CHEMICAL HERITAGE FOUNDATION

DONALD L. KLEIN

Transcript of Interviews
Conducted by

David C. Brock

By Phone

on

2 and 9 March 2006

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Donald L. Klein, interview by David C. Brock by phone, 2 and 9 March 2006 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0332).

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

DONALD L. KLEIN

1930	Born in Brooklyn, New York on 19 December

Education

1952	B.S., Chemistry, Polytechnic Institute of Brooklyn
1956	M.S., Inorganic Chemistry, University of Connecticut
1959	Ph.D., Inorganic Chemistry, University of Connecticut

Professional Experience

	Sylvania Electric Products, Inc., Woburn, Massachusetts
1952-1954	Chemist, Chemistry and Physics Department
	University of Connecticut, Storrs, Connecticut
1954-1958	Research Assistant, Teaching Assistant, and Assistant Instructor, Chemistry Department
	Bell Laboratories, Murray Hill, New Jersey
1958-1967	Member of Technical Staff and Supervisor
	IBM Corporation, East Fishkill, New York
1968-1987	Senior Engineer, Manager, and Technical Staff
1987-present	Consultant
	Dutchess Community College, Poughkeepsie, New York
1987-1988	Member of Faculty, Department of Physical Sciences; Lecturer in Chemistry

Honors

1982	IBM Invention Award
1994	Inducted into New Jersey Inventors Hall of Fame
1994	Jack A. Morton Award of the Institute of Electrical and Electronics Engineers
1999	Brooklyn Technical High School Alumni Hall of Fame

ABSTRACT

Donald L. Klein is the son of a Hungarian father and a Hungarian-American mother, who grew up in Brooklyn, New York. With his childhood friend, Neil Wotherspoon, Klein developed an early passion for chemistry, electronics, and amateur radio, interests that would follow him throughout his life and career. At Brooklyn Technical High School, he discovered an additional passion for metallurgy. He completed his undergraduate degree in chemistry at Polytechnic Institute of Brooklyn (now Polytechnic Institute of New York University), then found a job in the semiconductor industry to support his new wife (who also received a degree in chemistry). After working for a couple of years, he pursued a graduate degree at the University of Connecticut to study photochemistry under Dr. Roland Ward.

Klein was recruited to work for Bell Laboratories, and began working on the production of semiconductors. His group was involved in involved in developing etching techniques for semiconductors and methods to prevent different types of contamination in semiconductor production. In February 1966, Klein was in charge of a brainstorming session with several other Bell scientists to design a better process for building FET devices. They first identified the problems with current models and processes; out of that meeting came the idea of using a heavily doped polycrystalline silicon layer as the gate of an FET. The gate was to be supported on dual layers of a silicon nitride and silicon dioxide serving as the gate insulator. Using the FET as a model for integrated circuits, they fabricated and characterized hundreds of FET devices at high yield that exhibited close electrical tolerances. Klein and his colleagues published several papers on their new technology, and applied for patents on their process, though Bell's management was slow to appreciate the breakthrough its scientists had made. After a restructuring, Klein left Bell to work for IBM.

The rest of the industry, however, was quick to adopt and improve the silicon gate technology. There were legal disputes throughout the 1970s, but by that time Klein was at IBM developing photoresist technologies and more efficient processes for manufacturing electronic packaging.

INTERVIEWER

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

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INTERVIEWER: **David C. Brock**

LOCATION: **By Phone**

DATE: **2 March 2006**

BROCK: This is an interview with Donald Klein, conducted by David Brock, on 2 March, 2006, by telephone.

Dr. Klein, as…I warned you that I'd like to start at the beginning. I'd like to start at the very beginning and ask you when and where you were born.

KLEIN: I was born in Brooklyn, New York, December 19th, 1930.

BROCK: And did you…I know that you went to Brooklyn Tech [Brooklyn Technical High School]. Did you spend your entire youth in Brooklyn?

KLEIN: Yes, until I [was] married [in] 1952. So, for twenty-two long years I [lived in] Brooklyn. [laughter]

BROCK: Could you tell me about the…the family that you were born into, a little bit about your father [Kalman Klein], your mother [Emily V. Klein], and your early household?

KLEIN: My father had a kind of unique history of his own. He was Hungarian by birth. I think that's a big plus for engineers and scientists.

BROCK: It seems to be.

KLEIN: He came to this country as a youth of sixteen years of age, not speaking the language. Because he had an uncle in the middle of nowhere in Blackwell, Oklahoma, he went directly to Oklahoma and continued high school there. Unfortunately, when he finished high school he anticipated going to college, as his had uncle promised him. His uncle was willing to perform to that except that he told my father that he would have to continue working on the farm for a few years before he could attend college. At that, he ran away and disappeared into the West, stopping in Needles, California. If you know where Needles is, it's in the Mohave Desert. A very uncomfortable spot to live in […] he told me, because, he said, the temperatures reach way over a hundred most of the time. [A] particular lady, who was a Christian [Scientist], […] owned the boarding house that he lived in [and] told him if he didn't think about the heat he'd be perfectly comfortable. He tried to assume that she was right. It never worked [for him]. [laughter] [From there he went to] Seattle, Washington, and during World War II…—by the way, he had come to this country in 1909. He graduated from school, class of 1912. [His classmates] called themselves the "Dirty Dozen," both for the year and for the fact that there were twelve students in his class. Many years later, when my uncle (my father's younger brother) died—[…] he followed much the same path as my father […]—I went there to the school that my father always sang the fight song [about]: Blackwell Oklahoma High. I discovered that it was a very modern school, [which] had replaced the one that my father attended. The principal was happy to give me actual transcripts of my father's grades. What astounded me was the fact that he had a very, very high grade point average in all the subjects he took, including English, which included all the various plays and books he read. That was from the word go. It absolutely flabbergasted me that a Hungarian-speaking kid could be proficient in English [so fast].

BROCK: And did he…when he went to…eventually made his way to Seattle, did he then go to college?

KLEIN: No. Actually he didn't. This happened years later. World War I ensued. He was a very patriotic man. He volunteered, [and so did his brother, for the army]. He had experience in haberdashery stores and volunteered for the [United States Army] Quartermaster Corps, but he ended up as a medic. As a consequence, he was in the front lines in World War I, in France. He never wanted to look at blood again [after that], although he [expertly] bandaged me up repeatedly from my wounds [as a youngster]. He [stayed] on as a volunteer in France after World War I Armistice, because he wanted to see his own parents, and he anticipated [that] by being in Europe, he would have the opportunity of going back to Hungary. Never happened.
<T: 5 min>

While he was there, he did attend [the] University of Paris. They had special courses for Yanks in France. [He] took a number of courses. Ultimately in 1919, one year after the war was over, […] they shipped him back and, instead of discharging him in Seattle where [his unit] originated, they discharged him in New York City, because he was no longer with his organization, [his regiment being in] the 77th Division.

As a consequence, he never left New York City. [laughter] Not by choice, but by the fact that he met a woman there who [became] my mother. She was born […] in Brooklyn also, and was an [American native of Hungarian descent. My father] attended a number of courses at Pratt Institute, particularly in drawing and interior decoration. […] He was very proficient in watercolor painting. I had, at one time, a great number of his paintings that he generated in that period. My mother, believe it or not, was valedictorian of her class at [Manual Training High

School] in Brooklyn. […] She was always proudest to show [both her] sons that she knew physics. [She would] draw circuits that she learned in the physics class in high school. By the way, that included the electric doorbell, which she thought was high technology in those days. [laughter]

BROCK: By the time of your arrival on the scene in 1930, what sort of work was your father occupying himself with?

KLEIN: He opened a pioneering rug cleaning business. Rug cleaning was very primitive in those days, and he sat himself down [in] the New York Public Library and learned all he could about how you clean a rug. He opened a business, which was manually [operated] in those days. It was much later that the machinery was generated that would allow him less backbreaking work, and it grew into a very large firm, which, interestingly enough, my brother [Herbert J. Klein] took over many years later. My brother is seven years my senior, and I followed exactly in his footsteps up to the rug cleaning business and then I [diverged]. [laughter] He also went to Brooklyn Tech […] and also Brooklyn Poly. […] It's called Polytechnic [Institute of New York] University today. He's an electrical engineer [by training].

BROCK: […] Is there anything that you can point to in the household…obviously there was an emphasis on education, but where…do you see some element of the household contributing to both you and your brother going into technical subjects?

KLEIN: Yes. My brother was a radio amateur, which prior to World War II was a relative rarity. There were perhaps thirty-five to forty thousand radio amateurs throughout the United States. There are many, many times those numbers now. In those days it was considered high technology of the greatest type. You built everything yourself, and, indeed, he did. I was exposed to that type of technology. I, too, became a radio amateur, as [did] my wife [Ruth R. Klein]. My brother is still active, as I am, in Ham Radio.

BROCK: I see.

KLEIN: But, further, he contributed in a different way. For my thirteenth birthday he gave me a chemistry set.

BROCK: This is your brother?

KLEIN: Yes, […] my brother. Not only that, but in those days when you went to college they provided you with a kit of chemicals to be used […] in the experiments that were designed for the particular [course that he took]. He, not being a chemist, but rather an electrical engineer, […] took the standard one-year course in chemistry. […] I was the recipient of his excess chemicals from that kit. Not only that, but he was good enough to contact a number of [our] cousins, who also were going to college at that time, and they contributed further chemistry kits to me. So, I built a rather elaborate facility in the basement of my home. [laughter] My wife still marvels at it, not only for its **<T: 10 min>** largeness and professional [layout], but the neatness that I maintained [in] those days. She claims my [present] basement [laboratory] has never approached it [in neatness]. [laughter] That's not funny because she wants me to go down there after this [interview] and clean it up again. [laughter]

BROCK: [laughter] Well, I'll try and extend the interview as long as possible.

KLEIN: I wish you would.

BROCK: Okay. [laughter] At your brother's side, getting into ham radio, I suppose that was your real first acquaintance with electronics?

KLEIN: Yes. But, it really didn't develop until after World War II. Chemistry came first.

BROCK: Okay. And can…

KLEIN: I was aware of electronics but I certainly was not a radio amateur until after World War II, whereas he was a radio amateur, licensed radio amateur in 1938.

BROCK: It was really in your high school years that you got into the amateur radio yourself?

KLEIN: Yes. That is correct. By the way, that was primarily because there was a spot in lower Manhattan called "Radio Row." You may have heard of it. It's actually where the Twin Towers [World Trade Center] ultimately stood. They displaced all [the] electronic stores that existed at that time. I was there with my friend, and he was a great influence on me, Neil Wotherspoon. Neil and I used to travel on weekends up to Radio Row, but not so much for the electronic parts as the fact that there were several key chemistry stores there [also], professional chemistry stores, [from] which we got most of our supplies. But, as a consequence of that, we saw much of the electronic surplus that was available at next to no money, and that's how I got [started in] ham radio. [It was] primarily because for five dollars you could get an awful lot of electronics.

BROCK: An awful lot of surplus tubes? Would that be it primarily?

KLEIN: […] Yes, tubes and all the other components that [went with them].

BROCK: Sure. Was Neil Wotherspoon an age-mate of yours?

KLEIN: Yes, we were classmates through grammar school, high school, and undergraduate school.

BROCK: And you both had developed an early taste for chemistry?

KLEIN: That's correct.

BROCK: Was that…was it getting that chemistry set from your brother that set you on that track?

KLEIN: Yes, very much so.

BROCK: Could you talk about…?

KLEIN: Oh, by the way, if you need a spelling on Wotherspoon, it's W-O-T-H-E-R. Not what you'd expect. He pronounced it Weatherspoon.

BROCK: […] Thank you. Could you speak to what was it about working with that chemistry set that hooked you, or piqued your interest in chemistry?

KLEIN: The experimental part in which you could actually perform experiments and see results. Some of them were very poor. [For] one of the first experiments Neil and I ever performed, outside of emulating what the Gilbert Chemistry Sets could afford, we went in the backyard and generated carbon dioxide. […] We filled lots and lots of balloons expecting them to float in the air. [laughter] Of course they dropped like lead balloons, clearly. [laughter] We were very disappointed. When we discovered why, we switched over to generating hydrogen with very much better results.

BROCK: [laughter] I'm sure.

KLEIN: […] We got very, very serious about what we would do. We would often go to secondhand bookstores and buy serious, college-level chemistry books. Particularly organic chemistry intrigued us, […] so we made all the traditional dye sets that were [described] in those books, and many of the pharmaceuticals that we could generate synthetically. It became a very serious obsession [for] both of us. Neil actually represented a very important part of my chemical background because he was a very, very bright guy. […] He got his doctorate, ultimately, from Poly. He's still associated with that area, […] in Brooklyn, although in a different home than [the one] he shared with his mother. He too became […an] accomplished scientist.

As I was saying, Neil was a **<T: 15 min>** very bright guy, and we [played] a game in which we would pose [scientific] questions to stump [each other]. Each day we would come up with a question…and he had the damnedest questions that put me to reading a lot of books to find the right answer. [laughter] I, of course, tried the opposite, but he always seemed to know the answers [to questions] that I posed to him.

BROCK: It sounds like it was sort of a friendly competition, where you were both pushing one another deeper into your knowledge of chemistry.

KLEIN: Very much so. We enjoyed it. But, understand that we're talking about […] thirteen- and fourteen-year-old boys. I didn't reach Brooklyn Tech until 1945, so it was a few years later. Are you aware what Brooklyn Tech was or is?

BROCK: As I understand it, it's sort of what [one] would call a magnet school, if you will, for…

KLEIN: There was no such term. It's one of the specialty schools of New York City. Have you lived in New York at all?

BROCK: I don't, but I am…I'm aware of…I'm familiar with sort of Brooklyn Tech and the Bronx [High School of] Science and their sort of amazingly…

KLEIN: There's one other that you've neglected. […] Stuyvesant [High School].

BROCK: Oh right. Stuyvesant.

KLEIN: Stuyvesant was in Manhattan, Bronx School of Science was in the Bronx, and Brooklyn Tech was the equivalent school in Brooklyn. Bronx High School of Science and Stuyvesant tended to be more academic. I believe, from some of the people that I've met who were many years my senior, including my brother and some of his friends, that the original intent [at Brooklyn Tech] was to allow students to proceed through a very rigorous engineering and science course [to] come out to become technicians without going to college.

BROCK: Oh, I see.

KLEIN: Remember, this was done during the heights of the [Great] Depression [that] the school was originally set up.

BROCK: Sure.

KLEIN: […] The chemistry course that I took involved three years of chemistry beyond the […] first year introductory science courses. I went all the way through inorganic, qualitative and quantitative analysis, organic chemistry, physical chemistry, and, believe it or not, a chemical engineering course. It was a very, very well put together program, and at the end of that, the American Chemical Society gave each of the graduates […] an examination. […] If you could pass the exam you were deemed proficient as a "junior chemist," as they put it at that point. So, you were a certified junior chemist after four years in high school.

BROCK: I see.

KLEIN: […] Some of the things I learned in high school made college very, very simple for me, […] particularly the chemical engineering course. I used [it] many years later to great advantage at IBM [International Business Machine], because I ultimately became responsible for a chemical engineering operation of vast magnitude based upon my knowledge of that one course [and one in] college at Poly. Those two courses served me very well.

BROCK: Was that chemical engineering work at IBM associated with semiconductor manufacture?

KLEIN: No [it wasn't], as a matter of fact. It was a recovery plant: recovering [the] solvent that they used in [the production of] electronic packaging. [The process] used perchloroethylene, [which became] contaminated with molybdenum powder. That recovery system required very, very thorough control to avoid serious problems. It was the largest [solvent (perchloroethylene)] recovery system in the free world when I ran it.

BROCK: That was for the [ceramic] packaging […of electronic components]?

KLEIN: Right. Exactly. [For the mounting of] chips.

BROCK: I see. Well, going back to Brooklyn Tech, were there various tracks, if you will? So a chemistry track, where you would get that, sort of, depth of exposure? An electronics track, I suppose?

KLEIN: Right. And believe it or not, [in] things like aeronautical engineering. […] They had their own **<T: 20 min>** airplane, which they took apart and put back together. And an architecture track in which they…it was in a special laboratory, which was multi-storied so they could build an entire [two-story] house. It was a remarkable [choice of courses] that you could select.

BROCK: Was there an entry examination to get in, or…?

KLEIN: Yes. For all three of these science schools, Brooklyn Tech, Bronx High School of Science, and Stuyvesant. I passed Stuyvesant and Brooklyn Tech, but because it was much closer to where I lived in Brooklyn, I decided that I preferred that [to] the long trip to Stuyvesant. Stuyvesant, in those days, was in a very rough neighborhood and Brooklyn Tech was much preferable for me because of the ease of transportation and the neighborhood it [was situated] in.

BROCK: To have…you must then have…you went to the public schools before that as well?

KLEIN: Yes.

BROCK: Were you…were you showing an aptitude for science and math previous to taking the entry exam?

KLEIN: Certainly science. I can't say that about math. Math came later. I think more [and] more of it came in graduate school than anywhere else. [laughter]

BROCK: I see.

KLEIN: [People sometimes] speak about chemists [as] being flunked-out physicists [laughter] for that reason. But at least my grandchildren still respect me for my ability to help them [with] their [current] math.

BROCK: You were following in your brother's footsteps in going to Brooklyn Tech. Let's see…. What were your…as your high school career was coming to a close, had you set on an idea of yourself as becoming a chemist?

KLEIN: Yes. It was a toss up between chemistry and metallurgy. There was no equivalent metallurgical course at Brooklyn Tech, and metallurgy intrigued me. But, push came to shove. I decided I'd stay with the course I had the most background [in] and enjoyment. […] It also allowed me to continue using my [basement] laboratory as a chemistry laboratory rather than melting huge chunks of metal and forming alloys in the basement.

BROCK: Certainly there's that connection of materials between chemistry and metallurgy?

KLEIN: I agree. I agree.

BROCK: Could you…?

KLEIN: When we get to it, David, you'll find that Jack [John C.] Sarace, who was the third co-inventor of [silicon gate], was a metallurgist, [but] we never held it against him. [laughter]

BROCK: Was…in that early appeal, though, of metallurgy to you personally, was the aspect of looking at crystals at all a factor in that, or…?

