CHEMICAL HERITAGE FOUNDATION

ARTHUR L. BABSON

Transcript of an Interview
Conducted by

David J. Caruso and Sarah L. Hunter-Lascoskie

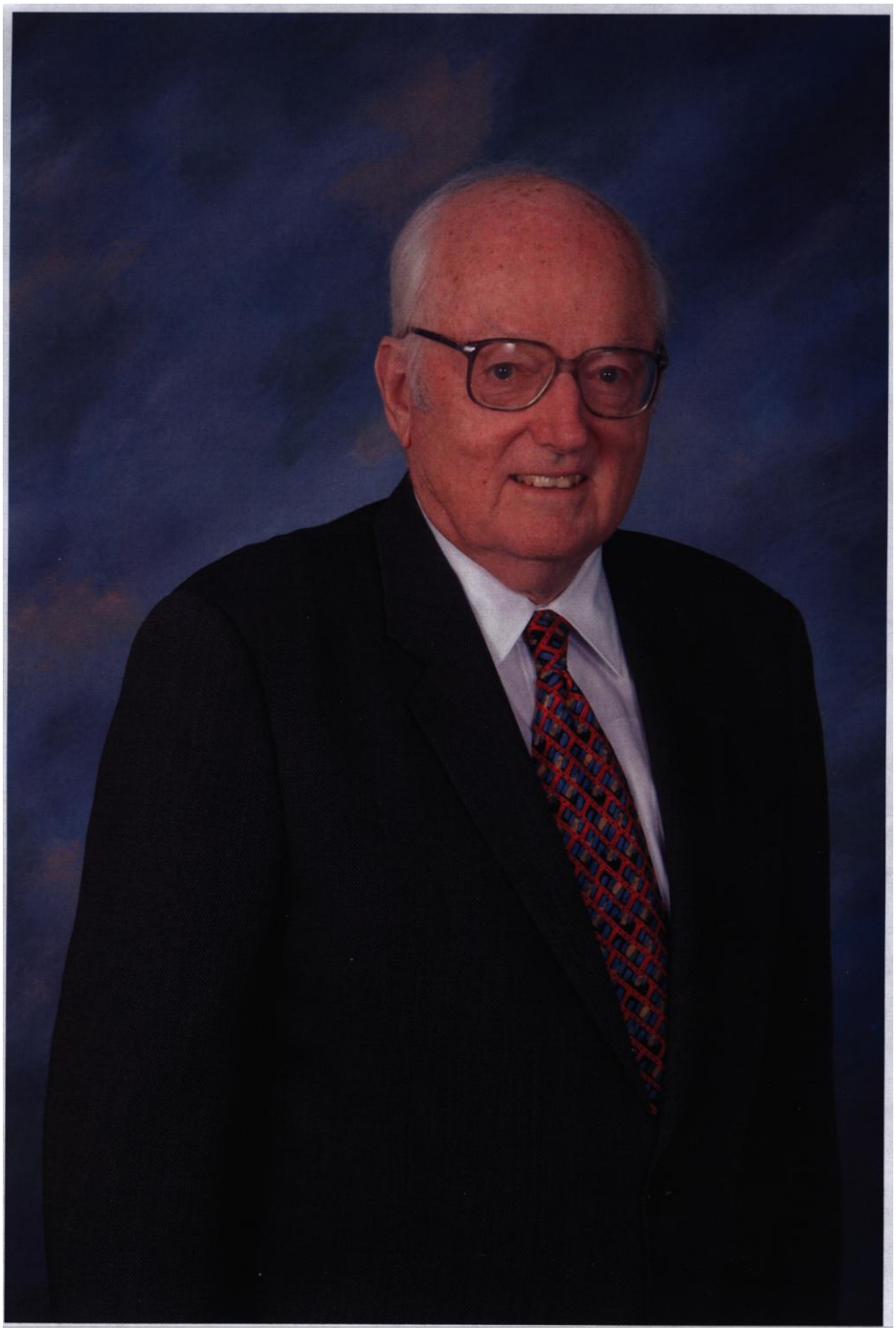
at

Siemens Healthcare Diagnostics
Flanders, New Jersey

on

6 and 8 December 2011

(With Subsequent Corrections and Additions)



Arthur L. Babson

CHEMICAL HERITAGE FOUNDATION
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ARTHUR L. BABSON

1927 Born in Orange, New Jersey, on 3 March

Education

1950 B.S., Zoology, Cornell University
1953 Ph.D., Biochemistry, Rutgers University

Professional Experience

University of Iowa
1953-1954 Postdoctorate with Theodore Winnick, Radiation Research Laboratory

Warner-Chilcott (later, Warner-Lambert)
1954-1962 Senior Scientist
1962-1967 Senior Research Associate
1967-1970 Director of Diagnostics Research
1970-1977 Director of Diagnostics Research and Development
1977-1980 Vice President, Research and Development, General Diagnostics Division

Babson Research Laboratories
1980-1987 President

Cirrus Diagnostics (formerly Pegasus Technologies)
1987-1992 President, Chairman and CSO

Diagnostic Products Corporation
1992-2006 Chief Scientist

Siemens Healthcare Diagnostics
2006-Present Chief Scientist

Honors

1975 Gerulat Award, American Association for Clinical Chemistry
1997 Inventor of the Year, New Jersey Inventors Hall of Fame
1998 Van Slyke Award, American Association for Clinical Chemistry

2010 Siemens Lifetime Achievement Award

ABSTRACT

Arthur L. Babson grew up in Essex Fells, New Jersey, one of two children. His father owned a General Electric appliance store, except during World War II, when he was an expeditor in Washington, D.C. Babson's mother was a housewife. Babson says he did not like school very much, at least until his high-school chemistry class, but he did like nature, the nearby woods, birds, and animals. He also liked to cause explosions, at home and on railroad tracks. To earn money Babson and his brother delivered mail, set traps at the gun club, caddied, and had a soft-drink stand on the golf course.

Babson began college and the Army Special Training Reserve Program at Rutgers University but was expelled for missing a single class. He then worked in a laboratory at American Dyewood until he was drafted. From Camp Kilmer he ended up in Japan, shortly after the atomic bombs were dropped; there he worked as a cook and on a wire crew—adding an instrument to his truck to assist with wire deployment and re-coiling—and he served on guard duty, where he developed booby-traps to alert him to anyone's approach. When he left the service and returned to the United States, he matriculated into Cornell University, where his father and brother had gone. He majored in zoology, took biochemistry, and decided to go to graduate school. After graduation he married his first wife; then he began his master's degree in biochemistry at Rutgers. He worked on protein nutrition in cancerous rats in James Allison's lab and decided to get a PhD with Allison.

When Babson had finished his PhD Theodore Winnick offered him a postdoc at the University of Iowa. Babson knew he wanted to do science but did not know what, and Winnick's offer was his best, so Babson and his wife and daughter moved to Iowa. A year of the postdoc was enough; Babson accepted a good offer from Ulrich Solmssen to work at Warner-Chilcott Laboratories back in New Jersey. It was there that Babson's career in diagnostics was launched. Tasked with developing a serum standard, he and his assistants invented Versatol, then Versatol-E (enzyme), which were successful for years; then they invented PhosphaTabs. During these years his second child was born.

Automating clinical chemistry started to emerge as Babson's core interest, and it became a clear program at Warner-Lambert, though Warner-Lambert's Robot Chemist lost out to Technicon's AutoAnalyzer. At Warner, Babson moved up in administration, moved away from the bench, and became Vice President of Research for General Diagnostics. Susan, who would become his second wife, transferred to his group. The Food and Drug Administration promulgated more regulations. Babson was active in AACC (American Association for Clinical Chemistry); he won the first Gerulat Award. Then a new layer of administration above Babson caused a number of people to leave Warner. Babson waited until his benefits vested and then left. A few years later Babson's nemesis was fired for falsifying results.

Meanwhile, Babson started his own company, Babson Research Laboratories, in his home. He patented a refinement of Blood Gas Control. He consulted for Ortho Diagnostics. Then he began work on a device to automate immunoassays (later named IMMULITE). John Underwood introduced him and his homemade demonstration model to Arthur Kydd, a venture capitalist, and the three established Pegasus Technologies, later changing the name to Cirrus Diagnostics. (Meanwhile, Babson Research Labs continued out of Babson's home for a few years, then shut down.) Cirrus started in a school classroom, rather a deterrent to hiring others, but Babson persuaded first Tom Palmieri, a mechanical engineer, and then Arthur Ross, an

electrical engineer, to join him. The business grew quickly, and by the time that Cirrus began manufacturing IMMULITE, it had taken over almost the entire schoolhouse. After building three prototypes (the A units), they moved on to building twelve production models (the B units); they sold their first production model (B1) to Morristown Memorial Hospital. Subsequently, Cirrus contracted with Lydo Manufacturing to build twenty-five more production models (the C units). Still interested in blood, Babson designed the Cardiac Risk Profiler to automate lipid profile diagnosis, but he was never able to sell it. From Babson's perspective, the Clinical Laboratory Improvement Act ended any hope for the CRP due to greater regulations for laboratories.

But IMMULITE withstood all its competition; it is the only such instrument still being sold of the twelve competitive systems that were available in 1992 when IMMULITE was introduced. Sigi Ziering, president of Diagnostic Products Corporation (DPC), bought Cirrus, and the joint company became DPC Cirrus. The second generation of IMMULITE, the 2000, automated sample loading. Babson received the Inventor of the Year Award from the New Jersey Inventors Hall of Fame. Next the company built IMMULITE 2000 XPi, for continuous flow instead of batch processing. Not yet satisfied, Babson and others then invented VersaCell, which automated sample selection completely. DPC Cirrus attracted the attention of Siemens Healthcare Diagnostics, which bought it and two other companies and combined them. Babson says the organizational structure is different, but the collegial atmosphere remains.

Babson likes to write essays, mostly with himself in mind as audience, and has written a whole book of them. He and his second wife, Susan, built their own house, taking three years to do it. The couple has taken a number of trips to Africa, especially East Africa. They are very involved in the Cheetah Conservation Fund. They have cats and dogs, but they have also raised two sets of raccoons. Babson points out that he has also won the Van Slyke Award from the AACC, and that he has just received the Lifetime Achievement Award from Siemens.

INTERVIEWERS

David J. Caruso earned a B.A. in the History of Science, Medicine, and Technology from the Johns Hopkins University in 2001 and a Ph.D. in Science and Technology Studies from Cornell University in 2008. His graduate work focused on the interaction of American military and medical personnel from the Spanish-American War through World War I and the institutional transformations that resulted in the development of American military medicine as a unique form of knowledge and practice. David is currently the Program Manager for Oral History at the CHF. His current research interest focuses on the discipline formation of biomedical science in 20th-century America and the organizational structures that have contributed to such formation. David is currently the president of Oral History in the Mid-Atlantic Region

Sarah L. Hunter-Lascoskie earned a B.A. in history at the University of Pennsylvania and a M.A. in public history at Temple University. Her research has focused on the ways in which historical narratives are created, shaped, and presented to diverse groups. Before Sarah joined CHF, she was the Peregrine Arts Samuel S. Fels research intern and Hidden City project coordinator. Sarah is currently a Program Associate for the Oral History Program at CHF and

leads projects that connect oral history and public history, including the oral history program's online exhibits. She also contributes to CHF's Periodic Tabloid and Distillations.

TABLE OF CONTENTS

INTERVIEWEE: **Arthur L. Babson**

INTERVIEWER: **David J. Caruso and Sarah L. Hunter-Lascoskie**

LOCATION: **Siemens Healthcare Diagnostics**
Flanders, New Jersey

DATE: **6 December 2011**

HUNTER-LASCOSKIE: […] Okay. Today is Tuesday, December 6th, 2011. I am Sarah [L.] Hunter-Lascoskie. We're here at Siemens Healthcare Diagnostics in Flanders, New Jersey. I'm joined by David [J.] Caruso, and we're here with Arthur [L.] Babson.

As Dave mentioned before, we like to start at the beginning. I know you were raised near or in Newark, New Jersey, but I don't know much else. So when and where were you born?

BABSON: I was born in Orange, New Jersey. Since two years old, I was raised in Essex Fells, [New Jersey], in a new house my parents built. I've been a New Jersey resident all my life, except for the Second World War—a year in Japan—and, of course, going away to college at Cornell [University], and a year of postdoc in the University of Iowa.

HUNTER-LASCOSKIE: But otherwise, all in New Jersey …

BABSON: All in New Jersey, yeah.

HUNTER-LASCOSKIE: So…

BABSON: I like New Jersey. New Jersey is a good, well-kept secret from the rest of the world.

HUNTER-LASCOSKIE: So had your family been in New Jersey for a long time? What were your parents doing?

BABSON: My mother, [Julia Norton Babson], was born in Chicago, [Illinois]. My father, [Rea Edwin Babson], was born in Brooklyn, [New York]. Where they lived prior to my arrival, I

don't really remember, because obviously I wasn't here. [laughter] But they pretty much lived in New Jersey all their life. Of course, they're dead now.

HUNTER-LASCOSKIE: Did you have siblings?

BABSON: Yes. I had a brother, [Rea Norton Babson], one brother, who was eighteen months older than I was. We fought like cats and dogs, as siblings will.

HUNTER-LASCOSKIE: And what did your parents do for a living?

BABSON: My father had a General Electric appliance store in Montclair, New Jersey. He was a dealer, General Electric dealer, in home appliances until he got put out of business by the Second World War, because GE was making armaments, not appliances, home appliances, at that time. So he worked for a couple of years during the war as a expeditor in Washington, [D.C.]. He'd commute every week on the train. Then, after the war, he went back and started his business again until he retired.

HUNTER-LASCOSKIE: Was your mother working too, or was she…

BABSON: No.

HUNTER-LASCOSKIE: …at home?

BABSON: My mother never worked. Well, she did work before I was born, but she never worked since I had been born.

HUNTER-LASCOSKIE: So you're growing up in New Jersey, and what did you like to do? Did you have any kind of early interest in science or technology? Did you visit your dad at work?

BABSON: Not really. But I did have an early interest in science, I think, because…I started school when I was four years old, which was too early, a year too early. I was always the smallest person in the class, including the girls, and always the youngest. Not very good at athletics, so I was always the last one to be chosen when choosing up sides for teams. I hated Field Day, when we had…we were obligated to compete in various activities.

So I was a bit of a loner. I think that might have sparked my scientific curiosity, because…well, I'm not sure why. As I say, I wasn't…I'm kind of a social misfit. So I went to science as a retreat, you might say.

HUNTER-LASCOSKIE: Now with this early interest in science, how was that…was it nature? Was it building things…?

BABSON: Nature mostly. We had about a square mile of woods behind our house in Essex Fells. I just loved <**T: 05 min**> being out in the woods. Actually, I'm still very much a nature boy. Currently I live in Mendham Township, [New Jersey], with… on a six-acre wooded lot. So I do love the woods, and I like to be out in nature.

HUNTER-LASCOSKIE: Did you kind of collect animals, do any kind of…?

BABSON: Yes. Well, I got interested in birds very early on. I remember a single incident that sparked my interest. I was walking home from school in Essex Fells one day, and I saw a little gray bird in the snow. It was obviously in distress, and I picked it up. Before I got home, it died. My brother and I had given to our parents an *Audubon's Birds of America* for Christmas the prior year.[1] We went in on it together, because neither of us could afford it ourselves.

So I looked up this bird in the *Audubon's*. I just leafed through it, and it was a Tufted Titmouse. As I say, it sparked my interest in birds, and I would memorize the entire *Audubon's Birds of America*, the pictures and the names. Every year I would go out into [the field]…and I'd keep lists of birds I had seen, and all the way through adulthood, really.

HUNTER-LASCOSKIE: Did this interest in nature and science translate into school? Was school something you liked when you were younger?

BABSON: Well, I didn't really like school until I took chemistry in high school, which was my favorite subject [and] also the easiest course I ever took, because I think I had a natural bent for it. But general science I also took in ninth grade, and that was very interesting. But chemistry really kind of turned me on.

HUNTER-LASCOSKIE: Did you have any particular teachers or labs that were really interesting when you were in high school that helped you?

[1] See, for example, John J. Audubon, *Audubon's Birds of America* (New York: The Macmillin Company, 1950).

BABSON: Yeah. My chemistry teacher was—and I don't recall his name, unfortunately—but he was great. My favorite teacher in grade school, in the eighth grade, was a person by the name of Grace Kass , [who] was a mathematics teacher. She was great, and I loved arithmetic.

HUNTER-LASCOSKIE: Was this an interest that your brother shared as well, or…?

BABSON: Not particularly, no. No, he was more into sports. He played hockey growing up. He actually played hockey at Cornell, [he was a] letterman at Cornell in hockey, ice hockey. But no, we didn't share that many interests actually.

HUNTER-LASCOSKIE: So going through school and being interested in nature, were you into anything like Scouts [Boy Scouts of America] or any other activity?

BABSON: Yeah. I was in the Boy Scouts. I collected a number of merit badges, but Scouts was too regimented for me. I didn't really like the militaristic approach, but I liked being out in nature. So I never got past Second Class Scout, I don't think. So, I can't say it was one of my major successes.

CARUSO: You never made it to Eagle Scout.

BABSON: No. I never made it to Eagle Scout, despite the fact that I had a number of merit badges under my wing.

HUNTER-LASCOSKIE: So since you were kind of more along the lines of being self-guided in your interest in nature and science, when you were in high school in your chemistry courses, did you have opportunity for maybe <**T: 10 min**> a lab, or self-guided study, or experiments?

BABSON: No. I don't…not that I recall. But I just liked the course, everything about it.

HUNTER-LASCOSKIE: In these chemistry courses did it become clear that it was something you wanted to pursue, or was it just an interest?

BABSON: I had no idea what I wanted to do when I graduated from high school. I graduated at seventeen, and had a year to put in before I had to go into the [United States] Army, the draft. So I got a job at American Dyewood [Company], in the research lab actually, doing quantitative analysis of guanidine hydrochloride, which was an experimental […] chemical, that they were making. I learned a lot in that job. I got the job because my father knew the president of the company. So that was a lot of fun and interesting.

HUNTER-LASCOSKIE: Did you enjoy that job?

BABSON: Oh, yeah, very much so

HUNTER-LASCOSKIE: Was it something you could see yourself doing?

BABSON: Yes. I very much enjoyed it.

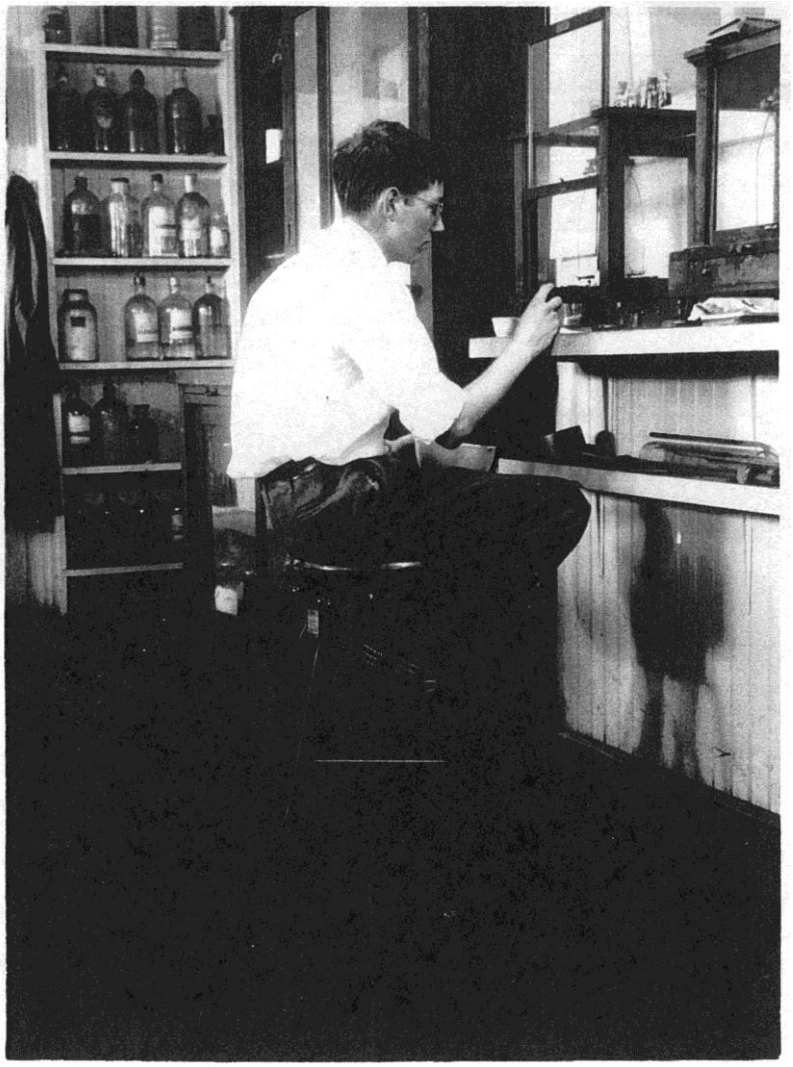
HUNTER-LASCOSKIE: Since your dad basically had the connection for the job, did they know about your interest in chemistry? Was it something that they were enthusiastic about?

BABSON: I don't know if MacKinney, who was the president of the company, [Paul] MacKinney…[knew anything about me], but as a favor to my father, and they were shorthanded, obviously, so he gave me a shot at the job. I think I did quite well at it, and enjoyed it very much. I worked for a man named Wallace Peck, who lived in Caldwell, [New Jersey]. I would walk the half mile to Bloomfield Avenue in Caldwell every morning, and he would pick me up on the way to work, and then drop me off and I'd walk home. He was a delightful gentleman.

CARUSO: One thing, I'm a little curious about given when you were born…. I mean you were growing up during part of the [Great] Depression.

BABSON: Yeah.

CARUSO: Did that have…I know you said that your father had this General Electric appliance store. I'm assuming he was doing pretty well. I know he had to close because of the war. Did the Depression affect your family or the people around you in any way?



Arthur Babson at age 17 working the analytical balance at the research lab at American Dyewood, 1944.

BABSON: Oh, yeah.

CARUSO: In what ways?

BABSON: Yeah. Well, we didn't have any spare money. We would go on el cheapo vacations.

CARUSO: Like where?

BABSON: Well, we'd go out to…they had a friend that had a house in Montrose, Pennsylvania, on a little lake. We would rent that house, or maybe we got it for nothing. I'm not even sure. We did vacations at my uncle [Durand van Doren]'s farm in Vermont. Let's see…. We also went out to Montauk, [New York], where we'd rent a house […] that didn't even have any running water—from another friend of my father's. My brother and I would […] have to earn all of our spending money, which we did in a number of ways, which kids nowadays don't even relate to. I mean raking leaves, shoveling snow, and…

CARUSO: I put my son to work.

BABSON: You …

CARUSO: Yeah. If he wants to buy toys, or at least, you know, when it's not a birthday or something, he needs to do work around the house.

BABSON: Good.

CARUSO: He's only four.

BABSON: Good.

CARUSO: So, I'm breaking him in slowly, but yeah, he definitely has to do a bit of work. When you were a bit younger, clearly you had to go to school….

BABSON: Yes.

CARUSO: Were there <**T: 15 min**>…you enjoyed exploring the woods, right. Were there other things that you would do after school to help the family out, or were there…were you interested in reading or any other sort of hobbies at that time?

BABSON: I…well, some of the other jobs that I had, because it was gas rationing at the time, and most people had A-cards, which would allow you to buy one gallon a week of gas, which didn't get very far. I had a business. The post office in Essex Fells didn't deliver mail at that

time. You had to go to the post office to pick it up. So I started a little business myself, delivering [a number of people's] mail on my bike. Every afternoon after school I would get on my bike, got to the post office, pick up everybody's mail, and deliver it.

CARUSO: How much did you charge for that service?

BABSON: I don't remember, but it wasn't very much, I'm sure. I had another, a regular job, working the traps for the gun club, where […]I would fire the clay pigeons that they would shoot with shotguns. That was kind of a fun job, and that was every week. But we were pretty resourceful, actually, at making money. We would set up a soft drink stand for example, […] along the fifth hole of the Essex Fells Country Club golf course, where we would sell soft drinks at a premium to the thirsty golfers. We also caddied, both of us, my brother and myself, at the country club. Anything to make a buck.

CARUSO: So it sounds like you were always quite busy.

BABSON: Yeah. Oh, yeah. Yeah.

CARUSO: When you were…?

BABSON: We didn't have time on our hands.

CARUSO: When you were actually home with your family, let's say at dinnertime, was there anything that your family talked about on a regular basis? Did your family like debating about politics, or talking about contemporary issues of the day? Or was it more just, "Did you make some money?" "Yeah." "Great."

BABSON: Yeah. I don't recall actually having deep conversations at the dinner table. I'm sure that my mother and father participated in [discussions] other than mundane matters, but I have no recollection of it to tell you the truth.

CARUSO: Did they have any opinions about your schooling? Were they, "You need to get an education," sort of parents?

BABSON: Oh, yeah absolutely. Absolutely. It was a foregone conclusion that I would go to college. Not necessarily graduate school, which I ended up doing, but by that time I was kind of in charge.

CARUSO: Did they, I mean when you got home at night, did they make sure that you sat down and did your homework? Did they work with you at all?

BABSON: Oh, yeah.

CARUSO: Okay.

BABSON: Oh, yeah. I had to do homework. That was…

CARUSO: So they were involved, making sure things got done.

BABSON: My mother particularly. Yes.

CARUSO: One other thing that, then—sorry about…

HUNTER-LASCOSKIE: No…

CARUSO: I'll turn it back to Sarah. In reading through some of the materials that you sent, you talked a little bit about—I'm assuming this was during your childhood, [but], I guess it could have been when you were older—but shooting off fireworks that the ingredients…

BABSON: Did I send you my memoirs?[2]

CARUSO: I don't…No, it wasn't memoirs. It was a piece that you wrote about kids today.[3] It talked about bike helmets and…

[2] See Appendix XI: Remembrances.
[3] See Appendix VII: The Brave New World of Guns for Everyone.

BABSON: Oh, that was one of the essays I sent you.

CARUSO: Yes. Yeah. So in that you talk about using…you could get the ingredients for black gunpowder at the local pharmacy. You liked making bombs…

BABSON: Yeah.

