed a different terminology and training than I had. It was frightening at first. I had a big transition problem; for a while I thought I was going to flunk unless I died before I flunked out. It was not altogether different from the City College experience: It hit me like a ton of bricks, and then I figured out the MO [*modus operandi*] and survived. The same thing happened in Berkeley, and it seems to me the same thing, watching my kids going through graduate school, seems to happen when people go to a graduate school different from their undergraduate school.

The other thing that was different was the student body, the interactivity of the student body. Here is Grove sitting in the class, utterly sunk and confused with the vector notations that he's never seen in his life, and busily writing things down and trying to follow along—that was after my shock has peaked. I'm staring at the blackboard and I'm utterly confused by how line 2 follows from line 1. I wrote it down but I don't get it. A hundred, maybe eighty, kids wrote it down and yawned. I said to the professor, "I'm sorry, I don't understand how line 2 follows from line 1." The instructor turns around and stares at line 2 and line 1, grabs an eraser, erases line 2, and realizes they're completely different. [laughter] The other seventy-nine students pull out an eraser as soon as this happens, erase and rewrite it. That incident, which is not an apocryphal incident, was also a very important one, because I discovered, "These toads don't know anymore than I do. They just don't dare to speak up." At City College, it was a feisty scene; you didn't get away with anything. You better have your act together there as an instructor, because the students will nail you. If you made an error in grading somebody's paper, it will take you only two minutes into the next recess before you get pinned to the wall. Berkeley was much lower key, much calmer, much less confrontational—in class, and out of class. That was one thing, which answers your question in part. The second one was an important event in my self esteem as a student, realizing that I'm really no dumber than the rest of them.

THACKRAY: Were you saying, "Gee, I'm in the right place, doing the right thing?"

GROVE: God, yes! First of all, remember I wanted to get out and get on with life. The course requirements, after the first year, were nebulous to non-existent. I had all the liberty of picking and choosing what kind of classes to take or audit, and I took advantage of it. I took some killer courses in math. I asked Acrivos what course I should take, and he sent me to a guy called Chambré [Paul L. Chambré], who in a previous life must have been like Jean Valjean from *Les Misérables*, the guy who lied, cheated, and became a hero (1).

[END OF TAPE, SIDE 1]

THACKRAY: Please continue with your description of Chambré.

GROVE: He was a really tough, demanding math teacher; with the personal skills and personal demeanor of this character of Victor Hugo's. But other than that I really didn't have to do anything. I talked with him, Andy Acrivos, and he sent me to a colleague of his. The two of them were collaborating on a theoretical-experimental combination thesis, which took some poor guy five years to construct and show that the apparatus is capable of doing the job. It was a circulating oil tunnel. They were feeling bad about the guy—they wanted to let him go, so I had the opportunity to take over an existing experimental setup. That's a ready-to-do experiment! [laughter] I liked two things about it. First of all, it was very consistent with the type of fluid dynamics I was interested in. It was experimental without my having to exercise my ten thumbs—I'm terrible with things. I was, and continue to be. But I like experiments. I don't like tinkering—there's a difference. The guy did five years of tinkering, and the experiment wasn't exactly ready for primetime, but almost ready for primetime. I had two advisors—one on the experimental side, and one on the theoretical side. I fit in there like—click—and I had a wonderful time with it.

I wrote up a thesis that—somebody is working on a book about me—so in connection with that, I looked up my own thesis because there was something in it that I wanted to verify (2). This is going to sound awful, but it is an amazing thesis. I was directed to a problem, whose theoretical solution goes back to [Gustav R.] Kirchhoff. The references are 1920 and 1897—things of that sort. I went in the face of prevailing dogma on the basis of my experiment and proposed a “Gordian knot” kind of solution that was completely against the classical beliefs. I had the guts to understand the experiments, what the experiment was saying. I had a Ph.D. advisor who, after a fair amount of due diligence, believed my data. Footnote: he moved to Stanford; negotiated to move the equipment over to Stanford; and put another student on there whose job was to redo my entire thesis, data point by data point. I didn't know that at the time, and he didn't know that he would have to do that. By the way, I passed. That helped me write a pretty important experimental paper; it led me, as first author, to elaborate the theoretical interpretation of what I saw (3).

It's all right for me to say that, because I'm talking about somebody who is more than forty years younger than me. There was an interesting intellectual courage. I'm impressed by that, I'm older than his father would be by quite a bit, by the person who did that. I'm sorry but it sounds awful to me anyway. I had a very good time. I was very proud. I wrote four papers (4). They published four papers out of my thesis—two of them in the *Journal of Fluid Mechanics*. Cambridge University-edited. The leading journal.

THACKRAY: Why not go into academe?

GROVE: It did not appeal to me at all. Remember I still had my parents, whose support I was responsible for. I wanted to do something useful. A Ph.D. thesis is the training of a researcher. I did something useful, from a practical standpoint. Lockheed Missile and Space Systems chased me, including coming to a ten-minute talk I gave in Chicago in the field. That was a hush-hush, research organization that was, I inferred, looking at the signature of the wakes behind submarines. In my thesis, there's one place where I mention some obscure guy called [Anatol] Roshko, a 1920 paper. Somebody from Lockheed called and said, "Roshko said such and such, not such and such." So there was somebody actually working on it, but I did not want to go there—that would have been a logical place for me to go to work.

THACKRAY: Are you saying you didn't want to go to Chicago?

GROVE: No, Lockheed was in Palo Alto. I did not want go into defense, military armament kind of work. I didn't want fences and security and this and that. Plus, I barely became eligible—you had to be a citizen. I was a citizen by that time, but barely.

THACKRAY: What were the options?

GROVE: Remember the comment—"If you don't like New York, you can try California." One of two professors that I had at Berkeley made an equally well thought-out comment, "What do you think you want to do?" I told him the story, "I don't want to work at Lockheed, dah, dah, dah." He suggested, "Why don't you go work in solid state—all the math that you learned under Andy Acrivos is applicable to solid state physics." I replied, "All right." I looked solid state physics up in the catalog. There was a course by a Professor [Shyh] Wang on solid state electronics that was about to start; so I showed up there and asked, "May I audit your course?" Dr. Wang said, "Sure." I audited the course. Whoever made that comment was right. I could keep up almost all the way without much effort. A diffusion equation is a diffusion equation. But everybody else in that class had a hard time of it. I basically was fluent in a like physical phenomena—I liked touching onto the behavior of the transistor, which was how it went from quantum theory to elementary transistor behavior. I began name dropping—slash—

dangerously, superficially ignorant in solid state electronics just in time for the interview season to start. What I did is the moral equivalent of applying at seventy places. I ended up having twenty-two interviews.

THACKRAY: How did you meet the people you interviewed with?

GROVE: Either on campus or I wrote letters to the people who didn't come. Half of them were from semiconductor firms; the other half were from businesses that I actually was educationally qualified for—like Lockheed. It was quite wise. The record looked like this—Fairchild rejected me once. Promptly, somebody else from Fairchild invited me to visit. Personnel organizations are notorious. I almost didn't go to visit them because I was so pissed off about the condescending rejection I got at the same time. Motorola [Inc.], I think made an offer to me. Texas Instruments [Inc.] did not make an offer to me. Somebody called Pacific Semiconductors [Inc.], with a long history, made an offer to me. Bell [Telephone Laboratories, Inc.] pursued me, and it ended up Fairchild versus Bell Labs [Laboratories]. Bell Labs was the place at that time, and a suitable department there made me an offer that I could come in and not decide on what job I wanted to have until after nine months. They were very nice to me. G.E. [General Electric Company] Schenectady Research Labs phoned me and asserted, "Why would we waste our time with you? If we wanted somebody with solid state experience we would go and get one of [Charles] Kittel's boys!" I was not their kind of guy.

THACKRAY: What was your answer?

GROVE: I responded with, "I have taken this course, and that course." He replied, "Blah, blah, blah. Next!" [laughter] I had offers, but by no means a clear sweep. My background was not right for the job. My personality, you either like or hate, so that wasn't a currency either. On the other hand, I think I did better. I remember DuPont pursuing me very aggressively. They didn't even ask me my religious preferences by then—that was three years later, in the 1960s.

THACKRAY: What would you have done at DuPont?

GROVE: Chemical engineering.

THACKRAY: You wouldn't have worked in solid state?

GROVE: No. These are companies such as Lockheed and DuPont.

THACKRAY: But classic, the Exxon's [Mobil Corporation] and Dow's [Chemical Company] as it were.

GROVE: Right. Exxon is a little anachronistic. Esso [Oil Inc.] may have been one of them.

THACKRAY: You had some offers on each side of the street.