KLEIN: Oh, definitely. […] One of my interests and hobbies had been […] rock and mineral collecting. The two are joined.

BROCK: Right. Right.

KLEIN: To this day I'm still a rock hound.

BROCK: [laughter] Let's see. Well, once you had made that decision to continue with chemistry, was it an obvious choice to you to go to Brooklyn Poly? Or, what were your thoughts at the time about where you might go to college?

KLEIN: I [had] thought of a number of colleges, but [I had the] consideration of staying at home near my own laboratory, and cost problems associated with living away from home. I really only considered the University of Wisconsin, and not for very long. I dropped that and held my breath to get into Brooklyn Poly.

BROCK: Was the…?

KLEIN: […] If I recall correctly my parents paid $150 a semester for me in those days. [It was] private school tuition.

BROCK: I was just going to ask about the financial dimension of financing your college education.

KLEIN: My parents helped me extensively with that, but, of course, it wasn't as difficult as it [is] today.

BROCK: Sure. Did you then commute from home to the campus **<T: 25 min>**?

KLEIN: Yes. You [could] hardly call it a campus. It was in beautiful downtown Brooklyn, [laughter] and trolley cars ran through the street which we had to cross from one building to the other.

BROCK: [laughter] Well, could you describe…you were at Brooklyn Poly from ‘49 to ’53, or…?

KLEIN: No, ‘52. That was an interesting thing. We almost crossed the question you should have asked, perhaps, [of] how [I contributed] to the cost of that education. I graduated [from Brooklyn Tech] in January of 1949. Brooklyn public schools had two graduations a year, but colleges weren’t acclimated to pick up students in mid-year. So they had a special arrangement for those who graduated in January, that you began your freshman year from January through June. From June through August you did the second semester to catch up with your second [half of] freshman [year], so as to be in sync again with the rest of the students. I had no free time that first summer, but thereafter I worked for my father. I was the greatest rug lifter you’ve ever seen. [laughter] […] I did that in high school as well, during summers.

BROCK: That was mainly picking up the rugs to be cleaned and delivering…?

KLEIN: That’s right. […] And delivering them after cleaning. I was the number one aide to […] some other people who were on the truck, driving it and lifting the rugs with me.

BROCK: Was there any chemistry connection to the cleaning of the rugs, or was this…?

KLEIN: Peripherally, yes. […] Stain removals, and so forth. There actually is a rather famous—or famous among rug cleaners—institute that deals with that. They have chemical laboratories, and I was always interested in reading about that end of the activity. [But] most rug cleaners had [at least] a small collection of chemicals for appropriate stain removal. Of course, there were [other] things like paradichlorobenzene, which were moth repellents in those days, and other chemicals involved in spot cleaning, and so forth—[mostly] organic solvents.

BROCK: Right. But unfortunately you seem to have been just transporting the rugs more so [laughter] than [performing] the chemical treatment?

KLEIN: That’s [where] they needed my brute force strength. [laughter]

BROCK: Well, coming out of Brooklyn Tech with such a…

KLEIN: By the way…just an aside. Have you ever seen very heavy iron doors that allow businesses to open them up and proceed into the basement from the sidewalk?

BROCK: Yes.

KLEIN: Well, I do remember that my father's foreman had to drill some holes for venting through that. Now, the steel was probably a quarter of an inch thick, and I saw him boring those holes over many days to make adequate holes. And they were very laborious in cutting it. I suggested to him that we [build] up a wax [ring] around it and drop in an appropriate mixture of nitric and hydrochloric acids and etch our way through. He never bought into that, but when you think about it, it ultimately became a good deal of my professional career after that—etching.

BROCK: Right. [laughter]

KLEIN: I still think about that to this day. That would have been my first application of real chemistry [to] the rug cleaning business. [laughter]

BROCK: To etch through steel. Well, what I was going to say was, coming out of Brooklyn Tech with such a strong background in chemistry, how did that serve you when you got into the chemistry course of study at Brooklyn Poly?

KLEIN: Very well, because with the earlier courses, I had been exposed to…as I told you, I had organic, physical, [etc.]. Many of them were repeats. Some of them [were] slightly [more] advanced, but it was not my first exposure. If I missed anything the first time around in high school, I picked it up the second [time] in college.

BROCK: And did you…you had the…the early focus in chemistry. Did you find your…was it at that time **<T: 30 min>** that you found yourself gravitating toward the inorganic?

KLEIN: That again, like metallurgy, was a toss-up. I had spent, with Neil, most of the time in my chemical laboratory in the basement making all the […] organic compounds, like I told you. It was a toss-up: inorganic or organic. I elected, after great thought as to what I should do, I decided inorganic was my forte. I was particularly good at analytical chemistry, because having so many years of laboratory experience on my own, I really had a deft hand in analytical chemistry. That was primarily what my bent was going to be, [becoming] an inorganic analytical chemist.

BROCK: What sort of…as you got into that sort of study at Brooklyn Poly, what sort of instrumentation were you using?

KLEIN: You're a very good shill. [laughter] Because of my interest in electronics at that time, I wanted to combine my interest in electronics and chemistry, and indeed in those days you had to do an undergraduate thesis at Poly. I elected to do something which was relatively new in those days. I elected to do radio frequency titrimetry, which was to generate an RF [radio frequency] signal, couple it to a chemical system, and you'd follow the changes within that system, as, for instance, you titrated it. Or, you could measure dielectric constant that way, and I did both. I designed my own unique RF titrimeter. It was interesting because my thesis advisor at that time, [who] ultimately left, asked to borrow that system and he took it to Canada. He was in the pharmaceutical industry; [he] used it to a great advantage to do non-aqueous titrations. […] You can pick up changes in non-aqueous media very readily by this RF technique.

BROCK: So this would be…you would look at the chemical systems' absorption of the radio frequency energy, or…?

KLEIN: Well, it's really a complex thing where you're changing both the impedance on the circuit as well as its resistance. Of course, changes [happened] in the system, so […] you could follow any change that occurred within either the resistance or the impedance of the solution…

BROCK: Oh, I see.

KLEIN: …without touching it. [That is, never needing electrodes to contact the solution under study]. The reason that you could do non-aqueous systems is that you never touched [the sample directly]. You were [coupled] externally [to the solution]. It was an RF field that you imposed around [the sample contained in] a very large test tube of about two, two and a half centimeter in diameter, and perhaps […] fifteen centimeters long.

BROCK: So was the test tube…did you have to make electrical contact then to the…?

KLEIN: It was a non-contacting system. You inserted the solution under study into…

BROCK: Into the coil?

KLEIN: …coil. That's right. […] There were various papers coming out that showed that you [could do it] capacitively, you could do the equivalent [measurement] in a capacitive way and, again, non-contacting. But I chose to do it inductively, and it worked excellently in a very simple, very stable system.

BROCK: I see what would happen then. So the changes in the chemical system would affect…you would keep certain variables in the external circuit constant, and the changes in the chemical system would change some of the electrical characteristics that are going on in the…?

KLEIN: You're a smart guy. Yes. [laughter]

BROCK: Okay. Very interesting.

KLEIN: […] We should back up to answer your question […about how] I became interested in polarography. I don't know [whether] you've ever heard of that?

BROCK: I have.

KLEIN: [It] was relatively new when I was in high school. To enter the Westinghouse Science Scholarship…[recently] it has become the Intel Science Scholarship […]. For many, many years Westinghouse ran [this] scholarship. You needed an original piece of work and I worked on polarography […]. What I tried to do was build…polarography **<T: 35 min>** normally was a point-for-point voltage current measurement in which you displayed the characteristics of a solution. But I tried to build an oscillographic system for that Westinghouse Scholarship. It worked [to] a degree. I just didn't have a sensitive enough oscilloscope in those days to do all [that] I wanted. [laughter] But I did develop a rather unusual polarographic cell. I don't know whether you're familiar with polarography, [but] it involves a dropping mercury electrode, a supporting electrolyte, and trace amounts of the material you're looking for introduced into [that electrolyte]. I worked out a glass-blowing nightmare, which I had a glass blower at Columbia [University make] for me. And that [cell] followed me into my first industrial job.

BROCK: Is that so?

KLEIN: […] I was interested in the cross between electronics and chemistry.

BROCK: Well, oftentimes people point to polarography and […] the pH meter as the first instances of [...] marrying electronics with, sort of, chemical/physical measurement.

KLEIN: Probably so. You might throw [the] electronic balances [in] but…

BROCK: Right. Well…that RF work was what you…that was your senior thesis at Brooklyn Poly, you said?

KLEIN: That's correct.

BROCK: Okay.

KLEIN: It was called *The Study of RF Titrimeters and Titrations*. [laughter] So I […] not only built and designed the titrimeter, but also used it extensively in various experiments [in] which I could show dielectric changes, as well as conductivity changes, in ionic solutions. I worked with Dr. [C.J.] Seagers, who was my thesis advisor at Poly, [who] took it to Canada and used it extensively. When he asked me if he could buy it, I named a price, which was $200 in those days. He came back and said, "There's not more than $25 worth of [war surplus] parts there." [laughter] We never came to an understanding, so he shipped it back to me. […] I think he copied it before returning it to me. [laughter]

BROCK: Was he an important mentor for you, or were other members of the faculty?

KLEIN: […] The most important one was a chemist who taught me everything from organic chemistry to…I started in qualitative analysis. But since he wrote the book at Poly…and his name was Paul Spoerri. I don't know whether you've ever heard of him?

BROCK: I haven't.

KLEIN: He was […] a Swiss chemist who had come to this country. [He was both a] very [good teacher] and very personable. I was, at one time, the president of the fine arts club, which [arranged] a music program once a week at Poly. He was the advisor. He always made sure that we had beer for each of those [noon-hour] meetings. [laughter]

BROCK: Was he particularly encouraging to you to continue your steps toward a chemical career?

KLEIN: No more so than most other teachers. [It's] just that I became closer to him than probably anyone else [on the Poly faculty].

BROCK: Did *Solid-State Electronics* or the transistor make their appearance to you during your time at Brooklyn Poly?

KLEIN: Well, yes and no. Actually, the predecessor of the transistor was the solid-state diode, silicon diode, which was used as a microwave detector during World War II.

BROCK: Yes.

KLEIN: Those were available as surplus for nickels and dimes. Neil and I made lots of circuits using diodes, including ordinary radio, crystal radios, which you could do very easily. But we went well beyond that. I was aware of the transistor, and that's why ultimately, when I graduated **<T: 40 min>**—I was very much in love with my wife [to be] who I had been dating for a number of years through high school and college, and instead of going to graduate school I got married the week I left Brooklyn Poly, and proceeded to join Sylvania Electric [Products, Inc.] in Boston, [Massachusetts], because […] they were one of the generators of semiconductor devices, one of the first commercial ones in the United States.

BROCK: In transistor production?

KLEIN: In diodes and transistors.

BROCK: Right.

KLEIN: [In this] particular division [they] made lots of light bulbs, fluorescent light bulbs and flash bulbs. That was big money for them, too. But during World War II they, in conjunction with Harvard [University] and MIT [Massachusetts Institute of Technology], which [were] doing the research work, were actually the production facility, as was Raytheon [Company]. They were getting free research […] support [there for] their production floor. But that's [why] I drifted toward Sylvania to do that work in that particular division. It was the electronics division. They also made vacuum tubes. I do remember when I first got there that somebody had a poster over [his] desk showing somebody […] with a broom sweeping [the transistors] out the door saying, "This is your fight, too." [laughter]

BROCK: What…how did that connection to Sylvania come about? Could you tell a little bit about how you got the job and the nature of the job?

KLEIN: […] Sylvania sent a representative to Poly—one of many—but, of course, I lit up when [I] recognized what they were, who [they were, and] what they did. It was a very obvious and sincere interest in making sure that I had a job at Sylvania. Most of the other organizations that I had dealt with were […] graduate school representatives. And I told you I was about to get married and not be able to afford to be a graduate student with a wife to support. Ordinary chemical facilities, that is, plants, were looking for chemists. Sylvania very closely coupled […] to what I wanted, the mixture of electronics and chemistry. I entered what was called the physics and chemistry laboratory. [This] sounds [grander] than it really was, but nonetheless it put me in proximity [to] some very, very interesting solid-state activities, of which I learned very quickly some of the complexities […].

BROCK: So could you talk about…was…the physics and chemistry laboratory, was that the main…was that a research and development laboratory for the division?

KLEIN: It was primarily development.

BROCK: A development laboratory?

KLEIN: [This was in] a manufacturing facility. You'll recognize, though, it's an early-on [operation]. It's set in a technologically-active Boston […] area. It started, actually, physically, in Boston; […] within a few weeks of arriving there they announced that they're moving from…do you know where Northeastern University is?

BROCK: I do.

KLEIN: One of the buildings that Northeastern is now housed in was part of the factory I worked in. It was on Forsythe Street, which is a back corner of Northeastern University. […] They announced that not only were they moving to Woburn, which is right off [Route] 128, but [that] they were also going to close down for their annual summer vacation for two weeks, which is usual in the Boston area but unusual for the New York City area. I never knew a company simply to literally shut the doors for two weeks. I told them when they came back at the end of two weeks, since I didn't have any money, that they would find a skeleton. [laughter] […] So what they did was suggest that what I could do is paint the [new] plant […] out on [Route] 128. Of course when I got there the painters recognized that I was not a usual painter. And what would you give a kid who insisted on the fact that he was a painter? You'd tell him to paint […the ceiling] of the facility. The [ceiling], of course, was several stories high in that particular **<T: 45 min>** building. It [was] very exciting. There was no air conditioning. It was summer. It was very heady business dealing with organic solvents in a closed facility. I did not like it. I found several hatchways that allowed me to escape to the roof once in a while for fresh air. [laughter] That was tough work for two weeks.

BROCK: You must have been happy to return to the lab?

KLEIN: I was. I was.

BROCK: What was the nature of your work while you were in the physics and chemistry laboratory there?

KLEIN: Well, actually, I was hired as an analytical chemist primarily. But I brought with me that polarographic cell that I talked about. It was still not completely understood what the doping levels were in semiconductors. I proposed that I could develop a technique to find out arsenic levels to a much better degree than they [had been] doing. They were doing it mostly spectrographically. Indeed, they assigned me an electrical engineer and, believe it or not, we built an [auto]-polarograph together and it worked very nicely.

BROCK: This would [have been to] take a sample of…what would it have been? Was it germanium and then…?

KLEIN: Well, it was primarily germanium in [this case]. […] Diffusion didn't come until years later. Even when I got to Bell Labs [Bell Laboratories], and IBM still years later after that, [it was not determined] whether germanium or silicon would be the winner. But germanium was the more obvious thing. And that's the problem they gave me: "Find out the doping levels of the impurities in germanium."

BROCK: I see. And they were, I guess, at this point…how were they diffusing? Were they…how were they putting…

KLEIN: Well, actually, they were…

BROCK: …were they grown junctions?

KLEIN: That didn't come until later, believe it or not.

BROCK: Yeah. I guess that was…

KLEIN: [They] were point-contact transistors originally. There aren't many people alive who [would think of] point-contact transistors [today]. [laughter] The next step after that was alloy diffusion, in which they would use indium or gallium [pellets] to make contact, and to diffuse simultaneously.

BROCK: So, they were…

KLEIN: [But] it did ultimately […] become diffused transistors.

BROCK: Right.

KLEIN: But we went through point-contact, alloy junction, and then diffused.

BROCK: Okay. So, at this point the…it was in…

KLEIN: That makes me a dinosaur.

BROCK: [laughter] It was in some sort of refining process of the germanium that they introduced the dopants [doping agents]?

KLEIN: Yes. As a matter of fact, I neglected to tell you they started from the word go. They literally [purchased] germanium dioxide, reduced it themselves in huge furnaces, and then refined it by zone refining. Are you familiar with zone refining?

BROCK: Yes, I am.

KLEIN: They started with GeO_2 and worked their way up. [laughter] […] One of my friends at Sylvania spent many, many years trying to grow the first single crystal of silicon. Silicon is a much more difficult material to deal with from a semiconductor standpoint. [A] much higher melting point and the fact that [when molten it] wets [almost everything it touches].

BROCK: Right. [laughter]

KLEIN: The thing they ultimately did do was to grow it in silicon dioxide crucibles. But as you grew it, the silicon dioxide [dissolved], and so you always had a high level of oxygen in the silicon (much easier to deal with a 960-degree germanium [melting point]). And it didn't wet everything like silicon did.

BROCK: For how long were you with Sylvania?

KLEIN: […] It was a wonderful two years. It was a postgraduate education.

BROCK: […] And in a very heady time in solid-state electronics as well?

KLEIN: Yes, it was. During that time I managed to lay my hands on some transistors and wrote a number of articles for a national publication of radio amateurs. I don't know whether you've ever heard of it, there's a publication called *QST*.

BROCK: *QST*? No, I haven't.

KLEIN: […] It's a national publication of the American Radio **<T: 50 min>** Relay League. They were very anxious to get articles about semiconductor device applications. So I wrote a number of articles for them. The first one was published in […] 1954, if I recall.[1]

BROCK: And, this was to show…?

KLEIN: An application of transistors. This was a point-contact transistor.

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[1] D.L. Klein, "A Transistor Self-Powered C.W. Monitor," *QST* 28 (January 1954); D.L. Klein, "The Paratone - An RF-Powered Monitor for Break-In," *QST* 25 (August 1954); and D.L. Klein, "A Circuit Symbol for the Junction Type Transistor," *QST* 36 (October 1954).

BROCK: I see.

KLEIN: Raytheon brought out a big advertisement in that same journal a few months later showing two people [talking] and one saying, “How can you afford to build…,” it was called a “monitor,” an RF monitor that I had designed and built. One of the characters in the Raytheon advertisements said, “How can you afford to build that circuit? Why not use inexpensive Raytheon transistors?” [laughter] They had already switched from point-contact to commercial diffused transistors.

BROCK: Interesting.

KLEIN: It was much less expensive. There was a very low-grade…in those days transistors were “post officed.” Do you understand that expression?

BROCK: I don’t.

KLEIN: You made a bunch of transistors no matter what they were or [were] intended to be and you hoped that by measuring their electrical characteristics you could find some that would be functional [for] a particular application. The very low-end diffused transistors that Raytheon made were made for amateur applications and were relatively inexpensive. A point-contact transistor that I originally published on was, I think, seventeen and a half dollars apiece. Now that was big money in those days. You could still buy a vacuum tube for under a dollar.