CARUSO: …using spent CO_2 [cartridges].

BABSON: Yeah, I did.

CARUSO: How did that…was that when you were a little kid? Was that when you were older?

BABSON: Not too little, but probably started in grammar school.

CARUSO: Okay.

BABSON: But…and continued through junior high and high school.

CARUSO: And was that just something fun that you did, or were you…?

BABSON: Mostly.

CARUSO: Okay.

BABSON: Yeah <**T: 20 min**>, mostly fun. I mean the Fourth of July was my second favorite holiday after Christmas. Of course, you could buy any kind of fireworks back then, and it wasn't restricted at all. So we were…I loved to blow things up.

HUNTER-LASCOSKIE: Did you have an audience to blow things up, or was this just on your…?

BABSON: Oh, sometimes I did. But sometimes I just did it by myself.

CARUSO: How did you go about figuring out how to do some of these things? I mean you mentioned home-brewed seltzer water. You used a spent CO_2 cartridge, and you detonated it electrically. I mean these…

BABSON: Yeah.

CARUSO: …aren't necessarily things you pick up in school.

BABSON: No.

CARUSO: So I was wondering how…?

BABSON: Well, once you have the formula for black powder, black gunpowder, then the sky's the limit. These little CO_2 cartridges, which would make home charged water, sparkling water, when they were spent you'd knock out the plug and fill it with black powder, put in two electric wires, which are shorted out. So when you plugged it in to the wall socket, they would…it would explode.

CARUSO: So you were doing this at home.

BABSON: Oh, yeah absolutely.

CARUSO: Did your parents know that you were doing this?

BABSON: Yes, they did.

CARUSO: Okay.

BABSON: They were very tolerant.

CARUSO: Tolerant of explosions. What about the…you also mention putting explosives on the trolley tracks on Bloomfield Avenue.

BABSON: Yeah. These would…I would use .22 long, spent cartridges, cartridge casings. Fill them with black powder. Put in a blue tip match head, which ignites on just anything. Crimped […] the .22 cartridge shell casing over and stick it on the trolley tracks. The trolley would run over them, and they'd explode with a very big report.

CARUSO: Did you ever get in trouble doing such things?

BABSON: No.

CARUSO: No.

BABSON: No. Never got caught.

CARUSO: Never got caught, even better. So I guess while you were making money at the local gun club, you were also…that's where you were collecting your shells for your explosions.

BABSON: No. No […], I had a .22 caliber rifle, and we used to shoot stuff: tin cans, and squirrels, and whatever we could shoot. So that's where I'd get my cartridge casings.

CARUSO: Okay, all right. So explosions, shooting things, that was part of your childhood …

BABSON: Yeah.

CARUSO: Interesting.

BABSON: Absolutely.

HUNTER-LASCOSKIE: So obviously you have this interest in science, chemistry, technology. Your parents know it's a foregone conclusion that you would go to college. What was the process like of figuring out where you were going to go? Did you want to stay close? Did you want to finally get out of New Jersey for a little while? What was that process like?

BABSON: Well, I applied at Cornell when I was seventeen, and was accepted. [Both my father and brother had gone to Cornell]. I applied in the School of Electrical Engineering, because my brother was a chemical engineer and I wanted to do something different than him. I thought you had to be an engineer to make a living. That's how little I knew. I really didn't know much about electrical engineering. I didn't even know the difference between an electrician and electrical engineer at the time.

But at the same time, I applied at Rutgers, [The State University of New Jersey], [which] had what they called ASTRP, Army Specialized Training Reserve Program, which was the forerunner of the […] ASTP, Army Specialized Training Program, which was where most of the officers came from. I figured if I had to go into the Army that it would be better if I was an officer than an enlisted man. It didn't work out that way, because I got kicked out of Rutgers because I cut a chemistry class of all things <**T: 25 min**>. The program was quite easy, I thought. I didn't really have to study at all, and I was still on the Dean's List.

But one afternoon, my friend and myself decided we didn't want to go to chemistry lab, we wanted to sack out. We knew the routine of the officer there, or probably the sergeant, I've forgotten…no, maybe it was an officer. We knew his routine pretty well, so we knew he wouldn't come around during that afternoon. So we decided to sack out in bed. Well, what we didn't know was that day, that very day, there was a new officer that was assigned to the group. He came around. He caught us in the sack, and an hour later we were […] hitchhiking home.

So that's when my father really laid into me and said, "You have two weeks to get a job." Actually, I got a job at Cullen's Photography working in the back room […] making prints and developing film. I had that job for a couple of weeks before I got the job at American Dyewood through my father's contact with Mr. MacKinney. I may be the only person in the world that was kicked out of Rutgers and went back to earn a Ph.D. [laughter] I don't know.

HUNTER-LASCOSKIE: You said your brother was in the chemical engineering program.

BABSON: Yeah.

HUNTER-LASCOSKIE: Were you aware of what he was doing? Did you talk about coursework at all?

BABSON: Not particularly, I don't think. I don't recall at least. He […] was a year ahead of me, but his education was interrupted by the war, too. He went into the [United States] Navy. Matter of fact, we were in Nara, Japan, at the same time. But never were able to…or no, it was Nagoya, Japan. But we were never able to contact each other, unfortunately.

HUNTER-LASCOSKIE: So when did you get enlisted and go into the war?

BABSON: When I got my "Greetings" from President [Franklin D.] Roosevelt on March the 4th, dated March the 2nd […]. I turned eighteen on March the 3rd. So they were waiting for me. So that's when I […received orders to report to Camp Kilmer, New Jersey]. I arrived after about two weeks thereafter. So I spent almost two years in the Army.

HUNTER-LASCOSKIE: Where did you go first? I know you said you were in Japan.

BABSON: Well, I did my basic training in Camp Blanding in Florida in the summer of 1944. Hiking through sand with a full field pack for miles and miles, oh, it was awful. Then I went to Camp Gordon in Georgia for advanced basic training. Then I shipped across the country to Camp Adair, Oregon, where I was going to be shipped out to Japan. […I] was on the first troop ship to cross the Pacific unescorted, not as part of a convoy.

So <**T: 30 min**> I got to Japan right after the atomic bomb was dropped, and I have a feeling it might have saved…[President] Harry [S.] Truman might have saved my life. Because at that time the Japanese were, even though they were obviously defeated, they were going to fight to the last man defending their country.

CARUSO: That definitely was some of the hardest fighting with all the islands that people had to…

BABSON: Mmm.

CARUSO: …that the U.S. military had to take.

BABSON: Absolutely.

CARUSO: So just that I'm clear on the timeline of things, you finished high school.

BABSON: Yeah.

CARUSO: You applied to Cornell and Rutgers.

BABSON: Right.

CARUSO: You decide to go to Rutgers […].

BABSON: Yeah, because it was free, mainly.

CARUSO: When you were going into Rutgers, did you have any idea of what sort of career you wanted to pursue? Or…

BABSON: No.

CARUSO: …it was just, you were going to go in and figure it out there.

BABSON: Yeah.

CARUSO: Okay. But soon into your time at Rutgers, because of your deciding to skip a class once, and having it be absolutely the wrong day to do so, you were thrown out. So you spent a year, about, working after that.

BABSON: No. It was less than a year …

CARUSO: Less than a year.

BABSON: Because it was probably July until March that I spent working […] at Cullen Photography for two weeks, and then American Dyewood for probably nine months or so until I went into the Army.

CARUSO: And so…

BABSON: Of course, then when I came back out of the Army, I realized I knew a little bit more. I knew the difference between an electrical engineer and an electrician. I decided I didn't want to be an electrical engineer. But that was the school I was accepted in, so after I got out of the war, that's where I had to start. I started in the spring term of 1946 […].

CARUSO: Okay.

BABSON: I didn't have a lot of the prerequisites for the engineering course, so I had a lot of electives, one of which was ornithology, another which was freehand drawing, another which was foundry. So I had a ball, but I realized that I really didn't want to be an electrical engineer, so I switched to zoology in my second year of Cornell, and had a ball.

Matter of fact…I used to take sugar and cream in my coffee. I learned […] to drink it black when we had an all night field trip in an ecology course studying ecological succession over the twenty-four hour period, the activity of different animals at different times of day and night. Somebody forgot to bring the cream and sugar for the coffee, and that's where I learned to drink it black.

CARUSO: So before we turn to your time at Cornell, I'd actually like to hear a little more detail about your time in the military, if you feel that there's more to say about it. Hear more about…I mean what was it like going to basic training? What were you doing in advanced basic?

BABSON: Well, I do remember…I still remember my Army serial number […12102461]. I used to remember my rifle [serial] number for many years […] that I had in basic training and advanced basic. Then when I went overseas to Japan, I was in four different field artillery outfits. I never saw an M-1 Garand the whole time I was over there. So memorizing my rifle serial number didn't help.

So I was in…I had a number of different jobs in the field artillery, different outfits. One of which <**T: 35 min**> was cook. I remember the conversation I had with the CO [commanding officer], who called me into his office—verbatim. He said, "Babson, have you ever cooked?" I said, "No, sir." He said, "Would you like cook?" I said, "No, sir." He said, "Report to the kitchen tomorrow morning at four a.m., you're a cook."

Actually, I knew…a friend of mine was the mess sergeant, and he was in charge of not only the enlisted men's mess, but the officers' mess. He arranged for me to get assigned to

officers' mess, which was a much better deal, because it was only two of us cooked for about thirty-five officers. We only worked every other day, but we worked a long day, about a fourteen or sixteen hour day. But we ate much better than the enlisted men, because we ate the same thing that the officers did. So that's why it was a good deal.

I was also in the wire crew. I had a few experiences, some not very pleasant in there, because I was assigned to the—and this was in Nara, Japan, I believe—I was assigned to the switchboard. The switchboard was fairly primitive. You had about a dozen lines that...and a little thing would drop down when a call came in. You'd push it up and you'd plug in a cord to the thing, and answer the phone. I was assigned to cover the switchboard at night on my first day, because at nighttime it's not so busy, so it would allow me to get used to the operation. They had a night alarm on the switchboard which would…besides this little thing coming down and flipping down, and going [Babson makes a noise that sounds like pprrrtt], they would turn on an alarm. They had a bed there, so that you could go to sleep periodically, because it was…I remember being awakened on my first night by a fire alarm going off. I woke up and the night alarm was on on the switchboard and was buzzing away barely. It […] didn't wake me up at all. I looked out and headquarters battalion was on fire, the whole building. Fortunately, nobody was hurt but there was a number of 55-gallon fuel drums sitting next to the building that went off one by one. It was this spectacular fireworks. I was terrified that I was going to get in trouble. The fire warden showed up the first time in the morning—he was a second lieutenant—and he was as worried as I was, because this was his first day as fire warden. But we escaped punishment out of that.

But one memorable occasion was, we used to go out on field trips and we'd have…the battery had four howitzers and they would be setup. Then we'd have…we'd have to run lines to the different parts of the battalion—battalion headquarters, the battery, the field forward observation posts, and so forth. The normal way of running these lines was you'd have a reel on the back of a three-quarter ton truck, which would be moving at about three miles an hour. Somebody walking behind the truck would push the wire off the road. Well <**T: 40 min**>, the forward observation post was probably almost a mile ahead of the gun battery. If…and there were four different…what do you call them? Not battalions, batteries, I guess, yeah. Four different batteries, so there were sixteen howitzers in a battalion. We had to pick up after the exercise. We had to pick up all the wire. This was kind of tedious, because you'd have to have somebody on the [truck] cranking up the reel, while somebody else was walking behind them making sure that the wire was [straight]. Well, the four wires on the side of the road, if your wire got tangled up with anybody else's, guess whose wire got cut?

CARUSO: Yeah.

BABSON: So if you were the last one to pick up your wire, you'd be spending an extra hour or two out there just splicing it back where somebody else had cut it. So I got an idea one day that I would figure out a way to lay the line way off the road. I cut down a bamboo tree about twenty feet long. I attached it to the side of the truck, turned the reel sideways, so we'd feed it

out off the thing. I had a pulley at the end of the bamboo pole, and a rope that you could raise it up and down so that you could go past the bushes and what not.

So we would lay our wire down at about twenty miles an hour […] instead of three miles an hour, and that was great. It would be far enough away from the other lines that it wouldn't get tangled up. So that was kind of a clever approach. I overhead one officer one day explaining to somebody else that…saying how clever he thought that was. He didn't say it to me, though.

CARUSO: So is there anything else from your military career that you remember as being significant or…I mean, I guess you were there after the fighting had pretty much stopped…

BABSON: Yeah. It…

CARUSO: …so you weren't seeing any action…

BABSON: Well, it was totally over. I mean…

CARUSO: So it was mostly just kind of hanging out, keeping routine?

BABSON: It was…well, it was the occupation, Army of Occupation. We did mostly guard duty there, which was boring as hell. But I did have one experience of guard duty. Well, we had an ammunition dump. We called it the "ammo dump," where we would store the howitzer shells. One of the [guard] posts […] was this ammo dump […]. I would always vie for this duty, because nobody else wanted it. It was kind of spooky in this…this was a manufacturing…I don't know what it was. They had a lot of lathes in it, and pits where we stored the ammo. But I would…I liked this duty because it gave me an opportunity to sit down and read, [to] read a book. To do this, I had to booby trap, essentially, all the entrances to the building, which I did by a number of different clever arrangements.

I remember one day, one typical day, there was a front entrance and a rear entrance, and I had a number of different booby traps that I would…I would salvage wire from these motors, you know <**T: 45 min**>, undo the windings and get these tiny copper wires and set up traps. I had one that was several yards away from the building, where if you walked about it, you'd trip this wire. The wire would go into a…come through the window, go into a shelf on top of the window, and trip a huge lampshade, which would come down, a metal lampshade, which would come down and make a terrible racket. Then I had at the front door, I had another trip wire which would send a wire reel bouncing down the steps. I had a sheet metal there that you couldn't avoid stepping on, which would make a racket. The backdoor I had another wire

which…I've forgotten what it…oh, yeah. I had a big metal sheet tipped up on a barrel, 55-gallon drum, and the wire would stick out, pull out a stick and the […] metal would come down on top of the barrel. I had a number of these things.

So I was pretty well protected, so that I would get advance warning of any inspection.

CARUSO: So…

HUNTER-LASCOSKIE: Did anyone set it off at all?

BABSON: Oh, yeah. Yeah. It was one…I remember one time during the day, and I was still walking around, because I hadn't finished setting up all my booby traps. I saw this officer come down and sneak by the windows. He thought he'd come in the back way and surprise me. I timed my walk until I was…until he and I would end up at the same backdoor. He had to crouch down to come in the backdoor. Bang, the thing came down, and I was right past him. I took my rifle off my shoulder, and spun around, and pointed it at his head and said, "Halt. Who goes there?" He was kind of scared.

But another time, the lights went off—and this was at night—I heard somebody come in the front door, because I heard the metal plate and the spool come down the stairs. I couldn't see. I was scared. So I yelled, "Halt. Who goes there?" No answer. No answer at all. I was about ready to let loose, when a little dog walked up to me, who had obviously set off the booby traps. But that was kind of interesting.

HUNTER-LASCOSKIE: Anything else from this time period, or…?

BABSON: Isn't that enough?

HUNTER-LASCOSKIE: They're great stories. So, any…?

BABSON: No. I can't think of any.

HUNTER-LASCOSKIE: So when did you return, then, from Japan?

BABSON: I returned…well, let's see. It was […] about December of 1946 […]. I started Cornell in the spring term, January that year. I remember I was going across the ocean and

coming back, both times, on Halloween. So, basically, I spent a year there, and then mustered out.

HUNTER-LASCOSKIE: So then you entered Cornell.

BABSON: So I entered Cornell…

HUNTER-LASCOSKIE: And…

BABSON: In the field…in the School of Electrical Engineering, which I already decided I didn't want to do.

HUNTER-LASCOSKIE: Right.

BABSON: But that's all they would let me…

HUNTER-LASCOSKIE: Right. So even though you weren't maybe as interested in <**T: 50 min**> your main coursework, did you get to take anything else that was interesting before you switched in your second year?

BABSON: Yeah. I took ornithology. I took freehand drawing. I took foundry. But…

HUNTER-LASCOSKIE: So you at least had a good time…

BABSON: But, then I…but in my second year I switched to zoology, because I liked animals. And that was a blast. I had a lot of courses, some courses, which were a lot of fun.

HUNTER-LASCOSKIE: Did you have to do a lot of fieldwork, and…

BABSON: Yeah.

HUNTER-LASCOSKIE: …go out on trips?

BABSON: Yeah. Yeah, a lot of fieldwork. But my last year I took biochemistry and that was a blast too. That's when I decided that I really needed to go to graduate school and get an advanced degree in biochemistry. So I went to Rutgers. Of course, I'd gone on the G.I. Bill [Servicemen's Readjustment Act of 1944], so it didn't cost my parents much of anything. I started out at Rutgers to get a master's degree in biochemistry and assuming that would be the end of my education. Actually, as I finished my first year in biochemistry, I had a job lined up with Squibb [Corporation], a laboratory job.

My boss at that time [James B. Allison] decided, or offered me a fellowship to go on to my Ph.D. This was a fellowship that had been started by Arthur McCollum who invented Flako Pie Crust. I don't know if it's even still available anymore in the stores. But he set up this fellowship, and I was the first McCollum Fellow. So that paid me a stipend, which was more than the graduate assistants were getting. I think they were getting like fourteen hundred dollars a year, and [for] which they had to teach a third of the time. I was getting two thousand dollars a year, I believe, and didn't have any teaching obligations. So it was great.

HUNTER-LASCOSKIE: So I want to go back, only a little bit. So why Rutgers? Why did you want to go back?

BABSON: Well, it was…why Rutgers? I believe I had applied for Rutgers and Cornell in graduate school. I was only accepted at Rutgers, so…

HUNTER-LASCOSKIE: And were your parents excited you were going on to graduate…

BABSON: Oh, yeah sure. Yeah.

HUNTER-LASCOSKIE: Who were you working with when you were at Rutgers?

BABSON: A gentleman by the name of Jim Allison, who was my advisor. He was a great guy. But he was in charge of the, what at that time was, Bureau of Biological Research, so-called, which was on top of a roller skating rink. [laughter] Yeah, in fact.

HUNTER-LASCOSKIE: An active roller skating rink?

BABSON: Yeah. I believe so. Now it's a huge building, independent building and very prestigious. But then, it was pretty primitive. But it was okay.

HUNTER-LASCOSKIE: What kind of work were you doing? What was…?

BABSON: It was…he had done…he was interested in protein nutrition in <**T: 55 min**> tumor bearing rats, in cancerous rats. It was not a terribly exciting project, actually. But it…I got a couple of publications out of it, I think.[4] I sent you a list of my….

HUNTER-LASCOSKIE: Mm-mm.

BABSON: Yeah. You have that. But it wasn't very earth shattering.

CARUSO: How was it transitioning from being an undergraduate to being a graduate student? Was it any different for you?

BABSON: Was it any different? No, I don't think so.

CARUSO: Okay.

BABSON: I don't think so.

CARUSO: And what was the structure of the graduate program like? Did you go straight in and start doing research? Did you…?

BABSON: Yeah.

CARUSO: I mean I know you…well, you started in a master's…

BABSON: I had a number of courses I had to take. Yeah. But I also started off in…I believe started off the first year in…. Well, no. No. It's not right, because when I was getting my

[4] A.L. Babson, "Some Host-tumor Relationships with respect to Nitrogen," *Cancer Research* 14 (1954): 89.

master's degree, I don't believe I had any independent research to do. I think I only did coursework. Yeah. I believe that. When I started…when I got my Ph.D., I started doing also research projects. Yeah.

HUNTER-LASCOSKIE: I know from your essays, during this time you also got engaged, and…

BABSON: Yeah.

HUNTER-LASCOSKIE: Now…

BABSON: Well, yes. I got engaged. This was before I graduated, actually, from…well, maybe it was about the time I graduated from Cornell and with degree in zoology. Well, how are you going to make a living with a bachelor's degree in zoology? My father-in-law, my potential father-in-law was not too pleased that his daughter, [Doris Lelong], was marrying this…engaged to this deadbeat that didn't have any means to make a living. So that's one of the reasons I decided I had to go back to graduate school.

HUNTER-LASCOSKIE: Did you meet her at Cornell?

BABSON: No. She lived in my home town. I met her in high school, and we'd stayed in touch for a long time. When I came back from the war, we got together again, and…

HUNTER-LASCOSKIE: So while you're in graduate school, obviously you're engaged. You have to think a little bit more about your career. What in the research you were doing was really piquing your interest? You said your research with your advisor, at first, maybe wasn't interesting. But was there something that was really getting you interested?

BABSON: I don't recall to tell you the truth. I like biochemistry as a field. But I don't remember one thing that piqued my interest, particularly.

HUNTER-LASCOSKIE: So when you were doing research did you collaborate a lot? Were you working with a lot of different students? Were you…?

BABSON: No. No. I was working on my own. Yeah.

CARUSO: Was that typical for students there? I mean…

BABSON: For graduate students.

CARUSO: Graduate students.

BABSON: Yeah.

CARUSO: So at the time it was [that] you worked with your advisor, or you had a project with your advisor…

BABSON: Right. Yeah.

CARUSO: …and then you were just in the lab doing your own stuff.

BABSON: Yeah. Right.

CARUSO: Okay.

BABSON: Pretty much.

HUNTER-LASCOSKIE: So while you're in graduate school, obviously I know that you went on to do a postdoc. Was that kind of the natural trajectory? Or was it something you looked for or you were recruited for while you were in graduate…?

BABSON: Well, to tell you the truth, it was a job. I went to the Federation [of American Societies for Experimental Biology and Medicine] meetings in Chicago, in the spring of 19…oh, what year was it <**T: 60 min**>? Well, the year before I got my Ph.D.

CARUSO: It was around '52?

BABSON: […] Was it ‘52 or ‘53?

CARUSO: I think you got your degree in ‘53.

BABSON: Yeah, ‘53. Yeah.

CARUSO: Yeah.

BABSON: Yeah, ‘53. I went out to the Federation meetings in Chicago to interview for whatever job I could find. One of my interviews was with Ted [Theodore] Winnick, who is from the University of Iowa, who I ended up working for. He gave me […] an offer, and it was one of the best offers I’d had, so that’s the reason, major reason, I decided to do a postdoc.

HUNTER-LASCOSKIE: Right. So when you were looking, you were just looking for a job.

BABSON: Any, any…

HUNTER-LASCOSKIE: It wasn’t a…

BABSON: …job. Yeah, looking for a job. Jobs were few and far between at that time, as I recall. So this was something to do.

HUNTER-LASCOSKIE: And so…

BABSON: And I already had a wife and a child, [Betsy Linda Babson], at that time, so…

HUNTER-LASCOSKIE: Oh, so you already had a child.

BABSON: Yeah. She was six months old…

HUNTER-LASCOSKIE: Oh, okay.

BABSON: My daughter. And so we drove out to Iowa City, [Iowa].

HUNTER-LASCOSKIE: And how did your wife feel about the prospect of moving to Iowa City?

BABSON: I don't think she…she wasn't terribly excited about it, but she was okay with it. Yeah. As I recall, she was okay with it.

HUNTER-LASCOSKIE: And was she…so she was at home, then, with your child at the time when you first…

BABSON: Yeah.

HUNTER-LASCOSKIE: …took the position.

BABSON: Yeah. We were living in Metuchen on the second floor […] apartment of a private home. She was…she had worked at the telephone company as an operator, I guess. I've forgotten…no, sort of a technical service representative. But when we had Betsy, my daughter, she quit and stayed home and we managed on my salary from my doctoral program. Didn't live high on the hog, but we survived.

HUNTER-LASCOSKIE: So then you make the move to Iowa City.

BABSON: Yeah.

HUNTER-LASCOSKIE: And as you said, you're working with Ted Winnick. What was that transition like? What were you doing there?

BABSON: Well, I was doing…that was…I was doing protein metabolism in tumor bearing rats, again, but using isotopes, using C^{14} amino acids. It was an interesting project, but again, not earth shattering. So…

HUNTER-LASCOSKIE: Did you have a lot of interaction with your advisor? Did you work closely with him, or were you pretty independent?

BABSON: As I recall, I was working pretty independently. But we would chat, obviously, frequently. One of the things that he sparked my interest in is in mineralogy. He was a collector of minerals. I became very much interested in that, and still have a mineral collection at home.

HUNTER-LASCOSKIE: Now you have…obviously we have your publication list. We know you have some publications coming out at the end of your graduate career, and the beginning of your postdoctoral. Was it something you were kind of thrown into, or was somebody helping you figure out how to write a paper, how to publish?

BABSON: Oh, I was able to figure it out myself, pretty much. I mean obviously I read a lot of papers. This was before the Internet, and you had to do library researches by <**T: 65 min**>…

CARUSO: Card catalogues and….