GROVE: Yes. Once I had offers from semiconductor industries, I concentrated on those, and very rapidly reduced them to what I should accept and what I wanted to accept.

THACKRAY: Was staying in California big, or not?

GROVE: I loved it here. I didn't quite love it in Palo Alto—the sleepy backwater and everything—but second best to Berkeley, certainly as compared to Morristown. [laughter] Back to New York! But were it not for the geography and were it not for Gordon [E. Moore], I might have gone to Bell Labs. I liked the people at Bell Labs. I didn't like them like I liked Gordon. I really liked my first impression of Gordon. Those were the two forces.

THACKRAY: What was the first Fairchild sequence? Can you talk about rejection at Fairchild?

GROVE: I wrote a letter applying to the personnel manager, whose first name I don't remember. His last name was Palmer [Robert B. Palmer]. I later on met him and it was dislike at first sight. He told me in a letter back, "We like our boys to come back closer to graduation. Go back to your courses and give us a call later." Literally within days I got a letter from the head of the chemistry section at Fairchild R&D [research and development]. He said, "I was visiting the Berkeley campus recently and your name came up in my discussions with various faculty. It sounds like you are a perfect fit for the kind of thing we are interested in. Could we persuade you to pay us a visit some afternoon?"

THACKRAY: Were letters like that also coming from, say, Bell Labs or one of these other places?

GROVE: Bell Labs didn't write me letters. Bell Labs sent people to my lab—senior people visiting me in my lab. That was a class act! I could see the beginning of kind of the decline of Bell Labs, but you could see why it was great for many years. They cultivated people for a long time. They cultivated faculty, so they'd pinpoint the Andy [Andrew S.] Groves or other promising people. Bell Labs stands out. Fairchild stands out.

THACKRAY: Who else?

GROVE: Lockheed stands out. At Lockheed, the guy pursued me. On the other hand, G.E. stands out and I can't get over the fact that the guy rejected me. Here's a funny story really. I had actually a good interview—I went out to TI [Texas Instruments, Inc.], their Dallas location, and I subsequently got to know two of the people who interviewed me. I couldn't figure out why they didn't hire me. Something wasn't right. My impression of the interview was such that I felt pretty good about it; I was going to get an offer. Not that I was that keen on that. That sort of thing didn't matter after a few of them. The interviews were pretty much the same as they are now. They flew me out to wherever, made me do a seminar on my thesis that most people didn't care about. They talked about what they do, showed me around. Then started selling—gave me dinner, put me on a plane, and thanked me.

THACKRAY: Did you do sort of one-on-one interviews with a number of people?

GROVE: I had a one-on-one interview that I remember with Gordon. I had a one-on-one interview with Jim [James M.] Early, who was the head of the section at Bell Labs. He subsequently came out here to run Fairchild R&D. He didn't do terribly well, but at Bell Labs he was highly regarded.

THACKRAY: Stay with Bell Labs and that one-on-one—was it because Early had explicit confidence in relation to your thesis? Was it because he was saying, "This is a hot property and I want to check it out"?

GROVE: It was like this: whoever was my contact person spent a lot of time with me, telling me, "You are really very good and you're really very interesting and we're very interested in hiring you." [Early] was in selling mode, "What can we do to make sure?" I told him, money is a problem. He upped my starting salary by one hundred dollars. I told him I wasn't sure whether I was interested in the chemistry part or the electronic part. He concocted an offer where I could do both of them in some period of time. Jim Early was in a selling mode by the time I got to him. Gordon Moore asked me about my thesis, all on his own, and listened, and got it! I had given many talks about my thesis; of course I spent a year and a half living it. Basically, he could have asked me for a thirty-nine second version and I would have delivered a

pretty decent thirty-nine second; give me a three-hour version, I would have done that. I don't remember him specifically asking me to do it in ten minutes, but he absolutely got the significance of what I did, and was very interested, and had good questions. I just came away very impressed with that young guy, in his thirties, running a good looking research lab; he had pretty good people there, and it was in California. He's really a smart guy—very personable, no airs. Gordon was a big selling factor, helping me see what I wanted to be.

THACKRAY: The Bell Labs offer was on the table at that stage?

GROVE: Yes. If it wasn't I knew it was coming also.

THACKRAY: Reputationally, if you'll hold it right there and talk Bell Labs—Fairchild, what do you say at that moment?

GROVE: If you are a microprocessor designer in college, you want to work at Intel. They could get anyone in that kind of activity because that's the place to be, that's the place that you want on your resume, that's the place for your formative years. If you're in solid state chemistry, solid state physics, solid state electronics, Bell Labs is the place. It's 1963—the transistor was invented fifteen years earlier—so the integrated circuit has just about been invented. Nobody knew how significant it was. In fact, Fairchild just shortly became the place to be because of integrated circuits. But it wasn't in 1962 when I was doing interviews. Bell Labs really had the reputation of being the place. My experience was very favorable, except for where it was, and except that Jim Early was no Gordon Moore. He ended up with whatever was left of Gordon Moore's job. That's interesting. The only thing that is even more interesting is calling on the customers in Silicon Valley—a salesman your age takes me into a building—and I tell him, that is where Tandem [Computers, Inc.] was before Compaq [Computer Corporation] took it over. Gordon and I can recite history of a thread of Silicon Valley. That was UNIVAC [Universal Automatic Computer] before it was Unisys [Corporation]. And the young man with me is rolling his eyes, "Let me tell you about our latest product, sweetheart." As we say, history is important but nobody cares about it.

THACKRAY: What percentage was it Gordon Moore and what percentage was it California?

GROVE: California I think was the important point. California dominated. But if it was a miserable experience, I had a wonderful alternative. It's interesting that later on when I did the MOS [metal oxide semiconductor] work at Fairchild, I became friends with the crew at Bell Labs that was doing similar work. They all knew that Bell Labs had tried to get me. Their prominence lasted for a while and then Bell Labs slipped away from the scene.

THACKRAY: Was there any other California offer?

GROVE: I want to say that PSI, Pacific Semiconductors [Inc.], gave me an offer, but I'm not sure. That was in Los Angeles.

THACKRAY: All right.

GROVE: Was there any other California offer, before Fairchild?

THACKRAY: Yes, that's what I was wondering—there's not much other option.

GROVE: The other companies wouldn't have known what to do with a Ph.D. Their research consisted of going to the Wagon Wheel [Pub] and sitting down and listening to what the Fairchild engineers were talking about.

THACKRAY: The classic chemical was really out of the window by this time.

GROVE: Once I had an offer from the "left column," it was—but not until then. I think I would have been horrified. There's a box where I keep careful notes of everybody I saw, and I wanted to dig out my notes about Gordon.

THACKRAY: Do you think you can find them?

GROVE: I know I didn't throw them out, but I couldn't find them. I tried once. Let me put it this way, when I work up my diligence and look again, and if I find it, I'll send you a copy. It was a very impressive interview.

THACKRAY: It is now an historical document, so we should archive it with due ceremony.

GROVE: It is quite amazing. I ferreted away a stack of pieces of paper like that and just last week I was deposed in a suit involving the creation of the Intel name, which was almost thirty-six years ago. I had a piece of paper where the various name candidates were listed in my handwriting. There were three columns of numbers—one, two, and three choices—number one,

number two, and three in each column, one in my handwriting, one in Gordon Moore's handwriting, and one in Bob [Robert N.] Noyce's handwriting. Definitely an historical document—it is likely to be the determinant in winning this lawsuit. It's so spooky that it's in Dr. Noyce's handwriting. A year ago a great friend of mine, Les [Leslie L.] Vadasz dug up the notes that he and I worked on when he explained to me the organization of the first memory device that we started to design, dated August 1968. We devised the first MOS random access memory. I have a few of them. When I showed Gordon his own handwriting, he said, "Yep, yep, yep." So I have a good sense of history.

THACKRAY: Have you made some arrangement of the disposition of your archives?

GROVE: Stanford wants to take them.

[END OF TAPE, SIDE 2]

THACKRAY: On the basis of a dozen interview sessions, I would say our experience was that Gordon Moore grew visibly more relaxed, but at the same time there's a sense in which I feel, at this moment, that perhaps I know you better than I know Gordon.