BROCK: Right. So…

KLEIN: That’s why the Raytheon advertisement about, “How can you afford to build [it]?”

BROCK: Yeah. [laughter]

KLEIN: […] So at Sylvania’s behest I built […] a circuit and published an application of a Sylvania diffused transistor.

BROCK: […] Why did you leave Sylvania after two years?

KLEIN: I had a mentor at Sylvania. His name was Carl Pitha. A very, very astute chemist, and he encouraged me to go to graduate school. I had been going to graduate school at Northeastern in the evenings, but let me tell you: I slept through most of the classes. [laughter] I even got my instructors to recognize the first hour of most of the three-hour course I would be asleep. I still have the notebooks in which I was taking notes, and you can see it change from a conventional printing into a scribble as I fell asleep. [laughter] I left early in the morning. I had a very long drive from—I lived in Back Bay in those days—to Woburn.

BROCK: Okay. That is a drive.

KLEIN: And with all the traffic I had to leave about seven o'clock in the morning to be at [Woburn at eight o'clock]. So it was a long day, coming home, grabbing a quick bite of supper, and on to Northeastern. I decided that that was not a life I would like to [continue with]. I did that for several semesters until I realized that it would take me forever, or I would fall asleep and they would throw me out of school. [laughter] I went to full-time graduate school after that.

BROCK: Did you look at a variety of places? I know that you went to the University of Connecticut. How did you…what was the story of getting there?

KLEIN: Well, first of all, I knew Roland Ward. He was a Scotsman, much like my friend Wotherspoon. He was rather famous in his own right because he's considered the father of solid-state chemistry, but not in the semiconductor sense. He was very big in phosphors. During World War II he had developed, with his graduate students, infrared phosphors. You've heard of sniper scopes and snooper scopes?

BROCK: Sure.

KLEIN: He was the guy who developed the phosphors for those.

BROCK: I see.

KLEIN: But he left Brooklyn Poly because, as every Scotsman should, he loved golfing. And, he found that the time to wait for a course in Brooklyn, was getting damningly long. So he looked for a place like UConn, and he moved to UConn, and he found the golf courses […] much more accessible any time he wanted. [laughter]

BROCK: So, was…**<T: 55 min>**?

KLEIN: I knew him from Brooklyn Poly, [and] knowing [his] background in solid-state, even though it was a different type of solid state, I applied and got into UConn.

BROCK: To work with him?

KLEIN: Yes. Specifically him.

BROCK: I see. How did you find the environment there? Did you have a teaching assistantship of some kind, or…?

KLEIN: Initially I did. I was a teaching assistant the first year. But, thereafter, I worked my way up through research assistant, and, ultimately, the grandest honor a graduate student could have, as an assistant instructor. You could never become a full instructor because they would not […] give you a graduate degree as a full-time member of staff. So I was a half-time member of staff. It got me a grand $4,050 a year, I believe, which was an enormous sum of money compared to my graduate research assistantships. I felt very proud of that.

BROCK: […] How [did] your research project for your dissertation take shape and what [was] the general context for the work […]? I know it was in photochemistry.

KLEIN: Yes.

BROCK: Could you speak a little bit about how that work developed?

KLEIN: How Roland Ward ever got into that end of the chemistry, which was rather foreign to all the other graduate students who worked for him, I'll never know. Except, of course, he probably had a very good reputation in U.S. Army Signal Corps activities. They were interested in finding xerographic techniques. Are you familiar with the term xerographic? Instantaneous photography?

BROCK: Yes.

KLEIN: He had a long-term contract to develop photosensitive materials for [this] particular application. Now, it wasn't an applied type of research. It was really a fundamental piece, but it was all aimed in that direction. Only one other person was really working on the program. Somebody had, just before I got there, gotten a Ph.D. in the same field working with some of the same materials. Roland Ward asked me if I would take that on as my thesis project. I found it intriguing and my first work was in the photochemistry of a solid-state transition-metal cobalt compound.   Almost instantly, because of my analytical background and interest in intriguing chemical technology, analytical techniques, I applied a type of technique that kept me busy for years. It all happened within probably the first week or ten days that I was working on that project. What I did was use paper chromatography [to both prepare exposure samples and carry out quantitative analysis of the effect of the photolysis].

BROCK: […] Okay.

KLEIN: You are familiar with it?

BROCK: Yes.

KLEIN: But this was not the usual two-dimensional type that most people do. I applied circular chromatography—which is a cute way of saying a "filter paper" (high quality rattan paper, if I remember [correctly])—deposited my photo compounds from a microburette in the center of that paper, and formed a spot of roughly a centimeter, centimeter and a half, which would be very reproducible because I could control the drop size that was impinging on the paper. I dried it. I had a maximum surface area for a minimum [amount] of compound. I was able to expose that and do very quantitative measurements in a very acute way. [After exposure, I would] cut a wick, a very narrow wick, into that paper […. Then] drop that wick into a petri dish containing a solvent, and the solvent rises through the wick, spreads out through the dot of exposed material, and lo and behold there was a [distinct color] change. The materials I originally worked with were bright orange in color. Exposure **<T: 60 min>** to ultraviolet [radiation] caused them to change to a bright green color. When I eluted them using the solvent system in that bottom of the petri dish, with another petri dish [put] on top to maintain a saturated atmosphere, and confine the solvents so they wouldn't evaporate too quickly, that it completely separated the complex compound [from the photolysis product]. The [original] coordination compound stayed rigidly in the center, and I was able to separate out a material which I identified very readily, and I could quantitatively elute from that paper and measure spectrophotometrically. I began to get very [good] quantitative results. I spent several years doing that. I got really good at cutting filter paper.

BROCK: And would you…were you using, then, an infrared spectrometer to analyze the paper chromatograph.

KLEIN: No. It was a [UV]-visible DU [spectrophotometer].

BROCK: Okay.

KLEIN: DU was the cat's meow in those days.

BROCK: Right.

KLEIN: Beckman, by the way. I'm sorry.

BROCK: The Beckman DU.

KLEIN: Right. I tend to use these [abbreviations] and expect everybody […] to recognize them.

BROCK: I'm familiar with the Beckman DU.

KLEIN: […] What I did was I eluted from the paper that green material, [the decomposition material, which] turned out to be blue by the way, after it was eluted away from the bright orange.

BROCK: Oh, I see.

KLEIN: I identified it as a cobalt II compound. […] It was eluted from that paper in a thiocyanate solution that forms a very deep-[red] complex. I was able to measure [in] the visible part of the spectrum quantitatively. Is that too fast for you?

BROCK: No. No. […] I've got it.

KLEIN: I was very proud of the fact that I worked out that technique, and I lived with it for years afterwards. [laughter] I should allude to the fact, also, that the first solvent system I tried, which consisted of water [and] acetone in hydrochloric acid, was a system that I stayed with for years after that. [I] never had to alter it. There was no better way of separating those two materials.

BROCK: […] Right from the very beginning you…

KLEIN: Yes. It was one of those things. I had done enough reading about paper chromatography that the first thing I tried worked.

BROCK: Right. So you developed your approach early on in thinking about the problem, and then it was…it was a rigorous experimental road then to…?

KLEIN: Yes.

BROCK: I see.

KLEIN: But that was [for] my master's […]. I was married at that time. I came to UConn with one child. I ended up with four before I left. [laughter] There was no other type of entertainment. [laughter] I decided that the better part of valor would be to aim for a master's degree in case anything happened. At least I would walk away with a graduate degree. In retrospect, it was a waste of time. I wasted the time it took me to do all that work and publish a thesis. […] It was edifying to have been so successful and find that it really did make a [bang]. We got published, believe it or not, in the *Journal of the American Chemical Society*.[2] I remember the editors saying that anybody who could get a straight line in a photochemical experiment deserves to be published.

BROCK: [laughter] Did you then continue the work toward your doctorate […]?

KLEIN: Yes. Unfortunately, the [great] decision was that it could be much more elucidating if I switched over to the same compounds in aqueous solution. And let me tell you, it was a hell

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[2] D.L. Klein, C.W. Moeller, and R. Ward, "The Photochemical Decomposition of the Halides of Tris(ethylenediamine) Cobalt (III) in the Solid State," *Journal of the American Chemical Society* 80(2) (1958): 265-9.

of a lot more difficult. […] First of all, I couldn't use my favorite paper [chromatography] any longer. [laughter]

BROCK: Right. What sort of alternate technique did you adopt?

KLEIN: It was very straightforward. It required a tremendous amount of analytical work though. And I worked in ordinary cells, Pyrex or pure silica cells. I was [asked] to do things like measure the quantum efficiency, which [would have been] a big deal […] at that time. And separate out and find all the degradation **<T: 65 min>** products that [had] ensued from the photochemistry. It turned out to be a darn interesting thing. Difficult initially, but I spent two and a half or three years on [it]. That's the second part. The first part [involved] tris(ethylenediamine) compounds in the solid state. [The] second one was the same compounds in solution.

BROCK: I see. And did you, being so engaged with that work, did you feel that you were separated from the sort of solid-state electronics world you had been in at Sylvania? […] What was that connection like? Or did it…?

KLEIN: […] Well, […] I was pursuing analytical chemistry to an nth degree, which I really enjoyed. I was working [out] my own techniques for elucidating all the chemical reactions that were going on. It seemed like real chemistry to me. [laughter] I was doing a lot of kinetics—kinetic studies—particularly in the aqueous system. It really was a very enjoyable experiment, or a series of experiments, but not solid-state.

BROCK: Right.

KLEIN: But to make up for that, I was very, very active as a radio amateur throughout this. My wife, by the way, is also a chemist [and a radio amateur].

BROCK: Oh, really?

KLEIN: Yes. I neglected to tell you that. In fact, she finished her undergraduate degree when she was pregnant with one of our children. [She took some of] Dr. Ward's courses that she had neglected to take at BU [Boston University] [to finish her undergraduate degree].

BROCK: Huh. And, did she…I mean, it sounds like she must have been very busy with your children, but did she become a practicing chemist, if you will?

KLEIN: No. Not at that time.

BROCK: I see.

KLEIN: No. And when you have four children, all close, relatively close, in age, it's a handful.

BROCK: […] I'm sure.

KLEIN: But she was encouraging to me, let me tell you. […] She became president of the Dames Club, something that may not exist any longer. […Dames was] a club of women whose husbands were graduate students. It was a national organization at that time. She was the local chapter president at UConn.

BROCK: [...During] the work for your Ph.D., what were your thoughts about the direction in which you wanted to head?

KLEIN: I wanted to go back to a mixture of chemistry and electronics. So I limited myself to looking for positions in that industry.

BROCK: Okay.

KLEIN: I had two offers from Philco [Philadelphia Storage Battery Company], which was a big operator in the Philadelphia region. I don't remember all the others. I do remember they were very…oh, yes, Sprague Electric, which doesn't exist any longer but was in North Adams, Massachusetts. One day Dr. Ward came into my laboratory, which was close to where his office was, and introduced me to a headhunter from Bell Labs, who wanted to know why I hadn't signed up for a Bell Labs [interview]. Frankly, I had completely missed [their recruiter coming] completely. But, [by appearing to] be aloof, I think that paid off because he worked very hard on recruiting me and setting up an interview at Bell Labs. After being exposed to that, I was very impressed with the Labs over Sylvania, […] Sprague, and the Philco facilities.

[…] I was just going to say, as an aside, and I don't know whether you want this in publication, but I was on a leave of absence from Sylvania. Towards the end of my graduate studies I sent them a note saying that I was now available to return to Sylvania. I got a note back saying that my background did not prove interesting to them. [laughter] We let it go at that.

BROCK: To rejoin the company?

KLEIN: Yes. At the actual Woburn site where I had left, under good **<T: 70 min>** circumstances.

BROCK: What was…I mean, that strikes me as…I find that very confusing […].

KLEIN: So do I, but it probably passed through the hands of some HR [Human Resources] guy who couldn't care less.

BROCK: Right. Unbelievable.

KLEIN: Who knows what positions [were open] at that particular time.

BROCK: Right.

KLEIN: It was kind of a strange letter to receive after being on a leave of absence in good standing.

BROCK: Yeah.

KLEIN: That's how I ended up at Bell Labs.

BROCK: […] When you went to interview at Bell Labs, were you interviewing for the chemistry department, or…?

KLEIN: Yes. But it was [in] research in those days, which didn't last very long. [laughter]

BROCK: So that was on the research rather than the development side of Bell Labs?

KLEIN: Yes, but I say it didn’t last that long because, by the time I accepted and actually showed up, that particular department which I was associated with turned into a development facility.

BROCK: Oh. [laughter] So…

KLEIN: I [went in] as a research chemist and ended up as a development chemist. The type of development at Bell Labs, though, was a very high caliber type of activity. Probably that was the greatest postgraduate education I ever had. There were more experts under that one roof at Murray Hill, [New Jersey], than at any university that I had ever been to, or have been in since. You could always find an expert in every single field you could think of.

BROCK: […] Well, could you talk about the group that you joined when you…. When did you first get to Bell Labs? In ’59?

KLEIN: In November ‘58.

BROCK: […] Would you describe the group that you joined and who was in it and what it was doing?

KLEIN: Yes. Its function was exclusively semiconductors, and the people in it were predominately chemists, although there were metallurgists, as I told you. Their function was to develop processes and materials for semiconductor applications. Now, in those days, there were such esoteric [questions] as wondering how you could package a germanium device and keep it stable. There were all kinds of arguments as to whether you needed a minimum amount of moisture, or whether you really needed a hard vacuum and the driest atmosphere you could have. Those were the types of problems you had in 1958. There were people trying to grow silicon single crystals. It was still rearing its ugly head after [all] these many years at Sylvania, where I told you one of the people had spent years trying fruitlessly to grow single crystals of silicon.

BROCK: In your group there were people working on that issue?

KLEIN: [Yes].

BROCK: I see.

KLEIN: But […] I joined a group which had some very, very well-educated and long-term experienced people. Probably one of my best friends at that time was Miles [V.] Sullivan. Miles was a rather inventive kind of guy. Did you ever see a drinking bird?

BROCK: Yes.

KLEIN: He's the inventor of the drinking bird. [laughter] He'd let you know very conspicuously that he was receiving all sorts of money from that system because once a month he carried a check [hanging] out of his pocket so you could see the amount of dollars that he was earning that month from his drinking bird. [laughter] He invented many other toys. Bell Labs would allow you to invent anything that didn't encroach on the Bell System. And so he became an inventor of toys.

BROCK: […] Interesting.

KLEIN: He was a very effective and a very, very ingenious kind of a guy. He and I got along famously, as did most of the other people. Strangely enough, one of the first days I was there, [I met a] youngster at a show in 1948—it was the […] fiftieth anniversary of the incorporation of New York City, [when] all five boroughs became a single city. My wife to be and I [had] volunteered to show science demonstrations for that great exhibition which they set up. She was doing Van de Graff generators in those days [that made] peoples hair stand on end, which was always a good [demonstration]. I was showing the applications **<T: 75 min>** of radioiodine to thyroid detection.

BROCK: Interesting.

KLEIN: Anyway, at that exhibit there was a youngster [Ron] who came up and was fascinated more with my wife's demonstration of the Van de Graff generators than [my demonstration]. He was a thirteen-year-old kid at that time, and of course I was a senior in high school, getting to be a senior in high school. He evidenced an interest in ham radio, then spent many a weekend at my house thereafter. He lived in the Bronx. [He] actually moved, at one point, to New Jersey with his family, but still came to my house weekends to study ham radio with me. I put him to good use because he was a climbing monkey, and fearless, as opposed to me who was an orthodox coward. [laughter] He helped me string up many antennas higher than I could have done myself. That first day at Bell Labs, who should I bump into but Ron Finne [Ronald M. Finne]. Ron was in the same group! We continued working in that same group, first together and then ultimately I became his manager years later.

BROCK: Interesting. How big was the group?

KLEIN: Well, it varied in size. The particular part I was in…they had a word for a manager in those days which was called a "supervisor." That particular group was probably about sixteen people.

BROCK: Right. Okay.

KLEIN: Some of which were professional scientists. Some of them [were] technicians. But […] at Bell Labs you could have a master's degree and be a technician.

BROCK: Right.

KLEIN: As a matter of fact, Ron […] had a master's degree from [the University of] Wisconsin. He was still a technician. Miles was a Ph.D. from Purdue [University]. There was a genius of a fellow who came from Yale [University], a very nice guy, Mike Yamen, who had worked for […Lars] Onsager. [Onsager] was a very famous physical chemist [and a Nobel laureate]. Mike Yamen was an equally brilliant chemist. The one drawback Mike had was that people said that you had to be very careful when he entered your labs because the knobs fell off equipment as he entered the threshold. [laughter] He was the sloppiest lab performer you have ever seen, but a genius in terms of theoretical chemistry. [laughter] That's the sort of people who were there.

BROCK: What was…in your smaller group within this larger group, what were you initially working on?

KLEIN: Stupid things. [laughter] […] I told you one of the big problems was to passivate or control the semiconductor diodes, the germanium diodes, which were very susceptible to moisture. […] Somebody had sold them a bill of goods that cadmium sulfide would be a great material to coat germanium with. Why, I don't know. Having absolutely no experience with vacuum systems, I had to set up a vacuum system to vacuum evaporate cadmium sulfide [onto] semiconductors. But we worked our way up from there. [laughter] There were lots of other things we could do later, [through] the years. I became […] the group's expert on etching, chemical etching of silicon, germanium, and gallium arsenide.

BROCK: Well, I did see that. I was looking at some references to your publications, and I saw that, in terms of what you were publishing, in the period from 1960, 1963, that you had a number of publications on etching silicon, and using HF [hydrofluoric acid]. And also…

KLEIN: […] Electropolishing silicon.[3]

BROCK: Yeah.

KLEIN: I did that in conjunction with Miles. Miles did the germanium part and I did the silicon [part]. We co-published.

BROCK: […] Could you describe both the etching work and the electropolishing **<T: 80 min>** work? What sorts of things were you exploring and what was motivating those studies?