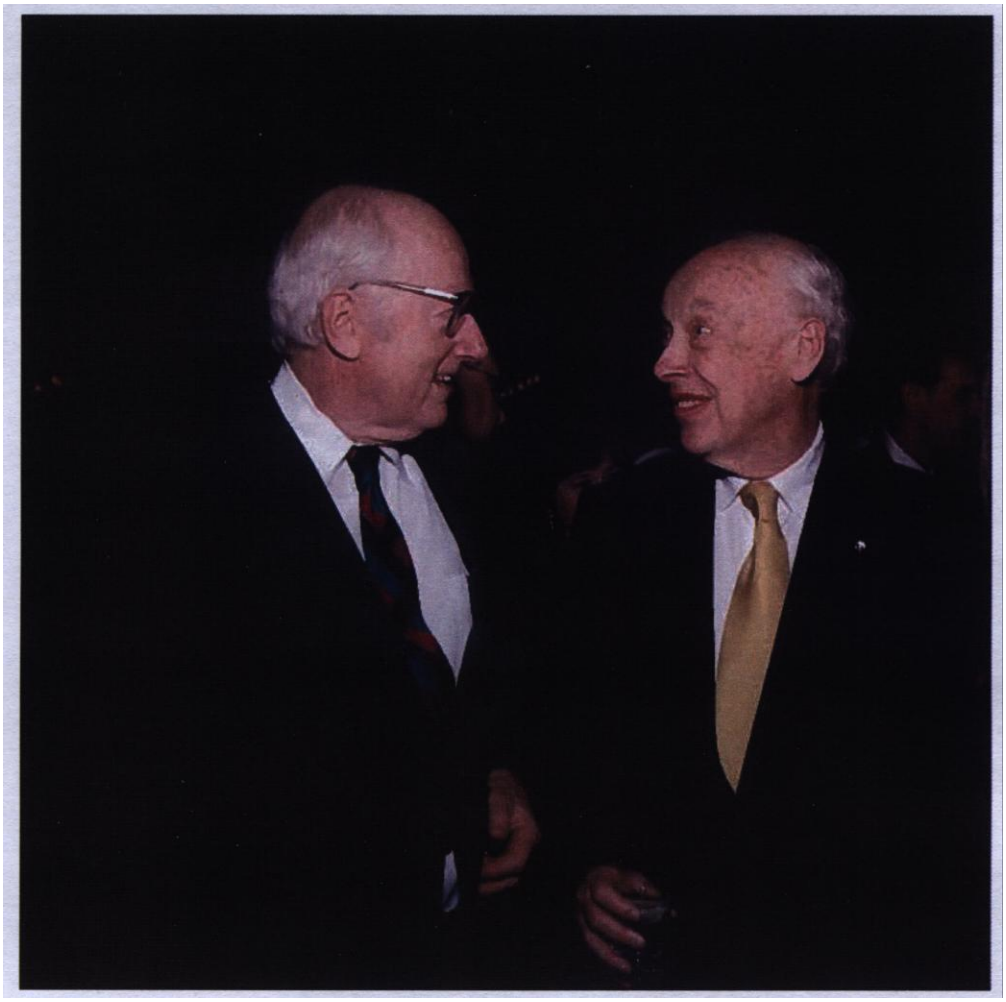
BABSON: Yeah, right. It wasn't as easy with…as it is now.

CARUSO: More broadly, did you notice any changes going on in these scientific worlds around this period of time? I mean in the post-war world, right, the U.S. is heavily invested in science. You have the rise of the National Science Foundation around this period of time. You're working with your postdoc…. You're in the radiation research lab…

BABSON: Right.

CARUSO: …which, I think, is significant as well. Did you see science as a good career choice at this period of time, or was it simply just that you were interested in it, and so you were going to make it your career?

BABSON: A little of both. […] I mean this was at the time when [James D.] Watson and [Francis] Crick came out with the double helix. Of course, I got to know Jim Watson personally, later, because he was on the board of directors of the Diagnostic Products Corporation. So it was an exciting time in science.



Arthur L. Babson with Nobel Laureate, James D. Watson at a DPC distributors meeting circa 2000.

CARUSO: Was your…I guess your father-in-law was approving of…

BABSON: Oh, yeah.

CARUSO: Yeah.

BABSON: Yes.

CARUSO: Okay.

BABSON: Yeah. He certainly approved of my actions in getting a Ph.D.

CARUSO: Did having a family change your relationship to your work at all?

BABSON: No. I don't think so.

CARUSO: So while you're pursuing your postdoc, you're working with Winnick. Was it the same sort of relationship that you had with Allison in terms of, he gave you a project you worked on it? Or did you have more freedom to do investigations?

BABSON: I think I probably had more freedom with the postdoc, but it was essentially his project that he was interested in. But I had more freedom in terms of how I pursued the project, absolutely.

CARUSO: Okay. Yeah. When you were at Cornell, you had an interest in science. You switched to zoology. I'm assuming you were doing laboratory courses. You had mentioned going out…

BABSON: Oh, yeah.

CARUSO: And doing that. So you're learning, I assume, some laboratory techniques…

BABSON: Yeah.

CARUSO: …during that period of time. The techniques that you learned in college, were they things that prepared you well for your work in graduate school? Or did you have to learn something new in graduate school to do the experiments that you were doing?

BABSON: Well, the newest thing…well…

CARUSO: Because I'm thinking of it from my experience in college laboratory courses, where, you know, there's a lab manual. You have an experiment. You follow the protocol and

you write up some data. That didn't necessarily prepare me in any way to go and do research on my own, right.

BABSON: Yeah.

CARUSO: Because you have to be creative. You have to start thinking of new ways. In college classes, you don't necessarily get introduced to the newest laboratory techniques. You're kind of doing things that everyone knows.

BABSON: Right. Yes, sure.

CARUSO: So I'm assuming in graduate school, or I'm wondering if in graduate school you had to learn new things? And if you did, how did you go about learning those?

BABSON: That's a good question, and I don't remember to tell you the truth. [laughter]

CARUSO: Okay.

BABSON: That was a long time ago.

CARUSO: Yeah. Well, just in case, I wanted to ask. You know that also led me to wonder with your postdoctoral years, if you're doing research on your own…. I think there's a lot in doing scientific research that's about being creative, right. Trying to come up with ways to answer questions that people can't…that haven't been answered yet.

BABSON: Right.

CARUSO: So I'm wondering how it is a scientist goes about doing that. I mean during the war, you were rigging up booby traps, right. As a kid, you were exploding things. You were coming up with your ignition devices. It seems like there is a certain level <**T: 70 min**> of creativity, and I was wondering if that mapped into your scientific and laboratory experiences at that time?

BABSON: I can't think of any specific examples, to tell you the truth. No. I can't think of any specific examples.

CARUSO: Okay. During the time in graduate school and as a postdoc, what sort of hours did you have? Was it like a nine-to-five thing for you? Or were you, "I was up all night doing research"?

BABSON: Yeah. Well, no. I never stayed up all night doing research. But no, I put in pretty regular hours, probably longer than most.

CARUSO: But it was still just sort of a very sort of routine Monday through Friday?

BABSON: Yeah.

CARUSO: Did you do weekends?

BABSON: Don't remember…

CARUSO: Okay.

BABSON: …to tell you the truth.

CARUSO: I think that also it was during your postdoctoral years, you met—I'm going to probably mispronounce this last name—George Kalnitsky.

BABSON: Yes.

CARUSO: Was it during your postdoc years that you…?

BABSON: George was…yeah. I was a postdoc. He was a professor at the University of Iowa.

CARUSO: Okay. So you just struck up a friendship with him…

BABSON: Yeah.

CARUSO: …or were you collaborating with him at this time?

BABSON: No. We never collaborated, but it was just a personal friendship.

CARUSO: Okay. Now during this period of time, you had this offer from Squibb, right, while you were…

BABSON: Yeah.

CARUSO: …doing your master's. Did you have a vision of…and the research that you were doing for your graduate advisor and for your postdoctoral position, it didn't…the way you made it sound, it wasn't necessarily research that *you* were interested in.

BABSON: No.

CARUSO: Right.

BABSON: No. It was the professor's projects really.

CARUSO: So did you know what you wanted to do in science?

BABSON: No.

CARUSO: No.

BABSON: No.

CARUSO: No, not at all.

BABSON: I had no idea. I had no idea.

CARUSO: So you just wanted to do something *in* science…

BABSON: I wanted to do science. Yes.

CARUSO: Okay. But you were open to doing anything?

BABSON: Right.

CARUSO: And mostly with biochemistry systems…

BABSON: Yeah.

CARUSO: …along those lines?

BABSON: Yeah.

CARUSO: Okay. So while you were doing your postdoc, which was just for one year…?

BABSON: It was just one year. I had just started, actually, started my second year, when I got a call from Ulrich [V.] Solmssen, who was then head of research at what was then Chilcott Lab or, no, maybe it was Warner-Chilcott Laboratories.

CARUSO: Warner-Chilcott, I think.

BABSON: Yeah. It was Warner-Chilcott Laboratories at that time. And [he] offered me a job as a biochemist in the research department. I had no idea who my boss was going to be. I didn't meet him until the first day of the job. I didn't meet my assistant until the first day on the job. It just sounded like a good opportunity.

CARUSO: How did Warner-Chilcott find out about you, then?

BABSON: Well, I knew, personally, Ullie Solmssen, who lived in Essex Fells…

CARUSO: Okay.

BABSON: …who was the…and, actually, I interviewed with him in his house, before, I think, before I went to Iowa. Yeah.

CARUSO: And so he just out of the blue calls you up one day…

BABSON: Yeah.

CARUSO: And says…

BABSON: He just out of the blue called me up one day. Of course, he knew my father very, very much. I expect my father had some influence on him. My father bailed me out a number of times. But he just…they were looking for a biochemist, Ph.D. biochemist at the time. I guess he must have been impressed with me, because I hadn't really spoken to him in like a year. He called me up and offered me a job.

CARUSO: Okay. Well, I guess that works.

BABSON: And it sounded good, so I accepted.

CARUSO: Did you know anything about Warner-Chilcott before then?

BABSON: I had applied there before I got my master's degree. I'd interviewed with a couple of gentlemen <**T: 75 min**>, not George Phillips, who ended up being my boss, but a couple of gentlemen there. I'm sure that they…and they didn't have a job at the time. But I'm sure that they influenced Ulrich Solmssen, or I'm sure he checked on them before he blithely offered me a job on the telephone.

CARUSO: And how did your wife feel about heading back to New Jersey?

BABSON: Oh, she was fine with it. Yeah.

CARUSO: Yeah. Okay.

BABSON: She wasn't crazy about Iowa either. There's nothing out there but corn […].

CARUSO: All right, so this is around 1954, 1955.

BABSON: Nineteen fifty-four. Yeah.

CARUSO: Nineteen fifty-four, okay. So you travel back across the country, and still just one child.

BABSON: Still one child, yeah.

CARUSO: And you mentioned just a couple of minutes ago that you took a job without knowing anything…

BABSON: Anything…

CARUSO: …about the position.

BABSON: Except that I had…I knew the facilities, because I had interviewed a year before.

CARUSO: Right. Did you know your salary or anything like that? Or was it…?

BABSON: Yeah. He gave me my salary, which was much more than I was getting as a postdoc.

CARUSO: Okay. So…

BABSON: But…

CARUSO: Tell me about…

BABSON: …piddly now.

CARUSO: So tell me about your first day.

BABSON: My first day I walked in and met my boss, [George Phillips]. He introduced me to my assistant, Sylvia Malament, and that's as much as I remember of my first day.

CARUSO: Okay. You…were you given any specific projects to work on immediately?

BABSON: Yes, I was. The then president of the research institute, a fellow by the name of George Mangum, had this idea that—and this was before there was a diagnostics industry, so to speak—had this idea that what the clinical laboratory really needed to improve the quality of their testing was a serum control, a known serum control, basically as a standard rather than making up aqueous standards, which was the routine at the time. Also, all these standards were made up by the laboratory, because, as I say, this was before there was a real diagnostics industry…

CARUSO: Right.

BABSON: …to serve this. So he had this idea that a serum standard, a standard in serum would be very useful for the laboratory and improve the quality of their testing. So that was assigned to me and Sylvia, as our first project.

CARUSO: Okay.

BABSON: And we…and it ended up as a product called Versatol that was successful for many, many years, and spawned a number of different products, serum quality control products,

particularly Versatol-E, which is something which led me…got me into […] enzyme assays. Versatol-E (E for enzyme), which was…so that was essentially my first project.

CARUSO: Okay.

BABSON: Sylvia and I would go out and routinely make a circuit of the local blood banks, who would give us their outdated blood. That's when it wasn't…once it was outdated you couldn't infuse it anymore, transfuse it, so it was worthless. They would throw it out. So we would say, "Don't throw it out, we'll take it."

CARUSO: So you went on blood gathering missions.

BABSON: We went on blood gathering missions, and that would be our raw material for what ended up as Versatol serum standard.

CARUSO: Now was this an area of science that you were at all familiar with before having to work on it? I mean, did what you learn or what you did during the…

BABSON: No. No.

CARUSO: Okay.

BABSON: It was totally new. Totally new <**T: 80 min**>.

CARUSO: Then can you take me through, because I mean Versatol was introduced in '56.

BABSON: Yeah, I believe so.

CARUSO: So there wasn't…it seems like you worked pretty fast on…

BABSON: Yeah.

CARUSO: …all this stuff. So can you take me through the process of how you figured all this out? I mean…

BABSON: Well, we decided to start with outdated blood plasma, because it was free.

CARUSO: Right. Free is always good.

BABSON: Free is good. But we had to process it, because, for instance, some levels were way out of physiological range, like potassium for example, which leaks out of the erythrocytes on blood storage, which is way elevated. So we had to get rid of it. So we figured we'd dialyze it. So we'd dialyze it, but then we had a problem that if you just used...dialyzed against distilled water, some of the proteins would precipitate out, because they weren't stable under the low ionic strength. So we figured out that we would dialyze it against magnesium acetate at, I think 10 or 20 millimolar, I'm not sure which.

CARUSO: How did you…?

BABSON: Because this was before magnesium was a routine analyte in clinical labs.

CARUSO: So how did you come to using magnesium? I mean…

BABSON: Well, we realized we couldn't use distilled water, and we had to use something with ions in it to save the proteins from being precipitated.

CARUSO: And so is this just you experimented and found out magnesium worked? I was just wondering if there was a specific reason why you chose magnesium to begin with or if it was just…?

BABSON: Well, magnesium…we chose magnesium because it wasn't an analyte of interest in the laboratory at the time. It is now, but at that time it wasn't, and acetate, obviously, is just a counter-ion, which has no significance.

CARUSO: Okay.

BABSON: And we'd take the outdated plasma, and the first thing we would do with it was lyophilize it, freeze-dry it in bulk. Then, because we wanted to precipitate out those proteins which were unstable to lyophilization, to freeze-drying, and that was nifty way to do it is to denature them, essentially. Then, we'd dissolve this out, this precipitated, or lyophilized, protein in a concentrated form, about twice the concentration that it was initially, because every subsequent step we would have would be a dilution of the thing. The dialysis would add water and what not.

CARUSO: Right.

BABSON: So we'd dialyze it, the concentrated proteins. First, we'd centrifuge it to get rid of the [denatured proteins] after-bulk lyophilization. We'd centrifuge it. We'd make it in a concentrated form, centrifuge it, and get a clear solution, and dialyze it. Then we'd add back weighed-in quantities of the constituents for which we wanted to analyze, urea, and uric acid, for example, bilirubin, sodium, potassium, chloride, and uric acid, whatever. There were a number of ways.

CARUSO: And was there a specific proportion that you were adding in for each of these?

BABSON: Yeah.

CARUSO: What was the portion based on?

BABSON: It was a concentrate based on the final concentration we wanted the lyophilized product to have.

CARUSO: Okay. Now you're talking about doing this research. Again, are you pretty much, it's just you and Sylvia working on…

BABSON: Yeah.

CARUSO: …your own in the lab?

BABSON: Pretty much, yeah.

CARUSO: Given that you're in a company, did you have a lot of oversight from your supervisor at the time? I mean did you have to give regular reports, or was this a very sort of open experimental…?

BABSON: Well, it was pretty much my project. He would oversee it to a certain <**T: 85 min**> extent, but I was the one making the decisions for it.

CARUSO: Okay. And did you…you were provided with enough resources at the time to accomplish anything? You didn't have to like…I know you were getting free blood.

BABSON: Yeah.

CARUSO: And, you know, that probably cut down the costs quite a bit. But did you…did they give you sort of a specific research budget that you were allocated for the project? Was it sort of a…

BABSON: I don't recall a specific budget for the project. Now at Siemens [Healthcare Diagnostics], of course, we have budgets for everything.

CARUSO: Right. Well, times do change.

BABSON: Yes.

CARUSO: Yeah. So it took you about two years to get all of this done. What was their response to this at Warner-Chilcott? I don't know when it became Warner-Lambert.

BABSON: I don't remember either, to tell you the truth.

CARUSO: So I'll probably say Chilcott for a while, and then I'll revert to Lambert every so often. But was the response…?

BABSON: They were very excited about it.

CARUSO: Okay.

BABSON: Yeah.

CARUSO: Did they have anyone else working on a similar project? Or was it just you…?

BABSON: No, it was just me…

CARUSO: You were the only one, okay.

BABSON: Just me…well, and Sylvia, of course.

CARUSO: Were they doing any other research into *in vitro* diagnostics at the time.

BABSON: No. No, this was…

CARUSO: Okay, so this was like…

BABSON: This was it.

CARUSO: This was it.

BABSON: Well, they had four products that they…were left over from other companies that they'd acquired. One was [Simplastin], a thromboplastin, used in [prothrombin time determinations]…and I've forgotten where that came from, maybe Chilcott Laboratories. One was a beta glucuronidase, […] called Ketodase, which was used in urinary steroid assays. Another was Evans blue [dye], which was [used in] an *in vivo* test for blood volume, [the fourth was Inulin, another sterile *in vivo* product used to measure kidney glomerular filtration rate…].

They gave these products…they didn't know what to do with them. They gave them to a gentleman called Raphael Cohen, […] and they called it the Laboratory Supply Division of Warner-Chilcott Laboratories, which was the forerunner of General Diagnostics. Ray was in

charge of these four products, and, basically, was in charge of any future products that would come out in the diagnostic arena.

CARUSO: Okay.

BABSON: And he was excited about it, very much excited about it.

CARUSO: Excited in what way? As new business?

BABSON: He thought it was a new…well, he'd get very excited…. When I came out with my first enzyme product called PhosphaTabs, which was a tablet of phenolphthalein diphosphate in a Tris buffer for a single test. We had test wells, we called them, which were little tiny […] test tubes. You'd add four drops of blood [serum]. You'd crush a tablet with a glass rod. Then you'd incubate it for a period of time depending upon the room temperature. Then you'd add a drop of sodium hydroxide to develop the phenolphthalein color after a timed incubation, and measure it […] against a color chart.

It was semi quantitative, but it was actually pretty accurate. […] Ray was so excited about this he thought I should get a Nobel Prize. Really.

CARUSO: Well, I mean…

BABSON: And he arranged for *Life* magazine to come in and interview me. They had a photographer, *Life* photographer, magazine photographer taking pictures of this little test. Of course, they never ran the article. But…

CARUSO: So it sounds like you really were at the forefront of a new type of developing…or a new type of industry.

BABSON: Yes. I like to say that I was in the diagnostics industry since before there was a diagnostics industry <**T: 90 min**>.

CARUSO: And so is this an industry…so given that your other research endeavors before this point weren't things that necessarily interested you, was this something that was finally…

BABSON: Yeah.

CARUSO: …interesting you?

BABSON: Yeah.

CARUSO: Why?

BABSON: Because I could see the potential. I could see the potential for products that would be…I mean the clinical lab at the time, the clinical chemist made up all of his reagents, all of his standards by weighing out chemicals. I could see that that was not the best way to do it. There was a commercial need for reagents and standards, and that it was wide open.

CARUSO: One thing that I find somewhat interesting is that—and we can talk about this a little more as we go through your time at Warner-Chilcott-Lambert—is that you're also publishing articles…

BABSON: Yes.

CARUSO: …during this period of time. Were these articles directly related to the research that you were doing?

BABSON: Yeah.

CARUSO: I could also see that as being potentially a big loss for a company, if they're letting you publish the exact thing they're trying to capitalize on.

BABSON: Well, in most cases the products were patented.

CARUSO: Okay. So you were allowed to publish after patenting.

BABSON: Yeah.

CARUSO: Why were you publishing? What was the relevancy of publishing those articles? Was it just something that you wanted to do?

BABSON: Yeah. It was something I wanted to do. It increased my stature in the clinical lab.

CARUSO: Okay, so it was a way for you…

BABSON: I became a household word you might say, because, at the time, when I finished with Versatol and started Versatol-E (for enzymes), because I realized that the state of enzyme assays in the clinical lab was abysmal, they really needed an enzyme control or standard. I'd started…that got me interested in enzyme assays and the simplification of enzyme assays. At that time the units were, of course, arbitrary units that you'd get, because you can't say there's so many milligrams of alkaline phosphatase or whatever, in a deciliter or whatever.

So it became the inventor's name or the describer's name associated with that, like they were Karmen units, there were Bodanski units, and there were Babson units. If you look at a label of Versatol-E, and I don't know if they even exist anymore, you would see there are all of these different enzymes. You could see so many Babson units. There were Babson units for just about anything. So…

CARUSO: So once…

BABSON: [There] was a little ego invested in this, too, I'm sure.

CARUSO: Well, there has to be, right. Otherwise, it's not as interesting.

BABSON: Yeah.

CARUSO: So you came out with Versatol. You were also becoming interested at the time with standardizing enzymes, right.

BABSON: Yes.

CARUSO: But where—if I remember reading correctly in some of the materials that you sent—where were your ideas about moving in this direction coming from? Because I think you had mentioned some work by Felix Wroblewski…

BABSON: Wroblewski, yeah.

CARUSO: So can you tell me how you sort of started to move into the specific field?

BABSON: Well, Felix Wroblewski was a researcher/clinician, essentially, who had a chap working for him called Arthur Karmen, who invented the spectrophotometric […] assays for GOT and GPT, which are glutamic oxaloacetic [and glutamic pyruvic] transaminase, now called aspartate [and alanine] amino transferase <**T: 95 min**>, and these respective spectrophotometric [assays] measured the absorbance of NADH at 340 nanometers. [They] required a spectrophotometer.

I got an idea, which was a purely serendipitous idea, that I could come up with a colorimetric assay, by reading…you mentioned George Kalnitsky earlier. I was reading an article by George in *Biochemical Journal*—maybe you've read this before—I was reading it just for interest, because I was just wondering what George was up to. It was an article on a method for measuring acetoacetic acid in tissue extracts. He mentioned that he had to heat the extracts to prevent interference from oxaloacetic acid. As soon as I read that, my…a light bulb went off. I realized that oxaloacetic acid was one of the products of the transaminase reaction. He was measuring […] acetoacetic acid by coupling it with a diazotized paranitroanaline, I believe. At that time, […] stabilized diazonium salts were used in the dying industry quite frequently. I figured that this is a good reagent. This is a good, reactive colorimetric reagent. So I collected a whole bunch of free samples of stabilized diazonium salts. We screened all of them and found one that reacted very well with oxaloacetic acid.

I got a patent on that.[5] As a matter of fact, that's the only patent out of the twenty-five patents I got at Warner-Lambert, that's the only one that ever produced any kind of income through licensing.

CARUSO: After the first Versatol came out—in some ways that was started at the instigation of George Mangum…

BABSON: Right.

[5] Arthur L. Babson, "Method of Determining Glutamic-Oxalacetic Transaminase," U.S. Patent 3,069,330, issued on 18 December 1962.

CARUSO: …did your relationship…

BABSON: Well, he had the idea.

CARUSO: Right.

BABSON: And he just turned it over to…

CARUSO: Right. But for this second project, was this just something that you said to your supervisor, "You know what, I want to work on this." And he said, "Go ahead."

BABSON: Yeah, pretty much. Pretty much.

CARUSO: So you were given free rein after your success with the first project…

BABSON: Right. It led to a whole line of different products.

CARUSO: Okay. So the company itself, did they start investigating much more? Did they have other researchers at the time also going into this new field of diagnostics? Or…?

BABSON: No. Pretty much, it was…

CARUSO: So it was just you…

BABSON: Well, yeah, they did. Well, George Phillips, my boss, he had another lady working or reporting to him called Jane Lenahan, who was really directed at coagulation reagents. I mean she had…I mentioned that we had a product called Simplastin, which was a thromboplastin reagent used in […prothrombin time determinations]. But she also worked on other coagulation projects. Eventually, I took over that responsibility, but at the time of my hiring, she was also working on diagnostic reagents in the coagulation field […].

CARUSO: Okay. Okay. So you're given a little more free rein to do your research. Were you given…did you need more resources? I know you mentioned that you were getting some free samples again to do some of the initial work. But did <**T: 100 min**> you have any budget per se at this period of time? Or is it just…?

BABSON: No.

CARUSO: No.

BABSON: Well, there was a budget that was for the entire biochemistry department. George Phillips had a budget. But budgeting at that time was done, mostly, by a chap that had no…was in charge of research administration. So he figured out everybody's budget. It was a crazy system to tell you the truth. But I had no budgeting responsibility at all.

CARUSO: Okay. You just had a little sort of free rein.

BABSON: Yeah. I had free rein, essentially.

CARUSO: When you started your work into enzymes were you still working with Sylvia. I remember reading about a Prunella Reid.

BABSON: Yeah. Prunella Reid was my second assistant.

CARUSO: Second as in you had two assistants, or second as in Sylvia had moved onto something else.

BABSON: No. Sylvia stayed with me for several years. Then she basically moved to another group, but by that time I had a number of people working for me.

CARUSO: Okay. What training did Sylvia and Prunella have? Were they just college-educated?

BABSON: Yeah. None, none at all. No specialized training.

CARUSO: So how did they work with you? I mean you at least had some experience in grad school…

BABSON: Yeah. They were working pretty much under my direction.

CARUSO: Okay. So were you training them…

BABSON: Yeah.

CARUSO: …what to do and things like that?

BABSON: Right. Yeah.

CARUSO: Okay. So there was a mixture of…it wasn't just…when I think of a technician today, technicians come in with a certain level of training. They might have to adapt somewhat to a new environment, but they're kind of pre-trained, whereas, to a certain degree you needed to do some of that teaching, some of that training for them.

BABSON: Yeah. Absolutely.

CARUSO: So let's see. So you and Prunella developed the tablet of Tris buffer.

BABSON: Yeah.

CARUSO: And the phenolphthalein phosphate and that became…

BABSON: PhosphaTabs.

CARUSO: …PhosphaTabs. And that was the first serum enzyme test.

BABSON: Right.

CARUSO: Right. That was, to a great extent, independent of sample size.

BABSON: Yeah.

CARUSO: Yeah. Okay.

BABSON: What are you reading from?

CARUSO: We develop what we call an interview protocol, where we take notes to make sure that we have a good sense of everything that we want to cover.

BABSON: See, I don't…this is not for me.

CARUSO: No. We read and digest everything that we can to incorporate into something that makes sense to us.

BABSON: Oh, all right.

CARUSO: It's crib sheets.

BABSON: Yeah, okay. I was curious.

CARUSO: Yeah. So how long did the work on the serum enzyme tests take approximately? Was that another like two-year project? Or…?

BABSON: Well, yeah, probably a two-year project. Well, there were a number of projects going on simultaneously. We had PhosphaTabs Acid, which was a test for acid phosphatase. Then we had…well, we had transaminase tests going on, TransAc.