GROVE: You probably do. What I'm going to say is sort of funny, but I'm thinking about the secret of my success over the first two decades in the semiconductor industry. This is a little tongue-in-cheek. A very plausible answer would be: recognizing Gordon's facial reactions better than anybody else. [laughter]

I would be in a meeting and people would be bashing each other's heads and all of that, and I'm running the meeting and I'm perfectly in charge and all that. I look at Gordon—something is wrong. I'd yell, "Stop! Gordon, what's bothering you?" And then, "Shut up! Gordon, tell us whatever you wanted to tell us," and he did. I could do that. He usually had the right answer and the right comment, the right concern. But somebody had to stop the traffic. I don't know that I did it all the time, but I did it a lot of the time, and it saved me and Intel from a lot of problems. Now my added value in the room was not just that, but my insight about Gordon had an added value. Nobody would have had access to Gordon's insight without my recognizing that it was time to stop the bulls. I would say that he was waiting for me—and he would probably give a sheepish little smile and agree with me. I don't remember the specific time, but he said, "You know me better than my wife—well not better, but as well as."

By the way, it happened to be done in a chemical engineering department—a small technical correction.

THACKRAY: About this technical, mathematical discipline that you have. As you look back now on your career, from Berkeley to today, what were you really bringing out of that background into your career?

GROVE: A couple of things. Comfort with differential equations, which allowed me to analyze things at a simple level—not very complex. Physical problems are sometimes described in differential equations. If you think like a differential equation, you can simplify them enough to put them into a proper form of differential equation. You can find typical closed-form mathematical solutions that are pretty useful. My early papers are perfect examples of that. All the work with the *Les Misérables* character [Chambré], courses, mathematics, and digging through the papers for my thesis, strengthened me—I was pretty comfortable after that. That helped me a great deal. It helped me also, indirectly, by allowing me to learn about solid state electronics and solid state physics so that I got halfway interested in a brand new field and, without too much fuss, became conversant enough to start working in that field.

When I arrived at Fairchild on a Monday morning, my supervisor, who was an electrical engineer, spent the morning giving me the background of a problem, and left me with an assignment to do a bunch of analytical work. I could not have done that without the analytical education. It actually wasn't that complicated, but I needed to be used to taking a physical problem and turning it into an equation, solving the differential equations, doing a family of curves, looking at a particular parameter. That turned out to be my first week at Fairchild. I was given a job and by Friday I published a report on it that was the electrical analysis with which we studied the silicon dioxide surface, electrically. It turned out to be a very important development. How lucky can you get? A, that you arrive Monday morning and someone gives you the assignment and goes away; B, you are armed with enough general platform to go after a problem like that and turn it into differential equations, solve the differential equations, and blah, blah, blah. And it so happens that I had the rudimentary programming skills so I could run up enough representations of that closed-form solution on a batch process computing service. I could plot a bunch of things by the end of the week. Very few people knew how to program in FORTRAN in 1963 at a Silicon Valley commercial company. I don't need to generalize, but I knew how to do it, and it fit right in.

BROCK: Had you had to learn the programming for your thesis work?

GROVE: Yes. I don't think it made it into my thesis particularly, but remember there was a Kirchhoff kind of approach. Those are described by differential equations and therefore that approach could be analyzed. I calculated what my experimental measurements would look like if Kirchhoff's theory applied. Actually, it probably did make it into the thesis (2). Anyway, it turned out that this divergence was big enough that you could drive a truck through it—not just in minor, in degree, but in the general look of it. This, for example, is the classical solution. The experiment suggests a classical solution is one solution, but there's another solution that looks like this. I don't have this theory to calculate it exactly like this, but these are some of the

conclusions that we can draw that model, which are, every single one of them, at variance with Kirchhoff's classical solution. Yes, I did. It's very analogous to what I did with surface states. This is what classical theory would look like; this is what the experiment looked like; this is the variance. Everybody thought about surface states, and I thought "They're not surface states, they're surface charge." Nobody writes about surface charge. Everybody talks about surface states. Everybody talks about Kirchhoff. Look at the data! It says there's the surface charge.

I presented the first paper later in the summer of 1963. It was a comparison of that stuff that I calculated and the experimental data I came up with. A fixed charge instead of surface states, which sounds like a minor difference; it isn't. I got nailed by all these experts who would sooner burn witches or equally burn me at the stake—like the witches of Salem—for being a heretic.

THACKRAY: Was there a lot of serendipity in that?

GROVE: Absolutely! But I had things in my tool bag, which is what your question is. I'd done exactly that kind of thing and I had a protector against classical dogma in Andy Acrivos. I had confidence. I didn't have a protector with surface states, but I didn't need one. Would I have had the intellectual courage to step out and do that had I not had that first experience? I don't know. The first time around, it worked exactly like it was supposed to. I had an advisor who advised me, to keep me out of trouble, but said look at the data and draw your conclusions. He protected me by putting his name on the paper and it got published by people who are not prone to publishing revolutionary insights or departures. I'd done it once, then the second time around, I did it without training wheels, without an advisor. It's a classic story of how academic training should work.

THACKRAY: Yes. Gordon mentions that Acrivos wrote him a note saying, "This is a very unusual guy." Just talk there about your relationship to Acrivos. It sounds as if you were in a very special relationship with him as opposed to any other student milling around.

GROVE: No, not then. I have become, not a close friend of his, but I see him. On St. Andrew's Day we are part of the celebration in New York. He left Stanford and retired and became a professor at City College. He's just retired from there. I have had kind of a low-key friendship of forty years with him now, but it developed slowly and after I left. He was always good to me, but he is an academic absorbed in his work and not that interested in people. We didn't have that much in common.

THACKRAY: Acrivos told Gordon Moore, "Grove is a very unusual person." What made him say that?

GROVE: I don't know. When I first heard of that letter I was stunned. I was kind of like, "I didn't know you loved me." [laughter] Not literally. By the time I knew of this it was kind of stupid to ask—what did you see in me when it was not obvious. By the time I found about it I was president of Intel or something or other.

THACKRAY: Your Fairchild interaction and hiring, was that 100 percent you and Gordon? Eighty percent you and Gordon?

GROVE: Let me make sure I understand you.

THACKRAY: Who else was involved in hiring you to Fairchild?

GROVE: The head of the chemistry department, somebody whose name I want to say is [Werner] Waring. Not the head of the physics department, who was my boss, because he was half gone already before I got there.

THACKRAY: Was that Tom [Chih-Tang "Tom"] Sah?

GROVE: Right. I was amazed in reading Gordon's description of the problem of Tom Sah, because I was very angry and my colleagues were very angry. Gordon was Tom Sah's boss and Gordon always looked like he never heard us complaining. Forty years later, he has heard every word, and he just chose to detach himself from it. He never did anything about it at the time. Tom Sah ultimately quit.

I don't know what Andy [Acrivos] saw in me, but he did write that letter. Gordon read that letter. He wrote it in 1962. As I think back, I was not that good in math. He had students who were dynamite in math. I was not that good at experiments—people including the guy who built my oil tunnel were a lot better at it. But I don't think he had many students who came up with tradition-breaking observations and insights. That's what I'm impressed by about myself, and I'm assuming that's what people were impressed by, but I don't really know. Asking me at this point is meaningless. As far as the Heisenberg Uncertainty Principle—*vis à vis* history has disturbed the facts.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

INTERVIEWEE: Andrew S. Grove [Interview 2]
INTERVIEWERS: Arnold Thackray and David C. Brock
LOCATION: Los Altos, California
DATE: 1 September 2004

THACKRAY: I wonder if we could begin as you join Fairchild [Semiconductor], and if you could say something about the physical plant reality of the Fairchild you joined, and the structural reality of how many people, doing what—just to set the scene.

GROVE: [It was] a relatively modern building. It was an “H” shaped building, and between the fingers of the “H” was an outdoor patio. A very attractive California design. There was a big contrast with Bell [Telephone Laboratories, Inc.], which was a messy hundred-year-old or eighty-year-old building at the time. The labs at Fairchild were separated, but only a short distance from the offices. The offices were small, but a number of them were individual offices, which was very important to me at the time. I didn’t think much of it at the time, but later on I puzzled over it: Gordon [E. Moore] and his associate director at the time—a guy called Vic [Victor H.] Grinich—had an office suite off the entrance, physically separate from the rest of the lab. You had to go through a complex of secretaries and administrative offices to get there. It was very isolated. At my arrival, I wasn’t very savvy about things like that. I had tried to drop in a few times to Gordon’s office, and it was painful to get there, and I realized it was a very undesirable setup. Four years later I was appointed assistant director, I was sort of the successor to Vic Grinich. When I became assistant director, I chose not to go to that wing and have Vic Grinich’s office, but decided to stay near where I was, which was at the crossroad between the cafeteria and the bathrooms—one of the intersections of the “H”—to make it very easy for people to come by and stick their heads into the office.

THACKRAY: What was the office size? Was it big or cozy?