KLEIN: Well, […] one of the things was, initially, [to determine] how to separate semiconductor devices. Remember, they were made as individual devices. Integrated circuits hadn't really come to the fore yet—they had been developed in 1959. But […] what they normally did up to that time was [that] the devices were made relatively close together [on a wafer, and] they were separated by a diamond scribe. You [actually scribed] and broke the wafers apart to get individual devices to be mounted. That particular process created two kinds of problems, serious problems. One was that the scribing itself could cause mechanical damage [in] the substrate, and the other was [that] the very act of scribing, and scratching, and breaking could create all kinds of silicon dust all over the place, on the devices. [The dust] hung on electrostatically. I'll talk about that later because I had got a very significant result after that, years later, which resolved that particular problem. They were asking, "Was there a chemical etching technique which [could] solve that problem and do the entire separation sequence by chemical means?"

BROCK: I see.

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[3] D.L. Klein and D.J. D'Stefan, "Controlled Etching of Silicon in the HF-HNO_3 System," *Journal of the Electrochemical Society* 109(1) (1962): 37-42; and M.V. Sullivan, D.L. Klein, R.M. Finne, L.A. Pompliano, and G.A. Kolb, "An Electropolishing Technique for Germanium and Silicon," *Journal of the Electrochemical Society* 110(5) (1963): 412-9.

KLEIN: To do that I studied the entire range of the binary system of HF-HNO_3, and did that with very careful [chemical] stirring at the wafer surface to cause the reaction to occur uniformly across that surface so that we could accomplish it. Incidentally, the way I separated [individual] devices was to coat them with gold [so that] lithographically [it could] delineate the gold into individual-sized devices and etch the gold first, and then use that gold as a mask in the HF-nitric acid system. To my knowledge, although I never made this claim, that was the earliest application of KPR, which was the Kodak Photo Resist, to an electronic problem [in those days]. But it worked very, very successfully.

BROCK: So, just…

KLEIN: [I] found out the optimum concentration of HF to HNO_3 for very uniform etching, and optimum temperature, and being at Bell Labs, worked out the kinetics of the system, and the activation energy of those reactions, and so forth. The thing that was most disappointing to me was that after several years (I think it was maybe a year and a half of this), I had finally arrived at a number for the activation energy for the reaction, as well as a mechanism, and I walked down the hall and asked ten or twelve people what they thought the activation energy was and they gave me numbers all over the place. But, when you added them up and divided by ten or twelve, whatever the number [I had], it came out within a fraction of what it had taken me a year and a half to [determine]. [laughter] That galled me to no end.

It was a very well-received paper. People needed to polish surfaces and I found a region in that composition of HF nitric acid that produced a very highly polished surface.

BROCK: Oh, I see. So, it was a…

KLEIN: […] Originally I was given the assignment to separate with a high degree of control. It ultimately also extended out to polishing of surfaces. It was a chemical [etch].

BROCK: […] How does that relate to the electropolishing?

KLEIN: Well, Miles initiated the work with a huge glass wheel, which we covered with a [conductive layer], with paper. He dripped electrolyte onto [the paper] and made the semiconductor one of the electrodes [of the cell].

BROCK: Oh.

KLEIN: Well, it was a gold-coated glass wheel [that] allowed another electrode to be interposed, only separated by that paper, which [was wet with] electrolyte. By rotating **<T: 85 min>** that wheel against the semiconductor surface you've got an exceedingly flat surface. What I did was I used the same technology and extended it to silicon, and we wrote a joint paper [with Miles Sullivan] after a long, long period of experimental work that we both did separately (but we jointly published together).

BROCK: Just one follow-up question about the separation etching work that you were doing, and the use of gold.

KLEIN: The gold was a mask.

BROCK: The gold was a mask? So would you…?

KLEIN: We had a mask in gold to make the mask, because nitric and hydrochloric acid would [attack the] photoresist—the photoresist was used for the gold masking. The photoresist was stripped, then the gold pattern that we left there was impervious to the HF-nitric acid system and we were able to use that to mask the devices.

BROCK: And the gold didn't…?

KLEIN: Does that make sense to you?

BROCK: Yeah, it does. I was just wondering if the gold was a contamination concern for the underlying device?

KLEIN: […] Yes, but it was on the backside. I neglected to tell you that.

BROCK: Okay. […] Obviously you can tell I was thinking, "Well, if it's from the top…."

KLEIN: No, you couldn't put gold over the device surface [itself].

BROCK: Right. Interesting.

KLEIN: The annoying part of this whole thing is [that] years later, when I got to IBM…and IBM and Bell Labs had a very close association because they had mutual cross-licensing of patents. We had many of [their] people come […] from IBM [to tour our facilities]. We shared that technique of electropolishing with them. They went back and applied it on an industrial scale, the likes of which you would never guess, and all their silicon [for many] years after that was produced by that technique of ultra fine flat surfaces. IBM tended, at least in those days and I still believe it's so, they would give inventors or developers of processes or techniques large sums of money. The person who carried that from Bell Labs to IBM, I believe, got $50,000 for that. [laughter] It made me feel very badly [that Miles and I never shared in their prize].

BROCK: Yeah.

KLEIN: But, anyway.

BROCK: Well, in this period let's just say '60 to '63, were you a member of the technical staff, I guess would have been your job title?

KLEIN: Yes. […There] were…let me see. TAs, technical assistants. STAs, senior technical aides. And […] AMTSs […], associate member technical staff, [laughter] which is what Finne became because of his rather extensive background (he was the fellow with the master's degree). He started as a technician and worked his way up to the highest rank he was going to get to at that time: associate member technical staff. And then there was a supervisor above me.

Something you should know about Bell Labs, at least in those days, was that it was as staid a place as you've ever wanted to see. I worked for the same boss, a supervisor, who then reported to a manager who had a number of supervisory groups reporting to him. We'd speak of it as a first and second level of management. [People] stayed with that same group, fixed, as I was a member technical staff for years, until they each got promoted one level higher. And that's when I got promoted. My relationship to my boss and my boss's boss remained absolutely constant.

BROCK: [laughter] Just that there was now a new layer beneath you?

KLEIN: That's correct. And that's a very awkward thing because, unlike the Army, if you get promoted from the ranks to an officership, you're usually put in another organization. I did not and that was a problem. At least to me it was, that I was now the manager, or supervisor, of people that I had, for years, worked with **<T: 90 min>**.

BROCK: Oh, I see. When did that change occur for you?

KLEIN: You know what? It's one of the few dates I can't tell you precisely without looking it up. I may be able to answer that question if you give me time.

BROCK: […] Well, it must have been prior to '66?

KLEIN: […Oh yes], much prior to that. But I can't tell you precisely. I would say '63 or ['64. It may be] even earlier. I'll have to look that up.

BROCK: Basically your mandate was to supervise this group and to participate, yourself, in this process and materials area?

KLEIN: Yes.

BROCK: Okay. Well, I was also looking at…reviewing some of the publications that appeared in print from you in the 1964 through 1967 time frame,[4] which I…I think this would…I'm guessing that this would be the work that's immediately preceding when you get into the silicon gate story?

KLEIN: […] Yeah. […] That's probably true. I'd have to look at the titles to tell you when and where.

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[4] R. Lieberman, W.R. Bracht, and D.L. Klein, "Surface Preparation of Semiconductors by Group IB Displacement Plating," *Journal of the Electrochemical Society* 111 (1964): 62C; D.L. Klein and C.W. Moeller, "The Photochemistry of Coordination Compounds in Solution. I. Tris(ethylenediamine) Cobalt (III) Chloride," *Inorganic Chemistry* 4(3) (1965): 394-8; K.L. Lawley, R.G. Sobers, S. Knight, and D.L. Klein, "N-Type GaAs for CW Microwave Devices," *IEEE Transactions on Electron Devices* 13(1) (1966): 201-2; R. Lieberman and D.L. Klein, "Temporary Protection of Silicon Surfaces by Iodine Films," *Journal of the Electrochemical Society* 113(9) (1966): 956-8; K.L. Lawley, J. Heilig, and D.L. Klein, "Preparation of Restricted Area GaAs Tunnel Diodes," *Electrochemical Technology* 5 (1967): 376-7; K.L. Lawley, J. Heilig, and D.L. Klein, "Preparation of Ohmic Contacts for N-Type GaAs," *Electrochemical Technology* 5 (1967): 374-5; and R.M. Finne and D.L. Klein, "A Water-Amine-Complexing Agent System for Etching Silicon," *Journal of the Electrochemical Society* 114(9) (1967): 965-70.

BROCK: Right. These were some of the topics of the work that was coming out, that was appearing in ‘64 to ‘67. There were…looked like some additional work on etchants for silicon?

KLEIN: Yes.

BROCK: There seems to be a direct extension of the sort of work that you were doing before. Then there was…I was interested to see there seemed to be some work on…

KLEIN: Gallium arsenide?

BROCK: Well, gallium arsenide, but also on preparing…the preparation of semiconductor surfaces using plating techniques.[5]

KLEIN: Yes.

BROCK: I was wondering if we could talk a little bit about what that work was about?

KLEIN: Well, to prepare—I'm glad you asked [laughter]—to prepare a semiconductor, after you've grown a single crystal of silicon, you slice it. Then you mechanically grind it flat. Then that grinding is followed by a polishing, mechanical polishing, using aluminum oxide. The aluminum oxide grit gets smaller and finer as you go. No one had ever realized, until we took some [electron micrographs] of it, that the aluminum oxide essentially stayed there almost forever…

BROCK: […] On the…

KLEIN: …electrostatically bound to that surface.

BROCK: Oh.

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[5] Lieberman, et al., “Surface Preparation of Semiconductors by Group IB Displacement Plating”; Lawley, et al., “Preparation of Restricted Area GaAs Tunnel Diodes”; and Lawley, et al., “Preparation of Ohmic Contacts for N-Type GaAs.”

KLEIN: Silicon […] almost instantly oxidizes in atmospheric air. So you have silicon which is electrically conductive, separated by a dielectric from another dielectric aluminum oxide. There were little balls of aluminum oxide and they stuck there tenaciously.

BROCK: On the surface of the oxide?

KLEIN: Right. And if you tried to take an electron scan to look at that surface, it was interesting to watch them as you charged them up. And they rebounded from that surface […]. But no one could remove them completely. And it occurred to me from an experiment I had been doing (because a physicist down the hall asked me how you would measure the surface area of silicon powders)…. Now, normally you measure the surface area with a technique, a gas absorption technique, but the equipment wasn't available for it. I came up with a thought that we could electroplate copper onto silicon by just immersing it in a copper solution. We began that work to measure surface area of […] silicon powders by measuring the increase […] of the copper deposit that we put on the surface. But then it occurred to me that that technique could be used to separate that aluminum oxide from the silicon wafers that had been polished. And indeed, the moment you put copper on the silicon, the **<T: 95 min>** aluminum oxide fell off because it was now [resting on] a conductive layer.

BROCK: Oh, I see. And then…

KLEIN: Then you stripped the copper, after you ridded yourself of the aluminum oxide. So it was a way of preparing very […] clean surfaces free of mechanical abrasives, which had been a real bugaboo, but no one had realized how bad it was until we began to look at them. But it was a very simple way. You simply dipped them into a copper solution. And to show you how science follows you, that was a technique I learned in machine shop at Brooklyn Tech when we were working on machining flat pieces of steel. The shop teacher showed us that [if] you took a solution of acidic copper and swabbed it on the steel, it would displace some of the steel and give you a copper surface on which you could scribe the structure you wanted to eventually chew out of that steel. So that was the connection that [gave me the idea of using] copper [displacement plating on silicon].

BROCK: Did that become a widespread technique in the…

KLEIN: Yes. Yes. And in fact it was very key to the silicon gate work [that] we did.

BROCK: Hmm.

KLEIN: It also became a [manufacturing] technique at IBM, when we published it, for them to prepare surfaces that way. They used it extensively for many, many years.

BROCK: In both…

KLEIN: Before I got there. [laughter]

BROCK: But in both…in production settings?

KLEIN: Yes.

BROCK: I see. Perhaps then we could talk…was that copper work, that copper plating work connected at all to […] a publication that I saw from you on protecting silicon surfaces by iodine films?[6]

KLEIN: No. But we also used that technique in the silicon gate. I got a call from a very famous low energy electron diffraction expert at Bell Labs who picked up the phone and said he had never seen a real silicon surface in all his work [with] low energy electron diffraction […]. If you build up any oxide layer on the surface, you're not looking at the surface of silicon. You're looking at silicon dioxide. He said, "Do you know of any way to avoid this?" Don't ask me why (I cannot retrace what my thinking was), but on that [phone] I told him how to do it. I told him to etch the surface with the usual nitric-hydrofluoric acid solution, and then quench it in [an iodine-saturated solution of glacial acetic acid]. I thought that it would completely cover the surface and prevent oxidation. He'd move it into his vacuum chamber, which was [part of] his low-energy electron diffraction equipment, pull a very hard vacuum on it, and then sublime iodine from that surface. […Then] he called me back […] to say it was the first time he'd ever seen a silicon surface directly. [laughter] Up until then, the only way he could do it was by cleaving a silicon ingot inside of his vacuum.

BROCK: Oh, right.

KLEIN: But here he could actually make a very flat, very nice surface without putting mechanical damage in it.

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[6] Lieberman and Klein, "Temporary Protection of Silicon Surfaces by Iodine Films."

BROCK: Right.

KLEIN: He was elated. That's that technique. [It was the first time I had invented anything as a consequence of a few minutes on the telephone.]

BROCK: […] That's very different from the copper work.

KLEIN: Right. But it pertains to a different problem: the oxygen reaction was clean silicon surfaces.

BROCK: Right. Well, could you then speak about the gallium arsenide materials work that you were doing also in this period?

KLEIN: I acquired that group that had been predominately working on gallium arsenide. At that same time a new device was being developed at IBM called a Gunn oscillator. I don't know whether you've ever heard of that?

BROCK: I haven't.

KLEIN: It was a microwave [diode oscillator…]. What it involved was making a microwave device […from] **<T: 100 min>** gallium arsenide [with very stringent electrical characteristics that were difficult to obtain. In conjunction with] Ken [Kenneth L.] Lawley, another member of the group that I acquired, we [developed] a technique which we described in the IEEE journal.[7] That's where you may have come across it.

BROCK: It may have been. I don't have the references in front of me at the moment.

KLEIN: [But] anyway, we were able to control gallium arsenide doping levels [and carrier mobilities required for Gunn devices. For the first time using that technique we were able to produce relatively large quantities of gallium arsenide of the required characteristics…].

BROCK: In the growing process?

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[7] Lawley, et al., "N-Type GaAs for CW Microwave Devices."

KLEIN: Well, yes, in a way. [laughter] Gallium arsenide was float-zoned refined. You know what float zoning is?

BROCK: Okay. That's where you're basically this temperature…you move a heater up and down it? Is that what…?

KLEIN: Well, it was usually [heated by] RF induction. But, again, you have a problem with gallium arsenide that it wets a lot of things. So you do it by actually only clamping the long crystal [at the] top and bottom, and moving [a narrow molten] region [within] a controlled atmosphere. It would depend upon surface tension, not letting the thing drop away. You would only make a small molten zone, and you're moving it [slowly] through the length of the crystal. What I did was suggest that what was happening is the vapor pressure is so different between [gallium and arsenic]—gallium has the longest liquid range of any element of the periodic table, and arsenic has a fairly high vapor pressure. So, when heated, you may have started with a stoichiometric compound of gallium arsenide, but [during float-zone refining], when you heat it to purify it, you cause a certain amount of the arsenic to disappear by volatilization. […] So we worked out a technique in which we could maintain an atmosphere of arsenic above this system to make sure that […] composition wasn't changed from the [stoichiometric value during the float-zone process].

BROCK: I see.

KLEIN: Is that more information than you wanted [about it]?

BROCK: No, no. I'm just working through it. […] It's basically a technique to keep a precisely controlled composition? Or to maintain the composition?

KLEIN: Yes and no. [We didn't know], except […] what we could do is either allow it to be slightly below stoichiometry or slightly above, in terms of arsenic, so that you could actually change the conductivity type.

BROCK: Right. So it was a way to both maintain it [and] control it. Yes, I see. So as we get toward the end of that period, you're now a supervisor in the group.

KLEIN: For several years, yes.

BROCK: For several years. […] I think that brings us to the beginnings of the silicon gate sequence. I'm interested in hearing your impression of what's the very first, the very beginnings, of the silicon gate story at Bell Labs.

KLEIN: Yes.

BROCK: I know that you convened a meeting in early 1966. February of 1966.

KLEIN: Yes. That's correct.

BROCK: […] Is that meeting where you would mark the beginning of the silicon gate story at Bell Labs? […] Could we talk a little bit about that meeting, then?

KLEIN: Yes. What I did was gather my whole organization together and went off to a conference room and spent most of the day discussing it. What had happened was that a very high level manager at Bell Labs posed a problem to me directly. He said that integrated circuits were now coming along and there was a tyranny of numbers. Even if you got, in each process step, very high yield, […] even with yields [as high as] ninety percent per step ([an optimistic number]), if you do five, or six, or seven steps, when you [do the arithmetic] you come out with very low [yield] numbers. Some people had tried to circumvent that [low yield] by building redundancy into the circuits, but that spaced devices further apart, required personalized circuitry joining of these devices, some of which worked and some of them didn't, and you had to bypass [the inoperable devices]. It was very inefficient use of real estate. Real estate represented time, that is, how fast a device could operate. You wanted to get the devices as close as possible and you wanted no redundancy if you could avoid it. So, he was asking the question…he put it this way: "Is there a process? Could you order a whole series of processes which would be go or no-go? They either all worked or they all failed, because it would be better [for] all to fail and you discard that particular wafer and start all over again."

BROCK: […] Who was that who posed you that challenge?

KLEIN: Bob [Willard S.] Boyle. […] He was my boss's boss's boss. [laughter]

BROCK: Okay.

KLEIN: And he's a very well respected electrical engineer and physicist. Or was [at that time].

BROCK: […] So you convened the group to think about these…essentially, it's a hundred percent or zero percent yield?

KLEIN: That's what he originally asked for, a go [or] no-go system.

BROCK: Right.