Arthur L. Babson with assistant, Prunella Reid, celebrating the introduction of PhosphaTabs Acid, May 1958.

CARUSO: TransAc. Were you also doing the work with the reactive dyes?

BABSON: Yeah.

CARUSO: At the same…

BABSON: Yeah.

CARUSO: Okay. So you had a lot of simultaneous projects…

BABSON: Yes.

CARUSO: …going on.

BABSON: Oh, yeah.

CARUSO: You're essentially…

BABSON: Oh, yeah, a lot of projects going on simultaneously.

CARUSO: So you're building the industry…

BABSON: Essentially.

CARUSO: Essentially. What was…I mean, in some ways, we've taken a tack that has focused principally on your scientific career for the past little while. What was going on for you personally? Or was anything relevant going on in your life outside of work at this period of time? I guess another question to, sort of, ask about is, how is your family—your parents, your brother—responding to, if in any way, to the work that you were doing?

BABSON: I think they were pretty much oblivious.

CARUSO: Okay. Did your brother go back into chemical engineering <**T: 105 min**>? Or did he…?

BABSON: […] Yes, he got a job in chemical engineering, a couple of jobs actually for a long period of time. I'm trying to think of the name of the companies that he worked for, and it escapes me. But he's still alive. I can ask him.

CARUSO: And your parents, they were just, you know: you have a job; they're happy.

BABSON: Yeah. Yeah.

CARUSO: And what about your wife, and your daughter? Was your family growing at all? Or…?

BABSON: Well, yeah. We had a son, Jim [James Norton Babson], which we started soon after I got the job.

CARUSO: Okay. Did having additional kids or…I guess, how did you balance your family life with your work life? It seems like you're getting a lot done at work. Were you working long hours, or…?

BABSON: No. Not particularly.

CARUSO: Okay.

BABSON: Regular hours.

CARUSO: Regular hours.

BABSON: Yeah, just pretty productive, I guess.

CARUSO: Okay. When you came home…I'm always curious to see…the way you grew up there was a lot of freedom. You were playing outside. You were playing in the woods. You were blowing things up. What was it like for your kids at that time? Were you involved in their schoolwork? Were you a soccer coach …

BABSON: No.

CARUSO: …or anything along those lines?

BABSON: No. I was never a coach. Was I involved in their schoolwork? Probably minimally.

CARUSO: Okay. Did they—I realize they were quite young at this time…

BABSON: They didn't have any scientific bent.

CARUSO: Okay. I was going to ask if they ever came to work, and played in your lab or anything.

BABSON: No.

CARUSO: No.

BABSON: No.

CARUSO: No. Okay. So I know that you were at Lambert—I'll just use Lambert now—for a total of 25 years.

BABSON: Twenty-five years almost to the day. Yeah.

CARUSO: I don't know the various positions that you held while there.

BABSON: Well, I was hired as a Senior Scientist. I got promoted to a Senior Research Associate, which was the highest laboratory position. Then I got promoted to Director of Diagnostic Research, then Vice President of Diagnostic Research, which was the top position I held there.

CARUSO: Do you have a rough timeframe for when those promotions occurred? Was it like every five years, every…?

BABSON: No, I don't remember to tell you the truth.

CARUSO: Okay.

BABSON: I could probably reconstruct that if necessary.

CARUSO: That's just…I was just going to try to have you go through your reflections of your career history. I thought maybe having some dates could serve nicely as a way to reconstruct the ways in which, not only your responsibilities were changing, but the ways in which I think the industry more broadly was developing, right. I mean clearly, you had some things that people were quite interested in coming out of Lambert. I'm assuming there are other companies that started to move in that direction.

BABSON: Yes.

CARUSO: So I'm just trying to get a sense of this twenty-year period where a lot of change is going on, what those changes are. I'm just trying to have a better understanding of all that was going on during this period of time.

BABSON: Well, the changes…the biggest change, I think, the most significant change was the invention of the AutoAnalyzer by Technicon [Corporation], now part of Dade Behring, now part of Siemens. At the time, we were also involved in automating clinical chemistry. We had a project which we hadn't developed. It was developed outside, called the Robot Chemist <**T: 110 min**>. We were neck-and-neck with Technicon, too. But when Technicon found that they could add analytes, channels, to their system, which was based on moving flow of reagents and samples just by bifurcating the stream, and they ended up with like the twelve-channel autoanalyzer, where we were using discrete reagent systems, and discrete cuvettes, there was no way we could keep up with them, and they [Warner-Lambert] dropped the ball.

CARUSO: Okay.

BABSON: Because the Robot Chemist was very similar in concept to a lot of the instruments that are in existence today. But they screwed it up.

CARUSO: So based on the various positions that you had, can you tell me a little bit about how your responsibilities changed over time at Warner-Lambert?

BABSON: Yeah. Well, when I was just a group leader, and we had another group that was doing microbiology reagents in a different department, not George Phillips's department, the

microbiology department—of course [George] had Jane Lenahan's group in coagulation reagents. Then we'd hired…no that was after I was promoted. We took Jane's group and Don Kronish's group, the microbiology, and my group, which was then several subgroups, and we put them together as the General Diagnostics Research. That's when I became Director of Research, and then subsequently, Vice President of Research for General Diagnostics.

So we combined these three groups under me…

CARUSO: When you say, "We combined"…

BABSON: Well…

CARUSO: …is that…?

BABSON: The company combined.

CARUSO: The company, so…

BABSON: Yeah.

CARUSO: Okay. So they were interested. They saw value in…

BABSON: They saw value and the future in diagnostics. They saw me as a valuable asset that was very productive in generating new products. So they decided that, well, it makes more sense to have this in one group rather than in…rather…

CARUSO: Separated.

BABSON: Separated. Yeah.

CARUSO: How did your work in the lab change as you moved up the ladder? You know, with Versatol and Versatol-E, you were…

BABSON: I was in there.

CARUSO: Doing the experiments.

BABSON: I was doing the experiments. Yeah, with Sylvia's [help], obviously…

CARUSO: Right. And so…

BABSON: …assistance.

CARUSO: Did that change? I mean as you became responsible…

BABSON: Sure.

CARUSO: …for a group, were you now behind a desk giving orders, or…?

BABSON: Yeah. Yes, pretty much.

CARUSO: Okay. Did you miss…?

BABSON: Didn't like it that much.

CARUSO: I was going to ask if you…yeah.

BABSON: No. I liked the hands-on work.

CARUSO: Were you able to get out sometimes, and do your…

BABSON: Yeah.

CARUSO: …own…? Okay.

BABSON: Yeah.

CARUSO: So did you have your own like mini projects that you kept to yourself? Or did you just go out and help…?

BABSON: Well, one of the things that I did was, I had…[the woman who became my second] wife was working at the time in my department. She was working with a chap called [Bob Megraw…]. At the time, Ray Cohen, who was then the head of General Diagnostics, decided that there wasn't any future in coagulation. So he transferred Jane Lenahan and her whole group over to marketing, which was a dumb idea because there was a future in coagulation, obviously. For many years, we didn't…or for several years, we didn't have any effort in coagulation, and the market was passing us by.

So that's when I took and gave it to my wife, my second wife now <**T: 115 min**>, Susan—the one in the pictures downstairs on the wall—the responsibility for coagulation. She developed a number of different products. Actually, she was one of my most productive scientists.

CARUSO: So did you…I assume you met…so, just so I have a very general time frame. When Susan was working, was she your wife before she started working for you, or was it something that…?

BABSON: No. She was working at Warner-Chilcott or Warner-Lambert in the pharmacology department. She became allergic to the rabbits that she had to work with.

CARUSO: Well, that would be problematic.

BABSON: So she had to transfer to a different department, so I took her on. Bob Megraw was the guy, the chap, that she was working with.

CARUSO: Okay. Okay. So then she's working on the coagulation…

BABSON: So after…yeah, I took her away from Bob Megraw and gave her responsibility for coagulation.

CARUSO: Okay. This was around 1970, 1972, just as an estimate […].

BABSON: Yeah, early '70s. Yeah.

CARUSO: Okay. You were the director at this point…

BABSON: Yeah.

CARUSO: …in the early '70s? […] What was the difference in terms of your responsibilities between being director and being vice president? Were you…?

BABSON: Very little.

CARUSO: Were you now making up budgets?

BABSON: Very little...

CARUSO: Okay.

BABSON: …actually. It was essentially the same responsibilities, just at a higher pay and higher…

CARUSO: Okay.

BABSON: […] title.

CARUSO: And were you now completely out of the lab at this point? Kind of?

BABSON: Yeah, pretty much. Pretty much.

CARUSO: Could you give me a little bit of information about how Lambert fit into the general industry at this time? I mean you were coming out with all these products. I'm sure there were other companies coming…

BABSON: Well, General Diagnostics was one of the leaders of the *in vitro* diagnostics industry. Another leader was Dade in Florida, which is now part of Siemens. There was another company out on the West Coast—or was it in Texas?—called Highland Laboratories, [Inc.]. There was a…those were our main competitors, I think at the time, in the early '70s.

CARUSO: Okay. Were you all trying to establish your own standards for the…you had mentioned the Babson units, and all this earlier on. Was there a general consensus at this point that there was *an* overall standard, and people were just coming up with tests to match that standard? Or were these individual companies still saying, "Look. There's the General Diagnostics's standard. There's the Highland Lab standard"? Was there a codification or a simplification standardization going on in industry generally?

BABSON: No. I don't think so. Each commercial organization was trying to establish its own product lines. Now the Feds were getting more and more involved in creating standards for the laboratories. So and…

CARUSO: Was this coming out of the FDA [United States Food and Drug Administration], the standards?

BABSON: Yeah.

CARUSO: Okay.

BABSON: Yeah, right.

CARUSO: And what sort of responses from your <**T: 120 min**> users were you getting about the products that you were producing? I mean you're talking…

BABSON: They obviously liked them or they wouldn't be buying them.

CARUSO: Okay, and…

BABSON: Because they had alternatives.

CARUSO: And at this time were you selling directly to physicians or was it…?

BABSON: No. We were selling to the hospital laboratory.

CARUSO: Hospital labs.

BABSON: Right.

CARUSO: Okay.

BABSON: And the private clinical lab, too, but mostly hospital labs.

CARUSO: Okay. Did General Diagnostics…was there any sort of regional aspect to it? You know let's say if Highland happened to be in Texas, did they handle the southwest and General Diagnostics…

BABSON: No.

CARUSO: No. So it was…

BABSON: It was all over…

CARUSO: …all over.

BABSON: And all over the country, and pretty much all over the world. We had a international division, which had a diagnostics responsibility, headed by a gentleman by the name of Luis Patinio. Luis basically was responsible for all the international sales of General Diagnostics's products.

CARUSO: Okay, all right. Looking at the time, this might be a good place to stop…

BABSON: You haven't partaken of any of our treats here.

CARUSO: We were busy talking. But I think on Thursday when we take up again, it might be a nice point to sort of finish up…I know that you received the American Association for Clinical Chemistry Award in 1975.

BABSON: Was that the Gerulat Award?

CARUSO: Yeah.

BABSON: Yeah, and the more prestigious Van Slyke Award a couple of years later. The other, third award, on my wall is the New Jersey Inventors Hall of Fame.

CARUSO: Right.

BABSON: Inventor of the Year Award.

CARUSO: Right. I remember seeing that as well. I think that might be a good place to pick up. It's a nice transition point, because, after your time as vice president, I guess by 1980, or maybe in 1980 exactly, is when you left to start, well, Babson Labs.

BABSON: Babson Research Labs, yes.

CARUSO: So I think that would be a good place to…

BABSON: Okay.

CARUSO: …probably pick up. Thank you.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INTERVIEWEE: **Arthur L. Babson**

INTERVIEWER: **David J. Caruso and Sarah L. Hunter-Lascoskie**

ALSO PRESENT: **Carroll C. Scribner**

LOCATION: **Siemens Healthcare Diagnostics**
Flanders, New Jersey

DATE: **8 December 2011**

CARUSO: […] So today is the 8th of December 2011. This is the second interview with Arthur Babson at Siemens in Flanders, New Jersey. I am David Caruso, and with me is Sarah Hunter, as well. I think, Sarah, you have a question.

HUNTER-LASCOSKIE: I was curious about the end of your time at Warner-Lambert. You'd spoken so much about working independently, and all of your scientific work in graduate school and postdoctorate, you're working by yourself. Suddenly over the course of some years at Warner-Lambert you have a huge group. What was that adjustment like going from working by yourself to managing a growing group of people?

BABSON: Well, I think […I had] fifty people or more. They were less productive than when I had five people, because…. I don't know, I just…I was a lot more productive when I was hands-on, totally hands-on, I think.

CARUSO: So you were productive as in, when you were working on something, you were literally productive with it.

BABSON: Right, yeah.

CARUSO: But as you became more and more responsible for a group of people, I guess there was a lot less of your own hands-on work?

BABSON: Right. It was a lot of administration, administrative work, and what not.

CARUSO: So directing…

BABSON: It was…

CARUSO: …directing other people what to do.

BABSON: Yeah.

CARUSO: Okay.

BABSON: Yeah.

CARUSO: Did you have as much input about things at that point with what was being designed and what was being investigated? Or were you essentially just relying on reports from people about the work getting done?

BABSON: Well, I had a lot of input, but not as much as I did when I was doing it myself, obviously. So I had to rely on other people. Other people weren't as reliable as I was. [laughter]

CARUSO: I know during this period of time, you have…you meet Susan, your second wife. How did…I mean she was in your group, so you were responsible for her. Were there any issues at Warner-Lambert in terms of you supervising someone whom you were married to?

BABSON: Yes.

CARUSO: Okay.

BABSON: Yes, definitely. There were a lot of petty jealousies. Susan was very productive; she wasn't given the credit for it that she deserved, because they said, "Oh, that's just Art," you know. It wasn't just me. She was a very productive scientist. But there were a lot of petty jealousies. When I was made Vice President of Research, my boss, George…

CARUSO: Is it…

BABSON: Masters. George Masters…

CARUSO: George Masters.

BABSON: …who was the president of the General Diagnostics Division, wisely told me not to have Susan reporting to me. So I had her reporting to Jim Turner for a while. Then she got a job in the international division of Warner-Lambert, and then eventually left and went to work for MedPath.

CARUSO: Okay. So she shifted around and…

BABSON: Yeah.

CARUSO: Okay. Now, again, as you moved up the corporate ladder and you were becoming more distanced from the…

BABSON: The bench.

CARUSO: …the bench, I'm sure that you had a lot of ideas about what could happen in the diagnostic realm. And I'm curious to know what it is that you thought was important to do in terms of standardizing diagnostics, and what sort of tests did you think needed to come up for physicians and things like that? I mean what was your thinking during this period of time about what you wanted to accomplish?

BABSON: Well, I wanted to simplify the tests that the laboratory was already running, and improve them if possible, to provide better results, to provide easier <**T: 05 min**> procedures. But the tests themselves, the analytes that were tested for, that were measured, were pretty much dictated by the need, you know.

CARUSO: Right, the medical field has its own…

BABSON: Right.

CARUSO: "We need to know something."

BABSON: Need to know what your glucose level is. We need to know what your uric acid level is, and so forth.

CARUSO: So were you…how did you keep abreast of what it is that physicians wanted or needed? Where you attending American Medical Association conferences? Or…?

BABSON: Well, I was very active in the American Association for Clinical Chemistry, let's say. That was the main meeting for the field I was working in, which was essentially clinical chemistry.

CARUSO: How long had you been involved in that?

BABSON: I was one of the early members. My membership number is two hundred forty-six, which only two hundred forty-[five] members before I joined. That was in early…oh, gosh, when would that be, a year or two after I joined Warner-Lambert.

CARUSO: Okay.

BABSON: Actually the Clinical Chemistry, American Association for Clinical Chemistry, they were kind of prejudiced against people from industry.

CARUSO: Why is that?

BABSON: [It's] because they thought it was their business. It was their…I don't know, to tell you the truth.

CARUSO: Was it mostly academics in the Association?

BABSON: Yeah, it was academics.

CARUSO: Okay.

BABSON: And they thought that people from industry were kind of like harlots.

CARUSO: So when you were involved with this group, were you just presenting your research initially at the meetings? Or was it…?

BABSON: No. Actually, I was on the board of directors of…what do they call it? I'm not sure whether it's board of directors. But of the New York Metropolitan Section, and then the New Jersey Section, so I was involved in the organization itself.

CARUSO: Okay. Were you one of the only industry individuals in the organization?

BABSON: I was one of the earliest industry individuals, I think. But now it's dominated by people from industry.

CARUSO: Okay. What was the purpose—I'm assuming the organization had annual meetings. What was the…

BABSON: Yes.

CARUSO: …purpose of the annual meetings?

BABSON: To present research results and whatnot, present papers. Initially they had…they didn't have commercial exhibits. It was just technical papers that were presented. But now it's totally dominated by commercial exhibits, essentially.

CARUSO: The work that was being done by the academics, was it any different from the type of work that you or, once more industry people started to come in, was it different from the work that they were trying to accomplish? Was there some fundamental difference between them?

BABSON: Not really. They were working on methodologies, new methodologies, and stuff. But the people from industry were working on products that performed the same function, so…

CARUSO: So the academics were coming up with ways to do things…

BABSON: Right.

CARUSO: …but industry was figuring out ways to bring it to market.

BABSON: Right.

CARUSO: So to realize what the sort of theoretical or experimental things…. Okay. So you were involved with the AACC for quite some time. In some ways…

BABSON: I'm still an emeritus member.

CARUSO: Okay. And you did receive…

BABSON: I don't have to…being emeritus member, I don't have to pay dues anymore.

CARUSO: So in the 1970s, you're vice president. Susan moves off. You're more removed from bench work. Almost completely removed, would you say?

BABSON: Pretty much. Yeah.

CARUSO: Pretty much. You received, in 1975, the American Association for Clinical Chemistry Award. Is it Gerulat <**T: 10 min**>…?

BABSON: Gerulat Award, yeah…

CARUSO: Gerulat Award. What is that award for, and…?

BABSON: Bernie Gerulat was a very early member of the Association, and particularly from the New Jersey Section. He died, and so this was kind of a memorial award for him. It was presented annually to a member of the association that they thought deserved it, I guess.

CARUSO: Sort of outstanding achievement in the field?

BABSON: Yeah, right.

CARUSO: So did you know that you were going to be receiving the award?

BABSON: No. Well, yes, I knew…

CARUSO: I mean did you well in advance…

BABSON: No, not…

CARUSO: That…no.

BABSON: …well in advance…

CARUSO: Okay.

BABSON: No.

CARUSO: Okay. So why is it that you received it in 1975? What was the reasoning behind…?

BABSON: You'll have to ask the committee.

CARUSO: Okay. I was just wondering if they had an award presentation, they might have mentioned was it for a specific patents that you'd come up with?

BABSON: No. I don't think it was for any specific…

CARUSO: Specific.

BABSON: One thing.

CARUSO: Okay. How did you feel about receiving that award?

BABSON: Oh, I was pleased, obviously.

CARUSO: Getting recognized for the work you had done…

BABSON: Yeah.

CARUSO: …up to that point. Now it's also - I'm assuming 1975 might be an interesting year in other respects, because within five years of that point you're leaving Warner-Lambert. So I'm kind of curious to know if there was something going on in the company that sort of led you to wanting to leave by 1980…

BABSON: Yes.

CARUSO: …or was it just…okay.

BABSON: Yes. They hired a person between me, and George Masters, who was the…what was his title? Bob Schiff, who I immediately took an instantaneous dislike to, and it's all spelled out in the memoirs. Are you reading that section?

CARUSO: I was looking for his name. I couldn't remember. But yeah, I just came up with Bob Schiff's name.

BABSON: Yeah. So he was the Senior Vice President for Research. He had responsibility for a number of different locations where at that time research was on the…main research was done

at Morris Plains, [New Jersey], of course. But he had…we had a group in Germany and then another group in Texas, I think.

CARUSO: So he was sort of this supervisor for all the vice presidents of research at…

BABSON: Yes.

CARUSO: …various locations.

BABSON: Right, senior vice president.

CARUSO: Okay. Was that…was he filling in for someone else who was already in that position?

BABSON: No. It was a new position.

CARUSO: It was a new position, okay.

BABSON: Yeah.

CARUSO: Did you know that the position was going to…

BABSON: No.

CARUSO: No. So this was…

BABSON: No.

CARUSO: …a surprise.

BABSON: It was a surprise. Yes.

CARUSO: And not a happy surprise.

BABSON: Not a happy surprise

CARUSO: Not a happy surprise. You mentioned that you took an instantaneous dislike to him. What was it about…I mean was it something about his management style that didn't fit well with…?

BABSON: You have to know the man. [laughter]

CARUSO: Oh, it's that kind of…

BABSON: Yeah.

CARUSO: …dislike. So was that the main reason that precipitated your leaving Warner-Lambert?

BABSON: Yes.

CARUSO: Yes. Okay.

BABSON: Yes, definitely.

CARUSO: Were there other things that no longer fit well? You know, Sarah mentioned the fact that you were…you kind of liked working on your research projects. Were you tired of the management aspect of things?

BABSON: Well, I never really enjoyed management, as such. It went with the territory though.

CARUSO: Okay. Did you ever consider asking to be demoted, so that way you could be back at the bench?

BABSON: No.

CARUSO: No. Okay. So 1980, after twenty-five years at Warner-Lambert, you decide to leave. Did you have a…?

BABSON: Well, I decided soon after Schiff arrived, but I didn't…I wanted to be <**T: 15 min**> vested in the Warner-Lambert medical plan, for example, before I retired. So I had to be a certain age. I've forgotten what it…was it fifty-five, I guess? I think. At that time, I had accumulated…my boss realized that, George Masters realized that Schiff was not going to be…there was a possibility that he and I would not see eye-to-eye. So he said, and I told him pretty much, that, "Hey, this guy is bad news."

He offered me severance if I decided after working…trying to work with Schiff for several months or whatever, that if I wanted to leave, he would give me severance. So I figured out that the day my severance would take me to the fifty-five years old, that was the day I was going to leave.

CARUSO: Were there other people at Warner-Lambert that had similar feelings towards…?

BABSON: Yes. Ken Sumner, who was my manager of, or director of clinical investigation. He left almost immediately.

CARUSO: Okay, wow.

BABSON: And he said, "I don't trust Schiff." That's just what I remember him saying to me exactly. He turned out to be right, because two years after I left, or within two years after I left, Schiff was summarily canned, along with all the people he brought in with him, for lying about results on a cancer test…

CARUSO: Oh, wow.

BABSON: …which the data showed that it wasn't going to work, and he hid that from…

CARUSO: And I guess it was kind of good you got out when you did.

BABSON: Yes.

CARUSO: Did you have a plan for what you wanted to do when you were leaving?

BABSON: Yeah. Well, I knew I wanted to be my own boss. I knew I wanted to stay in the medical field. So that's why I essentially started the Babson Research Laboratories, which was an S corporation. I made…actually built a lab in my house, which I provisioned with leftover stuff from Warner-Lambert to a great extent. And I started working on projects.

CARUSO: So what was your home-built lab like? I mean what sort of devices did you find you needed to start your new company?

BABSON: Well, I needed a spectrophotometer, for example. I needed balances. I needed a water bath. Some of these things, a lot of these things, I was able to purchase, and some I was able to get from my prior employer.

CARUSO: Okay. So you had spent so much time studying diagnostics, and you wanted to continue it, was there a specific area that had interested you? You had mentioned when we were first talking, the success of Versatol. You became interested in enzymes. Then there was Versatol-E. Then there was an interest in coagulation for you. Was there a new area that interested you? Or it was just like a continuation of…?

BABSON: Well, the most successful product in General Diagnostics was Blood Gas Control, which was an ampouled control for pH, P_{CO2}, P_{O2} for blood gas controls, for controlling blood gas measurements. I had an idea…and these all had to be…they had a gas phase <**T: 20 min**>, and they had to be equilibrated at […] 37° [C], for example, before they could be used. They were essentially injected in the blood gas analyzer.

I had an idea for coming up with a…and they were single use ampoules. I think, in the second year of sales, they were like a six million dollar product, which was unusual, unprecedented you might say, for General Diagnostics. I came up with an idea for making a multiple use [control], which didn't require equilibration, because it didn't have a gas phase […].

CARUSO: Okay.

BABSON: And I got a patent on it, and tried to peddle it, and had a number of companies that were interested in it.[6]

CARUSO: So my first question is, how did you come up with the idea? Again, yesterday when we were talking about Versatol-E, you'd mentioned some of the work of other scientists, their publications. Your friend, who was publishing some interesting stuff, you were just looking up what he was up to. How is it that you came about with this idea? I mean was it just again…?

BABSON: I don't know. [laughter]

CARUSO: So it just came to you.

BABSON: Yeah, just came to me.

CARUSO: Okay. So it's a problem that needed to be solved.

BABSON: Problem that needed to be solved, and…

CARUSO: And you were going to solve it.

BABSON: I thought it was…I thought I could come up with a good solution, and I did. But, unfortunately, I was never able to sell it. It gave me a lot of contacts in different companies in the diagnostics industry.

CARUSO: So you saw yourself selling your ideas to diagnostic companies, not that you would necessarily become a diagnostics company.

BABSON: No. No. I didn't want to sell to customers.

[6] Arthur L. Babson, "Multiple Use Container for the Packaging of Fluids," U.S. Patent 4,559,052, issued on 17 December 1985.

CARUSO: Okay. So you wanted to be sort of a…

BABSON: Individual laboratories, let's say.

CARUSO: Okay. So you were going to be a researcher that contracted with or sold your inventions, your ideas, to the diagnostic…

BABSON: Right.

CARUSO: …companies, which would then sell it on to the users.

BABSON: Right, yes. Although I did end up selling to MetPath, which was then the largest laboratory in the world, in New Jersey, a couple of products, which together were worth ten thousand dollars a month to me: [prostatic acid phosphatase and urinary alcohol]

CARUSO: Wow. Okay. Was that in the early portion of Babson, or was this…?