GROVE: I would say fifty thousand square feet, but at the time I knew the building, I wasn’t very good with square footage. People started work relatively late. I was not aware of it at the time but with an Intel [Corporation] eye, looking back, it was a very casual work environment. People also left relatively early by Intel standards. It was a bit of a country club atmosphere—again I didn’t realize it at the time, but looking back later, it was that. We had very good people—just no pressure.

THACKRAY: Because they owned the territory? Why no pressure?

GROVE: I guess no pressure because very little was expected in terms of tangible output. I think we talked about the difficulty of transferring technology to the manufacturing world. For all practical purposes, everybody involved had given up on that. They just did their stuff, wrote their paper—it was almost an academic environment. It was very rare to see people from [Fairchild Semiconductor] Mountain View [manufacturing facility]—the manufacturing arm—roam the halls of the company or come to meetings. It was an entity left to itself. You asked for my first impression—I’m giving you my reflections two years later. I didn’t know any of the manufacturing people because I never saw them, until that became my self-appointed mission, which was in 1967 or so.

THACKRAY: You’re entering the physics department. What does that consist of—number of people?

GROVE: Twenty. Twenty-ish.

THACKRAY: Twenty-ish Ph.D.’s?

GROVE: Half Ph.D.’s and half non-Ph.D.’s. Led by a guy that we talked about, Tom [Chih-Tang “Tom”] Sah, who was there half the time at best, doing his own work behind closed doors much of that half time. I don’t remember, as I sit here, a single department meeting. Talking of the no-pressure, no-output—the three of us that ended up collaborating, Bruce [E.] Deal, Ed [Edward H.] Snow, and I met by accident. We were all working on carefully selected—almost as if it was carefully selected—parts of the same issue.

THACKRAY: This was not from on high?

GROVE: No. If it was from any master plan, I never found that master plan. We started collaborating by literally running into each other in the cafeteria lunch room. Hi, I’m so and so, I’m so and so. “What are you doing?” “I’m working on a MOS capacitor.” “Gee, that’s funny. I’m working on the MOS capacitor, too.” Claws come out. “What do you do?” “I’m trying to grow pure oxide.” A sigh of relief. “I’m trying to analyze the theoretical capacity.” The other guy relaxes. Then without anybody telling us, we informally started to work together. If there was a master plan to pick the right three—and subsequently, I’ll tell you how the fourth came—it could not have been any better designed by background and inclination. The fourth person was a fellow by the name of Des [Desmond J.] Fitzgerald, who was a transistor engineer working in my neighborhood on an unrelated project. They were going to lay him off. I thought he was very good; and I traded one of the people who I supervised, who I did not think

was very good, for him. Des survived the layoff—the layoff came a year into my presence there. The other guy didn't really much mind being laid off either. I'm not sure Des would've minded—these were relatively young people.

THACKRAY: These were technicians.

GROVE: No. Master's degree engineers—bachelor's and master's. Anyway, Fitzgerald became the fourth member of the group. We fairly rapidly extended what we learned about the surface physics to the behavior of bipolar transistors, and Des was knowledgeable on that. In various combinations the four of us published several dozen papers (5).

THACKRAY: There was a physics department, a chemistry department—

GROVE: We had nothing whatsoever to do with the chemistry—I had no idea what they did. The microwave device department was run by a guy called Irv [Irving H.] Solt. Presumably they were working on microwave transistors.

Digital integrated electronics department [DIED]. If you want to make a joke about it, it's not inappropriate to call them DIED because they were dead! [laughter] I have no idea what they did—they had their own private line, they had their own simulation stuff. They were working on a branch of integrated circuits called CTL [capacitive threshold logic], which was an approach to bipolar integrated circuits, but they were the butt of jokes in terms of lack of productivity. They had a computer systems organization under a guy called Rex Rice, doing advanced computer architectural work unrelated to everything else. Then there was a service organization called device development that was supposed to run the lab to fabricate the experimental devices. Now, I did not figure these things out until quite some time later. I mean, I found these things out by sitting around the cafeteria and people explaining them to me.

THACKRAY: There was no introduction program?

GROVE: No. Now an interesting relevant point—one of the engineers working in the DIED department, working on MOS [metal oxide semiconductor] integrated circuits, who at some point started to work on MOS integrated circuits was Les [Leslie L.] Vadasz. Somebody told me there's another Hungarian, so we met, and we became friends. He was then and for some years later terribly underappreciated and undervalued. He was a small guy and he was very skinny then. But he was a very knowledgeable, very smart engineer—he had very good judgment. When the Intel time came around he was the first person I wanted to recruit. History bore out my judgment. But I think we only did something together in the last year or so of my

stay at Fairchild. Because the MOS integrated circuits presumed the ability to fabricate and keep alive a MOS device, that took a while in coming.

THACKRAY: The description you're giving sounds almost like the same description you might give if you'd been hired from outside into, let's say, the Berkeley [University of California, Berkeley] physics department. I'm thinking of the looseness of what you encountered.

GROVE: I don't know about the Berkeley physics department, but I think the Berkeley chemical engineering department had more discipline and structure, which is where I came from. I liked that it was like that, but the trouble with freedom is: you like your freedom, but if you want to accomplish something, if he has his freedom and you have your freedom, the two of you pursue different things. You're dependent on his collaboration to move your work along: his freedom renders your freedom useless. I mentioned the device development lab—there was absolutely no discipline. They took forever to do their job, picked their own priorities. It seemed to the outside that they worked at half speed, which is exactly why the DIED person said, "I want to have my own lab." He got his own lab—his own lab didn't cost us anything. There was no internal discipline to the place; and there was no external discipline or expectations that were put on the lab and put on the manufacturing organization to support the lab. The head of device development over some period of time was a guy called Pierre [R.] Lamond. A very hard driving, kind of mean, but sharp guy. He moved over to manufacturing in some kind of promotion. He came back, and in so many words, he said, "R&D's [research and development] filled with a bunch of turkeys, except Andy [Andrew S.] Grove is not a turkey." He started paying attention to the fact that we were applying what we learned to transistor behavior, Fitzgerald and myself. He actually took that out of our hands, processed it through his manufacturing line at five times the speed of the device development lab, and put it into manufacturing, and I got a couple of patents processed.

THACKRAY: How long had you been there at that moment?

GROVE: Three years. Then subsequently he joined [Charles E.] Sporck. Sporck left to take over National Semiconductor [Corporation]. He took Pierre Lamond as his number two. Pierre ended up running the operations at National Semiconductor. National Semiconductor, for a period of time, was very successful. He turned around and tried to recruit me—by this time it's four years, into 1967.

THACKRAY: Were you tempted?

GROVE: I was very tempted. I kind of liked Pierre; I didn't know Sporck. But I was so frustrated and bored. I have to tell you something related to this. My first book, *Physics and Technology of Semiconductor Devices*, was published in 1967, because in 1963, 1964, 1965, we did the basic work that I talked about earlier (6). In 1966 I put all of this together into a course I taught at Berkeley. Out of that course I wrote the first manuscript of the book. I poured my twenty-something energy and ambition into—first the research. I couldn't apply it to products for all the reasons we were talking about, so I put it into a course. The course was very enjoyable and I developed the scheme that I would give the lecture, prepare a lecture—prepare lecture number seven, let's say; give lecture number seven, go home, dictate into a Dictaphone the material that I just gave, give it to one secretary to transcribe, and when that was done I picked up the material—the transcript of lecture number seven—and edited it. Simultaneously, I was working on creating lecture number eight, editing the draft form of number seven, and doing the final draft of number six; which kind of kept me out of trouble. In 1966 I put all of this together into a course I taught at Berkeley. At the end of nine months I had a book basically ready to go. When the book came out, it was like somebody blew out the light. I had the book sitting on my desk when Pierre called.

THACKRAY: It was an anticlimax.

GROVE: Exactly. Baby blues. Work took nothing. You know the circumstances of what happened then? I don't want to repeat that.

THACKRAY: You haven't said.

GROVE: You haven't heard that? I was telling somebody about that. There's a guy writing a book on me. Pierre wooed me; I interviewed. The timing was perfect. I accepted. Great. Trip to Gordon—I was a section head. In name I had a department head I reported to but it was—

THACKRAY: A section within physics?

GROVE: A section of the physics department. The physics department by now would have had a caretaker—a manager who really did not get into the substance of anything. It was an appointment of convenience when Tom Sah left. I had no problem with him but he really wasn't my boss. Gordon reviewed every paper of ours and made good comments, and I remember he understood my thesis very well. I had a relationship with Gordon. So a trip to Gordon; I swear to God, Gordon choked up and had tears in his eyes. He said, "I was always hoping that some day you would succeed me in this lab. I guess it's not to be." He was very depressed in a quiet way. I felt pretty lousy but there was nothing else to say, so I said goodbye and went back to my office. Gordon reported to [Robert N.] Noyce. Actually, Gordon reported

to Charlie Sporck's successor, who was a drunk salesman, an alcoholic salesman—a drunk, alcoholic nothing at this stage. A fellow by the name of Tom [Thomas] Bay. Tom Bay's was one of the two divisions that reported to the real general manager, who was Noyce.