KLEIN: What we came away with was the idea that semiconductor processing had been built ass-backwards […]. [laughter]

[But] usually what you did was you got a very clean surface, a semiconductor, you put insulating layers of one sort or another on that surface, etched away portions, and then built up the next layer, etched away portions, and you did this repeatedly. What I posed to the group was that we build it the other way around and we start with all the [critical layers in place as much as possible] so that the etching operations would not in any way affect the critical surfaces that we were interested in. The FET [field-effect transistor] was relatively new at that time. People who were making [the gate electrode] usually made it with metals like aluminum. And the big bugaboo of field effect transistors—and, as an aside, I don't know whether you know it or not, but the original idea of the transistor was to build an FET.

BROCK: Yeah.

KLEIN: It never worked [laughter] because they had no control over the surface states of the devices. […] It's a very sensitive measure of how clean your surfaces are. An FET is particularly susceptible to sodium [ions]. The greatest contaminant in semiconductor work is sodium, and sodium is kind of prevalent in this world. As a consequence of that we chose to consider making an FET in a reverse manner—reverse over the conventional [procedures]. We were going to use all the technologies we knew at that point first to generate an ultra-clean surface, and then to preserve that surface while we built the device.

BROCK: Can…just to interrupt you, sorry. Is the question [then] that if the generalized context is about finding a better way—let's come up with a better process for making integrated circuits…

KLEIN: And a material set.

BROCK: And the materials…materials process set to make integrated circuits. It seems that you chose a very particular beginning point and that is…

KLEIN: An individual FET.

BROCK: Well that […] you're going to make, you're going to concentrate on field effect transistors, and consequently, […] then only somewhat [on the] reasonably stabilized MOS IGFET [metal-oxide-semiconductor insulated-gate field-effect transistor] technology.

KLEIN: […] Can you call it MOS when there's no metal there? But that's all right. [laughter]

BROCK: Well…

KLEIN: The idea's the same.

BROCK: […] I guess the then-somewhat-established **<T: 110 min>** approach for fabricating these field effect transistors?

KLEIN: Well, as I told you they were made in a more conventional way initially.

BROCK: Yes.

KLEIN: […] Starting by putting layers on, shaping them, and then adding metals to make contacts.

BROCK: Right.

KLEIN: The critical part of FETs are: one, the surface contamination; [and two], the gate, which sits between the source and drain. It had to be very, very critically aligned between there, and that required precision photolithography. So, the idea was that we were going to circumvent these problems by making a system that would be self-aligned, that would start out with the most pristine surface we could ever think of, and maintain the pristine nature throughout the entire process because we would never open it again to the atmosphere.

BROCK: I see.

KLEIN: You sound dubious or confused at that point?

BROCK: No. […] I was just pausing to try and formulate my question in a different way.

KLEIN: Okay. Go ahead.

BROCK: […] I guess what I'm trying to get at is, in a way, your group was getting together to try and devise a better mousetrap. What I was trying to talk about, what I was puzzling about is what you were choosing to improve upon? So, there are lots of different…

KLEIN: The yield of FETs were very poor. Not only that, but their characteristics, their uniformity, were very, very poor because of the surface states left on that silicon surface. That's the problem that [William B.] Shockley had run into when he tried to build an FET. That was the original attempt of making a transistor. He gave up until years later when it was understood how to control some of those characteristics. This was an nth degree of that in which we wanted very fine control over the electrical characteristics of the device we would generate.

BROCK: So it was clear to you at that point that if […] one could develop a high-yield process with the right materials for making field effect transistors and integrated circuits with field effect transistors, that that would be…

KLEIN: [Of] uniform quality…

BROCK: That would be…

KLEIN: …better electrical characteristics.

BROCK: Right.

KLEIN: That was the key to it. [Yes.]

BROCK: […] But that field effect transistors were going to be sort of…if they could be produced successfully, they were going to be the sort of superior semiconductor device of the future?

KLEIN: Yes. [laughter]

BROCK: […] Because you could [have spent] your time working on bipolar transistors […].

KLEIN: Oh, I did [at the time], too, but this was the biggie. By the way, to show you, I left […] Bell Labs in ‘67. I went to IBM, and it was perhaps ten years before IBM caught onto the idea that an FET could do better than a bipolar device. […] They also came up more recently, since my retirement, with a germanium silicon alloy as a substrate, which I had been working on with Ron Finne in the ‘65 timeframe. [laughter] So, it shows you how large a gap there is between the development [and] research work and actual production. But that's neither here nor there.

BROCK: But in terms of…in thinking about the time when you convened that meeting in February ‘66, I mean, certainly in the sort of silicon semiconductor research community, the field effect transistor was…

KLEIN: Was what?

BROCK: Was *the* **<T: 115 min>** exciting thing?

KLEIN: Yes.

BROCK: That's where people were…there was a lot of exciting work going on at that time in that area.

KLEIN: Well, as much as I told you about the tube engineers wanting to sweep out transistors when I first got to Sylvania, the same sense existed in terms of competition between bipolar and FET devices. There were bigots on both sides of that argument.

BROCK: [laughter] Well, do…

KLEIN: You think that scientists are level headed and logical?

BROCK: I don't think I was suffering under that illusion. But, to go back then to this meeting in February '66…

KLEIN: We have to backpedal a little bit because I haven't explained something.

BROCK: Okay.

KLEIN: Two things. One is that I acquired another group along here. Remember, I had started out with my own group. [There was] this group that I had originally belonged to, as a member [of the] technical staff, [and] then I acquired a gallium arsenide responsibility of another group. Then I acquired [the responsibility for] what was called a Models Laboratory. A Models Laboratory was supposed to evolve processes which you could go in as if you were going to a restaurant and [pick out] a menu of processes and materials and build a device. I had the responsibility of that. It was run by one of my associate members of technical staff who was [directly] responsible for it. But the people who operated there were actually the lowest level [technical] people in the laboratory. I've forgotten what they were called, but they were [classified below that of] technicians. They were simply operators. By using standardized processes and materials […] they learned how to perform them to an elegant degree. My friend Werner Bracht, who was the AMTS I talked about, would confer with [the technical development personnel], and if necessary we would evolve techniques [to accomplish a semiconductor process step]. He built a whole smorgasbord of processes where anybody could go down with the appropriate set of photolithographic masks and he [and the operators] could build them devices to establish a credibility of a process or electrical design that they wanted. We called that the Models shop.

BROCK: That's very interesting. So it was sort of a development production line, or research production line?

KLEIN: […] Yeah. It itself did no development […]. But, of course, we supported that—"we" being the other […] members of technical staff, supported Werner. […Initially, he had] an appropriate set of menus that he could proceed [with for] anybody in the lab [so as to] make devices, [using] these [standardized] sets of available processes and materials.

BROCK: I see.

KLEIN: That's why you may look at this strange picture that appears in both publication and in the patent drawings [of the silicon-gate FET]. This happened to be a readily available set of masks that we had.[8]

BROCK: Oh, in the Models [shop]?

KLEIN: It has nothing to do with what we wanted. It was relatively big, but it was a set of masks that was available to us to actually try this whole thing out. [Not an integrated device]. This was an individual FET of huge dimensions compared to what we were aiming at. But […] masks are probably the time limiting thing in most development work. It takes a long time to develop masks, [or] at least it did in those days. And so we used what was available. Somebody had once designed this set of masks that we could use, and we used them because it would show the principle which we wanted to demonstrate.

BROCK: […] About how many people were in this meeting where you came up with this…?

KLEIN: Well, it started with probably at least ten, twelve people. Most of [them were] the engineers [or] members of technical staff, and the associate members of technical staff. Werner Bracht joined us because it was through his facility, the Models shop, that this thing would be built.

[…] Interestingly enough, he ultimately showed up at IBM after **<T: 120 min>** Sprague Electric. He left Bell Labs to go to Sprague Electric years later. And Sprague Electric closed up, as you may know. He came looking for a job [at IBM] and I was more than happy to help him procure one. Ron Finne also left the Labs, but he went to Japan, and he's still there to this very day.

BROCK: In the semiconductor industry?

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[8] J.C. Sarace, R.E. Kerwin, D.L. Klein, and R. Edwards, "Metal-nitride-oxide-silicon field-effect transistors, with self-aligned gates," *Solid-State Electronics* 11(7) (July 1968): 653-60; and R.E. Kerwin, D.L. Klein, and J.C. Sarace, "Method for Making MIS Structure," U.S. Patent 3,475,234, Filed 27 March 1967, Issued 28 October 1969.

KLEIN: Well, he's in the electronics and other high technology [industries]. He actually acts as a liaison, primarily between U.S. and Japanese firms. At least once a year he comes by and spends several days [in the U.S.] to get [some home cooked meals] and a bed to sleep in. [laughter]

BROCK: Well, people that we haven't yet talked about are…who I…

KLEIN: Kerwin [Robert E. Kerwin].

BROCK: Kerwin, sure. Yeah. And Sarace [John C. Sarace].

KLEIN: Yes.

BROCK: Could you talk a little bit about their roles or their place within the group at this time?

KLEIN: […] I [interviewed] Kerwin at an ACS [American Chemical Society] [meeting] and he joined us in 1964. He came out of Carnegie [Mellon University]. MIT, if I remember, was where he got his master's degree, and Carnegie for his PhD. He was, essentially, a polymer chemist. [That organic background is key to the invention].

BROCK: Interesting.

KLEIN: There weren't a hell of a lot of them [at Bell Labs]. There were some. There was an entire group of polymer chemists developing cable insulation. But he came to my group specifically to work on photolithography. He made major contributions right off the bat. He did it as fast in photolithography as I did with paper chromatography for my master's degree […].

BROCK: Working on resists?

KLEIN: Working on resists, improving them, understanding what controlled them, et cetera, et cetera. But he came to that [meeting] because, obviously, lithography would be a part of the processing.

BROCK: Right.

KLEIN: And it was he, when we discussed the…we're jumping ahead slightly […]. I described how FET worked. Most of the people in the group had never heard of [them, and] didn't understand them. I was able to explain exactly what we were after and how they functioned, what the stumbling blocks were, and particularly the fact you cannot heat metals like aluminum towards the end of the process while you're doing a diffusion of the [dopants], because it's completely incompatible. The aluminum will melt and alloy and you've got all kinds of problems with that. It was he who came up with the dumb idea, [laughter] kind of naively […], "Why don't we use silicon?" We explained that you couldn't put silicon on the surfaces we were dealing with. We were dealing with oxide and nitride surfaces, and that it would not be a single crystal, it would be polycrystalline. He didn't necessarily understand the significance of that. He'd only been in the Labs two years at that point. As I said, [he had been working in the] entirely different aspect [of organic compounds. It was] not that he didn't know about semiconductors, but he didn't understand them intrinsically. Maybe that's a bad word to use. [laughter] Intrinsic semiconductors are something else; it has an electrical meaning. But, when we thought about it, we said, "Why not try it?" Now none of us had had experience with polysilicon. [Do] you understand what polysilicon is?

BROCK: I do.

KLEIN: […] It has many, many crystalline interfaces. And we didn't know how it would behave. We had no experience etching it. We had no experience diffusing it. The whole idea was that it would form the electrode on the gate and that it would become heavily doped as we diffused into the conventional silicon single crystal on each side of it. It turns out that because it was polysilicon with many crystal interfaces, they were able to pour enormous amounts of dopant into it while we did the more conventional slow diffusion into single crystal silicon.

BROCK: So, it…

KLEIN: As a consequence it became a metalloid. It essentially had conductivity of a not [too] bad metal.

BROCK: Just so I know that I understand correctly: it was because of its polycrystalline **<T: 125 min>** form that during the diffusion process, when you're forming the source and the drain […] that the gate electrode material, the polycrystalline gate electrode material, would more readily…. The dopants?

KLEIN: The dopants. Yes.

BROCK: Would more readily diffuse into the polycrystalline material?

KLEIN: Yes.

BROCK: Interesting.

KLEIN: But we had to establish that for sure.

BROCK: Right.

KLEIN: We also had to establish that we could control etching on [them too]. We did all that. That was part of the job that we did. There were other experts who knew how to put down silicon nitride. That was a relatively recent material [in semiconductor structures]. We knew how to etch it because that was a recent development. Silicon nitride is one of these materials that is impervious to most etchants. It turns out [the etchant is] hot phosphoric acid, which is as close to a universal etchant as you'll ever find. [laughter] We knew how to [deposit] very thick oxides by what is called EOS (ethyl orthosilicate) deposition. By [thermally] decomposing the […] orthosilicate we could lay down very thick layers of oxide. That's one of the ultimate layers that we had. I don't know whether you've looked at the patent to see the idea of self-centering. Also it's shown in the paper how if you were off center with the silicon gate, it would reorient itself directly in the center.[9] It was a self-centering device.

BROCK: Right. Because, […] essentially, the gate material was acting as a diffusion mask?

KLEIN: Right. But also an etching mask.

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[9] Sarace, et al., "Metal-nitride-oxide-silicon field-effect transistors, with self-aligned gates."

BROCK: That’s right. Yeah. So when…

KLEIN: Unlike the aluminum, which could not, first of all, survive a diffusion, but secondly couldn’t survive an etching operation, which would etch the oxides and nitride [layers].

BROCK: Right.

KLEIN: The polysilicon could survive [them].

BROCK: […] Dr. Klein let me just pause my recording here for a moment […].

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INTERVIEWEE: **Donald L. Klein**

INTERVIEWER: **David C. Brock**

LOCATION: **By Phone**

DATE: **9 March 2006**

BROCK: This is a second interview with Donald L. Klein, conducted by David Brock on March 9th, 2006, by telephone. Dr. Klein, as we were just discussing, I thought we could return to the beginning of the year 1966 and talk about the challenge that one of your colleagues, Willard Boyle, gave you about developing a go/no-go process for semiconductor manufacture.

[…] Would you tell me a little bit more about the context of how he issued that challenge to you and if it was in the context of manufacturing, dealing with the yield problem for manufacturing integrated circuits or discrete devices?

KLEIN: Well, integrated circuits had certainly been introduced at that time, but it was the tyranny of numbers […], because semiconductors required multiple processing [steps]. Even in high-yield [for] single steps, when multiplied out by eight or nine steps, would yield very low levels of working devices. And it was anticipated that the problems of integration […] would preclude having redundant devices because they would cause large areas [of the devices] to be consumed. Since interconnections in integrated circuits represented [circuit] time, it would defeat the whole purpose of making small devices spread further and further apart because of the need for redundancy. So that was ruled out.

BROCK: I see.

KLEIN: […] The fact is that you would have to do a discrete wiring pattern after you found the devices that were operationally good, so that it would require a personalization of the wiring pattern. That would be another reason to defeat that type of redundant technique [that] was being considered.

BROCK: I see.

KLEIN: Does that make sense to you?

BROCK: It does. […] In terms of go/no-go processes themselves, […] in which one would achieve either one hundred…

KLEIN: A very high yield….

BROCK: Yeah. Or…?

KLEIN: …or a disaster, and you [would simply toss] the device away. By the way, that was his challenge. Whether we answered it or not is another question.

BROCK: [laughter] I was wondering about go/no-go processes themselves. Was that […] nomenclature, that process concept, something from general manufacturing art or chemistry?

KLEIN: No. It was something that he simply threw out. I believe he coined it at that meeting where he enjoined us to do that.

BROCK: So it was not a term of art?

KLEIN: No.

BROCK: Okay.

KLEIN: […] One thing I should tell you is that [the group I led was a group of chemists and metallurgists]. Bell Labs was predominately led by physicists, and we felt particularly challenged by this because we felt that devices, up until that point, had always been made in the wrong way. […] They would be made layer by layer, each requiring a new photolithography step. Photolithography is not necessarily a high-yield process, particularly for small devices.

BROCK: Right.

KLEIN: So, what we took as a challenge, or at least what I took as a challenge. was to devise a technique in which we would build it the way a chemist or a metallurgist would wish to do it rather than the way a physicist had been traditionally making devices. We took, in particular, an FET at that time—FETs were relatively new, even though they were the original type of device that had been sought by Shockley […]. The biggest challenge in FET [was] the [control] surface states **<T: 05 min>**. Surface charges were what prevented Shockley from achieving that type of device years earlier, and it was still a problem within the [electronics] community. I think we spoke about sodium [ions] being a particularly bad contaminant in FET devices, and the world is permeated by sodium. [laughter] So, we took that as a challenge. We weren't asked to make FETs. We were just asked to develop a go/no-go process, and we elected to go that way because it would allow us to do several things: incorporate processes that we had developed over the years, to clean surfaces to make them their pristine best, and then seal them by layers that we would introduce while the [structures] were still clean before we did wet chemical processing, where the majority of contaminants arise.

BROCK: […] Your choice to concentrate on a process for making field effect transistors, either discreet devices or included in integrated circuits, was based on the group's prior engagement with surfaces. Is that fair to say?

KLEIN: Well, that was one of the prime means of testing whether this type of procedure or process would be successful, locking in an ultra-clean surface before wet chemical processing would be invoked.

BROCK: So, in essence, the idea…and to contrast the chemist's approach from the physicist's approach that you were just mentioning, your starting point was that you would first create these critical surfaces in as pristine and clean, and therefore stable, a form as possible, and then do additive and subtractive steps in terms of the wet chemical processing that would leave those critical surfaces as intact as possible?

KLEIN: You got it.

BROCK: Okay. [laughter] […] Good.

KLEIN: […] The further test of that whole thing is a photolithographic one, because the three elements, not meaning chemical elements, but the three electrical elements of a FET are the source, the gate, and the drain. The gate is particularly most controlling and sensitive to contamination, but also, from a photolithographic standpoint, it had to be centrally located within […] the source and drain areas. It represented a photolithographic challenge as well. So, we were going to address that by this process. That meeting took the majority of the day, believe it or not, to work out all of the details of building a chemist's dream of what a semiconductor FET would look like and the process steps that we would invoke. At that meeting, Bob Kerwin was the guy who suggested…, when we came to a critical position of how would we get the gate electrode in place before we did diffusions? In those days gaseous diffusion was the way of doping. Ion implantation did not come along as a process until later. He […] had been very active in our photolithographic work, and knew a modicum about semiconductors. Nonetheless, he was at that meeting. For obvious reasons we were going to have to do photolithographic steps, but he came and blurted out, "Use silicon [as the gate electrode]." We explained to him that you can't lay silicon on anything other than a silicon surface to come up with single crystal. Then it suddenly occurred to us that perhaps he was right. Silicon sitting on top as an electrode for the gate contact would be satisfactory [and could] meet all the needs and reactions of…silicon could survive thermal processing, unlike metals. Traditionally, aluminum had been used for **<T: 10 min>** that purpose, and aluminum could not survive diffusion temperatures. So what was blurted out, probably naively, was actually a very critical comment. We had to test it. [Deposition of silicon on amorphous silica would be expected to lead to polycrystalline silicon.] We had no experience with polysilicon. […I had responsibility] for a group called the Models Laboratory. [In it, it was possible to select] standard [processes] by […] choosing [from] a menu […as practiced there]. [In that way] you could [readily] build devices [to evaluate a new design. In essence it was like choosing to build devices by selecting from Column A, Column B, and Column C]. We began [by looking at polysilicon]—how we could shape it and how it would behave in diffusion. We were very, very happily surprised that we could not only shape it well, but during the diffusion process the polysilicon, because it was [poly]crystalline, had many grain boundaries in it. [It] would soak up the diffusants that were normally meant to diffuse into the source and drain areas. [They] would soak into the polysilicon that was sitting on top of an insulator to form the gate structure. […] The resistivity of that polysilicon film dropped to about four ohms per square. I don't know whether you're familiar with the term?