BABSON: No. That was…well, I've forgotten exactly when. No, it was later on, I think. I had sold one product, a non-isotopic immunoassay for prostatic acid phosphatase, which I developed, and to Ortho [Diagnostics] [Johnson & Johnson] down in Raritan, New Jersey. They marketed it for a while. But then they really screwed up the marketing and they abandoned it for a while, or after about less than a year, I think, and gave it back to me, the marketing rights. So I made a presentation to MetPath and said, "This is a good test. You ought to have it. You ought to use it." I convinced them, and they started selling it, or they started using it, I mean, not selling it.

CARUSO: So in the early years of Babson Laboratories, initially it was you, right.

BABSON: Yeah.

CARUSO: How did that company grow in the first few years? I know you eventually moved out of your…

BABSON: It was always me. I was the sole…the first employee, and the only employee. So I did…I was making reagents for Becton Dickinson, [and Company], for the Clay Adams Division, several reagents—four, I think—and selling them in bulk, which they repackaged in <**T: 25 min**> single test units for their Accustat analyzer, which was sold to the physician's office marketplace. I was also consulting for them for like five years. I ended up, after selling the prostatic acid phosphatase test to Ortho [Diagnostics], I ended up consulting for them for many years.

CARUSO: Okay. So, really, in those first several years, it was just you.

BABSON: Just me.

CARUSO: It was you in your home lab…

BABSON: Just in my home lab.

CARUSO: …your home lab with…the contacts that you were able to make in the diagnostics industry, was that because people knew you from Warner-Lambert?

BABSON: Yeah, pretty much.

CARUSO: Okay. So you had this reputation…

BABSON: Oh, yeah.

CARUSO: …people knew about you.

BABSON: Right.

CARUSO: Now one thing I did notice was that—and I think I probably know the answer to this next question—but I noticed that once you left Warner, I'd say for most of the 1980s, you really stopped publishing.

BABSON: Right. Yeah.

CARUSO: Is that just because you were so focused on your own…

BABSON: I was so busy making money.

CARUSO: Okay. Making money. But if you had the chance, would you have been? Would you have continued publishing, do you think?

BABSON: Maybe, I don't know. I already had a number of publications…

CARUSO: You did.

BABSON: Quite a few.

CARUSO: That's true. That's true.

BABSON: There wasn't any money in publishing, right. At that time I was interested in earning a living.

CARUSO: Okay. So outside of the initial work and the consulting work that you were doing in those first few years of Babson Laboratories, did you have any specific vision for what you wanted to achieve with the company? Or was it just, "Whatever work needed to be done, I was going to do it"?

BABSON: Pretty much. You know, I didn't have any high vision.

CARUSO: Okay.

BABSON: I wanted to make a buck any way I could.

CARUSO: Were you doing…so putting work aside, were you doing anything in your own personal time, your leisure time, that was interesting you? I mean yesterday, or the last time, we spoke about you liked to go for walks in the woods. When we were having lunch the other day, I commented how there's beautiful land behind the building here. It seems like you could go for walks here. Were there things that you were pursuing in your own personal life at the time that…what were you doing when you weren't working? Or were you just working full-time?

BABSON: I was working pretty much full-time. Yeah.

CARUSO: Full-time, seven days a week, no breaks.

BABSON: Seven days a week, probably. Yeah.

CARUSO: Okay. So after those first few years, I know that eventually, and I don't know if I have a clear understanding of when everything happened, but there was Pegasus Technologies. There was Cirrus Diagnostics. There was sort of an evolution of what you were doing, and also…

BABSON: Well, this is totally separate from Babson Research Labs.

CARUSO: Okay. Can you…?

BABSON: This was a new company. Well, I had an idea for automating immunoassays by spinning a tube on its vertical axis to do separations. I got a patent on that.[7] I actually wrote the patent application myself, because I already had twenty-five patents, so I knew…

CARUSO: You had some experience.

BABSON: Yes. I had…I knew what the routine was. With the idea of selling it outright or licensing the technology, because I didn't want to…I didn't visualize myself developing it, Babson Research Labs.

[7] Arthur L. Babson, "Vessel and Procedure for Automated Assay," U.S. Patent 4,639,242, issued on 27 January 1987.

CARUSO: Why not?

BABSON: Well, I didn't want to be in the manufacturing…

CARUSO: Okay.

BABSON: I made bulk reagents for MetPath, and for Clay Adams, Becton Dickinson, but that was great business. But I didn't want to have employees, for example <**T: 30 min**>. I like working for myself. So I got this idea. I wrote up a patent application, and got a patent on it, and that's the basis of the technology you see here. I was doing a lot of consulting with a chap by the name of John Underwood. John was…and I was essentially a technical backup for John. John was a consultant for marketing and whatnot, but he knew his way around the diagnostics industry pretty well. So I showed John this idea. I said, "John, who do you think's a good customer for this?" He took one look at it, and he said, "Art, this is too good. You don't want to sell this. You want to start a company." And I didn't. I didn't want to start a company. But he was very persuasive, and he convinced me.

He introduced me to an early stage venture capitalist, Art Kydd, when we were on a consulting trip to a company in Minneapolis [called London Diagnostics…]. Art was a director of this company, and I showed it to Art, and he said, "Yeah, this is pretty good." So, in a subsequent visit to Minneapolis…and I had taken a battery-driven model of a tube spinner. As a matter of fact, I have—it doesn't exist anymore—but this was my first wooden model. I have a metal model that simulates or almost exactly the same design, which I had a model-maker make for me here. But so I had this, and this up at a restaurant, which Art Kydd had invited a few of his wealthy investors to, and I demonstrate the principle of axial centrifugation to do separations in immunoassays on the dining room table in the restaurant. On the basis of that, and subsequent investigations Art made on my background, he decided to start the…put up the early stage of venture capital.

CARUSO: So…

BABSON: And that was incorporated as Babson Technologies [on March 17, 1987]. But before we had our first hire, besides me—I was the only employee at the time—I decided that, and John Underwood agreed, that we don't…this is not a good name for a company, because I don't want people thinking they're working for me. I [want] them to think they're working for their own company. So we changed the name to Pegasus Technologies.

[…] At a Clinical Chemistry meeting, in Philadelphia I think it was, in 1991, '92, I walked into the exhibit hall, and there on a huge banner on the wall, "Pegasus: Automation in Immunoassay." So we figured we'd had to change the name of the company, and this was after

we had several employees. So I got all the employees together, and I solicited names for the new company from all the employees. We had seventy or so submissions. We all sat down <**T: 35 min**> at the conference room table, which was a four-by-eight sheet of plywood, in the schoolhouse. We went through this list and IMMULITE was one of the suggestions. But we decided, "Oh, no, that's…we'll save that name for the…

CARUSO: The product.

BABSON: …the product." Sirius, the star, the main star in, I think it's Orion, was suggested. "Yeah." We kind of liked that, but then somebody said, "Well, how about Cirrus?" "Yeah." We liked that even better. So we changed the name to Cirrus Diagnostics. I cleared it through my lawyer to make sure that there wasn't any other…

CARUSO: No other company with…

BABSON: …companies named that, at least in this industry.

CARUSO: So I'll probably to refer to things as Cirrus…

BABSON: Yes.

CARUSO: …acknowledging that there were some name changes over time. What was the relationship between Babson Laboratories and Cirrus? Or is it that Babson Labs shut down?

BABSON: None.

CARUSO: None.

BABSON: None. Well, Babson [Research Labs] survived for the first couple of years after [Pegasus], but eventually…

CARUSO: Just shut it down.

BABSON: Just shut it down. Yeah.

CARUSO: And during the time when both existed, were you…was Babson Labs still you in your home lab? Or…>

BABSON: Yeah.

CARUSO: Okay. Then you would go offsite when you working at Cirrus?

BABSON: Well, when we first rented a room in the schoolhouse, we rented one room in the second floor. It was the chemistry lab, I think, [because it was the only room with a sink].

CARUSO: At this point, who is the "we"? Is it just John, and…?

BABSON: No. Well, the board of directors of Pegasus, then Cirrus, was John Underwood, Art Kydd, and myself. I was the President, CEO, Treasurer…

CARUSO: Chief Technical Officer.

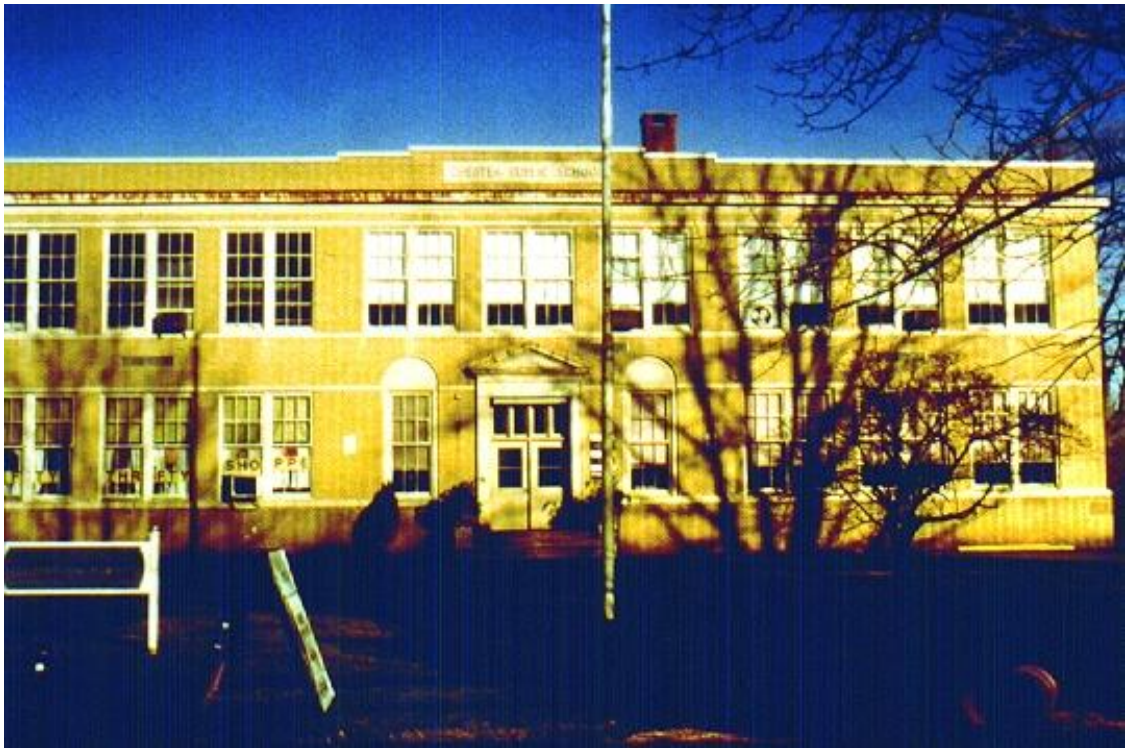
BABSON: Chief bottle washer. And we didn't have any other employees for a long time. I mean this…we incorporated in [March 1987…]. We didn't have any employees except me. I decided that my first employee should be a mechanical engineer.

CARUSO: Why is that? Because you had to build a device?

BABSON: We had…I had the idea, and the concept, but we didn't have any mechanical design except for my wooden model, which obviously wasn't adequate.

CARUSO: Was this your first foray into actually building devices? So previously, when you were at Warner, it was coming up with tests, not necessarily devices.

BABSON: Yeah [except for Simplate, General Diagnostics's bleeding time device].



The Old Schoolhouse, Chester, New Jersey

The Original Office, including Chemistry, Drafting, Engineering, Management

The Original Chemistry Lab

CARUSO: All right, so this is your first device.

BABSON: This is my first actual instrument, you might say.

CARUSO: Instrument. Okay.

BABSON: Yeah. So I had a hard time convincing…I had one room in the schoolhouse. I bought two secondhand desks. I had built a drafting table. And that was essentially <**T: 40 min**> the company. So how would I convince a mechanical engineer, who were a pretty conservative bunch anyhow, to come to work for a fly-by-night diagnostics? It took me a while. My first hire was Tom Palmieri, a mechanical engineer. He started on [March] 15th, 1988 […]

CARUSO: Okay.

BABSON: And…

CARUSO: So almost a year after the incorporation.

BABSON: Yeah, almost a year.

CARUSO: What were you doing in the time between incorporating and hiring Tom? Was it just searching for people? Were you…?

BABSON: No. I was actually sketching out concepts for…

CARUSO: For the design.

BABSON: …the mechanical design, so that when I eventually did get a designer, I'd have some direction for him to start in. But we…but after that, the company grew, in terms of people, by leaps and bounds. It's that first threshold going from one employee to two…

CARUSO: To two…

BABSON: …employees, convinced other people that it wasn't a bad idea.

CARUSO: Did you necessarily know that you had a market for this device?

BABSON: Oh, yeah.

CARUSO: Had you been shopping the idea around to anyone at this point? Was it still…?

BABSON: I had…I don't know if I had shopped it around. No. I don't think I was shopping it around. I don't remember shopping it around. Once John convinced me that I shouldn't shop it around, that I should start a company, I didn't.

CARUSO: Okay. So you have your first hire in [March of] '88 […].

BABSON: Yeah.

CARUSO: So how did the company grow at that point?

BABSON: Well, I had a mechanical engineer. Then I needed an electrical engineer. I needed a biochemist to do the assay development. I hired Arthur Ross, who was an electrical engineer. He responded to an ad we ran in the *New York Times*.

CARUSO: Really.

BABSON: I got Doug Olson, who was…eventually turned out to be president of the company, when we were acquired by DPC [Diagnostic Products Corporation]. I got him through a series of contacts. […] Johnny Teipel, […] who I met at Ortho [Diagnostics] when I was consulting for them. He knew of somebody, of Doug, who was working with a startup at that time, which failed. He knew him through a third party. So nobody…it was a strange chain of events. I called Doug up, and he was interested. We had an immediate rapport.

I remember he was…I was interviewing him in my house in Mendham, because we just had the empty room at the schoolhouse. My wife, Susan, also participated in the conversation.

Then she excused herself, and went out of the room. The telephone rang. It was Susan. She said, "Hire him." So she recognized his talent, too. And so I did.

CARUSO: Okay. So how long did it take you to effectively create your first instrument to go from these ideas, and these plans to making something…

BABSON: A working model?

CARUSO: A working model.

BABSON: Yeah. Well, it took about nine months.

CARUSO: Nine months. Okay <**T: 45 min**>.

BABSON: Yeah, because we had venture capitalists, also called vulture capitalists. They have…they're very much concerned about targets, target dates. You have to…they say…well, they offered me a whole bunch of stock in the company, but only if I met these milestones. They had a milestone at the end of the year, that first year, to have…to demonstrate the feasibility. So we did. [On December 31, 1988 Doug Olson was able to produce an acceptable dose/response curve for digoxin].

I remember we were concerned about the label that we were going to use, whether or not it was going to be acridinium esters, which is a [chemiluminescent] label used by some of our colleagues here at Bayer [AG], or an enzyme label with a chemiluminescent substrate, which had just been announced in the literature. So we built our first [breadboard] models such that it could use either.

CARUSO: Oh, really.

BABSON: We evaluated both of them, actually, as labels. We decided that the enzyme label was much preferred.

CARUSO: Based on just general testing results?

BABSON: Yeah, based on actual results.

CARUSO: Okay. Now, how did you go about manufacturing this first device? Was this like…?

BABSON: We built three units. The gentleman on Tuesday that hosted us for lunch [Gene Hochmuth], was one of the major builders. He was an electrical engineer, but he actually constructed the first units in the schoolhouse. We had…we made three units, we called the A units. Basically one of these we took to a Clinical Chemistry meeting in [1990]. We also then, by this time, we'd grown so much, we took over essentially the entire schoolhouse…

CARUSO: Oh, okay. Wow.

BABSON: …the schoolhouse building. We had the entire second floor, and most of the first floor, and part of the basement even. We built twelve instruments we called the B units. Those were actually…we sold one of those to our first customer, which was Morristown Memorial Hospital, in Morristown, New Jersey. Then we contracted for the C units, which were twenty-five, which we […] paid a million dollars for, forty thousand apiece, to a company in Mountain Lakes, New Jersey [called Lydo]. Then we moved out of the schoolhouse into our own facilities in Randolph, New Jersey, where we set up the manufacturing area ourselves.

CARUSO: So I mean was it…in terms of actually manufacturing the device, did the school have a machine shop that you were…

BABSON: No.

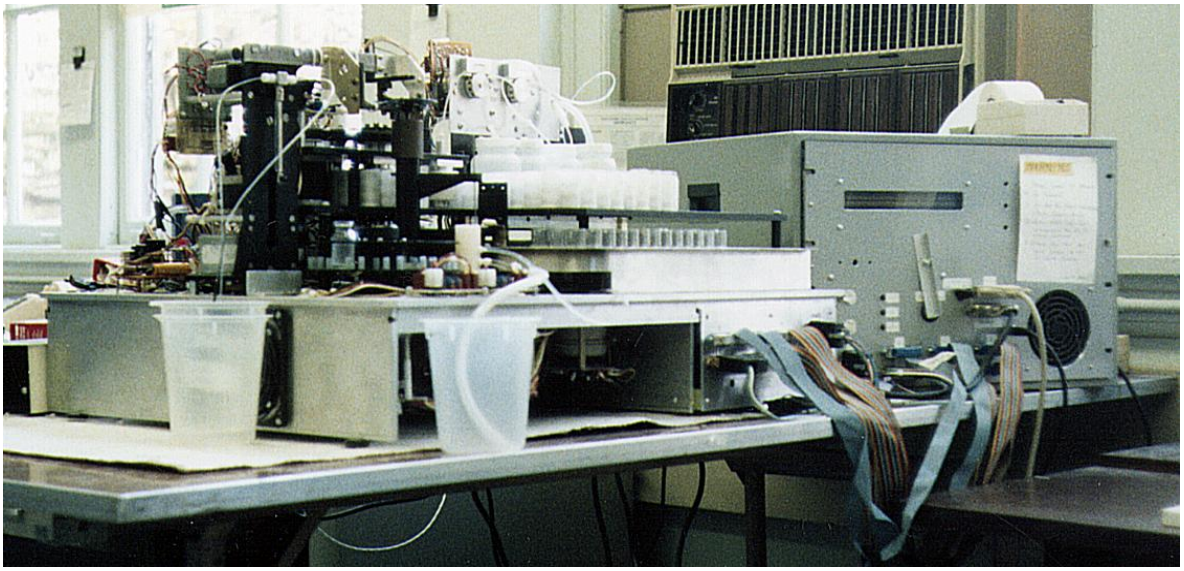
CARUSO: …making all…?

BABSON: No, we worked outside, with outside machine [shops]…we essentially did the final assembly. We weren't doing any machining at all.

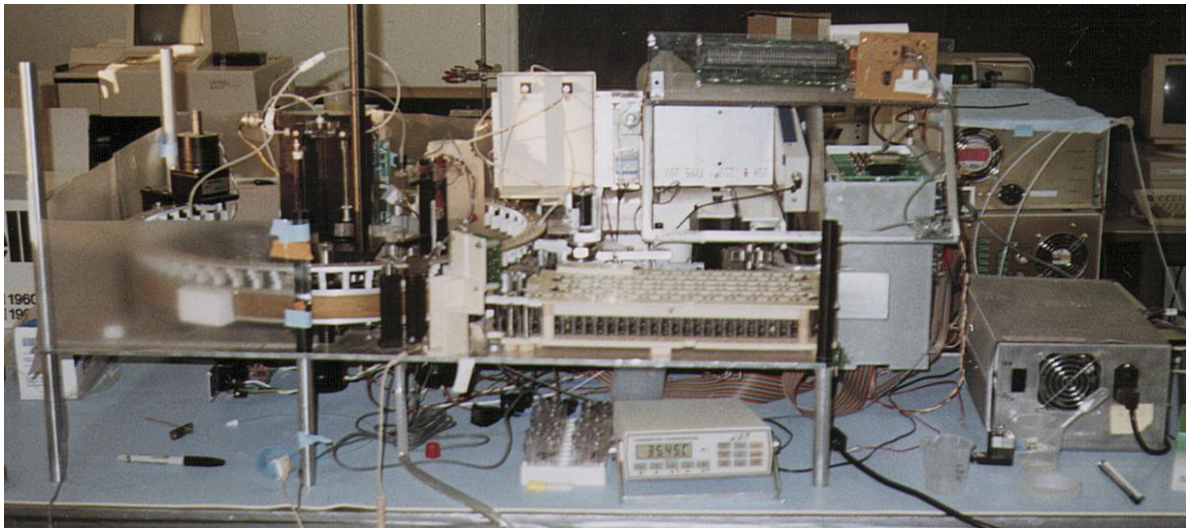
CARUSO: Okay. No in-house machining…

BABSON: No.

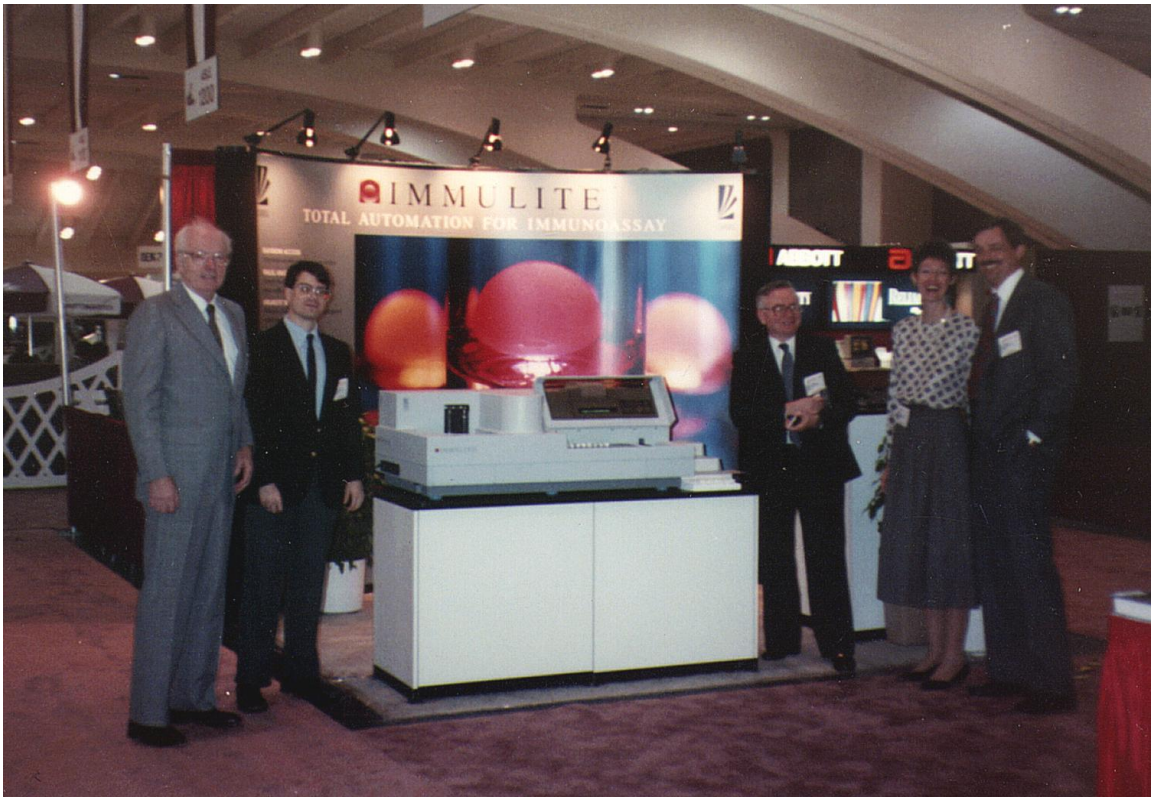
Cirrus Diagnostics employees at the old Chester schoolhouse—March, 1989. Back row: Jack Amato, Ray Hicks, Doug Olson, Greg Giter, Tom Palmieri, George Feldstein, Pradip Dutta; front row: Art Babson, Arthur Ross, John Underwood. Missing: Earl Nause.



First IMMULITE Breadboard completed in 1989



Second IMMULITE Breadboards completed in 1990 for display at AACC



First American Association for Clinical Chemistry display
San Francisco, California, 1990. Left to right: Arthur Babson, Arthur Ross, Robert Fennell, Pam Eden, and Douglas Olson.

CARUSO: So you were getting the parts made for you, and reassembling them onsite.

BABSON: Yes.

CARUSO: Okay. Now for someone who liked to work on his own, rose through the ranks and had to do a lot of managing without being hands-on anymore, leaving that position, so that way he could work hands-on with things again, to then have another company or have a company that you were building and was growing and expanding, how did you respond to those changes? I mean, you said that Underwood convinced you that you needed a company. You have a company, and now you have tons of people that you're responsible for again. Was this in some ways different from what you were experiencing at Warner? Or…?

BABSON: Much different.

CARUSO: How so?

BABSON: Much different, because <**T: 50 min**> Cirrus Diagnostics was a tightly knit organization. It wasn't…and there was no politics involved. It wasn't part of a huge corporation, like Warner-Lambert, a division, small division of a huge corporation. So it was…and it was our baby, you know.

CARUSO: So there was a different, complete culture there.

BABSON: Yeah, totally different culture.

CARUSO: Okay. Was there any formalized structure to Cirrus at that time? I know that you obviously were in charge, but did you start creating a hierarchical structure in any way?

BABSON: No.

CARUSO: Or is it…it was very, just like…

BABSON: It was…

CARUSO: …like people coming up with a name for the company. You were sitting around a table.

BABSON: Yeah, right. .

CARUSO: And just…

BABSON: Well, there were different functions, I mean like…

CARUSO: Right, right.

BABSON: Mechanical engineering, and electrical engineering, but it was a very collegiate group of so-called equals, and we all pitched in.

CARUSO: Okay. So you make your A units, your B units, your C units.

BABSON: Well, we contracted for the C units…

CARUSO: Contracted for the C units.

BABSON: Yeah.

CARUSO: But it was still something you were producing. That's all I…

BABSON: Yeah.

CARUSO: …really meant. Did you see this as being *the* device that Cirrus was going to make? Or did you start thinking about what the next thing was going to be for Cirrus?

BABSON: Well, we started on the…actually, I had started on the next thing, before we completed the IMMULITE, which was what we called the CRP, or the Cardiac Risk Profiler.

CARUSO: Right. That was the automated lipid…

BABSON: Yeah.

CARUSO: …profiling system.