That same afternoon, if my recollection is right, Bob [Robert N.] Noyce saunters into my office. I'd never seen him—I'd certainly never seen him in my office. I'd never seen him at R&D. He sits down across from me and leans back and makes some comments about my book—blah, blah, blah, blah. “We don't want you to leave.” We went back and forth as to why I was leaving. At the end of the conversation—it was a Friday—Noyce gave the assignment, “Think of the circumstances under which you would change your mind.” “Are you serious?” He said, “Absolutely.” He laughed. I took it at face value and figured out what I really would like to do; and what I needed to be able to do was to solve the problem of becoming useful for the lab, not just for me. I discussed it with my wife [Eva Kaston], called Gordon on Sunday, and set up a meeting at Rickey's for breakfast on Monday morning. I basically told him just what I would like to do; and in order to do that I had to be assistant director of R&D, with technical authority—I didn't want to have any dealings with the department heads. The department heads in my opinion were useless, without a single exception. The section heads—the people who were at my level—that's where the work was being done. I told Gordon, “You manage the department heads.” The department heads were all substantially older than I. I wasn't looking forward to dealing with them. I want to be able to work with the section heads and I want to be able to go to Mountain View to represent the key developments.

Gordon agreed with all of this and made a very gracious announcement of my two-level promotion. Although I did not want to deal with the department heads, the promotion put me ahead of them. To my knowledge none of them objected. Then I called Pierre from a phone booth at Berkeley and I told him I changed my mind and why, and he said, “I knew it!” He had told me before all this happened, “We're going to offer you two departments.” I said, “You're kidding. What would I do with two departments anyway?” When I told him my decision, he said, “I told you I would offer you two departments, but it sounds as if they've just given you four departments.”

THACKRAY: This is all in the context of Gordon Moore, director of research. On one level I hear you saying, “That guy couldn't direct research.”

GROVE: At one level you'd hear that and at the other level you'd hear that we did state-of-the-art research from a little lab running circles around much bigger organizations around the world. Gordon was then, and continues to be, a technical leader. He is either constitutionally unable or simply unwilling to do what a manager has to do. If you wanted to listen to him he was there, if you wanted to take his advice he was there. He had wise comments if you sought them out, which were better or on par with stuff I got from Andy [Andreas] Acrivos. He was like a thesis advisor. Would he interfere in some conflict between X and Y and Z? Not on your life. The reason Fairchild disintegrated was because neither Bob nor Gordon was capable of removing a Tom Bay, who was staggering in at eleven o'clock for a nine o'clock meeting, breathing

alcohol. I mean, it wasn't subtle. Nor was it hearsay. I cooled my heels in Tom Bay's office for two hours before he showed up. It was about a huge technical problem involving recall of integrated circuits on a major scale. The meeting was about that—

[END OF TAPE, SIDE 1]

GROVE: In retrospect—this is not a direct observation, it is an inference—Sporck must have supplied the decisive mass and the operational muscle to the company, knocking heads and telling people what to do and what not to do and correcting behavior and doing all that stuff. When Sporck left, he left a hole, not just in an organizational chart but in the dynamics of the company. Part of this is R&D where, within the four walls, Gordon's inspiration had a magical effect on people like me and my colleagues who really cared about what we were doing, but allowed the DIED department to do nothing; allowed the computer system department, to the best I can tell, to produce nothing. He left it to chance that we would collaborate; he left it to chance that any product of ours or any knowledge of ours would ever cross organizational boundaries. In this context, I think my role at Intel was the Charlie Sporck role. It worked. I wasn't a dumb Charlie Sporck because I had a very strong Gordon Moore, and I hung on every word of his. But I wasn't waiting for Gordon to deal with the organization—training programs, non-performance, and transfer issues—any more than Charlie was. Charlie wrote a book, you know (7). I think that's probably overstating it. He didn't write it; he went around with a tape recorder and got perspectives on the early days of that period from different people. It's unedited, random, but you ought to get a copy of that. Have you interviewed him?

BROCK: He's on our list of people to speak to, yes.

GROVE: I don't know what he is like these days, but he used to be a very blond, very headstrong guy. Age makes him more headstrong, but less blond. [laughter] But he would definitely have a very useful perspective on the questions that you are asking. He was technically Gordon's boss over some period of time.

THACKRAY: Staying again in that period in Fairchild, and thinking of that university analogy—what's the difference between Gordon and a star professor at Caltech [California Institute of Technology] or Berkeley? Is that really what he is?

GROVE: I never thought of it that way, but I don't see an obvious flaw in that comparison. A star professor who hates lecturing, doesn't write papers—Gordon's publication record doesn't give justice to what I've said. Gordon's talks ranged from “eh,” to mediocre. Parts of Gordon's talks, taken by themselves, can be brilliant, but he doesn't weave the stuff together. I never understood why, because when I handed him a manuscript his critical assessment was exquisite.

If he was in academia and he was gauged on his own publications he would not have added his name to every fucking paper. [laughter]

You see in my experience with Andy Acrivos, who agonized over every word of every paper, in fact the two major papers I wrote, the experimental basic paper, he was playing the senior faculty role (3). But the theoretical paper he took over and wrote and let me comment on it (8). Gordon never did that. Andy Acrivos was more of an activist. But then he had another student that I did not get along with—who did not get along with anyone, me included. He was in my class and we were more or less working on the same issue. Andy never lifted a finger to get us together, so again the analogy works.

THACKRAY: If Gordon's isolated in his corner suite, what is he actually doing in this era?

GROVE: Once I became assistant director, I went to see him just about every day. What I did is, on my way out, as I was nearing the exit, I would walk into his office and lean against the wall and talk to him. Those were very useful conversations. He held a weekly staff meeting, which was not an inspiration to the future manager in me. Also either a weekly or bi-weekly staff lunch in a now-defunct restaurant, a back room, where the department heads that I mentioned had literally two or three drinks over lunch. Then fifteen years later Jimmy Carter [James Earl Carter, Jr., thirty-ninth President of the United States] started raving against the three-martini lunch. The only three-martini lunch that I had ever seen, before or since, were Gordon's staff meetings. The conversations were correspondingly useless. I don't know what he did beyond that.

THACKRAY: When you first arrived, that first Monday morning—

GROVE: On that Monday morning I sat down with Tom Sah, who had a one o'clock plane; he took out a pen and a pad of paper, and explained to me what he wanted me to do. I was so confused. I had arrived somewhere between eight and nine. At nine o'clock I get called into Tom Sah's office; I sit down. He had a raspy voice, "I'm leaving for a meeting; let me explain what I want you to do." He spent several hours bringing me up to date on surface states and surface effects and capacitors and this and that. I want you to calculate this distance and that and he gave me some hints as to how I might go about it. Then he left. I retreated to my nice little office. I was fresh from fluid dynamics, all of the math—the error functions, complementary error functions, and diffusion equations. I analyzed the problem, and went back to Tom Sah's secretary and asked if there was a computer someplace that I could use. I forget how many steps later, I finally got an account at a Control Data [Corporation] service center which was about a mile from the lab. That building is still there—I don't know what's in it. I went over there, got an account, got a bunch of forms. I had to put in the forms because I was doing the work in FORTRAN using a Hollerith symbol, which means that every placement had to be just right or the computer would return an error message, which it did multiple times.

Now the time comes and I've got all my data, every space counted correctly. Ultimately I managed to get through and the computer calculated a family of curves which would have taken a very long time to calculate manually. By Friday I published an analysis of the curves in a report ready for Tom. In some fashion that report circulated—I certainly didn't know who to circulate it to, so I assume it was Tom's secretary who had a distribution list. On Monday I got a note from Bob Noyce, who I had never met—"I just read your report on MOS—it's very nice work," signed "Bob." I thought I'd died and gone to heaven. I found out who Bob Noyce was—general manager and Gordon's boss's boss. He reads technical reports over the weekend. Oh God! [laughter] I have that note some place. I cherish that note. That was the first and the last time I heard from Bob Noyce during my stay at Fairchild.

THACKRAY: Was he reading it because it had got to Gordon, and then him? Was he really reading it?