BROCK: No. Very much like a metal, though?

KLEIN: Well, it behaved like a metalloid at that point.

BROCK: Right.

KLEIN: It was loaded with [the] diffusant that [was] going into the single crystal areas of the source and drain, but soaked into the polycrystalline materials sitting on top of the insulator. But I jumped ahead a little too fast. The whole idea was that after we did the best we knew how to [in preparing] the silicon surface free of mechanical damage—which I've talked about before, and we normally [etched] that—we used the best etching techniques we knew and also did a copper displacement to remove […] abrasive particles which had been used in the earlier steps of forming a flat silicon wafer. We then locked that surface by doing what is called a steam oxidation to form an oxide over silicon. Steam oxidation formed SiO_2, which was the highest quality of silica in those days that we knew how to make […].

BROCK: Okay.

KLEIN: What we did was we formed a very thin layer of [silicon dioxide] to lock that surface as we left it as pristine as we knew how, then laid down a relatively new [thin film of silicon nitride…this material] had been worked on by others in the area […]. Silicon nitride has the marvelous characteristic of not allowing sodium ion migration. So the silicon nitride sitting on top of the silica represented a barrier [towards] sodium migration […]. We had silicon with silicon dioxide with a very thin layer of silicon nitride above it.

BROCK: Was the silicon nitride, in addition to being a sodium barrier, […] also important for its different reactivity to the etchants?

KLEIN: Yes. Exactly.

BROCK: Okay. Could you…?

KLEIN: What we were going to do was, ultimately, do a number of steps of photolithography, but each one would be self-aligning and self-limiting. That was the whole key. You could use an etchant on silicon nitride, which would leave silica impervious […. This was] hot phosphoric acid, by the way […].

BROCK: Right.

KLEIN: Silicon nitride dissolves in hot phosphoric acid whereas silica does not.

BROCK: So that would allow you to just etch that silicon nitride layer?

KLEIN: That's correct.

BROCK: And that very thin oxide layer **<T: 15 min>** above the clean silicon surface would remain intact?

KLEIN: Yes, masked by the silicon nitride.

BROCK: I see.

KLEIN: But that's not the end of the process. The next layer that was added was a […] relatively thick layer of silicon dioxide put down by chemical vapor deposition.

BROCK: Okay.

KLEIN: Typically it was [with] tetraethylorthosilicate, a gas which you could thermally decompose and build a relatively thick layer above the initial two layers [of] silicon dioxide [and] silicon nitride. […] Not as good quality in terms of electrical characteristics, but here it was primarily used as a spacer.

BROCK: As a physical spacer or as an insulator or…?

KLEIN: [Both]. Well, it certainly was an insulator. The idea was that if we had to carry electrodes along the [upper silica] surface, we could physically separate it from the key elements of the device. It would remove the electrodes [from interacting electrically with the underlying silicon].

BROCK: Okay.

KLEIN: Now you see, what we've done is prepare a pristine silicon surface, laid down the best we know how [of] a silicon dioxide layer, of a very thin layer of silicon nitride, and a very thick layer of chemical-vapor-deposited silicon dioxide.

BROCK: Right.

KLEIN: This is the first time, then, that the […] device will [encounter] wet chemistry [but in a noncritical electrical area of the FET]. We used photolithography to expose the rough outline of the width we needed for the source, the drain, and the gate electrode, and simply etched down using hydrofluoric acid through the SiO_2 layers. This would open a […] relatively large gap all the way down to silicon nitride. Silicon nitride does not react at nearly the same rate with HF [as] the SiO_2 layers […]. Then what we would do is, looking down on that silicon nitride, we would put in the gate [electrode], and at that time we also introduced electrodes on the surface that would be the contacts for the […] source and drain.

BROCK: […] That was through a deposition of the polycrystalline silicon? Was that also a vapor deposition […]?

KLEIN: Yes. Although it could be easily a chemical vapor deposition, much like you would use for […] epitaxial growth. Are you familiar with that term?

BROCK: I am, yes.

KLEIN: Okay. Now we have a big hole down to a silicon nitride layer. What we did was use a second step of photolithography to outline where we wanted the silicon nitride to remain and where we were going to etch down [through that layer…] by hot phosphoric acid.

BROCK: Okay. To define the source and the drain?

KLEIN: Right. What's key to this whole thing is that the silicon nitride layer that was left [beneath] the gate would be self-masking for all the other steps in the operation and would always remain centered, because, while we used the HF to remove the other silicon dioxide, it would self-center the gate protected by silicon nitride and would always remain in the center of that structure. That may be a little difficult for you to follow, but if you look at the pictures we have in publication I think you'll recognize it.

BROCK: Right. So underneath the polycrystalline gate there would be both…?

KLEIN: Silicon nitride **<T: 20 min>**.

BROCK: And then the silicon…

KLEIN: Dioxide.

BROCK: […] And then the silicon surface?

KLEIN: Yes.

BROCK: Okay. Yeah. I can see that.

KLEIN: And the polysilicon would be sitting on the upper surface of the very thick oxide. On the other side there'd be an interconnect for the […] source and drain. By removing it that far from the surface of the silicon, it would have less influence upon the electrical characteristics, […] depending upon field effects. You don't want those other electrodes to have influence on the silicon. That's why [there is a necessity] for a thick SiO_2 layer, whereas the [polysilicon] sitting on top of the silicon nitride, down in that trench, was very close to the silicon and therefore could be used to influence the conductivity.

BROCK: I see. […] In some things I've read I've heard part of this structure that you just described referred to as a "sandwich" structure.

KLEIN: Well, the sandwich would be the sandwich of the silicon nitride and the silicon dioxide.

BROCK: The nitride between the…?

KLEIN: Under the gate.

BROCK: Okay. That's the sandwich ?

KLEIN: The "sandwich" structure.

BROCK: I see. So the […] bread is the two oxide layers and then the filling of the sandwich is the silicon nitride layer. Is that correct?

KLEIN: No […]. The polysilicon is sitting on top of the gate, [just] sitting on silicon nitride, is the bread. The silicon nitride and silicon dioxide under that gate are […] the multiple layers of the filling to that sandwich. And the next silicon, the single-crystal silicon, is the other end of the bread.

BROCK: Okay. Understood.

KLEIN: So, you have silicon, silicon dioxide, silicon nitride, polysilicon.

BROCK: Okay. And that…

KLEIN: And that's your sandwich.

BROCK: Okay. Thank you. […] Was it in this day-long meeting that the process, the essentials of this process, that we just talked through were laid out?

KLEIN: Yes. And each person there with his own specialty was given the task of investigating whether there were any showstoppers in any of those [procedures] that we conceived of that day.

BROCK: I see. Was…?

KLEIN: For instance, how easily was polysilicon shaped by photolithography? What would be the conductivity after you did a diffusion? […] Problems like that.

BROCK: Right.

KLEIN: But each one of those steps was individually tested.

BROCK: Did you, in creating this sort of chemist's dream process, did you deal at all with issues about the formation of the initial single-crystal material for the silicon? Or, was that…?

KLEIN: That was routine.

BROCK: That was routine.

KLEIN: What wasn't routine was that we were applying everything we knew to make that surface as good as we knew how. You understand how this differs from conventional device manufacture? Usually, you would grow an oxide layer on top of silicon, with or without all the intricate steps of surface cleanliness that we invoked. You would etch […] that very thin layer. Now you're stuck immediately in the second step of the process, having exposed it to an aqueous solution and photolithography, which can contaminate all the surfaces. This is why ours was a backward scheme. We're going to put all the layers in place before we ever exposed it to wet chemistry.

BROCK: I see.

KLEIN: It was completely [the] reverse of all procedures up to that point. Usually you made a layer, shaped that layer, added another layer, shaped that layer, and so forth. But each one gave you the opportunity to expose the material to contamination as you went through **<T: 25 min>** wet steps.

BROCK: Right. Right.

KLEIN: Wet steps we invoked in the process to make our FETs were such that the original surface never saw wet chemistry in the critical portions under the gate. They were always self-protected by the two layers of insulator—silicon dioxide and silicon nitride.

BROCK: So, in essence, the single crystal surface was exposed only at one point for the dopant diffusion?

KLEIN: Exactly. Well, it was exposed to wet chemical etchant to get down to that surface.

BROCK: Right. And then a diffusion. And then was it covered with another oxide layer after that?

KLEIN: No. […] It could have been but we didn't.

BROCK: Okay.

KLEIN: You could, again, if you wished, at the end of this whole thing, encapsulate it in a chemical vapor deposition [of] silicon dioxide. You could have.

BROCK: Right. But it was the surface states of the single crystal surface beneath the gate that were critical […]. Not so much the electrical surface states on top of the source and the drain?

KLEIN: Yeah.

BROCK: Okay. I understand.

KLEIN: That's the critical aspect of it. Lo and behold, when we tested the devices, they all failed. [laughter] And we held our heads for a long time on that one.

BROCK: What was the…?

KLEIN: The problem was that we had built stress into the system. The expansion coefficients of silicon, silicon dioxide, and silicon nitride are [all] different […]. As a consequence we had enormous amounts of stress at the interface under the gate electrode…

BROCK: Mechanical stress?

KLEIN: …because of the different thermal expansion [in the various thin films].

BROCK: Yeah. Okay. Okay.

KLEIN: Yes. The impact was that it created all kinds of […] surface states, which made the devices fail. And then […] information came to us—I don't remember the details of how…I believe it was recent literature that pointed out all we had to do was anneal that system in a hydrogen ambient. All the devices that had failed suddenly became wonderfully good. [laughter]

BROCK: […] At what point would you do the anneal?

KLEIN: After all the devices were formed.

BROCK: […] That somehow repaired these stresses?

KLEIN: Well, it relieved the stress within the system.

BROCK: I see.

KLEIN: If you ask me what [the anneal] temperature [was], I cannot tell you at this moment. You'd have to read the paper.

BROCK: [laughter] I can do that. Well, to go back to that day-long meeting, do you remember where it was that you met?

KLEIN: Oh sure […]. In a conference room a few doors removed from Bill Boyle's office.

BROCK: Okay. And was this…to my ear, it sounds like an atypical group meeting or am I wrong on that?

KLEIN: Atypical in what sense […]?

BROCK: It just seems like to spend a day inventing a new process from the ground up…well, that's my question to you: was this a typical sort of a group meeting, or…?

KLEIN: Yes, in that respect, [but] we were looking at it as a challenge that chemists could do better than physicists had done lo these many years.

BROCK: Okay. […] From my background research I understand that after this day-long meeting, then you recorded the outlines of the process, I suppose, in your lab notebook? Is that right?

KLEIN: Actually, it wasn't entered into the lab notebook but into an internal memorandum, showing the steps of procedure and what was expected to happen at each step. That particular document […] **<T: 30 min>** was dated February 19, '66, and I don't remember whether we ever established a precise date in February. But that became a very key matter in patent litigation, and established when we first started the work. Thank goodness we showed due diligence after that, because it appeared that all the work was noted in various lab notebooks […].

BROCK: Was…then I understand that it was quite soon after the February meeting, in early March of 1966, that you had a meeting with Hugh Cleveland?

KLEIN: Yes. He was my immediate manager.

BROCK: […] Was that meeting to establish […] the making of actual devices according to this process?

KLEIN: Yes, with enough devices so that we could show some statistical information about the procedure and how efficient it was.

BROCK: Was Cleveland himself a chemist or a physicist?

KLEIN: He was a chemist.

BROCK: Okay.

KLEIN: But the key to the patent litigation […] was the fact that he could commit a number of resources necessary to support an operation that showed continuity and diligence.

BROCK: That's what I was just going to ask about. What were the resources—in terms of people, time, materials, and equipment—that this new project required?

KLEIN: I can't tell you at this time. […] You may have a better look at it than I do. I don't have the court documents, but on that document Cleveland penned in next to my outline of the entire process, we came to an agreement as to how many people would be required for each of the steps of development. That was considered in court very critical that a manager at a higher level than I would make a commitment of [resources…] for that purpose.

BROCK: To show the continuity of effort?

KLEIN: And diligence. [laughter]

BROCK: Right.

KLEIN: A court term, not mine.

BROCK: Right. [laughter]

KLEIN: But real people were being committed, which is a cost factor, of course.

BROCK: So I guess it must have been…was it in April […] of '66, or right around then, that you had that first batch of non-working devices?

KLEIN: Yes.

BROCK: Okay. And then it seems that by…

KLEIN: The following month, [in] May, we had actual operating devices. I believe that was when we introduced the annealing process.

BROCK: I see. And…

KLEIN: Again, I want to stress that this is a very unusually-shaped FET. For one thing, it's—physically—relatively large. [Remember], one of the most difficult things we had to contend with was the availability of photolithographic patterns, masks.

BROCK: Yes.

KLEIN: Those take an inordinate amount of time to generate and make. What we did was we hunted around and found a set of masks that were available and could serve our purpose as a […] model device. So you see [there are] extraneous electrodes on there which we made no use of but took up some of the surface.

BROCK: […] I didn't realize that there were extraneous electrodes.

KLEIN: Well, one of them is and one of them isn't.

BROCK: Okay. [laughter]

KLEIN: We were willing to use that as a vehicle to test the whole procedure.

BROCK: In essence to get the work going fast?

KLEIN: Right. Exactly. […] Meanwhile, you understand that there were parallel efforts being worked on at the Laboratories to build FETs at high yield using aluminum contacts. In essence, we were in a **<T: 35 min>** competitive race [of] who could make FETs the best and fastest.

BROCK: An internal as well as an external competition?

KLEIN: Primarily internal.

BROCK: […Were] your aluminum competitors devising different process steps to try and improve the yield for field-effect transistors?

KLEIN: Yes.

BROCK: I see.

KLEIN: But they were building their devices in conventional ways.

BROCK: Right. […] In essence, there was a team, or your entire group was working on…was this the major project for your group in this period through May 1966? Or…?

KLEIN: It wasn't the entire group. The entire group had met for the formulating meeting in February, [and] made their contributions or did not make their contributions, as the case may be. It really devolved down to a specialist in each of the steps concurring that it could be done, and then it came further down to a handful of people who were devoted to [practicing] that process. I don't remember the exact numbers. I would say that no more than two people, two full-time people, besides [Bob Kerwin and I]. Bob was responsible for the photolithography. I was leading the program. The metallurgist in the group, named Jack Sarace took on the bulk of the hands-on type of operation. We subcontracted […] some of the electrical [characterizations] to the groups that normally would be building FETs. They were willing to cooperate [in measuring] our devices as we produced them.

BROCK: I see.

KLEIN: We had the whole use of the models facility that I told you about.

BROCK: Okay. When you had […] working devices, […in] May [and] June [of] 1966, what was the internal reaction among your group, you personally, and then Bell Labs sort of more largely?

KLEIN: Where do you want me to begin?

BROCK: [laughter] Anywhere you like.

KLEIN: [The feeling was particularly of excitement]. I remember that the first wafers that had been checked by a fellow named Joe [Joseph J.] Kleimack, […who] told us those devices were good. I happened to be standing there looking over his shoulder while he was making those measurements. Bob Kerwin, at some point in the litigation, indicated that I came running down the hall yelling, "It works! It works!" almost like a "Eureka!" moment.

BROCK: [laughter] Right.

KLEIN: I was very enthusiastic and very happy over the results. Bob Kerwin, who felt that his contribution was very important, was also very happy about it. Jack Sarace, who had most of the hands-on activity other than the Models Laboratory, was particularly enthusiastic about it. But […] my immediate management at that time did not necessarily support this effort because they didn't fully understand it.

BROCK: They didn't understand the implications?

KLEIN: Yes. It's hard to believe, but that was so. I told you there was a certain amount of competitiveness in this whole thing. We had to build up statistical data that showed it wasn't a fluke, that we had made a single wafer that could carry dozens and dozens of devices, [so we produced] hundreds and hundreds of devices. We proceeded from that point [onward]. I do remember that Jack Sarace went to **<T: 40 min>** a meeting [of Bell Labs] in Allentown, Pennsylvania. At that time the Bell Telephone Laboratories had sub-laboratories at each of the manufacturing sites of Western Electric, and one of them was at Allentown, Pennsylvania. [Sarace] gave his talk about how we were building these devices and how they operated. It was received well, but I don't think anyone gave up what they were doing at that point to get on the bandwagon. It was somewhat about this time […], somewhere along the way something happened at Bell Labs which was astounding. The astounding part was a reorganization.

BROCK: This is in the latter half of 1966?

KLEIN: […] I have to go back and look at the records, but probably. Unfortunately, my boss, Hugh Cleveland, […] lost his position and had what was deemed a lateral arabesque, which meant that he lost control of the organization he had put together and led for many, many years. I worked for him, essentially, for [eight] years with no [major] change in the organization. His competitor in the parallel organization, which was predominately a materials group dealing with things like batteries and solar cells […], came to the forefront and my group was folded into that. Now, this [the leader of the materials group] considered us the [competition], if you will, because we represented the organization which was parallel to his and he saw it as a great competitor. He did not treat us terribly well.

BROCK: […] What was the other fellow's name?