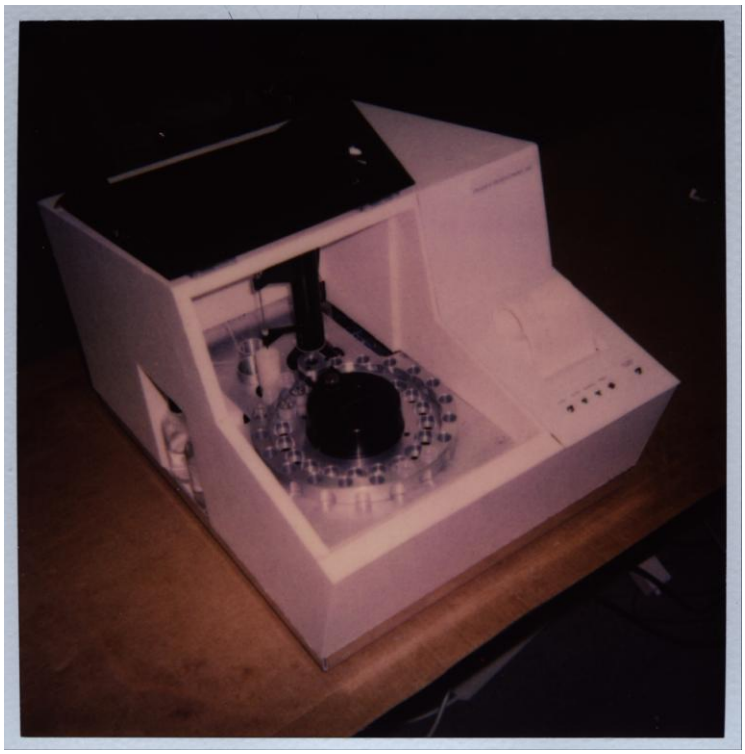
BABSON: Yeah, because at that time, good cholesterol, bad cholesterol, and triglycerides were all the rage, you know, for screening for susceptibility for potential heart attacks. At that time, we had…. The venture capitalists…Art Kydd had limited resources. I thought that we could put something together pretty quickly—and we did—that would, based on the same principle of axial centrifugation—by moving fluids around—we could come up with a lipid profiler, which did these LDL [low-density lipid, (bad) cholesterol], HDL [high-density lipid, (good) cholesterol, total] cholesterol, and triglycerides […].

CARUSO: There's nothing on the market for this at the time?

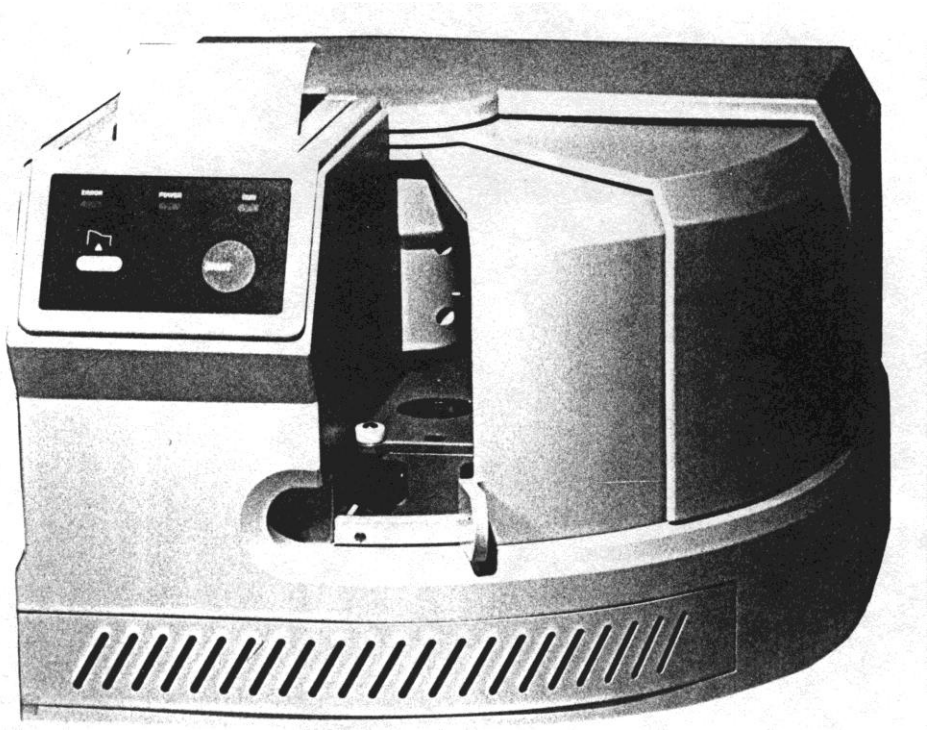
BABSON: There was a…. Yeah, there was one product, […Cholestech LDX]. I don't think there was a dedicated analyzer for this, that just did these analytes. We came up with one, and we went to Becton Dickinson. They said, "Yeah. That's great." But…oh, we went to them initially with a [prototype that used] modified IMMULITE [assay tubes], which would do twenty serum samples in about thirty minutes, do a profile, complete lipid profile. We built this. We went to Becton Dickinson, demonstrated it. It worked very well, but they convinced us that, "No, we don't really want an instrument that does twenty serum samples in thirty minutes. We want an instrument that does one blood sample, whole blood sample in twelve minutes," because they wanted to market it to the physician's office laboratory, and the physician wants to have the results while the patient is still there. The average patient visit was like twelve minutes, or something like that.

CARUSO: Twenty minutes, yeah.

BABSON: Yeah. So they said, "This is what we really need." So we went back to work, and <**T: 55 min**> then in several…in a few months, we came up with the thing, which is in my office. I don't know if you remember…



Batch Analyzer



Cardiac Risk Profiler

CARUSO: Yeah.

BABSON: …you saw it.

CARUSO: Yeah.

BABSON: We built three of those, or had them built for us, really (we didn't build them). Went back to Becton Dickinson and they said, "Well, that's pretty good. But how much?" We had an estimated manufacturing cost which was too high for their method of distribution, because they had several layers of distribution that they went through before they sold to the actual physician office laboratory. So it was too expensive for them.

CARUSO: Did you think of trying to market this directly yourself?

BABSON: No. No. The strategy was to work with a third party that would bring in some early cash flow into the company to make us less dependent on the venture capital. But we had no intention of manufacturing it or marketing it ourselves.

CARUSO: Okay.

BABSON: But…so when Becton Dickinson pulled out, we started scrounging around for other potential customers. [Unipath in Mountain View, California] was very interested. [The president of Unipath was Chris Monahan, an old colleague of mine from General Diagnostics. We made a presentation to Chris and ten of his top people in May 1990]. The lawyers were actually putting the final papers together, when the FDA released the proposed CLIA, Clinical Laboratory Improvement Act, regulations. So this Act was in 1988, CLIA 88 it was called.[8] But they didn't release them until [1990], and these proposed regulations would essentially put the physician's office market out of business, because they didn't differentiate between different size laboratories. Every laboratory had to have a clinical chemist or…

[8] U.S. Department of Health and Human Services, "Medicare, Medicaid, and CLIA programs: Regulations implementing the clinical laboratory improvement amendments of 1988 (CLIA-88)—proposed rule," *Federal Register* 55(20) (1990): 896–920, 959.

CARUSO: Oh, so they were instituting specific standards that would have made it too expensive for a physician to have…

BABSON: Yes.

CARUSO: …a laboratory in his or her office…

BABSON: Well, of course, these were the proposed regulations, but by the time they got adopted, they were much less onerous. But so Chris Monahan […] did an about face and said, "No thanks."

CARUSO: No thanks.

BABSON: So we never were able to find another customer.

CARUSO: What about for the original idea that you had for getting twenty samples done in thirty minutes? Was there any way to revert back to that? Because now it sounds like that would be much more useful…

BABSON: Well…

CARUSO: …in terms of a…

BABSON: It had limited use in the clinical lab, I think. But, no, we never explored that again.

CARUSO: Okay. So after the Cardiac Risk Profiler, what is it that you wanted to do with Cirrus? What was the next idea?

BABSON: Well, the main idea was the IMMULITE, the immunoassay analyzer. Yeah.

CARUSO: Okay. So you decided just to continue with that.

BABSON: Yeah, right. We just continued with that.

CARUSO: Okay. So how did that change and grow over time? I mean, you know, clearly you had your original device. But it's…

BABSON: Well, we had…at the time we had one customer, Morristown Memorial Hospital. At that time, we were looking for an acquirer, because the venture capitalists, they didn't want to start a standalone company. They wanted to get in and get out.

CARUSO: Right. They wanted someone to buy you up.

BABSON: Yeah, right. Yeah.

CARUSO: Make profits off of it. Right.

BABSON: Yeah. So we…now how did we get…I'm trying to think how we made contact with Diagnostic Products Corporation initially, and I don't recall to tell you the truth <**T: 60 min**>. But they were…oh, yeah. I had written Sigi Ziering, who was the President of Diagnostic Products Corporation, and asked him if he was interested in marketing or making reagents for the instrument. I think maybe we were considering going it on our own. He said, "No." He had their own division, had their own things they were working on. Well, it's a joke, the instrument they were working on. But…

CARUSO: In what sense? It just was not a good instrument.

BABSON: No. It was terrible, terrible idea. So I wrote to him again, and he came out and visited. He got…he had a spark of interest. He…so we were visited by three people from DPC, one of which was a woman that brought a whole bunch of samples. There were like two hundred samples with her, which [had been assayed at DPC]. She ran them on the IMMULITE in the schoolhouse. She was…the results were beautiful.

At that time, we had a little offsite manufacturing operation for the reagents. We had developed seven. We had approval, government approval, FDA approval, for seven different analytes at the time—TSH [thyroid-stimulating hormone], LH [luteinizing hormone], FSH [follicle-stimulating hormone], HCG [human chorionic gonadotropin], digoxin, T4 [thyroxine] and T-uptake]. We were going to go it alone. But then, Sigi came in and he got…basically, we

had a visit from several of his international distributors. They were…they convinced him that, "Hey, this is a good thing." So he made us an offer, and which we took.

CARUSO: You couldn't refuse.

BABSON: Well, no, we wanted…actually, he offered us twenty-eight million dollars for the company, and we thought it was worth more. We had one of our venture capitalists negotiating with him, but he wouldn't budge. We tried to get him up to at least thirty. But…so he acquired us in 19…

CARUSO: Ninety-two.

BABSON: Ninety-two, yeah.

CARUSO: Okay. So…

BABSON: Ninety-two or '93?

CARUSO: I had 1992 listed, but again, I can always be off by a little bit. What did the acquisition mean for your operations? I guess one question is, when did you move out of the schoolhouse?

BABSON: Shortly thereafter. Shortly…

CARUSO: Shortly after the acquisition.

BABSON: Yeah.

CARUSO: Okay. So the…was that because DPC wasn't really happy with having their…having you work out of a schoolhouse?

BABSON: Yeah.

CARUSO: Or was it just that the company needed a change?

BABSON: No. Well, we needed…once were acquired, we obviously weren't working with third parties to manufacture anything. We wanted to manufacture it ourselves, and there was no way we were going to manufacture [in the schoolhouse]. Even though we built twelve units there, […] the B units, one of which is down in the lobby, B-1. I don't know if you…

CARUSO: Is that one of the ones that…right in the entryway?

BABSON: Yeah.

CARUSO: Okay.

BABSON: Right next to…on the same side as the receptionist, that's the first IMMULITE that we sold to Morristown Memorial, which was the first one that was built in the schoolhouse.

CARUSO: Oh, wow.

BABSON: That was built in the schoolhouse. So we had to set up…find a manufacturing facility. And that's why…

CARUSO: So why…

BABSON: …we moved to a rental unit <**T: 65 min**> in Randolph, New Jersey.

CARUSO: Okay. So why is it that you wanted to do the manufacturing in-house? Did you just find that better quality control? Or…?

BABSON: Better control, yeah, all the way around. Sure.

CARUSO: Okay. What was your role in the company, then, after the acquisition? Did it change in any effects? I guess what I'm curious about is, since I don't know…I mean with

acquisitions you could have a situation where everything is taken, you're really incorporated into this new company, and what you had previously stops existing. But there is also the chance that you're bought by a new company, and they see that you work well as is, and they kind of leave you alone, as long as you…

BABSON: Yeah. They pretty much did leave us alone. Although, the President of DPC, [Sid Aroesty, also] became President of Cirrus [Diagnostics, which became DPC Cirrus…].

CARUSO: So there was the name change…

BABSON: So he divided his time between the…

CARUSO: The two.

BABSON: Between the East Coast and the West Coast.

CARUSO: Okay. So nothing else changed with how…

BABSON: No, pretty much…

CARUSO: …it functioned.

BABSON: Well, we…

CARUSO: You moved.

BABSON: Obviously, we moved, and we added a whole lot of people, mostly in manufacturing.

CARUSO: Did you have specific factors that went into your choice of the new site in Randolph? Is there anything…?

BABSON: Yeah.

CARUSO: What was…what were those?

BABSON: Well, it had to close by, because we didn't want the existing employees, which we were about thirty by then, in the schoolhouse, didn't want [them] to have to commute long distances. So it was close by. It […] was a nice facility. We could essentially fit it out—what do you call it?—fit it out to our own…

CARUSO: Specifications, needs.

BABSON: Specifications. So it was essentially empty space, and that was desirable. So we essentially designed the space. […] The front part was administration or research labs and what not; and the back was manufacturing.

CARUSO: Were you still…okay. Were you still working nonstop as you had been?

BABSON: Oh, yeah. Oh, yeah. I didn't slow down at all.

CARUSO: You didn't slow down, you didn't take a break or anything like that?

BABSON: No.

CARUSO: No. Okay. Were you still at the bench doing work? Or were you away from the bench more?

BABSON: I was pretty much away from the bench…

CARUSO: Away from…

BABSON: …at this time.

CARUSO: …the bench. Okay.

BABSON: Yeah.

CARUSO: So you were back…

BABSON: Very heavily involved, though, in all of the plans.

CARUSO: Okay. Were you handling a lot of the research aspects of things…

BABSON: Yeah.

CARUSO: …or the administrative?

BABSON: No, a lot of the research, too.

CARUSO: Okay. And after the acquisition, after you moved to the new location, what were your responsibilities to your parent company? Is it that you had to manufacture a…did you have to drum up your own business? Did you…?

BABSON: No. Did we have to drum up our own business? Well, we weren't…DPC had…they had a very strong marketing group, including in there a number of international divisions. So they had marketing. They did the marketing for us, essentially.

CARUSO: So you just had to build the devices and refine their operation?

BABSON: And they also—DPC—had reagent manufacturing. As a matter of fact, they initially made a decision that they didn't…and we had a reagent group here in New Jersey, too. And <**T: 70 min**> they said, "Well, we don't need these people anymore." So they let them go and shortly thereafter they had to hire them back, because they realized, "Hey this was a dumb idea. All of the expertise was resident in these reagent manufacturing people."

IMMULITE

IMMULITE 1000

IMMULITE 2000

IMMULITE 2000 SMS

So they shut down the…we had a small reagent manufacturing facility, where we coated beads and packaged them, and whatnot. But we had…the expertise was resident in the people. So they had to hire them back.

CARUSO: So what did you see DPC Cirrus doing? Or I guess the way to phrase this is, what did you want to change about your product?

BABSON: Well, we had the IMMULITE, which we're not manufacturing anymore. We're manufacturing a second generation, IMMULITE 1000 we call it. But I realized we needed…this was a relatively small analyzer that required a lot of operator interface, had to load the different tests on, that it didn't have, essentially, a reagent carousel, because reagents were essentially loaded on manually. So I thought we needed a higher-throughput, second generation instrument, which turned out to be the IMMULITE 2000. So we were already starting to work on that.

CARUSO: Okay. So was it…so it sounds like you wanted to automate things.

BABSON: Yeah, totally automate it.

CARUSO: So was there a reason for automating it? Did you…

BABSON: Sure.

CARUSO: …have feedback from people saying that, "We're making mistakes when we're loading these things into the machine," or…?

BABSON: Not particularly, but the laboratories were, at that time, getting…well, right now, they're totally automated. You walk into a laboratory, you don't see anybody working at the bench.

CARUSO: Right.

BABSON: They're just…

CARUSO: Machines in the background making noise. Yeah.

BABSON: Yeah. They're just loading samples on the machines. So we could see that was coming. Yeah.

CARUSO: Okay. So you were getting on the automation track because it was happening more…

BABSON: Yeah.

CARUSO: …generally in the medical field. So what aspects did you want to, of this device, did you want to automate? You mentioned one thing, the loading. Were there other parts that you wanted to automate more, or change in some respect?

BABSON: No, not really. We increased the throughput. […] We totally automated the sampling and whatnot.

CARUSO: Okay. Now, the device is…the models on the wall here…[9]

BABSON: Yes.

CARUSO: …are those the devices from this period of time, or did we pass by the devices that we were talking about earlier?

BABSON: Kind of concurrent. Yeah.

CARUSO: Concurrent. So could we spend a couple of minutes just talking about these various…

BABSON: Sure.

[9] The interview took place in the executive conference room where several of Dr. Babson's working wooden models were displayed.

CARUSO: …wooden designs, because, as I mentioned, I find them quite interesting. I guess part of what I'm curious about is how you were using these models? Were they simply just a way to convey your ideas to other people? Were you just comfortable…did you like building models, and so…?

BABSON: Well, it was a lot cheaper to build it out of wood, than to build it in a machine shop out of metal.

CARUSO: So this was sort of a proof of concept device.

BABSON: Proof of concept, essentially. Yeah. These were all proof of concept devices.

CARUSO: Okay. So could we talk about <**T: 75 min**> them, possibly in sequence, like we did earlier?

BABSON: Okay.

CARUSO: You can sit down, or we could stand up, whatever you prefer.

BABSON: We don't need to stand up. [These models are all] for the IMMULITE 2000 […]. The IMMULITE Test Unit has the bead packaged in it. The IMMULITE 2000 used bulk beads and bulk reagents on the carousel, and bulk assay tubes. These are the assay tubes for the IMMULITE 2000.

CARUSO: Okay.

BABSON: We needed a way to orient the tubes [so they can be transferred to] the incubation carousel. This was a concept using essentially a rotating hook-hopper feeder they call [it], to see whether or not this was a feasible approach to doing that, taking bulk-loaded tubes and orienting them into a queue and then individually…

CARUSO: So looking at the design here, what I get is that, you don't drop a whole bunch of tubes into a large…

Rotating Hook Hopper-Feeder

BABSON: Hopper.

CARUSO: …hopper. Is it just purely random that the tube will get picked up?

BABSON: Yes.

CARUSO: Is that what you're betting on?

BABSON: Totally random.

CARUSO: And so as things turn around, as thing move, it's churning this bulk…

BABSON: Right, yes.

CARUSO: …number. So, eventually, some…one will be oriented correctly in order to be picked up.

BABSON: Right. That's exactly right.

CARUSO: Okay. So the mechanism constantly rotates. It's constantly churning things around to pick something up, and then drops it down into a little chute, where it's perfectly oriented.

BABSON: Yeah.

CARUSO: And then it makes its way into the analyzer.

BABSON: Right.

CARUSO: Okay. So you mostly built that…you built it out of wood because it was cheaper. Actually, I think it's wood…

BABSON: Well, it's cardboard [and wood].

CARUSO: …cardboard and some tape. A lot of tape, I noticed to keep things in place. So was this…did this start as an idea that you had in your mind?

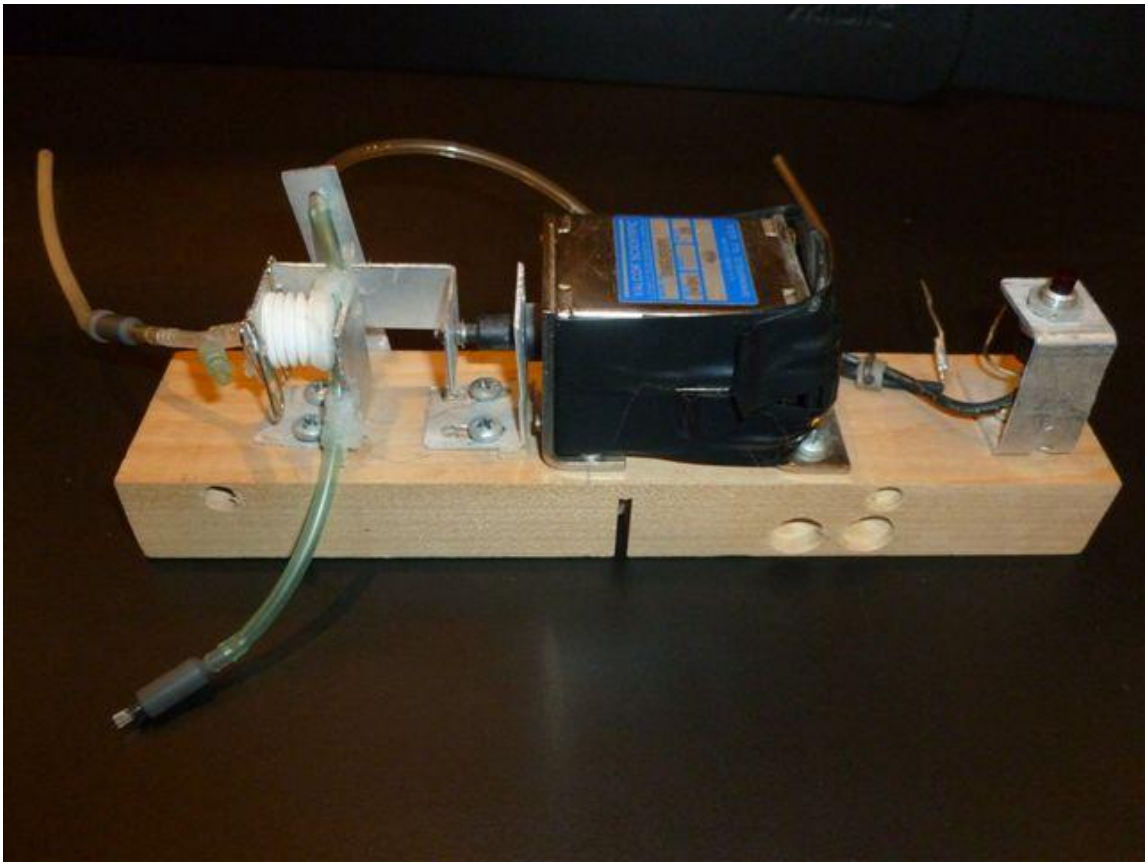
BABSON: Yes.

CARUSO: And then it came through, so then you built it. So it wasn't like you were sitting there playing with things, and you…?

BABSON: No. No. I knew what I wanted to build before I started.

CARUSO: Okay. What about this next device?

BABSON: That next device was…we needed to…after we incubated the bead with the sample and the reagent, and so forth, we needed to separate the bead, the liquid from the bead. Then we had to wash the bead. We were washing it with an injector pump, which would dribble, which was bad. So this device was to come up with an injector pump which would incorporate what we called suck-back: after the volume of fluid, the fixed volume of fluid, was delivered, in this case water, it would suck back a little bit.



Injector Pump with Suck-Back

CARUSO: Right. So that way you wouldn't get the dribble.

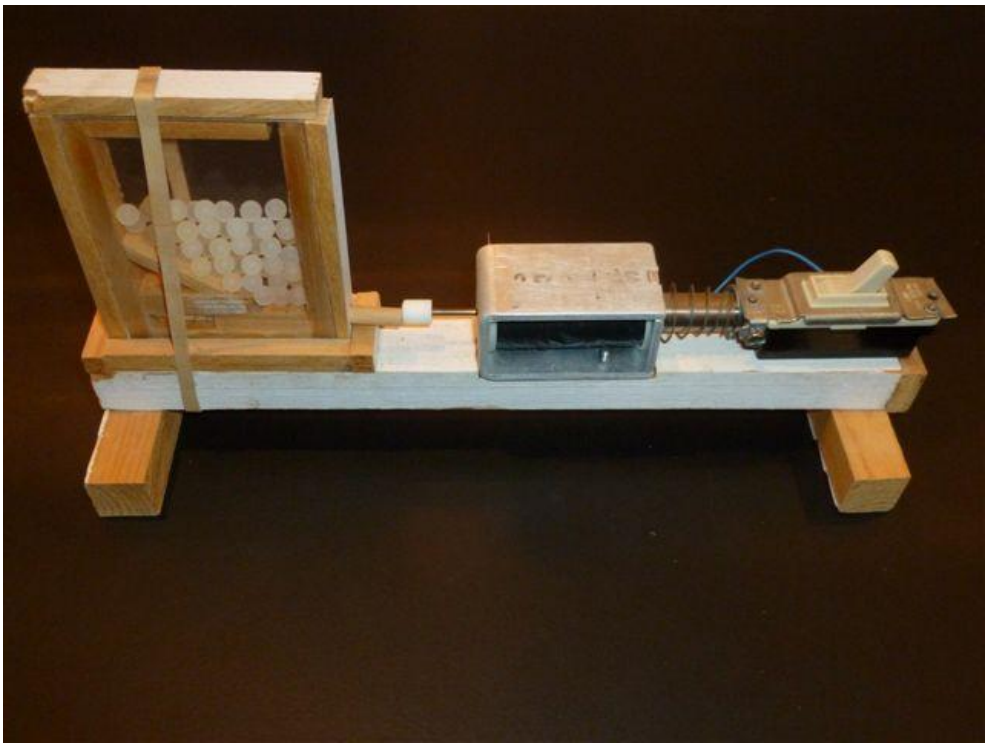
BABSON: Well, you wouldn't…yes, an anti-dribble injector pump.

CARUSO: Okay. This was just, again, to test an idea.

BABSON: Yeah.

CARUSO: Okay. What about the next one?

BABSON: Well, that was when this guy bit the dust—[Babson indicating a bead pack with a spiral groove to keep the beads in a line]. That bead pack, which was designed for us by an outside firm, [often didn't work. Static electricity would build up and prevent the beads from rolling down the spiral groove]. We needed an alternative. So this was a concept that would see whether or not we could bulk load the beads and singlicate them, and deliver them one at a time to the […] incubation ring. That worked very nicely.



Bead Loader with Singlication

CARUSO: So this uses a solenoid essentially to move a lever which opens a hole, which would allow one…

BABSON: Right.

CARUSO: …[bead] to come through.