GROVE: He may have been copied on it, but I don't have the original report. I know what was in it, but I don't remember who was on the distribution list. Tom Sah's secretary may have automatically copied the management chain. It may have been the custom. I did not send it to Bob Noyce. I was very impressed that somebody that high up would read it. With my first assignment I came to the attention of the group general manager. With that you would think that the skid would be greased. He sent the compliment—that's another story. I mean, it's a story that you've heard. Let me put it this way, I learned a lot about management with a multiplier of minus one. [laughter] I came to Intel with a ferocious determination not to build another Fairchild. I wanted to avoid many of the practices and cultural elements. On the technical end, we tried to be like Fairchild R&D. People knew what they were talking about, there were good technical people. Things just didn't work for a variety of managerial reasons, every one of which seemed like I chose to do the opposite at Intel. For example, we didn't have separate R&D. We still don't have separate R&D. I mean, there are manufacturing organizations that are dedicated to development of new technology, but they are run by the manufacturing organization. The whole transfer issue had the technical difficulties but it didn't have the organizational difficulties.

THACKRAY: If you stay with Fairchild, was Gordon unhappy with the structure of research?

GROVE: How much time have you spent with Gordon?

THACKRAY: A lot.

GROVE: Can you gauge his happiness?

THACKRAY: I don't think he was happy, but I didn't really hear deep discontent.

GROVE: He never grouched to me. He occasionally encouraged me, but I don't think his heart was in the encouragement. He was resigned. I'm saying these things from forty years of retrospection. He was no different than he is now; he's just kind of a passive, shy guy. You can't figure out what he means in the objective context, let alone the emotional context. The total number of times I have seen him animatedly annoyed or unhappy or angry I can count on my thumbs.

BROCK: What were they?

GROVE: At Intel in the late 1970s, Bob Noyce said in a meeting, "Now that we are in the computer business, blah, blah, blah." Gordon exploded, "We are not in the computer business! We build computer development systems." Gordon was more forceful on that than on any other issue—Bob cowered! [laughter] We didn't go into the computer business. There must have been another one—I have a thumb to spare for another Gordon story. [laughter] But that's the most obvious one that I can count.

THACKRAY: In 1967, you had the issue of whether you were leaving. How aware were you of the fact that Bob Noyce and Gordon both were thinking of leaving? How did you get into that?

GROVE: That's a very interesting little scab. [laughter]

THACKRAY: Pick at it please. [laughter]

GROVE: There was a Solid State Devices Conference in May or June of 1968. Gordon was at some sort of a Fairchild planning meeting and he was only coming a day later. Gordon arrived and he and I went for a walk and I was busily updating him about what I heard at the meeting. I can see that he's distracted. I started asking him, how was the planning meeting, figuring that something happened there. He said, "It was interesting, but I was not really interested." I stared at him. "I've decided to leave Fairchild." "What are you going to do?" "I'm going to start a new semiconductor company." To which I say, "I'm going with you." I said it faster than I just said it. Gordon didn't say no. I don't remember what he said—I mean he didn't exactly hug me. [laughter] But then he hasn't exactly hugged me or anyone else in my presence—then or any other time. We start feverishly talking about what was to be Intel. In a few minutes he said, "By the way, Bob Noyce is joining as my partner." I don't remember the phrase he used. To

which I said, “Oh.” I was not a Bob Noyce fan. I had witnessed Bob Noyce being absolutely inactive and paralyzed as the Tom Bay/Fairchild was disintegrating. I subbed for Gordon at Noyce’s staff meetings a number of times and it was awful. This was a long time from that one-line compliment. I had very little appreciation for Bob. Gordon said something to the effect, “He’s better than you think,”—blah, blah, blah. I got over it. My reaction to Bob’s presence was not very good. That was all that I knew then.

Later on we had to work out my getting out of Fairchild. I had to go to a meeting, present a paper—that was the last thing I did for Fairchild—go to some kind of radiation physics meeting, and present a paper on behalf of Fairchild. I took a few days’ vacation at Glacier National Park in Montana. When I came back we caucused in Bob’s study, which was where we started. I was very concerned that somebody should pay me something, because I was living from paycheck to paycheck. They guaranteed that they were going to pay me even before the company started. The company started very quickly. In due course, I developed an appreciation for Bob, but not as a manager.

He was a very smart guy, lots of ideas, some of which were brilliant, most of which were useless. He was perfectly tolerant of you picking the gems out of the ideas and throwing the rest of it away. He didn’t insist on his own way. He was very approachable. Bob was a paradox, and very private, very approachable the first inch and after that you can’t go any further. But the first inch was good enough to charm every customer, every partner, and lead everyone to adulation. An ability to think of doing things that other people would say, “You can’t do that.” He would say, “Why not?” Some of the time he would be right. But that was a very good dynamic over time, because he would tolerate me saying, “That’s bullshit.” Then we would argue. I would drop the bullshit label some of the time. In the mid-1980s he was the leader of taking a free market administration, talking them into taking the strongest, still to this day unique, stance of prescribing the percentage of users of semiconductors from the U.S. sources to Japan—it hasn’t happened since. He was the spirit behind that. He was a good mentor. I came along and argued with people like Dick [Richard G.] Darman and stuff like that. The faculty advisor I had in those kinds of things was Bob.

THACKRAY: Roughly, is Intel schematically: Bob Noyce equals charm and politics; Gordon equals research; and you equal management? Is that the best sketch?

GROVE: It’s a pretty good sketch, but I’ll tell you a more specific description of it. Peter [F.] Drucker, *Practice of Management*, 1954—his best book (9). Long before he writes the same thing in thousand-page versions. At some point in the 1970s, I came across that book and I really loved it. It’s my favorite book. You come to a chapter called, “The Ideal Chief Executive.” I read that chapter, make two copies, and hand them to Bob and Gordon, with some comment that that’s us. The “Ideal Chief Executive” is three people—in one sentence: “an outside man, a man of thought, and a man of action.” That’s almost exactly what you came up with. We couldn’t have had a better casting for those roles.

THACKRAY: Was it implicitly clear on day one that this was the scenario?

GROVE: No. A major power struggle took place between day one and two and a half years into it.

[END OF TAPE, SIDE 2]

GROVE: Bob, probably Bob more than Gordon, hired a guy that used to do marketing—as compared to the sales manager who was Tom Bay—this guy called Bob [Robert F.] Graham. He was the marketing manager in early Intel days. He left when he did not get the top job. They considered him very good and he was recruited to be Intel’s sales and marketing manager. A very smart guy, very ambitious—maybe a year or two older than me but a lot more sophisticated than me. I’m about to shift from facts to opinion. Fact: the two of us rapidly began rubbing each other the wrong way. Opinion: later on it dawned on me that he recognized me as a competitor at a time when the whole staff of the company would sit around this table and he’d start competing with me by putting me down. In real life, it took me a long time to become suspicious of it, but by 1970 or 1971 I was utterly miserable.

I’ll give you one example of what he would do. I ran engineering and manufacturing and he ran sales and marketing. He would go on a trip and he would have one of his henchmen nose around in the design labs checking up on whether we were doing the day-to-day work of what we were supposed to be doing. I would see this junior marketing guy, and I would say, “What are you doing here?” He gave me an evasive answer. I pounced and pinned him against the wall, figuratively speaking, and he told me that Bob told him to check on the progress of this and that circuit design—it was six months away from delivery—so he can check on my status report next time I give it, with inside information. Shit like that—the company was only a few dozen people at the time. In the face of all of this, in 1970 we were in Washington at a technical meeting. Gordon and I were taking a walk in the Washington Zoo and Gordon very clearly told me that someday I would run Intel. It didn’t mean a thing to me—I thought it was a very nice compliment—but I did not have such ambitions. But I did have ambitions of working my tail off without somebody persecuting me. I was miserable, and I told Gordon I was miserable. He never did a fucking thing about it. Bob ultimately realized they had to make a decision between Bob Graham and me, and they brought in a new sales manager. I never had this discussion with Gordon, but I was definitely heading towards leaving. I was miserable. I can’t understand that he would have let me go, given what he told me about his hopes, when there were tears in his eyes, he said, “The day you walked into this office, I thought I saw my replacement.” This was at Fairchild. We’ve seen this “movie” before. Bob Graham is dead by the way. He and I said hello once—he died about five years ago—I saw him in a restaurant and said hello. That was the one time we talked since 1971.

THACKRAY: Go back to the start-up moment—Bob Noyce, Gordon Moore, and yourself—you’ve all had the Fairchild experience, and here’s Intel. Can you characterize in some way what Bob Noyce and what Gordon Moore took out of the Fairchild experience to Intel, before then going to yourself?