KLEIN: […] His name was Donald [E.] Koontz. [Almost] the same initials as mine […]. I remember at one of the meetings where we were dealing with a federal patent suit in Delaware, they had him come in as a […] witness. When the [Bell] attorneys interrogated him, they dismissed him because he was the most [antagonistic] person [they] had ever met. [laughter]

BROCK: […] The silicon gate process for making a field-effect transistor [was] under Hugh Cleveland? Then [it was] under Donald Koontz, in a separate materials organization? That was where this competing metal gate process innovation…?

KLEIN: No […]. I may have misled you. It was not making FETs by the aluminum process. That was a group led by physicists, for the most part. At some point you'll see a fourth name tacked onto the article that appears in [*Solid-State*] *Electronics*.[10] [Do] you see the name there?

BROCK: […] R. Edwards [Roger Edwards].

KLEIN: Right. He was an electrical engineer [and] physicist who was responsible for a group trying to develop the aluminum FET.

BROCK: Okay.

KLEIN: His name appeared on that document primarily for two reasons. By the time the document appeared I had left Bell Laboratories.

BROCK: By 1968, when it came out?

KLEIN: Yes. […] I left actually in '67.

BROCK: Okay.

KLEIN: […] I believe in July of '67. The other reason he was there [was because] he did much of the statistical analysis associated with it.

BROCK: I see.

KLEIN: After I had left, despite the fact that **<T: 45 min>** Bob Kerwin and I wrote that article, they tacked his name onto it [because he supplied] the statistical analysis […]. [laughter] He [was] a nice fellow. I don't mean to deprecate him. It's just that I was surprised when the paper actually was published that there was a fourth name on it. He's not on the patent.[11]

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[10] Ibid.

[11] Kerwin, et al, "Method for Making MIS Structure," U.S. Patent 3,475,234.

BROCK: No, he's not. […] I'm a little unclear. Why did…in the reorganization, when Cleveland…when your group became associated then with Koontz rather than Cleveland, he viewed your group as just part of the old order or something like that?

KLEIN: Yes. Exactly.

BROCK: Okay. I see.

KLEIN: And also, truth be told, he was not as conversant with semiconductor processes as was Cleveland. I remember sitting in his office soon after he took over my group. […] He thought that it was stupid of us to have built surface field-effect devices because you'd save real estate if you made it vertically into the surface.

BROCK: Vertically into the surface?

KLEIN: Yes. [laughter] Make it in the silicon.

BROCK: Oh, I see.

KLEIN: Don't laugh, because many years later they did really make some transistors that way.

BROCK: I can't picture it.

KLEIN: But at the time he suggested it, it was a sour grapes attitude that we were stupid to have used the surface of the silicon up the way we did.

BROCK: I'm not even sure I understand the vertical suggestion. That would be…

KLEIN: He wanted […] the gate and [the source and drain] to be buried inside the silicon.

BROCK: Sort of in the width of the silicon wafer?

KLEIN: In a vertical strata inside the silicon.

BROCK: Oh.

KLEIN: You shouldn’t quote this guy too much […]. It was kind of stupid at the time, and he didn’t want to hear that it was impossible to consider that structure in those years.

BROCK: Right. Well, in the same way…in the sense that there was this internal competition between silicon gate and metal gate groups, did you have a sense in […] 1966 of any external rumblings of people getting interested in using silicon for the gate material, or other types of metals?

KLEIN: No, we were not aware. Later on [in] one of the […] trials, I believe it was in [...] Toronto, [Canada], someone pointed out in the litigation that was attempted to use against us, that people had used polysilicon to make semiconductor capacitors, but I was not aware of that.

BROCK: Okay. As I understand it, […] after you had those good devices back in May/June, then it was Sarace who continued working on the process through the fall?

KLEIN: Oh yes. Koontz, in his inimitable way, began to strip me of the people that were reporting directly to me until finally I was the manager of a single person, Sarace.

BROCK: Oh, I see.

KLEIN: So […] I left Bell Laboratories. [laughter]

BROCK: Right. […] But you were there at Bell Laboratories through the patent process for…

KLEIN: Well, the disclosure that went into that patent. The patent wasn’t issued until quite some time later.

BROCK: Right, but [you were there for] the patent application.

KLEIN: Yes. I helped write that patent, [of course].

BROCK: Okay. Then you **<T: 50 min>** […] left Bell Labs right before the first public disclosure of your work, […] in that August 1967 paper?[12]

KLEIN: I was already working at IBM at that time.

BROCK: Did you…?

KLEIN: Yes, I attended. [laughter]

BROCK: You attended? Did you help give the paper […]?

KLEIN: No.

BROCK: Who…?

KLEIN: I wanted Sarace to get credit for [the work he had done in fabricating the devices].

BROCK: Okay.

KLEIN: […] Within the structure of Bell Labs he was an associate member of technical staff. [He was] a degreed metallurgist but had worked his way up from a technician to that in-between rank. It would be like a warrant officer in the military.

BROCK: Okay.

KLEIN: […] For all the work that he had done in carrying this through, […he felt particularly] isolated, alone, and he felt the scorn of his manager, which was Don Koontz at that time. I thought that he ought to get credit for presenting that paper.

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[12] Sarace, et al., "Metal-nitride-oxide-silicon field-effect transistors, with self-aligned gates."

BROCK: Does that explain…well, I had a question, a couple of questions about that venue to give that paper at a Metallurgical Society meeting. Did that have to do with Sarace?

KLEIN: No. It was just appropriate for the […] other papers that were being presented. […] At that time lots of things were difficult to publish in existing journals. […] I published a number of papers in the *Journal of the Electrochemical Society*, which seemed to be the right venue for most of this. But because I published in the *Electrochemical Society* [and] the American Chemical Society recognized that both Kerwin and I were members, they wrote an inquiring letter as to why we didn't publish in an American Chemical Society publication. […] They really weren't accepting [semiconductor articles] at that time.

BROCK: Right.

KLEIN: The same way we published in an English publication, for that paper, with Bob [Kerwin] and [Edwards]. We published in *Solid-State Electronics* because it seemed the right venue. The Metallurgical Society was probably the right place to give it at that time—I still get their programs for their meetings to this day—and for many years thereafter they used to have whole sections based upon the silicon gate devices.

BROCK: At the Metallurgical Society?

KLEIN: Yeah, and its electrical characteristics. But, as I said, for years later they kept a particular part of each of their meetings related to those devices. It's gone out in other directions. They [now often] speak of improved dielectrics and so forth.

BROCK: Right. But it was then, and is today still, a venue for people presenting their work in semiconductor technology. Is that true?

KLEIN: I can't answer that…

BROCK: Okay, but certainly at that…

KLEIN: …because I don't get that journal.

BROCK: …time?

KLEIN: At that time, certainly.

BROCK: Okay.

KLEIN: Today, there are publications that appear in several of the…well, *Electrochemical Society* has become very large in taking […] papers dealing with solid-state.

BROCK: Right.

KLEIN: […] But I've noticed more and more there are many, for instance, lithography articles based on materials of lithography in ACS journals these days. It's just that we were about thirty years too soon.

BROCK: [laughter] […] In addition to that August 1967 […] paper at the Metallurgical Society meeting in New York, do you recall any other public presentations about your silicon gate work around this time, late '67, early '68?

KLEIN: Yes. […] There was a paper in the IEEE [Institute of Electrical and Electronics Engineers] *Spectrum* in October of 1969 **<T: 55 min>**, by [Leslie L.] Vadasz, [Andrew S.] Grove, [Thomas A.] Rowe, and [Gordon E.] Moore from Intel.[13] That was one of the earliest publications that I remember seeing, outside of the Bell Labs publication.

BROCK: [You're talking about the] Metallurgical Society paper […] that appeared after you left Bell […]. That was in the journal…

KLEIN: […I left on] the last day of June.

BROCK: That came out in 1969.

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[13] L.L. Vadasz, A.S. Grove, T.A. Rowe and G.E. Moore, "Silicon-Gate Technology," *IEEE Spectrum* 6(10) (October 1969): 28-35.

KLEIN: Two years later.

BROCK: Right. The Sarace, Kerwin, Klein, and Edwards paper in *Solid-State Electronics* from 1968 was the major public statement of your group's work?[14]

KLEIN: Yes.

BROCK: Okay. What […] reaction did you get after those public disclosures?

KLEIN: It was slow in coming, but even Bell Labs realized that they had flubbed. While I was […] at IBM early on, they sent someone from […] Bell Labs to ask me to file an additional patent related to the device structure itself. The original process really referred to the processing and materials set, and they wanted to take out additional patent coverage for the device itself. But, for whatever reason, and I really don't know what happened, they never were able to establish a device patent.

BROCK: Based on the silicon gate?

KLEIN: Yes. It probably was because there had been public disclosures without a patent disclosure. […] Bell Labs was very, very careful in always getting patent coverage before disclosing publicly.

BROCK: Right. Well, was…by the time your 1968 paper came out in *Solid-State Electronics*, you had left for IBM in the summer of 1967 and it seems like Sarace also had departed sometime slightly later?

KLEIN: […] I can't tell you when, but later. [laughter]

BROCK: [He] went to RCA [Radio Corporation of America]?

KLEIN: Right.

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[14] Sarace, et al., "Metal-nitride-oxide-silicon field-effect transistors."

BROCK: Was that just in the context of…you know, it was essentially the disbanding of the group?

KLEIN: Well, the group didn't disband per se. I think he felt that he could use that as an entry into more professional levels than he was being received at within Bell Labs.

BROCK: I understand.

KLEIN: […] He parlayed that, ultimately, if I remember, to his final job at Rockwell [International] on the West Coast, in California.

BROCK: Oh.

KLEIN: And, he did very, very well. He became a rather elevated member of their organization.

BROCK: […] I want to talk about your transition to IBM and your work there. Maybe we could carry this silicon gate story a little bit farther afield and then return to that, if that's okay?

KLEIN: Sure.

BROCK: As I understand it, and using the appearance of your *Solid-State Electronics* paper of 1968 as sort of a benchmark, it seems that by the time your paper came out that Fairchild Semiconductor, […] General Micro Electronics, Hughes [Semiconductor], and possibly, Intel [Corporation] [were] just beginning to get into silicon gate work. Were you aware that these other groups were responding […] to your work, [as] in the case of Fairchild and Intel? [And] these other groups, General Micro Electronics and Hughes, [seem] to have come **<T: 60 min>** across it in a parallel fashion? Were you aware of this activity going on at the time, that this…/

KLEIN: No.

BROCK: Okay. When did you begin to get a sense that it was really picking up broader interest in the community?

KLEIN: Well, I became aware…the thing that startled me, and we're talking now thirteen years in advance, through the 1980 timeframe, I received a copy of an affidavit that Gordon Moore presented, which indicated that he was one of the founders of Fairchild Semiconductor […]. Along with […] Bob [Robert N. Noyce], […] who unfortunately [has since] passed away. [Noyce] heard about the presentation we gave at the Metallurgical Society and realized the importance of what we were saying. Apparently [he left] Fairchild, and [with] Gordon Moore, [formed] Intel based upon the process and the device of a polysilicon gate, self-aligned system. I have the affidavit in front of me. It's very interesting. It's not long, but it's very interesting in what he has to say.

BROCK: […] Maybe we could talk about it if I could have a look at [it].

KLEIN: [Now], I pushed you ahead thirteen years.

BROCK: Well, that's fine. I'd love to have it. […] My understanding is that Ed [Edward H.] Snow from Fairchild Semiconductor was at that meeting and heard that paper, […] and came back and they started some work on it….

KLEIN: I always have kicked myself, because I wish I had the entrepreneurship that these guys did: they simply […left] Fairchild […] as a group and set up an independent company. If I remember correctly, […] Moore and [the others] who started that company ended up as some of the richest men in the world.

BROCK: Well, yeah, and it was…

KLEIN: I believe they were in the top fifty of this country, in income.

BROCK: Well, […] it became a very large concern, Intel.

KLEIN: But here's what he said…. It's only a few pages long, but let me read you a paragraph.

BROCK: So, this is an affidavit from a court case?

KLEIN: This is an affidavit. […] It says, "In the matter of German Patent Application," it gives a number, "Western Electric Company, in the matter of an opposition thereto by Siemens AG and Telefunken." He's giving a deposition. He introduces the deposition by telling who he is and what he does.

BROCK: This is Gordon Moore being deposed?

KLEIN: This is Gordon Moore. "In 1967 I was director of Research and Development of Fairchild Semiconductor. At about that time I heard of a development of R.E. Kerwin, and others of Bell Laboratories, of a new technique for making MOS devices"—MOS is metal oxide semiconductor— "through the use of a silicon gate electrode. I'm not sure who first explained this technique to me, but I believe it may have been Edward Snow, an engineer at Fairchild Semiconductor who attended an AIME [American Institute of Mining, Metallurgical, and Petroleum Engineers] meeting in Boston in 1967, and there heard the description of it by R.E. Kerwin, et al." He's in error there, because it was Sarace who gave it.

BROCK: Right.

KLEIN: But it doesn't much matter. "Upon learning of this silicon gate technique and its advantages, I was convinced that it could be a major advance in MOS integrated circuit technology. This new technique provided gate electrodes which were inherently self-aligned and it made use of polysilicon electrodes and interconnections which could be subjected to the high temperature of diffusion treatment. Thus, the technique represented a marked advance over the conventional use of metal gate electrodes and metal interconnections which were not self-aligned with diffusion regions." Do you want me to continue?

BROCK: Sure.

KLEIN: "I was fully aware of the state of the MOS integrated circuit **<T: 65 min>** art at that time, and I was quite surprised to learn that doped polysilicon is satisfactory as a […] signal conductor in MOS integrated circuits. This is because such circuits are used for digital switching purposes, and if the resistance of the signal conductor is too large, the circuit time constant, which is proportional"—with one hand I've got to turn the page—"to the product resistance and capacitance. Likewise, if it becomes too large, then the switching speed will be reduced. Since reasonably fast switching speed is an important requirement of such circuits, only good conductors can be used. At that time, it was thought that only certain metals were sufficiently good conductors. [Therefore], it was quite surprising and fortuitous that polysilicon could be used as a conductor because it is compatible with the other materials of the integrated circuit. It is capable of withstanding high temperature treatment, and can be converted to highly insulated silicon dioxide, among other advantages." Then it says, "In 1968, along with Dr. Robert Noyce I founded Intel Corporation. We believed that the fabrication of semiconductor memory chips making use of silicon gate technology represented an opportunity which justified the risk of starting a new business." And it goes on from there. All I [have to] say is that this guy had more insight than I did in terms of business. [laughter] I've often said that I'd like a micro cent for every gate, silicon gate, that was ever made.

BROCK: [laughter] Yes, well, that would be…

KLEIN: Well, since a few years back they were making 10^{17} gates per year by this process, [it] would represent a pretty big hunk of money.

BROCK: I'm sure.

KLEIN: [It'll never happen]. At Bell Labs I received a dollar when I started working the first day, and that was to buy up all my patent rights, [whereas] at IBM, they [would generously] reward you by what they considered a percentage of what the value of your particular patent was. I would have rather invented it at IBM. [laughter] Typically the rewards were in the $50,000 category. Now, remember, this was thirty years ago.

BROCK: Right. Well, I wanted to continue [...to talk] about silicon gate. […] I know that there were these legal disputes in the 1970s about these patent infringement claims, about the group at Hughes, and the group at General Micro Electronics. I [also] know that in the end it was the work at Bell that was victorious in these legal disputes.

KLEIN: Yes. Sure.

BROCK: Well, what I was wondering, is: do you think this is an instance of near-simultaneous innovation, or, I'm just curious about, if this is a case of this idea percolating up in three very separate, non-interacting contexts? What does that tell us about the general context of the semiconductor technology at the time, or the role of materials and chemistry in it? […] Do you have any thoughts about it?

KLEIN: Well, when the time is ripe in a particularly fast-moving technology like solid-state electronics, great ideas do not necessarily originate at one spot. […] If you recognize what the needs are, [they] can be addressed in a number of ways. We chose a particular materials process [set] that allowed us to move ahead, but others were capable of doing the same thing.

BROCK: Okay.

KLEIN: So, I'm not contesting that others couldn't think of it. When this [came] to federal courts, both in this country and in Canada, we always prevailed that we [originated] this particular concept, but others were busily moving [on]. What particularly irked me at one point in time was that **<T: 70 min>** paper that I described from Intel in '69 points out—I can find the paragraph in which he says, we "Didn't recognize one of the significant advantages of polysilicon." And, I must admit I didn't at the time. But it was when you separate two electrodes, and a single crystal silicon, and the polysilicon separated by an insulator, [that they] represent, in essence, a battery, because they have different work functions. Or, I'm sorry, I mis-said that. If you put a metal like aluminum on top, instead of the polysilicon, you have a [difference in] work function between the silicon and the aluminum, and therefore a built-in bias. And, that Valdez, et al., paper says that one of the things we missed was to say that there was no such bias when you use polysilicon over an insulator over silicon. That made it even better.

BROCK: I see. Because it would…

KLEIN: The same materials, in essence.

BROCK: I see.

KLEIN: So, there's no work function difference.

BROCK: Okay. The lack of that work function is an improvement as well?

KLEIN: Right.

BROCK: I see.

KLEIN: If I hunt in that paper I could probably find where he admonishes that we didn't see it. I don't think that's important. Unless you wanted me to do it […]?

BROCK: That’s okay. I have a copy of that paper, so I’ll look. […] One of the issues I’d like to explore is your continuing involvement with silicon gate technology after the summer of 1967.

KLEIN: There was no involvement.

BROCK: Okay.

KLEIN: For me to leave Bell Labs and enter at a high level [position] in IBM, it was agreed that I would be hired only if I didn’t operate on material sets and devices that I had been working on at Bell Labs.

BROCK: This was an agreement that IBM made with Bell Labs?

KLEIN: I believe so. I have no way of proving it. But I believe it was done at very high levels, especially at IBM, to consider taking me into their corporation.

BROCK: I see.

KLEIN: [The promise] that I would not be working intimately with the problems that I had been at Bell Labs, for at least a period of time. What that period of time was, I don’t remember, probably a year or two.

BROCK: Well, before we go into the IBM story then, may I just ask you to reflect on the subsequent development of silicon gate technology and its role in semiconductors across the last, thirty/thirty-five years? […] We talked about [how] they’re making 10^{17} gates a year. [When you look] at the importance of silicon gate and the pervasiveness of it as a technology, what are your reflections about that?