BABSON: One bead. It also…there's what we called a "disturber" in there <**T: 80 min**>, which would reshuffle the beads [and keep them from bridging over the exit hole].

CARUSO: Right. So nothing would get stuck.

BABSON: So that some…the beads are kind of rough, and they would bridge. So you have to keep mixing them up. That's the same with that loader, re-loader.

CARUSO: Okay. So the last device, if you want to explain…which is using string, and rubber bands as well…

BABSON: Yes. That…

CARUSO: This is an agitator.

BABSON: That's to [mix] the solid phase, which is on the bead, and the reactants, which is the sample and the reagent, which are in solution. If you didn't mix the contents, the reaction rate would be dependent upon the diffusion from the liquid phase into the solid bead surface, and that's very slow. So to speed up the kinetics of the reaction, you need to mix the bead, keep the bead moving and within the assay tube. This device would, basically, knock—bump—the bottom of the tube periodically back and forth. That was incorporated in the IMMULITE pretty much.

CARUSO: So were they…I'm wondering if these models sort of stayed as is once they were incorporated. Like if we looked at the device, would we just see metallic versions of these things, or were they modified, then, somewhat from their original design to complement the machines?

BABSON: Oh, yeah, very much so.

CARUSO: Okay.

BABSON: Yeah.

CARUSO: So it's mainly to capture specific…

BABSON: It's just…it's just to demonstrate a concept.

CARUSO: Okay.

HUNTER-LASCOSKIE: So would you be bringing these into the conference room, and demonstrating them for a group? Or…?

BABSON: Sometimes, yes. Yeah.

HUNTER-LASCOSKIE: So I'm guessing they sparked some…

BABSON: When they…

HUNTER-LASCOSKIE: …good discussion, or…?

BABSON: I had a time convincing people that this pump with suck back, for example, would be precise enough. So I collected a lot of data, and it turns out it was more precise than the […] the pump we were using.

HUNTER-LASCOSKIE: Oh, so you'd basically be giving a presentation…

BABSON: Yeah.

HUNTER-LASCOSKIE: …backing these…

BABSON: Right.

HUNTER-LASCOSKIE: …up.

BABSON: Yes.

CARUSO: Did you have a wood shop here? Wood shop in the…

BABSON: I had my wood shop at home. Yes.

CARUSO: At home, so this is something you would do at night, after…

BABSON: Yes, right, absolutely.

CARUSO: Okay. Were you using that wood shop for anything else at the time? Or was it just your own personal…?

BABSON: Own personal wood shop.

CARUSO: Okay.

BABSON: Yeah.

CARUSO: Were there other aspects, other models that you developed to come up with new concepts for the…?

BABSON: I…sure there were. Yeah. There's one in the picture there—[Babson pointing to a framed picture on the wall surrounded by his wooden models]—that, I think, I have it in my bookcase […] in my office.

CARUSO: Okay.

BABSON: But they only…these were the ones that would fit on the wall, comfortably.

CARUSO: Okay. So now, this automation, this is happening in the early '90s.

BABSON: Yeah.

CARUSO: Was that when it was taking place? Are there other changes going on inside DPC Cirrus at the time? Were you still continuing to expand…?

BABSON: Well, we were growing, sure.

CARUSO: And what was the market like for your device? Was that growing as well? Was it a steady market?

BABSON: Yes. Yes.

CARUSO: And were you…?

BABSON: And there were a number of competitors at the time, too.

CARUSO: Okay, like what other companies?

BABSON: [Dade (Baxter Healthcare) had the STRATUS, PB Diagnostics had the OPUS, Syva had the SR1, Technicon (Miles) had the ImmunoOne, Amersham had the AMERLITE, Pharmacia had the DELFIA, Becton Dickinson had the AFFINITY, Toso Medics had the AIA 1200, Boehringer-Mannheim had the ES 300, Abbott had the IMX, and Ciba Corning had the ACS 180. Of all these products only IMMULITE is still being sold].

CARUSO: Were their devices—I know this is going to be somewhat biased—but were their devices on par with what you were creating? Were there things about those other devices that you…earlier you had mentioned that, I think it was DPC…DPC was…had their own…

BABSON: Well, DPC was working on a <**T: 85 min**> system which…

CARUSO: Had some problems.

BABSON: Had a lot of problems. Yes. Yes.

CARUSO: So were your competitors creating devices that had similar characteristics, in terms of the data that it could generate, to yours?

BABSON: Our device was unique in that we used the actual centrifugation to do separations, and washings, and which turns out to be very efficient. Other devices would use magnetic particles, let's say as a solid phase, and then squirt in water, suck it out again, squirt it in, suck it out, and it didn't work very well. Not as efficient.

CARUSO: So is it roughly around 1996, the mid to…I'd say the mid '90s that your more automated immunoassay analyzer? I'm just looking at when your patents were. There's a group of them. The two washing systems, the sample dilution system, and dilution well insert, the automated immunoassay analyzer, those seem to be…those patents are being issued somewhere in the mid '90s. So I'm wondering if that's roughly when the more automated version of the IMMULITE system was coming out?

BABSON: Oh, when did we market the IMMULITE 2000? […I believe it was in 1997].

CARUSO: Sure. I was wondering if it was around that general period of time. So it seems like you've…so somewhere in the mid 90s, you've developed a lot of automated techniques for this system. I know in 1997 you received the New Jersey Inventors Hall of Fame Award.

BABSON: Yeah, Inventor of the Year Award from the New Jersey Inventors Hall of Fame.

CARUSO: Right. Can you tell me a little bit about that award, how it came about? How…?

BABSON: I believe John Underwood nominated me for the award. They had…New Jersey Inventors Hall of Fame had a committee that evaluated these nominations, and they made the selection. We had a ceremony down in Newark, [New Jersey]. I had to wear my monkey suit. It was…they had a number of different awards that they had, annual awards. I was only one of several recipients.

CARUSO: Was it acknowledging your entire career? I mean you've done everything, pretty

much, in New Jersey.

Arthur L. Babson receiving the New Jersey Inventor of the Year Award.

BABSON: Yeah, pretty much. But it was mainly for the IMMULITE system. Yeah. I'm pretty sure.

CARUSO: Okay. So after you came up with the IMMULITE 2000, did…what was the next step for this system? Did you see…were you looking to tweak this system more, or were you trying to think of new ideas to pursue? Were you receiving anything or any sort of guidance from your parent company, DPC at the time, about directions to go?

BABSON: No. DPC—hi, Carroll [Carroll Scribner entered the room]—DPC was totally dependent on us for the direction.

CARUSO: Okay.

BABSON: Except for marketing, of course. They were in charge of marketing.

CARUSO: Right.

BABSON: But in terms of the technical aspects of the new systems, they didn't have any contribution to that at all.

CARUSO: Okay. So what is it that you wanted with these systems? Where did you see them going?

BABSON: The newer systems, the IMMULITE 2000?

CARUSO: The IMMULITE 2000, or what would come after that?

BABSON: Well, what has come after that is <**T: 90 min**> the IMMULITE 2000 XPi, which was just introduced…what a year or two ago? Two years ago, yeah.

CARUSO: So what is it that you saw, or what is it that you wanted to accomplish with the IMMULITE system after you came out with the IMMULITE 2000? What is it that you were trying to address that you needed a new model for? Was it just incorporating outside technologies? Or the general change in technological development that you needed to incorporate into the device?

BABSON: Well, we needed…currently on the IMMULITE 2000, to change samples you'd have to put the instrument into pause. Then it would take a while to re-crank it up again. We wanted that totally automated, so that's where the XPi came in, where we had total automation of the sample loading.

CARUSO: So it's now a continuous flow instead of like a batch system.

BABSON: Yes, more or less. Yeah. That's a good way to put it.

CARUSO: Okay. What else, if anything, is going on in terms of modifying these systems? I'm just wondering if…

BABSON: Well, we were working on new systems like the…what we called the…what is now called the VersaCell, which was then called the […] SMS, Sample Management System, which would basically totally automate the sample handling. The operator just loaded, bulk loaded, trays of samples, and the robot would then pick up the sample, put it in the IMMULITE, where it would be pipetted. Then the robot would put it back into the sample tray. So this was a product […] which is essentially our next product.

CARUSO: Okay. Was this something that, again, you were coming up with these ideas about how to push diagnostic, clinical diagnostics, in new directions? I mean, talking…IMMULITE was, "I had an idea one day. I thought people might need this. So it just…." That's what you worked on. I was wondering if you were having other ideas along those lines about devices that you might want to pursue or that you're thinking of pursuing at some point?

BABSON: No. No. We were an immunoassay company. We were just trying to improve the quarter-inch bead technology.

CARUSO: Okay. So can you take me a little it through any other changes that were occurring within the company? I know that you…Siemens acquired you in 2006.

BABSON: Right.

CARUSO: So I'm wondering how that sort of came about, and also, if there were other changes going on within the company?

BABSON: You have to ask Siemens that.

CARUSO: Okay.

BABSON: But they decided to get into the *in vitro* diagnostics business. They weren't in it at the time. So they just willy-nilly bought up three different companies and put them together.

CARUSO: So did that affect you at all in terms of how this company was being run? Or is it just, it was an acquisition for them, and they were happy with what you were doing, and they were leaving you alone?

BABSON: No. They didn't leave us alone. They threw their weight around, you might say. But it's been good.

CARUSO: Okay. At what point did you wind up coming over to this facility here? I mean, I guess what time did you build this facility and come over here?

BABSON: Nineteen ninety-one, I think was…

SCRIBNER: No, 2001.

BABSON: Oh, 2001. Yeah, 2001…

SCRIBNER: December.

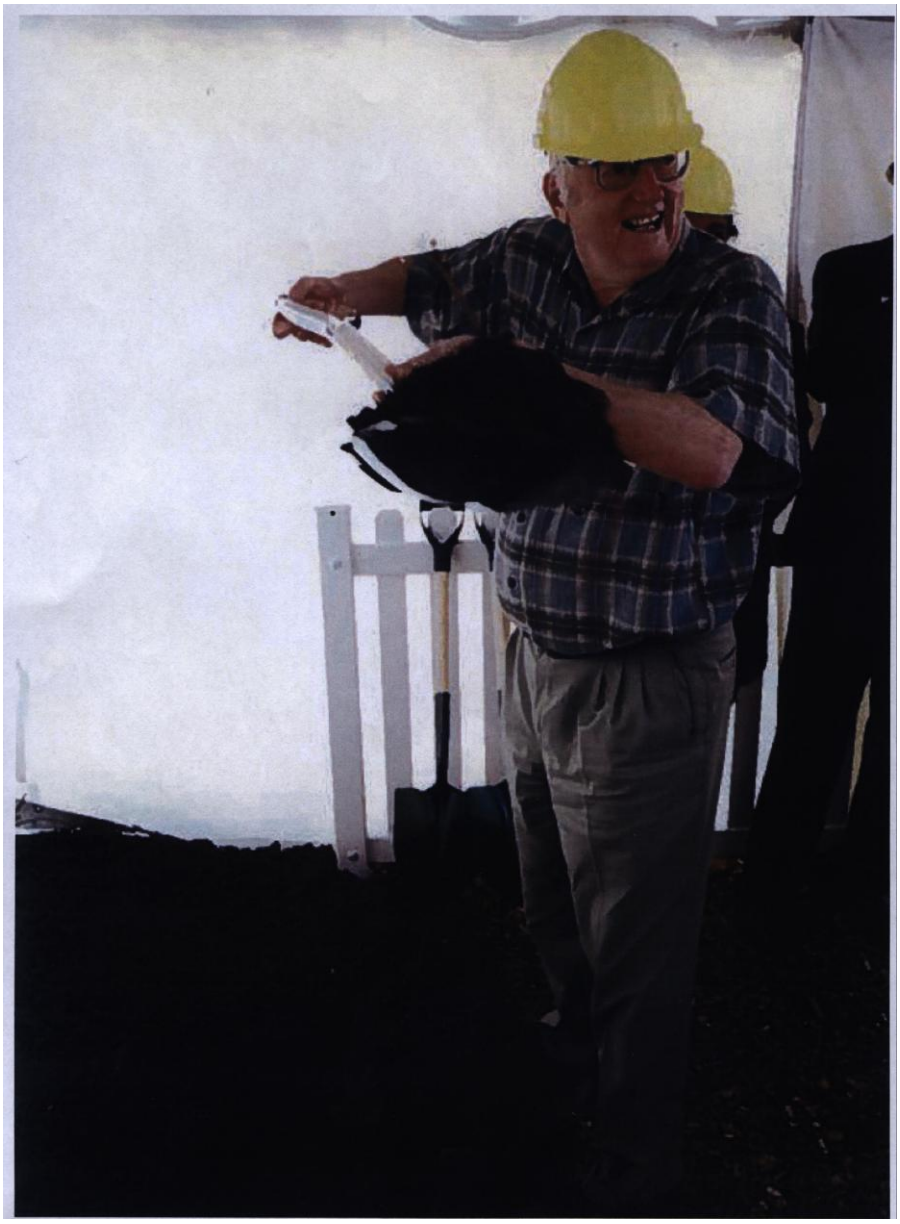
BABSON: Right.

SCRIBNER: Two-thousand one […].

BABSON: Ten years ago.

CARUSO: So what precipitated the building of this facility <**T: 95 min**>, and the move?

BABSON: Well, we ran out of space at the rental facilities in Randolph. We just needed more space. So we bought this property, designed the building to our specifications, and built it and moved in.



Groundbreaking ceremony for new building in Flanders, New Jersey—27 September 2000.

CARUSO: And this was still the…

BABSON: We have twenty-eight acres here, so we have room for a lot more expansion.

CARUSO: Lots of expansion. You're still manufacturing everything on site.

BABSON: Everything on site. Yeah.

Flanders building dedication ceremony, 24 May 2002. Foreground (left to right): Michael Ziering, President of DPC; James D. Watson, Board Member; and Arthur L. Babson, Chief Scientist.

CARUSO: Okay. Did the character…or how would you describe the character of the company over time? Clearly, I mean, initially, if you were selling only a few devices here and there to a very local group of individuals, I would assume that managing…

BABSON: No. We had worldwide distribution right from the beginning.

CARUSO: Right from the beginning.

BABSON: Well, not right…after the DPC acquisition.

CARUSO: Right. I'm talking about thinking back to before the DPC acquisition, when it was just Cirrus.

BABSON: Yeah.

CARUSO: Clearly as you build these devices, you also, in many respects, take responsibility for those devices, right. So out in the field, if something goes wrong, I assume that you have to go…not necessary you specifically, but someone from the company has to go and work with people, maybe you bring people in to understand how to use the device. How did those aspects of the company develop and change over time?

BABSON: Well, they grew by leaps and bounds, you might say, because we had, at the time, at maximum…we had how many people in field service?

SCRIBNER: At maximum?

BABSON: Yeah.

SCRIBNER: Probably fifty or sixty right before we…

BABSON: So we would…and we had customer training here. Of course, this wasn't at the time of the DPC acquisition. We had Gene Hochmuth, the gentleman that took us to lunch. *He* was field service. He started it and he built up the group, essentially. But and we did customer training here. We started off small, and built a group. Now we have a lot of people.

CARUSO: Are things still operated roughly the same way now that they were when you were coming up with the name Cirrus? Is it still just people sitting around a desk coming up with ideas? I'm just trying to understand whether the structure of the organization, the way…

BABSON: The structure of the organization since Siemens acquisition has changed dramatically, because we're totally integrated in several locations.

CARUSO: Right. I'm just wondering if the culture of the ideas, how things are thought up and implemented…in the schoolhouse you worked closely with a lot of individuals. It seems like

people working together to come up with devices, instrumentation, how to build it, and things like that, does that aspect of things still exist now? Or has that changed?

BABSON: Yeah, I would say it still exists now, except it's distributed. Some of our new projects, for example, are distributed between Flanders, [New Jersey]; Glasgow, Delaware; [and] Tarrytown, New York.

CARUSO: And how is that sort of managed? Do people from this facility travel to other facilities? Is it just like a movement, a shuffling of people around on a regular basis for meetings, and things like that?

BABSON: Yeah.

CARUSO: Okay. So everyone's kind of kept in the loop about everything…

BABSON: Right.

CARUSO: …that's out there.

BABSON: Right.

CARUSO: Okay. All right. Now, some of the materials that you sent <**T: 100 min**> us to prepare for this interview, they weren't technical documents, right. They weren't published papers in…published scientific papers. You sent one piece that talked about belief systems, the world being round, the world being flat…

BABSON: Oh, those are my personal essays.

CARUSO: Your personal essays.

BABSON: That's got nothing to do with the company.

CARUSO: I know that has nothing to do with the company. But I'm actually interested in hearing a little bit about what inspires you to write those personal essays, and for what sort of audience are you writing those personal essays?

BABSON: Well, the audience is mostly me.

CARUSO: Okay.

BABSON: I just…I get inspired by…for instance, the one that…"Guns for Everyone," was inspired by an incident in the schoolhouse, where…not the schoolhouse, our schoolhouse, but where some kids were killing each other.

CARUSO: And you had the one about the litigious society. I'm just curious what motivates you to write those pieces. I actually haven't met many people that sit and write personal essays that…

BABSON: Really?

CARUSO: …actually reflect on things beyond their career. So I'm curious why it is that you write those things.

BABSON: I don't know to tell you the truth. I've got a whole book of essays, if you'd be interested.

HUNTER-LASCOSKIE: Is that something you've done throughout your career, just kind of when you have the time? Or is it only something that's maybe more recent?

BABSON: Well, and by "more recent," you mean last decade or so?

HUNTER-LASCOSKIE: Right.

BABSON: Yeah, probably.

CARUSO: Okay. Are there any things that…I don't actually have any more questions myself. What I usually like to do at the end of the interview is try to give the interviewee a chance to talk about something that's either related to your work, to your research, or anything more broadly, that I haven't asked about. Sometimes I throw this question out and people start talking to me about science education in the United States.

Sometimes I throw out this question and people get into the fact that, actually, for the past twenty years, they've devoted most of their free time to volunteering at the Society for the Prevention of Cruelty to Animals. So I'm trying to capture as much as I can in this oral history, but there's a lot of stuff I don't know about. So I like to give you a chance to bring up some topics that you find interesting that we could possibly discuss. But if you have nothing…

BABSON: Well…

CARUSO: …that's also fine, as well.

BABSON: One of the things we didn't mention was that Susan and I—[I mention this] in the memoirs—Susan and I built our own house.

CARUSO: Okay. When was that?

BABSON: In 19…shortly after we were married. That was early 1980s. Took us three years.

CARUSO: Did you literally build it yourselves?

BABSON: Literally built it myself.

CARUSO: So…

HUNTER-LASCOSKIE: Wouldn't that require some…

BABSON: Every board, and nail…

HUNTER-LASCOSKIE: Did that require a lot of learning on the job, or was that something that you and Susan had to figure out how to do it? Did you have some experience?

BABSON: We…well, I've always been into building stuff, but—as demonstrated by the models on the wall here—but I always wanted to build a house. We were lying in the hammock one day, and I said, "I'd like to build my own house," and she was very enthusiastic. She said, "Yeah. Let's do it."

CARUSO: So where were you going to…where did you wind up building?

BABSON: Well, right…actually, right next door to where we were living.

CARUSO: Oh, okay, so an easy commute.

BABSON: We bought the six-acre lot. Susan and her prior husband, David, had already bought it. When, after Susan got divorced, and I got divorced, and we got married, we bought it back from David. That's where we're living right now. So, we…it was very convenient, because we were living right next door <**T: 105 min**> to where we were building the house.

CARUSO: So you'd go out at four in the morning…

BABSON: Yeah. We would go out about…well, not four in the morning. But every afternoon after work, go home, change our clothes, go over to the building site, and work until dark, and go back and have dinner, and go to bed.

CARUSO: Anything else while you were building, other than building that house?

BABSON: Anything else I was building…?

CARUSO: Anything else you were building, or were there other things you were doing while you were building the house? I'm assuming you still had some free time—just joking…

BABSON: Well, we got interested in Africa, and we spent a lot of time in Africa, mostly on safari, but also involved with the Cheetah Conservation Fund in Namibia. I was on the board of directors of the Cheetah Conservation Fund, an example. We spent a lot of time and resources supporting them.

CARUSO: What was the pull for you? I mean what was the…

BABSON: I liked animals.

CARUSO: Liked animals.

BABSON: I love animals. Yeah. If you end up with…reading the…did you look at the pictures?

CARUSO: I was hoping…I was hoping that you would bring up the raccoons without me prompting.

BABSON: Well, I've always been interested in animals, always liked animals. We have three cats at home. But…and Susan feeds—I don't know if you noticed that picture on my desk of the two bears and the cat in the sun room…

CARUSO: I haven't. I'll have to take a look. But…

BABSON: Yeah. Take a look at that. Susan feeds all the animals and we raised a couple of sets of raccoons. The first set was like…wandered in one day, and we opened the door, and they walked into the bedroom—our bedroom is on the ground floor. The second set fell out of the nesting tree. We had to nurse them with a bottle, bitch's milk. But they were a lot of fun.

CARUSO: How much time did you spend in Africa doing…well, how much time did you spend there…

BABSON: Well, we made…I think we've probably been there at least a half dozen times, probably more.

CARUSO: What parts?

BABSON: Kenya, Tanzania, Zambia, then Namibia.

CARUSO: And how long would you…?

BABSON: We'd go for like a month. Yeah.

CARUSO: Okay. Is there a reason cheetahs specifically?

BABSON: Yeah. We always liked cheetahs. Susan got involved with Laurie [L.] Marker who is head of the Cheetah Conservation Fund in Namibia. We've been there a number of times.

CARUSO: I noticed, actually, I think it's the December, maybe the November issue of *National Geographic* is all about big cats, and conservation.[10] So it's just interesting that…I mean, well, there was something I didn't necessarily know about you.

BABSON: Yeah.

CARUSO: Are there any other things that you would want to discuss, any other topics that you're interested in, or you would like to talk about?

BABSON: I don't think so.

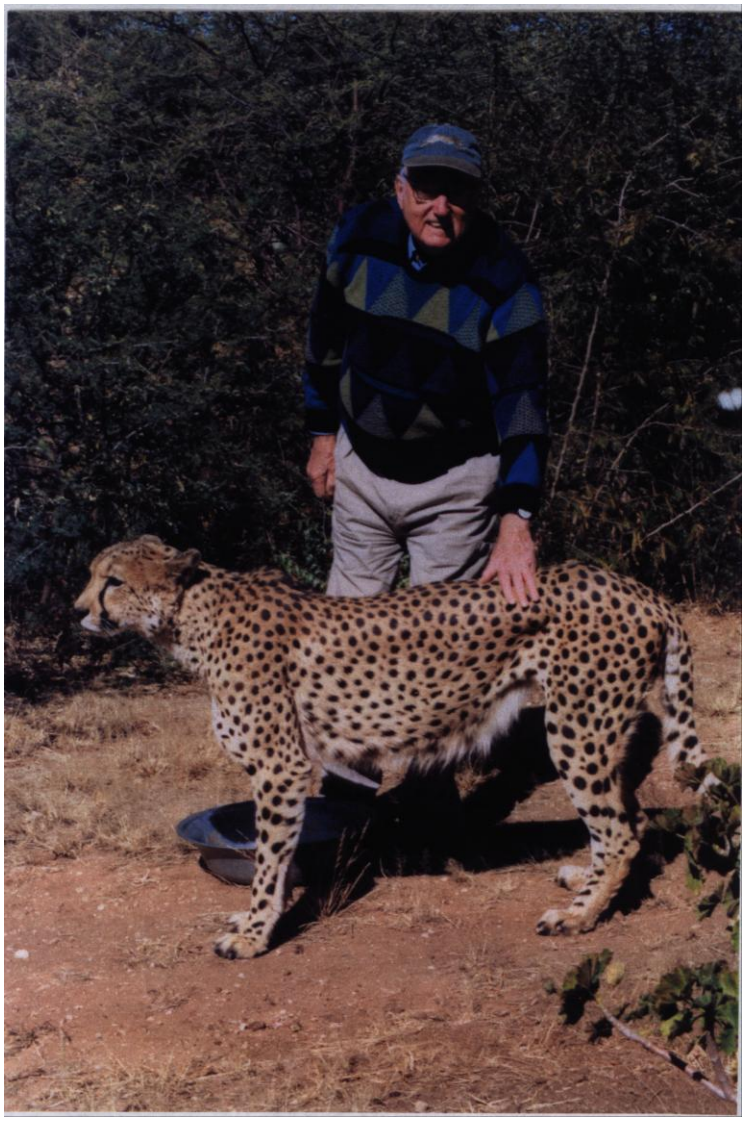
CARUSO: No. The remembrances that you provided, it's dated December of 1995. So I'm not sure…

BABSON: Yeah. There's an update in the end there. I don't know if you've gotten to it yet. It's…

CARUSO: Two-thousand seven addendum.

BABSON: Two-thousand seven, yeah.

[10] *National Geographic* (December 2011).



Arthur L. Babson with Chewbaka in Namibia.

CARUSO: I mostly focused on the pictures and didn't get past those. Okay, all right. The Van Slyke Award for Outstanding Contributions to the Science of Clinical Chemistry in 1998…

BABSON: Yeah.

CARUSO: Didn't know about that one.

BABSON: Oh, you didn't know about that one…

CARUSO: No.

BABSON: On my wall in my office <**T: 110 min**>

CARUSO: But I didn't see your office before conducting the interview. So what was the Van Slyke Award? Was it…was that the only other award that you received?

BABSON: That was the only other award that I got from the Clinical Chemistry Association.

CARUSO: And that must be your continued work in the…

BABSON: Right.

CARUSO: …field.

BABSON: Right.

CARUSO: Okay.

BABSON: Yeah.

CARUSO: Anything else, any other…?

BABSON: Other, there was the New Jersey Inventors Hall of Fame, Inventor of the Year Award, which is also up on the wall. The one that Carroll showed you, that was the Lifetime Achievement Award from Siemens, which I got just a year ago.

CARUSO: Right. Okay.

BABSON: In Germany. I think it's the first and only Siemens Lifetime Achievement Award.

Arthur L. Babson receiving the Siemens Lifetime Achievement Award, Munich, Germany, 19 January 2010.

CARUSO: Wow.

BABSON: So that was kind of nice.