GROVE: Bob Noyce took out of it that he wasn’t going to work for anyone else. He wasn’t going to work for an East Coast company, which was his bane at Fairchild. Somebody in Syosset was making decisions about a business that he didn’t know anything about and they were utterly arbitrary about it. Gordon, I think, basically wanted the equivalent of the same thing. He wanted to set up a cost system the way he understood cost systems; he wanted to run analysts’ meetings the way he wanted to do them. Gordon actually has strong feelings about a small number of things, compensation being one. He had very well-defined ideas. To this day, Intel compensation is based on Gordon’s philosophy and practices.

THACKRAY: The animating ideas being what?

GROVE: Low, performance-oriented, variable, and equitable as compared to opportunistic. That’s how I would sum it up. He always placed a great deal of value on silicon technology, and he was the technical leader of the company so long as the company was defined by silicon. I don’t think he was ever comfortable with the microprocessor business, either the technology or as a business. As a business the microprocessor business is not a value-performance-cost. It’s an architecture-maximum return, increasing return economics as it has been since software dependence. It’s a highly intricate game theory kind of game. I thrived in that.

THACKRAY: Gordon’s *métier* was—?

GROVE: I build a much better mousetrap.

THACKRAY: Interesting research issues.

GROVE: The technology to do something that other people cannot do. Which was the first thing he did at Intel. For a while it worked. Then it didn’t work and we had to play a different game. It’s not that I could see the possibilities of microprocessors going into them, but I could see the horrors of the memory business by staying in it. Then as we went into that—my co-teacher at Stanford [University] has a phrase; most strategic planning is really instances of what he calls “strategic recognition.” You bumble along until an opportunity comes and you realize the potential of it and you mine it. I think that describes what happened to me—the increasing return world of Machiavellian software dependence is a custom of power and balances and stuff.

Microprocessors—that's not Gordon. It could have been Bob, but Bob was then gone from the business. He actually died before the possibilities of this were at full blast. Bob would have still needed a Charlie Sporck or me to enforce the discipline. You've got to all march this thing very carefully—with military precision, particularly because of antitrust laws and equity. There were mine fields all over. That wasn't Bob's *métier*, to use your phrase. But he would have relished the possibilities of doing it. For Gordon, it's not his kind of thing. You need a sociologist. You see the magic of it was this three-man office. We got along. We understood each other's contributions and didn't tread too much into it, but pushed them close enough so that the contributions could couple to each other. I don't think it is a very frequent occurrence.

THACKRAY: What was the mechanism of interaction—in weekly meetings or in just dropping by?

GROVE: Both, through most of the time. We did have weekly meetings and my perception was that, even when I wasn't running them, they were not like Fairchild meetings. I didn't run them for many years. But they were weekly meetings. The group in the meetings was different. It wasn't a bunch of middle-aged has-beens who never were; they were a bunch of young, ambitious people who wanted to get on with developing things and get them in the market. The meetings were better and the personal interaction was excellent. I just complained about Gordon's inaction—with one exception, every trouble or any misgiving I have ever had about Gordon was a case of inaction. I never had any problem with his thoughts or my interaction with him or his understanding of it. The more I think about the academic advisor the better that sounds. If I wanted to do something I would walk into his office—"I want to such and such," and he would tell me what's wrong with it. But he wouldn't tell me, "Do this or do that," he would rarely give me specific instructions. But I had such profound respect for his wisdom that he didn't need to. My interaction with Bob was actually more social. Bob and I became casual friends.

THACKRAY: Socially interacting, did you visit each other?

GROVE: Skiing—he taught me to ski. His house, our house. My wife and I got along with his first wife and our kids were similar ages. We would do things together in various combinations.

THACKRAY: Where were Gordon and Betty [I. Moore] in this?

GROVE: We never socialized with them. I actually have no idea what their social life is. I don't know anyone who socialized with them. By the way, that was not necessary. The relationship between Gordon and myself was reading each other's minds, and so long as he didn't have to do anything, trust that he wouldn't. He never disappointed me there. I'd love to

see his face when he reads this! [laughter] But you tell me—is his description of the situation during that period of time at variance with mine?

THACKRAY: No. I think fundamentally it's the same.

GROVE: How does he describe his relations with Bob?

THACKRAY: There's very little affect to the description—positive or negative.

GROVE: Emotional affect?

THACKRAY: Yes—it's just a fact of life. Bob did this; I did that. How would you describe their relationship?

GROVE: I don't know. I'm going to give you a posthumous—way past Bob's death. Once and only once I saw deep irritation in Gordon when somebody was giving the long-dead Bob credit for something that he thought was unfair. It's an art to recognize when Gordon is irritated, with the exception that we talked about. He got emotional and that blew me away and suggested to me that lower level versions of that emotion must have existed. But I never picked that up. What is interesting to me about it is Bob, being the charmer with the high profile, overshadowed Gordon. Gordon was the intellectual power behind Intel. Gordon was in charge then, but by the time Bob's light faded on the scene, my light was glowing. When I saw that emotion I thought, I wonder how he feels about me. But he never gave me the slightest indication that he was bothered by the attention I was getting.

THACKRAY: Let me just ask you one last thing, if I may. I have the enviable, or the unenviable, task of introducing Gordon next week when he gets the Perkin Medal of the Society of Chemical Industry, which is for technical innovation. If you were introducing Gordon, what would you say? I'm picking your brains I'm ashamed to say.

GROVE: He's the brains behind the first half of Intel. Intel is the brains behind the computer industry—the modern distributed computer industry, which arguably through the Internet, dah, dah, dah, has changed things as much as electricity. You're dealing with a person who is equivalent to maybe six people in electricity—because it is recognized that the microprocessor has changed everything. Gordon has nothing very much to do with the microprocessor, but he has to do with mass-produced complex integrated circuits, without which microprocessors would not even be a thought. That's his historical place.

[END OF TAPE, SIDE 3]

[END OF INTERVIEW]

NOTES

1. Victor Marie Hugo, *Les Misérables* (Bruxelles: A. Lacroix, Verboeckhoven & Ce., 1862).
2. Andrew S. Grove, "An investigation into the nature of steady separated flows at large Reynolds numbers" (Ph.D. thesis, University of California, Berkeley, 1963).
3. A. S. Grove, F. H. Shair, E. E. Petersen, et al. "An experimental investigation of the steady separated flow past a circular cylinder," *Journal of Fluid Mechanics* 19, no. 1 (1964): 60-79.
4.
 - a. F. H. Shair, A. S. Grove, and E. E. Petersen, "The effect of confining walls on the stability of the steady wake behind a circular cylinder," *Journal of Fluid Mechanics* 17, no.4 (1963): 546-550.
 - b. A. S. Grove, E. E. Petersen, and A. Acrivos. "Velocity distribution in the laminar wake of a parallel flat plate," *Physics of Fluids* 7, no.7 (1964): 1071-1079.
 - c. A. S. Grove, F. H. Shair, E. E. Petersen, et al. "An experimental investigation of the steady separated flow past a circular cylinder," *Journal of Fluid Mechanics* 19, no.1 (1964): 60-79.
 - d. A. Acrivos, D. D. Snowden, A. S. Grove, et al. "Steady separated flow past a circular cylinder at large Reynolds numbers," *Journal of Fluid Mechanics* 21, Part 4 (1965): 737.
5.
 - a. A. S. Grove, E. H. Snow, B. E. Deal, and C. T. Sah, "Simple physical model for the space-charge capacitance of metal-oxide-semiconductor structures," *Journal of Applied Physics* 35, no. 8 (1965): 2458.
 - b. B. E. Deal and A. S. Grove, "General relationship for the thermal oxidation of silicon," *Journal of Applied Physics* 36, no. 12 (1965): 3770.
 - c. B. E. Deal, A. S. Grove, E. H. Snow, and C. T. Sah, "Observation of impurity redistribution during thermal oxidation of silicon using the MOS structure," *Journal of the Electrochemical Society* 112, no. 3 (1965).
 - d. B. E. Deal, A. S. Grove, E. H. Snow, and C. T. Sah. "Recent advances in the understanding of the metal oxide-silicon system," *Transactions of the American Institute of Mining, Metallurgical and Petroleum Engineers* 233, no. 3 (1965): 524-9.