KLEIN: Well, I’m sorry I was never associated with it. In fact, many years later when IBM became interested in FETs, I was never consulted. No one ever came to me and said, “Could you tell us about what you did in detail?” They certainly have progressed into FETs in a big, big way. The advantage of FETs only became obvious later on. [They] were running in competition with bipolar devices. At the time, the bipolar devices could run rings around FETs in terms of speed, and speed is important in computation particularly. So that was a competition within IBM of whether FETs or bipolar devices would excel in that chip design. Ultimately

FETs [have] led the way, particularly **<T: 75 min>** because they have an enormous advantage over bipolars when they got smaller and faster, because size determined the speed of the devices. They had an advantage that they used much lower levels of current, and current times the voltage in which they operate represents heat, and heat is a bugaboo in semiconductor devices, particularly when you build millions of them, ultimately, on a small chip. The other thing is that they can be stable without drawing current. You can turn them on and they will stay on, or turn them off and they will stay off, so that the amount of current used to switch them from one state to another is very minimal. Whole operations have been based on the so-called CMOS [complimentary metal-oxide-semiconductor] device. They're complimentary. MOSs became very dominant in the strategy of electronics, particularly in computing work after that because of all the reasons I told you and the fact that the design leads to very large scales of integration.

BROCK: […] Well, perhaps then we could turn to the story of your transition from Bell Labs to IBM? How did that…?

KLEIN: [But] you understand that I'm restricted now, [laughter] in what I can do when I came to IBM. I was asked to set up a group to advance the technology of lithography.

BROCK: Okay.

KLEIN: I spent years operating in lithography, both in terms of exposure technology and particularly in the building new photoresist materials.

BROCK: I see.

KLEIN: Ultimately that led to IBM to actually [start] making their own photoresists. I don't know whether you know the history of photoresists?

BROCK: I know that there was originally Kodak Photo Resist, KPR, and then a number of firms started getting into the photoresist game as the market developed. […] How would you characterize the photoresist story?

KLEIN: [In] the photoresist business […], I was one of the [first], if not the first person to ever to use photoresist in electronics. I don't want to make that claim because I can't prove it, [laughter] but […] I shaped gold on the backs of semiconductor wafers so that I could work on an etching technique when I first came to Bell Labs. […] By the way, I think I said it wrong. A correction on that description, when I think about it, […] the photolithographic layer was used as a plating mask to create the gold dots on the back of the wafer so that we could use that as a mask of the nitric acid/hydrofluoric acid etch that separated devices.

BROCK: Okay.

KLEIN: But, to my knowledge, that was the first publication to use […] lithography in semiconductor work at all.[15] It's not an important point but it was early on. That followed me in a haunting way when I got to IBM. They decided that they really needed to understand the technology of lithography that was a limiting step in how small devices could be made and how reproducible they could be made.

BROCK: Sure.

KLEIN: I set up a group of chemists who were interested in understanding how photolithography really operated as a material set. KPR, as you mentioned, which stands for Kodak Photo Resist, was originally developed for two large-scale purposes. One was to make photographs, or photographic plates, for printing. And the other was a lithography to create patterns on tableware.

BROCK: Plates?

KLEIN: You can make stencils [that] way. There wasn't a terribly demanding technological requirement of what the photoresist had to do in terms of resolution, or even **<T: 80 min>** how pure it had to be, as long as it stuck to its Chinaware or metal plate it was perfectly satisfactory. It was certainly not terribly satisfactory in terms of photolithography because the quality of KPR left much to be desired. For one thing, the adherence to silicon dioxide and the other layers that we wished to shape photolithographically required an understanding of surface chemistry to increase that adhesion. Now, other photoresists came along. They were there when I was still at Bell Labs. Like KTFR […], Kodak Thin Film Resist, that was made specifically for electronics application. So, it followed from KPR that we came to KTFR. But even though it was a higher quality material than the KPR it left much to be desired. […] People spent a lot of time developing surface techniques for improving the adhesion of the photoresist because it's exactly in the patterned area of a resist where you get the greatest demand of survivability in holding, adhering, to that surface, right where the removed photoresist exposed the surface that you wanted to etch. That was a very demanding sort of thing. All kinds of hocus-pocus technology began to develop as to what adhesion promoters you could [improvise].

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[15] Klein and D'Stefan, "Controlled Etching of Silicon in the HF-HNO_3 System."

BROCK: That would essentially be the compounds to add to the photoresists?

KLEIN: No. Primarily to add to the surface of the material you wished to shape before you applied the photoresist.

BROCK: Okay.

KLEIN: And a great deal of both science and art evolved from that, along with a lot of work trying to develop an improved photoresist, because KTFR had the ungodly characteristic in thin films of reacting with oxygen and preventing its full exposure.

BROCK: Okay.

KLEIN: In fact, Kodak never in those days published how the [darn thing worked], so we spent a lot of time understanding how photoresists operated. […] We knew it was a polymer, but it was based [in part] on a polymer plus a photo activator, and how that photo activator worked was very key. We spent a lot of time taking photoresists apart, understanding their function and then deciding that we could ultimately make a better photoresist ourselves.

BROCK: […] Was that the primary focus of the early period of your work at IBM, then?

KLEIN: Yes […].

BROCK: When did you […] and your colleagues […] reach this decision that you would try to manufacture your own photoresist?

KLEIN: In the early ‘70s.

BROCK: What sort of volume were you looking at in terms of how much photoresist you would need to produce?

KLEIN: You [have] got to understand that a few drops went a long way in photoresist. […] At that time, there were other companies like Kalle. I don't know if you've ever heard of Kalle [Kalle Corporation], or Azoplate [Azoplate Corproation]?

BROCK: I haven't […].

KLEIN: Other companies were making the opposite sense photoresists. Photoresists made by Kodak at that time were called negative resists: where you exposed them to light the patterns stayed; where you didn't expose to light you could [dissolve them] away with solvents. That's how you developed an image pattern. But a German firm called Azoplate, which is really part of Kalle, […] developed the opposite […], a positive resist in which where you exposed it [to light] was removed by a solvent process and where you had **<T: 85 min>** not exposed it, it remained intact. So, they were exactly mirror images of each other. A positive resist would allow you to remove material where you exposed it. A negative resist would retain the pattern where you exposed it.

BROCK: I understand.

KLEIN: They worked by entirely different mechanisms and we spent a long time trying to understand [them]. But we did ultimately.

BROCK: […] So was it a positive or negative resist that you chose to produce internally at IBM in the early '70s?

KLEIN: Positive. Now, I should hasten to point out that the initial photolithography in semiconductors, and for years afterward, were what's [been called] "contact printing." […] You would make an image in a mask, a photographic mask, hold it in hard contact with the photoresist film on the semiconductor and expose to appropriate wavelength of light. […] But that had a tremendous disadvantage. Oftentimes, in […] lifting that mask away from the surface of the semiconductor, carried particles of the photoresist [were] mechanically torn from the surface.

BROCK: Sure.

KLEIN: That may seem simple to you, but it wasn't. You would think you could simply clean [the photoresist from the photographic mask] and start again, but what it did was also reduce the effectiveness of the photoresist film. You've actually torn holes [in the photoresist] on the semiconductor [films]. So it became a whole evolution of how you could expose photoresists without contact printing. [We] worked […] on a technique using coatings over the photomask so [contact against the material with the photoresist] would not pull the photoresist off. That worked reasonably well, and that had the additional advantage of operating as an intermediate refractive material. We actually got very high quality exposures. [But] still, in all photomasking with contact printing [had] its [limitations] and was abandoned after a great number of years. What [we were] looking for were other […] techniques for exposing the resists.

As a matter of fact, I had written a memo while I was still at [Bell], and I do recall that it was […] many years later that someone from [Bell Labs called IBM to ask] where that memo at Bell Labs was. [They wanted to know] because that memo outlined the progression that I saw in the photolithography business even in those days, going from light exposure to projection exposure, to electron beam exposure, and finally to using ion implantation. It was considered a seminal paper to Bell Labs. They went back and hunted it up. No one paid attention when I wrote it at the time. So a philosopher in his own country is not well received.

BROCK: [laughter] Was…?

KLEIN: Did I explain that we started that work at Bell Labs, believe it or not, in a very, very limited way and that's what I carried on at IBM, even though they didn't consider me as a lithographer. That's why I was allowed to operate in those regions when I came to IBM.

BROCK: Okay. And was…that internally-produced IBM photoresist…. Did that have…what was its life course like at IBM? Was it…? What was the story of the photoresist within IBM, the internally-produced photoresist? Did it come to pervade IBM, or was it…?

KLEIN: Well, yes, in very [demanding high resolution] applications.

BROCK: I see.

KLEIN: I hasten to add, I started to touch on positive photoresists. If I recall correctly, positive photoresists cost $1,000 **<T: 90 min>** a liter.

BROCK: Wow.

KLEIN: So, when you asked me, backing up to your question, "How much volume did we generate," I can't remember, frankly. But you didn't need [very] many dozens or hundreds of quarts before you were really in a big operation worth a lot of money.

BROCK: Did it require a large physical plant, too?

KLEIN: If you were a chemical engineer you would have said that we were operating a semi-pilot plant. It was a true chemical engineering setup to produce these devices, but, believe it or not, at that point I was moved, as IBM often does, into other work. So, I was not responsible for fully setting up that plant.

BROCK: […] It sounds like that was, then, a pretty significant change in the early 1970s. […] Where did you move?

KLEIN: I was still at the same site, but I had other responsibilities at that point.

BROCK: Were they also chemical in nature?

KLEIN: Semi. [laughter] And I don't mean that as a pun. Yes.

BROCK: […Could you] discuss them?

KLEIN: Well, it was a whole hodgepodge of things. I ultimately ended up, for years, in packaging technology, electronic packaging technology. IBM had its own unique package for many, many years based upon ceramic technology with many layers of metallurgy denoting interconnections in layers of ceramic.

BROCK: And, this is packaging for integrated circuits?

KLEIN: That's correct. [That required] some of the same technology in terms of shaping those lines and entering operations of putting ceramics together, [and] some of it is quite different than semiconductors. But that's where I ended up—a good deal of my background—to meet those needs.

BROCK: Just to understand, in this IBM packaging they were using layers of metal interconnections in the packaging to connect different areas of the integrated circuit in the package?

KLEIN: You really want [to hear] a tutorial?

BROCK: I would.

KLEIN: [laughter] Before I ever got there, there came a time—and you can find it within *Fortune* magazine in the '60s, in which, if I recall, the title of the magazine was, "You Bet Your Company" —they made a decision that they would make a simple integrated circuit with individual devices (if you can believe that), integrated in terms of resting on a surface of ceramic.[16] Can you envision this? You don't have any photographs in front of you?

BROCK: I'm trying to picture it. So, this is to take discrete semiconductors devices and…?

KLEIN: And mount them on a ceramic insulator in which there were metalized patterns on the surface.

BROCK: Okay.

KLEIN: It was about half a square inch. The backside of this ceramic contained what looked like vacuum tube pins. […] Think about vacuum tubes. [They were along] the periphery of this entire ceramic so that you could plug it into a socket, which would make contact right through the ceramic onto the forward surface where all the devices were mounted. Can you envision that?

BROCK: Yes, I can.

KLEIN: That was called SLT, Solid Logic Technology, and that is what made the first big breakthrough in solid-state computers, large computers. [It] made the so-called 360 machine. You've heard of that one?

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[16] T.A. Wise, "IBM's $5,000,000,000 Gamble," *Fortune* (September 1966): 118-23, 224, 226, 228.

BROCK: I have.

KLEIN: What [they did] was they built that technology into more and more complex structures, convincing themselves that they had the best of all packaging techniques. Instead of using a single piece of ceramic, they made layers of green sheet—scientists call […] anything that had not been fired that has a binder and is semi-flexible **<T: 95 min>** [green sheet]. What [you did] was you patterned these, stacked them up, and then fired them so it became a single solid hunk of ceramic with all kinds of interconnecting lines in various layers. Again, they used the same type of pin arrangement on the back, and you mounted your devices on the uppermost surface. But now we're talking of integrated circuits, truly integrated circuits.

BROCK: In terms…are the devices loaded on the top layer, so to speak, or…?

KLEIN: On the ceramic.

BROCK: […] Are these discrete transistors?

KLEIN: No.

BROCK: Okay. What are they?

KLEIN: They graduated from the earliest application into the beginning of integrated circuits, resting on top. This is actually a means of making many integrated circuits interconnected on a ceramic substrate and interconnected through the substrata of wiring on the different layers of ceramic, which, after firing, burnt off the binder that allows you to manufacture this in a semi-flexible form and become hard as [a] rock ceramic after firing.

BROCK: And that would, sort of…okay […]. In essence, this would allow you to…it's sort of a modular customizable…

KLEIN: Hey! That's what they called them: "modules." [laughter]

BROCK: […] I think I have heard of this, although not in the…

KLEIN: There [are] a great number of articles over the years that appear in *Scientific American* that you could find if you searched for it, that described these. It kept expanding into more and more layers of ceramic and metallurgy, varied metallurgy, and bigger and bigger dimensions, until they finally got up to many, many centimeters, rather than the half-inch, which I described to you where they started. They got into many inches.

BROCK: Okay. And when…and for how long did […] IBM pursue this technological path?

KLEIN: All the years I was there.

BROCK: When did you leave IBM?

KLEIN: I left in 1987. They ultimately farmed it out to overseas operations to manufacture. When the extent of the development had become limited and there was no need for further research and development, they farmed it out. But, I think I mentioned the first time we talked about having the largest solvent system recovery in the world.

BROCK: You did.

KLEIN: Well, part of it was to use perchloroethylene to clean the screens that [we] used to lay down the molybdenum pattern that was used on these sheets of ceramic. This was because the conductors in those days were molybdenum, because molybdenum was compatible with the firing cycles that you would need to make that ceramic into a brick.

BROCK: I understand.

KLEIN: They used a special paste loaded with molybdenum, and the screens had to be cleaned after each screening operation. And they used voluminous quantities of perchloroethylene to do that.

BROCK: Okay.

KLEIN: And, at one point in my life, I was given the job of essentially setting down the controls necessary to maintain that large operation.

BROCK: […] That must have been a tremendous volume? I mean, this was essentially the…you're talking about the packaging for the logic hardware?

KLEIN: It was used in both logic and memory.

BROCK: But for the critical components of, sort of, the main line of the mainframe business? Is that…?

KLEIN: Oh sure. But they ultimately appear in other systems as well.

BROCK: In smaller computing systems?

KLEIN: Yes.

BROCK: Okay.

KLEIN: As you know, the PCs that [IBM ultimately] sold, […] were **<T: 100 min>** ultimately manufactured to a lower standard requirement in those days, so it could be made out of vendors' products and assembled from [there]. […] Most recently, they've sold that whole operation out to a Chinese firm and PCs are now made by the Chinese, in their country.

BROCK: Right. Lenovo [Lenovo Group], is it?

KLEIN: Yeah. You got it.

BROCK: That is definitely a continuation through that whole period, those twenty years that you were with IBM, from the photoresist technology through to this advanced ceramic packaging technology, is very much a continuation of the chemical story.

KLEIN: Yes.

BROCK: […] Okay. Well, the only…. It's my understanding that with integrated circuits, and even with discrete transistors, that the packing was a huge cost factor in the manufacturing costs?

KLEIN: Yes, it was.

BROCK: I think that it…the packaging doesn't get the attention that it might, because, for some reason, it seems sort of less exciting than making the active device […]?

KLEIN: [laughter] So much so, […] that when I told them I was going to leave Bell Labs because I couldn't take my new boss any longer, they offered me the opportunity of moving […] my office and facilities a few miles to another Bell Labs location to […] enter the field of packaging. I turned up my nose at it. [laughter] I thought it was rather ironic that I spent a lot […] of my years working on packaging technology at IBM. And yes, I thought of it as a much lower level of technology. But, in itself, it's rather a complex technology. […] In many ways [it] challenges [and requires] the same efforts that you would need in semiconductor development.

BROCK: Right. Well, I'm sorry. You were going to speak to my observation about the continuation of […] the centrality of chemistry to your work in…

KLEIN: Oh yes. That's always been true.

BROCK: Well, let me just look at my question…if you'll pardon me while I look at my question list here. I think I may have exhausted all the questions that I had written down. Is there…is there anything about…have I committed any grievous sins of omission with either respect to the silicon gate story or your experience of the chemist's role in modern electronics?

KLEIN: I don't think so. [laughter] We may have breezed over some of it rather fast, but you'll have to decide to see if there's continuity to what we've been speaking about.

BROCK: Okay.

KLEIN: I'll be glad in the future to fill in any blanks that you see in it […].

[END OF AUDIO, FILE 2.1]

KLEIN: […] Okay. I neglected to tell you that there are two types of FETs. [There is] what they call "enhancement" and "depletion" modes. What happens is that the most difficult form of [FET] device is an enhancement, an N-type […] FET. Because there are electrons floating around in most FET [surfaces], […] you have to apply a bias to prevent the thing from conducting.

BROCK: Okay.

KLEIN: […] What we were producing was the first [enhancement N-type FET] devices that [did not require bias to turn them off]. The most difficult type was an N-type of FET that was off when you started, and you could bias it on. You didn't have to bias it off. That was considered a great advance at the time. […] If you look at the paper, it's inferred from some of the curves on that.[17]

BROCK: Is that then an enhancement mode device?

KLEIN: Yes.

BROCK: Okay, and so what was it about the…?

KLEIN: A clean surface.

BROCK: I see.

KLEIN: We didn't have any degrading of that surface by either structural defects or by debris or contamination. They were clean surfaces, and so they had no free electrons roaming around.

BROCK: It was the case that with your device it was enhancement mode, and yet it had an advantage in that you didn't have to bias it to keep it turned off?

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[17] Sarace, et al., "Metal-nitride-oxide-silicon field-effect transistors."

KLEIN: That’s correct.

BROCK: Okay. I’ve got it.

KLEIN: And that’s a tremendous advantage. Also it means you need to deal with lower voltages to turn it on.

BROCK: Lower gate voltage?

KLEIN: [You would need higher gate] voltages to turn it off if it had free electrons in that surface […].

[END OF AUDIO, FILE 2.2]

[END OF INTERVIEW]

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