- e. D. J. Fitzgerald and A. S. Grove. "Mechanisms of channel current formation in silicon p-n junctions," *Fourth Annual Symposium of the Physics of Failure in electronics*. (1965).
- f. A. S. Grove, B. E. Deal, E. H. Snow, and C. T. Sah, "Investigation of thermally oxidised silicon surfaces using metal-oxide-semiconductor structures," *Solid State Electronics* 8, no. 145 (1965).
- g. A. S. Grove and D. J. Fitzgerald, "The origin of channel currents associated with p+ regions in silicon," *IEEE Transactions on Electron Devices* 12 (1965): 619.
- h. E. H. Snow, A. S. Grove, B. E. Deal, and C. T. Sah, "Ion transport phenomena in insulating films," *Journal of Applied Physics* 36, no. 5 (1965): 1664.
- i. B. E. Deal, E. H. Snow, and A. S. Grove, "Properties of the silica-silicon system," *Semiconductor Products and Solid State Technology* 9, no. 11 (1966): 25-33.
- j. B. E. Deal, E. H. Snow, and C. A. Mead, "Barrier energies in metal-silicon dioxide-silicon structures," *Journal of Physics and Chemistry of Solids* 27, no. 11-1 (1966): 1873.
- k. D. J. Fitzgerald and A. S. Grove, "Radiation-induced increase in surface recombination velocity of thermally oxidized silicon structures," *Proceedings of the IEEE* 54, no. 11 (1966): 1601.
- l. A. S. Grove and D. J. Fitzgerald, "Surface effects on p-n junctions: characteristics of surface space-charge regions under non-equilibrium conditions," *Solid State Electronics* 9 (1966): 783.
- m. A. S. Grove and E. H. Snow, "A model for radiation damage in metal-oxide-semiconductor structures," *Proceedings of the IEEE* 54, no. 6 (1966): 894.
- n. E. H. Snow and B. E. Deal, "Polarization phenomena and other properties of phosphosilicate glass films on silicon," *Journal of the Electrochemical Society* 113, no. 3 (1966): 263.
- o. B. E. Deal, M. Sklar, A. S. Grove, and E. H. Snow, "Characteristics of the surface-state charge (Q_{ss}) of thermally oxidized silicon," *Journal of the Electrochemical Society* 114, no. 3 (1967): 266.
- p. E. H. Snow, A. S. Grove, and D. J. Fitzgerald, "Effects of ionizing radiation on oxidized silicon surfaces and planar devices," *Proceedings of the IEEE* 55, no. 7 (1967): 1168.

- q. D. J. Fitzgerald and A. S. Grove, "Surface recombination in semiconductors," *Surface Science* 9, no. 2 (1968): 347.
 - r. D. J. Fitzgerald and E. H. Snow, "Comparison of surface and bulk effects of nuclear reactor radiation on planar devices," *IEEE Transactions on Electron Devices* 15, no. 3 (1968): 160.
 - s. S. T. Hsu, D. J. Fitzgerald, and A. S. Grove, "Surface-state related 1/f noise in p-n junctions and MOS transistors," *Applied Physics Letters* 12, no. 9 (1968): 287.
 - t. E. H. Snow and B. E. Deal, "Polarization effects in insulating films on silicon—a review," *Transactions of the Metallurgical Society of AIME* 242, no. 3 (1968): 512.
 - u. D. J. Fitzgerald and A. S. Grove, "Surface-related failure mechanisms in integrated circuit arrays," *Proceedings of the 1970 annual symposium on reliability* (1970): 249-50.
6. Andrew Stephen Grove, *Physics and Technology of Semiconductor Devices* (New York: Wiley, 1967).
 7. Charles E. Sporck, *Spinoff: a personal history of the industry that changed the world* (Saranac Lake, New York: Saranac Lake Publishing, 2001).
 8. A. Acrivos, D. D. Snowden, A. S. Grove, et al. "Steady separated flow past a circular cylinder at large Reynolds numbers," *Journal of Fluid Mechanics* 21, Part 4 (1965): 737.
 9. Peter Ferdinand Drucker, *Practice of Management* (New York: Harper, 1954).

INDEX

A

Acrivios, Andreas, 2, 8-9, 17-18, 24, 26
Allied Chemical Company, 3
American Institute of Chemical Engineers [AIChE], 1

B

Bay, Thomas, 24-25, 29-30
Bell Telephone Laboratories, Incorporated, 10-13, 19
 building design, 19

C

California Institute of Technology [Caltech], 25
California, University of, Berkeley, 2, 6-7, 9, 11, 16, 22-23, 25
 chemical engineering department, 22
 course taught by Andrew S. Grove, 23
 physics department, 22
Capacitive threshold logic [CTL] circuits, 21
Carter, James Earl Jr., Thirty-ninth President of the United States, 27
Central Intelligence Agency [CIA], 3
Chambré, Paul L., 8, 16
 as Jean Valjean of *Les Misérables*, 8, 16
City College of New York, 1-5, 7, 17
Columbia University, 4
Compaq Computer Corporation, 13
Control Data Corporation, 26
County Adoptions Department, Solano County, 7
County adoption agency, Santa Clara County, 7
County Welfare Department, Marin County, 7

D

Darman, Richard G., 29
Deal, Bruce E., 20
Dow Chemical Company, 11
Drucker, Peter
 Practice of Management, 29
DuPont de Nemours and Company, Inc., E. I., 3, 10
 anti-semitism, 3

E

Early, James M., 12-13
Esso Oil, Inc., 11

F

Fairchild Semiconductor, 3, 7, 10-14, 16, 18-20, 22, 24-25, 27-32
 building design, 19, 26
 employee relations and, 19
 collaboration at, 20, 22
 digital integrated electronics department [DIED], 21-22, 25
 managment dynamics, 25, 27
 staff meetings at, 26
 work environment in, 20
Fitzgerald, Desmond J., 21-22
Fluid dynamics, 1-2, 4, 8, 26
 research centers, 2
FORTRAN, 16, 26

G

General Electric Company, 10, 12
Graham, Robert F., 30
 discord with Andrew S. Grove, 30
Grinich, Victor H., 19
Grove, Andrew S.
 Fairchild Semiconductor
 collaboration at, 20-21
 first report at, 27
 promotion at, 24
 fluid dynamics, interest in, 1-2, 4, 8
 graduate thesis, 8-9, 12, 16
 Intel Corporation
 management dynamic with Robert N. Noyce, 29
 management style at, 27
 personnel policy at, 5
 obtaining summer employment in California, 5
 on the transistor, 9
 Physics and Technology of Semiconductor Devices, 23
 proficiency in differential equations, 16
 relationship to Gordon E. Moore, 32
 relationship to Robert N. Noyce, 32
 undergraduate interests, 1, 4
 undergraduate thesis, 1
 wife, Eva Kaston, 2, 4-5, 7, 15, 24

H

Hollerith symbols, 26

I

Integrated circuit, the, 13
Intel Corporation, 13-15, 18-19, 21, 25, 27-31, 33
 corporate structure, 27
 formation of, 28-29
 management dynamic, 29, 32
 microprocessor planning, 31-32
 staff meetings, 32

J

Journal of Fluid Mechanics, 9

K

Kirchhoff, Gustav R., 8, 16-17
Kittel, Charles, 10
Kolodney, Morris, 1, 6

L

Lamond, Pierre R., 22-24
Les Misérables, 8, 16
List, Harvey, 1
Lockheed Missile and Space Systems, 9-10, 12

M

Martinez, California, 2, 5-6
Massachusetts Institute of Technology [MIT], 3, 6
Metal oxide semiconductor [MOS] integrated circuit, 20-22, 27
Moore, Betty I., 32
Moore, Gordon E., 11-15, 17, 19, 23-33
 emotive subjects, 28, 31, 33
 lecturing style, 25
 management style, 24
 Perkin Medal of the Society of Chemical Industry, 33
 relationship to Andrew S. Grove, 32
Motorola Incorporated, 10

N

National Semiconductor Corporation, 22
Noyce, Robert N., 14-15, 23-24, 27-33
 Intel Corporation
 management dynamic with Andrew S. Grove, 29
 personality, 29, 33

P

Palmer, Robert B., 11
Pfizer International, 3
Pittsburgh, University of, 2

R

Rice, Rex, 21
Richmond, California, 4-5
Roshko, Anatol, 9

S

Sah, Chih-Tang "Tom", 18, 20, 23, 26-27
Schmidt, Alois X., 1-2
Shapiro, Irving S., 3
Silicon Valley, 13, 16
Snow, Edward H., 20
Solt, Irving H., 21
Sonoma State Mental Hospital, 5-6
Sporck, Charles E., 22-25, 32
Spinoff: a personal history of the industry that changed the world, 25
Standard Oil Company, 4-5
Stanford University, 6, 8, 15, 17
Stauffer Chemical Company, 5

T

Tandem Computers, Inc., 13
Texas Instruments Inc., 10, 12
Tidewater Associated Oil Company, 2, 5

U

Unisys Corporation, 13
Universal Automated Computer [UNIVAC], 13

V

Vadasz, Leslie L., 15, 21

W

Wagon Wheel Pub, the, 14
Wang, Shyh, 9
Waring, Werner, 18
Wisconsin, University of, 2, 6