CARUSO: Yeah. That sounds great. Is there anything else you want to talk about? No.

BABSON: Not really.

CARUSO: Okay. All right, well thank you very much.

[END OF AUDIO, FILE 2.1]

[END OF INTERVIEW]

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APPENDIX I: OUR RESPONSIBILITY TO THE FUTURE

President's address given at the Morristown Unitarian Fellowship
September 28, 1958

In many respects the most audacious of all the great religious insights that have come into the world was the apparently absurd conviction of Jesus that men must love one another. "A new commandment I give unto you that ye love one another." In reality, this new commandment was not an absurd and arbitrary rule laid upon man from the outside. It was, rather, the most profound insight into man's nature that had yet been achieved. Man is in sound psychological health to the degree that he relates himself affirmatively to his fellow man. By love Jesus meant not the possession of a person but the affirmation of that person, granting him the right to his unique human-hood. When Jesus said, "Do unto others as ye would that others do unto you," he was asking us to see others with the same honest concern with which we see ourselves.

In his "Reverence for Life" Schweitzer has taken the great ethic of Jesus and extended it to include all living things. I would like to extend this ethic one step further to include future generations as yet unborn. We should be concerned not only with the welfare of the present inhabitants of this planet but also with the welfare of those who will inherit the earth from us. It is not enough, however, that we should be concerned with the welfare of the future, we should act positively to ensure that future.

This year marks the centennial anniversary of two unrelated events which have a bearing on my address this morning. One was the birth of Theodore Roosevelt and the other was the reading of Darwin's paper on the origin of species.

When he became president in 1900, Theodore Roosevelt did not hesitate, in the face of political opposition, to use his power and influence to correct long-standing abuses in the management of land, water, forests and wildlife. His achievements in conservation are many, and the policies and programs that he initiated still influence our thinking and our action. During his administration more than 100 million acres of public land were set aside as national forests. More than 50 game preserves and bird sanctuaries, 85 national monuments and many national parks were created by executive order. The land available today for such projects is vanishlingly small and is becoming smaller every day. Indeed, we must be doubly vigilant lest private interests encroach on our existing national parks and forests.

We are indebted to Darwin because he placed man in the proper relationship to his environment. He showed the man is ultimately governed by the same natural laws to which other species are subjected. Briefly, Darwin noted the universal tendency of organisms to vary and to transmit these variations to their offspring. Darwin's second point was that organisms tend to reproduce far beyond their capacity to survive, and the relatively small proportion of individuals surviving did so, not by random chance, but largely because they were best fitted to cope with their environment. The emergence of human intellect, however, has brought an abrupt change in the slow process of organic evolution. Man, a single species, has become an important

geological and ecological influence on the earth. With his machines and his technology, Man advances his competitive position more in a century than he could in a million years of biological evolution. Because of his overwhelming competitive advantage, his numbers continue to increase prodigiously at the expense of other creatures. He threatens to sweep from the earth whole orders of life, and in so doing he may initiate irreversible changes that will sweep him off also. Man is fast outgrowing the planet.

Ever since agriculture began, frontiers have marked the boundaries between the natural and the artificial ways of life. So long as the wilderness frontier has existed, the implication of man's artifice has been obscured. The establishment of the vast American frontier gave the illusion of an endlessly expanding horizon, when actually it signaled the beginning of the end of all frontiers. The little pools of cultivated, urbanized territory have expanded, coalesced and swept over the earth. Within our lifetime the wilderness frontiers of the world will be gone. The fateful meaning we must see in the passing of the frontier is this: when man obliterates the wilderness he repudiates the evolutionary process that put him on the planet. In a deeply terrifying sense, man is on his own.

His responsibility to the life principle lies heavily upon him. If the study of nature leads one to any sense of the kind of significance and meaning that we associate with religion, that feeling must be aroused by the insistent proliferation of order that works through the obscure processes of evolving life. Today the future of the products of that long and patient waiting rests largely in man's hands. A living species embodies a unique accumulation of genetic individuality and adaptive wisdom. It is one more insurrection against the second law of thermodynamics. Each form has attained its own private expression of individuality and beauty, and when it perishes the universe is poorer. Surely no other of man's transgressions is so fundamentally evil as the heedless destruction of these irreplaceable evolutionary life-lines.

As regards the extinction of birds, North America has the worst record of any continent on earth. Audubon once estimated a single flock of passenger pigeons to number over two billion birds. The last passenger pigeon died in captivity in 1914. The Labrador duck, the great auk, the Eskimo curlew, the Carolina parakeet are all gone. The majestic California condor, the largest flying bird in the world, which has survived since the Pleistocene has been reduced to a remnant of about sixty individuals. The ivory-billed woodpecker and the whooping crane are also on the verge of extinction. Even our national emblem, the bald eagle, is on dangerous ground. In Florida, one of its strongholds, Charles Broley, who has banded more eagles than anyone else in the world, estimated that there were 500 active nests in 1940. This year he doubts if there were 80 nests that produced young in the whole state.

The primary factor responsible for the rapid decline in American wildlife has been the wholesale destruction of habitat resulting from population pressure and industrial expansion. The sad part is that much of this destruction is unnecessary and is the result of abuses and excesses such as unwise drainage, pollution, overgrazing, burning and over-cutting. With sound management practices land can be used for other purposes and still support wildlife. Drainage, on the other hand, is different. You cannot possibly drain wetlands and still have the wildlife they produce. The waterfowl, muskrats, shorebirds and fish go down the drainage ditch with the

water. Every acre that is drained is a total loss of a habitat and the wildlife it once supported. About half of the original 140 million acres of swamp and marshland in the United States is gone. Besides the wildlife they produce, wetlands serve as natural reservoirs for fresh water, the supply of which is becoming more critical every year. All in all, it is a confused, illogical picture in which we drain natural wetlands of high natural value to produce subsidized crops which we already have in surplus, while we find it necessary to subsidize the retirement of already producing land, while we spend huge sums of money to create new sources of water.

So much for wildlife. Now what about man?

It is an awesome thing to contemplate a graph of world population growth. In 6000 BC when recorded history begins the population of the world was about 10 million people. It took 3000 years for the population to double. The last doubling of the world population took only 100 years. The recently published *United Nations Demographic Yearbook* for 1957 reports that the population of the world is increasing by 47 million people a year, a rate that is calculated to double the estimated 2.7 billion inhabitants within the next 40 years. Perhaps the most striking observation is the speed at which population forecasts go out of date. Just three years ago in the August 13, 1955 *Saturday Review* there was an article by Paul Henshaw entitled, "80,000 Hungry Mouths a Day." Now there are 130,000 new hungry mouths a day.

"All right," you say, "This is admittedly a problem for the rest of the world, but not here in the United States. We've got lots of room." On the contrary, in several respects the problem is greater in this country than in many parts of the world. In the first place, the per capita consumption of energy and raw materials in the United States is ten times that of the rest of the world. Our resources cannot support at anywhere near our present standard of living, population densities such as are found in Asia. Secondly, in some countries such as India and Japan, the government recognizes the problem and is trying to deal with it. Our government, on the other hand, tries to convince us that a rapidly growing population is a good thing and should be encouraged. In the third place, the population of the United States is growing faster that the world average.

The very fact that we have a margin of safety not enjoyed in many parts of the world is both a challenge and an opportunity. Yet the idea of plenty in food, energy, and minerals dominates the discussion of our future. Our almost hypnotic fascination with outer space is one more thing that diverts attention and energy from the prosaic business of setting our terrestrial space in order. And it has fostered an incredible escapism. One hears too frequently the sober assertion that we need not worry about depletion of natural resources now that interplanetary travel is just around the corner. This obsession is not a detached phenomenon, but is the culmination of a new faith – the belief that technology will solve any problems that may confront humanity. Our present attitude toward terrestrial space exemplifies our selective use of science. Terrestrial space is our one natural resource that cannot be increased by any amount of technology. Yet the power of applied science has been overwhelmingly employed to exploit land, while those aspects of science that could illuminate its wise and lasting use are still largely ignored. Equally serious is the increasing vulnerability that comes from utter dependence upon elaborate technological systems. If technology should be our servant instead of our master, its

advancement should be in the light of all scientific knowledge and not merely of those facets that are of immediate use.

This then is the crux of the population problem: it is essentially a problem in ecology, but the ecological aspects are largely overlooked. The growth of any population, plant or animal, can be resolved into two phenomena, the capacity of organisms to increase in numbers and the capacity of the environment to support organisms of that type. All species are potentially capable of increase far beyond the capacity of the environment. Various technological solutions to the population problem have been proposed including restoration of damaged agricultural land, irrigation of the deserts, new methods of exploiting marginal land and algae farming. These are all proposals for increasing the carrying capacity of the earth for human populations and thus would stimulate population growth. Nothing permanently is gained by increasing the carrying capacity of the environment except a larger population. We can visualize a temporary relief of population pressure while the now arid Great Basin and Southwest are becoming as densely populated as Manhattan Island and while our lakes are being transformed into algae factories. At the end of the cycle of population growth we would not have our recreation areas, but we would still have our population problem.

It is a completely obvious but commonly overlooked fact that any positive rate of population growth must ultimately cease. The only real solution to population growth will come when the average death rate is equal to the average birth rate. It is up to us whether this equilibrium population will be at the limit of environmental capacity, as it is with most species, in which case our standard of living will be at the survival level, or whether we will use our intelligence and hold population to a level that does not deplete our natural resources or overcrowd the living space – and I am thinking of crowding in terms of psychological space, and our responsibility to other life forms more than of food and shelter. I am assuming that it is not enough for man to live by bread alone, but that intangible as well as tangible values are necessary to justify his persistence. The moral issue is not how many people can exist in the world, but what kind of life will be possible for those who do.

In the long run, of course, it is clear that if people desire to achieve low death rates – if they persist in using the techniques of death control – they must match these low death rated with equally low birth rates. This means that they must make use of the techniques of birth control. The sooner these techniques are adopted on a wide scale, the sooner hunger and privation can be eliminated from the world's population. The main problem in the adoption of birth control techniques, unfortunately, is less with technology than with the necessity of changing people's beliefs, customs, and attitudes. For the solution will come only when voluntary, responsible family limitation is the accepted custom. The first step toward such a goal should be a comprehensive educational program aimed at informing the public about population problems and changing the social attitudes toward birth control. We need more articles in the press and in lay journals on the consequences of overpopulation. I have a whole sheaf of articles on overpopulation culled from various scientific journals, but only two from The Saturday Review and one from *Esquire*. And we need more articles written by biologists and ecologists and less by technologists and businessmen. I believe businessmen are particularly responsible for fostering the idea that a rapidly growing population is a good thing. What's good

for General Motors is not necessarily good for the country. Any country that bases its economy on a growing population rather than a stable one, because it is easier, does so at the expense of later generations.

There is tremendous social pressure in this country today toward large families. Witness the perennial picture in the paper of the woman who had her tenth baby delivered free of charge; the applause when the quiz show contestant announces that there are seven little ones watching the show at home; books like "Cheaper by the Dozen"; ads portraying the All-American family with never less than four children. Today, despite the staggering cost of education and the increasing cost of food, the large family is in vogue. It sounds incredible, but I am convinced that it is through the influence of fashion that many modern families work out their response to the problem of population and space.

There is much the government could do to alleviate the problem such as reduce the tax deduction for dependent children, make contraceptive knowledge more available, and liberalize abortion laws to name a few. But the government will not act unless forced to do so by popular opinion.

With a population set to double in less than 40 years, with a national space which, though vast, is finite both in area and quality, with each individual making growing demands on our natural resources, we have on our hands a problem without precedent in human history. But if we recognize the problem and believe it worth solving, we can solve it. I would like to believe that the tremendous forces of organized religion and of popular education will overcome the superstition, the ignorance, the apathy and the psychological hindrances that now stand in the way of population control.

It lies within man's power to hold his own numbers to a level that will ensure the maximum spiritual and material benefits for this and future generations. This is a challenge and an opportunity never before presented to any species on earth. It is up to us to use our God-given intelligence and meet this challenge. The greatest moral responsibility mankind can assume is to provide for the welfare of future generations. When we continue to indulge in irresponsible fertility, eradicate entire species of living organisms, and depredate our natural environment that cannot be replaced by any amount of technological ingenuity, we are forsaking that responsibility.

Arthur L. Babson

APPENDIX II: LETTERS TO THE EDITOR, 1958-2005

Population Control

Published in the *Newark Evening News* — December 2, 1958

Periodically you editorialize on the "blessings" on our high and increasing rate of population growth. Your editorial of Nov. 15, "Everybody's Welcome," was typical. The importance you see in population growth is in the increased demand for consumer goods and the consequential stimulation of business.

Any country that bases its economy on a growing population rather than on a stable population penalizes future generations. It is a commonly overlooked fact that any positive rate of population growth must ultimately cease.

It is up to us whether we hold population to a level that does not deplete our natural resources or overcrowd the living space. I am referring to crowding in terms of our psychological needs and our responsibility to the other life forms. We must not obliterate the wilderness, for wilderness is a spiritual necessity to the people of America, an antidote to the high pressure of modern life, a means of regaining serenity and perspective.

The moral issue is not how many people can exist in the world, but what kind of life will be possible for those who do. In the long run, of course, it is clear that if people desire to maintain low death rates — if they persist in using the techniques of death control — they must match these low death rates with equally low birth rates. This means that they must make use of the techniques of birth control. The great difficulty lies in the necessity of changing people's beliefs, customs and attitudes toward birth control. For the solution will come only when voluntary, responsible family limitation is the accepted custom.

It is high time that newspapers which have such great power in molding public opinion cease looking at population growth with the myopic and selfish viewpoint of the businessman and assume the moral responsibility to use their power to help overcome the superstition, ignorance, apathy and psychological hindrances that now stand in the way of population control.

Arthur L. Babson
Morris Plains

About 'Fanny Hill'

Published in the *Morristown Daily Record* — December 18, 1964

Superior Court Judge Morris E. Pashman has declared *Fanny Hill* to be 'patently offensive," "utterly without redeeming social importance," and appealing to "prurient interests." As a consequence the book may now be banned in New Jersey.

Certainly a lack of redeeming social importance is irrelevant and no criterion on which to ban a book. If it were, the mystery addicts and science fiction fans would necessarily be deprived of the thousands of socially unimportant novels that they find so diverting. The law giving the State jurisdiction to censor our reading is based on the old puritanical notion that sex is a necessary evil, and any evident interest in sex is debauching, vile, downright prurient. However, as beauty is in the eyes of the beholder, so prurience is in the mind of the reader.

It is difficult to comprehend how any book could offend anyone, since one has the option to read it or not to read it. Unfortunately, Judge Pashman was doubly offended, because he felt compelled to read Fanny Hill through twice in order to form an opinion about it. That his opinion should determine what adults may or may not read is an affront to a free society.

To defend Fanny Hill as a work of literary art depicting the life and manners of 18th century London is sheer nonsense. The book is pornography, and the fact that it was written over 200 years ago indicates how difficult it is to obtain good pornography these days. This reader found it patently inoffensive, mildly erotic, and somewhat boring soft-core pornography. The author assiduously avoided the use of any of the four-letter words that abound in many books of "literary merit." It is not the quaint language of Fanny Hill which offends the puritanical mind, but the depiction of the sexual act as a pleasurable experience by a young woman. And herein lies the redeeming social importance of the book. If the onus of sin and the resulting burden of guilt were removed from sex there would be less frigidity and impotence and more happiness in marriage in this country today.

Arthur L. Babson
Morristown

Smokers Stink

Published in the *Morristown Daily Record* March 6, 1972

The current campaign to get smokers to kick the habit based on the health hazards of smoking appeals to the wrong sense of values. Despite the demonstrated fact that smoking can cause cancer of the lungs, larynx, esophagus and oral cavity, is the most important cause of chronic bronchitis and emphysema, significantly increases risk of death from coronary artery disease, and decreases life expectancy as much as eight years, smokers are impervious to these warnings, whether they come from the Surgeon General or the American Cancer Society. Americans could care less about health hazards. Witness the thousands of people who swarm over the ski slopes every weekend. On the other hand, Americans are extremely concerned about not being personally offensive. They spend millions on deodorants, mouthwashes, breath mints, perfumes, etc. They make a fetish out of cleanliness.

Smokers stink! Their hair stinks! Their clothes stink! They can stink up a whole room! Few odors can compete with the breath of a heavy smoker. Smoking causes yellow fingers and yellow teeth.

Smokers are dirty. They drip ashes and tobacco, burn holes, scatter matches and butts, foul the air, and generally make life unpleasant for non-smokers. I think the time is propitious for a test case by a non-smoker to insist on his civil right to breathe uncontaminated air in public places. In the meantime, I would appeal to smokers who are truly concerned with the quality of the environment to take up thumb sucking. It is equally satisfying, and it doesn't pollute the atmosphere.

Arthur L. Babson
Chester

Brave New Suburbia: The Genetic Lawn

Published in *The New York Times* July 13, 2000

The efforts of the Scotts Company to develop grasses that are genetically modified to withstand spraying with pesticides and herbicides, as well as grasses in designer colors, are very disturbing (front page, July 9). I would like to suggest an alternative.

I live on six acres shaded by trees. While the trees do not allow enough sunlight to support the kind of lawn that The Scotts Company envisions for all suburbanites, they encourage a luxurious growth of moss that carpets large areas of my yard.

The moss does not require any fertilizer or pesticides. It never needs mowing or watering. If it does dry up during a prolonged draught, it comes right back to life with the first rain. The only maintenance required is to pull up the occasional weed.

Genetic engineering holds great promise of important advances in medicine and agriculture. Orange grass is not one of them.

Arthur L. Babson
Chester, N.J.
July 10, 2000

No sympathy for smokers

Submitted to *The Star Ledger* July 20, 2000

The punitive damages awarded by the Florida jury to hundreds of thousands of nameless smokers is absurd. Everyone knows smoking is bad for you. Everyone knows it is addictive. Everyone knows the tobacco companies have lied about the dangers of smoking. If smokers have chosen to believe them rather than the surgeon general that's their problem. Smokers should be the last people to profit at the expense of the tobacco companies. They chose to smoke with full knowledge of the risks therein.

It would make more sense to distribute any punitive award to the class of nonsmokers who have for too long put up with the annoyance and stink of smokers, the established danger of second-hand smoke, and the added taxes and medical insurance premiums resulting from self-inflicted damage to smokers.

Arthur L. Babson
Chester

On Human Cloning

Submitted to The Observer Tribune January 17, 2003

In a letter to the editor published in *The Observer Tribune* January 9, Pete Skurkiss raised the question of whether or not human clones would have a soul. He argues against cloning because, without souls, clones could be "consciousless monsters....ultimate psychopaths, answering only to their own desires and lusts." We certainly have too many such people on the planet right now and, presumably, they all have souls. Skurkiss also seems to equate *in vitro* fertilization with "playing God."

There is no question that sooner or later someone will succeed in cloning a human, but only to show that it can be done. There is no reasonable incentive do this, and the risks far outweigh any benefits. Certainly cloning is not "a way to perfect man." Dolly is no improvement on the sheep she was cloned from. There are justifiable reasons to ban human cloning which should not be confused with stem cell research which has huge potential for human benefits. Whether or not embryos created for the latter have souls can only obfuscate serious discussion about this important new science.

Of course, human clones have been around for a long time. Identical twins are clones. The Orthodox belief is that a soul is created by God at the moment of conception. What happens if the fertilized egg divides, the resulting pair of cells become separated, and each results in a baby? Does each baby have half a soul? Do identical triplets each have a third of a soul?

Arthur L. Babson
Chester

Corps of Engineers

Published in *The New York Times* June 26, 2003

Your June 24 editorial "Time to Re-engineer the Corps," supporting independent oversight of the Army Corps of Engineers, did not go far enough. The Corps — "an incorrigibly spendthrift agency whose projects over the years have caused enormous damage to the nation's streams, rivers and wetlands," you call it — is an entrenched bureaucracy that has no current justification for its existence, if indeed it ever had.

The corps always thought big. Big dams, big drainage projects, big levees, big dredging and straightening of rivers, big jetties, big ways to improve on nature. The taxpayers will fork over $8 billion to try to fix the damage the corps has done to the Everglades. It's time to muster out the Army Corps of Engineers.

Arthur L. Babson
Chester, N.J.
June 25, 2003

Strange alchemy

Published in *The Star Ledger* July 9, 2005

Your July 5 report on the Group of Eight summit discussing President Bush's opposition to the Kyoto Protocol quotes him as saying, "I believe we'll be able to burn coal without emitting any greenhouse gasses." Coal is carbon. Burning carbon produces carbon dioxide, the major greenhouse gas. If Bush knows of some alchemy to obtain energy from carbon without producing carbon dioxide, I urge him to share it with the world.

Arthur L. Babson
Chester

Frightening Statistics

Submitted to *The New York Times* July 16, 2005

Two statements in Hassan M. Fattah's article on the July 16 front page about the anger of British Muslims strike me as very frightening: "The bombers are an exception among Britain's 1.6 million Muslims." "A recent poll commissioned by *The Guardian* found that 84 percent of Muslims surveyed were against the use of violence for political means..." This implies that 16 percent of Muslims in Britain, or over a quarter of a million people, condone this method of protest.

Arthur L. Babson
Chester, N.J.

Unintelligent Design

Submitted to *The New York Times* August 23, 2005

Proponents of so-called Intelligent Design like to cite the complex blood coagulation mechanism as evidence for an intelligent designer. Blood coagulation involves a sequential cascade of several proteolytic enzymes triggered by tissue damage and leading to a massive

release of thrombin at the site of injury. Thrombin converts soluble fibrinogen to an insoluble mesh of fibrin strands forming the foundation of the blood clot.

For over fifty years I have been designing *in vitro* medical diagnostic tests and devices for the clinical laboratory. The blood coagulation mechanism has provided me with an opportunity to develop a number of commercial products for the diagnosis of its many deficiencies. In my work I have adhered to the guiding principal that simpler is always better. The fewer variables in any test or device the more robust the final product.

No designer in his right mind would come up with such a convoluted and complex scheme that is so fraught with opportunities for failure. When you cut yourself shaving, the blood coagulation cascade does a good job of stopping the bleeding. When it goes awry it can be a vicious killer. Indeed, the blood coagulation cascade could only have arisen through the blind blundering of organic evolution.

Arthur L. Babson, Ph.D.
Chester, N.J.

Intelligent Design

Submitted to *The New York Times* September 20, 2005

A person could spend his entire life studying evolution and barely scratch the surface. But how does one study or teach the theory of intelligent design a.k.a. creationism? What is the curriculum? Where is the literature establishing the scientific basis for the theory? The answer, of course, is that intelligent design is not a theory. It is not science. It is a belief. Undoubtedly a large segment of the population, perhaps even a majority, believes in intelligent design, but that is beside the point. Perhaps the science teachers in Dover, Pa. should just acknowledge this fact and get on with the job of teaching science.

Arthur L. Babson
Chester, N.J.

Sell the gold in Fort Knox

Submitted to *The New York Times* October 25, 2005

Thank you for the front-page article calling attention to the appalling environmental destruction accompanying gold mining using cyanide heap leaching, a practice shamefully promoted in Third World countries by the World Bank ("Behind Gold's Glitter: Torn Lands and Pointed Questions" NY Times October 24). It is hard to believe that 100 tons of rock can be profitably processed to produce a single ounce of gold, a metal with few commercial uses.

By restricting the sale of gold the Federal Reserve is supporting its high price. It is costing the U.S. taxpayers money to store the 8,134 tons of gold in federal vaults. I suggest that we sell that gold on the open market. It wouldn't produce the current value of $122 billion, because the price of gold would drop severely from the sudden increase in supply. However, the environmental gains would be immeasurable from putting marginal gold mines out of business, which is where they belong.

Arthur L. Babson
Chester, N.J.

APPENDIX III: THE PEPPERONI THEORY (1973)

I sat there in the meeting gnawing on my two and one half inch stick of pepperoni and sucking on my coke trying to convince them that this was all part of a scientific experiment. You see, I've been studying the relationship between voiding velocity and urinary acid phosphatase concentration trying to prove a theory this urologist friend of mine has. It's an involved hydrodynamic thing relating Bernoulli's Principle to prostatic secretion that I won't go into here. I've been using myself as a subject for these studies, which is always a mistake. It's like taking Abnormal Psychology 201 during which you find out that you have all the symptoms of the various mental disorders that have been described and some that haven't. I'll never forget the time I was convinced that I had bladder cancer, because some new test I was working on became bright red with my urine sample. It turned out to be nothing more serious than an overdose of Vitamin C contracted during the previous breakfast.

At any rate I had been conducting these acid phosphatase studies for some time without demonstrating any startling effects until last Friday. Friday's morning samples were singularly lackluster, and I only repeated the series in the afternoon with the view of collecting one more set of negative data to put my friend's theory to rest. I should have been satisfied with the data I had. In the afternoon samples my acid phosphatase had increased 800%!

Now biologists are forever relying on statistics to prove that one set of data is really different from another set of data. This is the way that obscure interrelationships are discovered and scientific knowledge is advanced. When P=0.05 or less, this signifies that the probability that the observed differences between two sets of data occured by chance alone is only one in twenty, sufficiently small that the differences can be considered significant and therefore real. Of course, statistical theory teaches us that one experiment in twenty will by probability alone show a significant difference between two sets of data which is not real. It is not unreasonable to assume that 95% of the experiments done in biology which demonstrate nothing are considered failures by the experimenters and therefore not reported, and the 5% that are reported as significant are the ones that really are not.

But 800%! That is a different story. Eight hundred percent does not require the crutch of statistical theory to support what is obviously a real difference. But why?

I racked my brain to uncover the underlying cause. And then I remembered that before lunch on Friday I had partaken of several large pieces of pepperoni. Pepper is a known irritant to the gastrointestinal tract. Surely the pepperoni had stimulated the release of intestinal acid phosphatase into the circulation from whence it had made its way into my urine.

The test of any scientific theory is predictability. If in fact a theory is correct the results of an experiment designed to test that theory should be predictable. Thus the two and one half inches of pepperoni and coke. After the meeting I rushed back to the laboratory, and after a brief visit to the lavatory to collect appropriate samples, I was back at the bench to prove the pepperoni theory. The results were exactly the same as that morning.

Did the coke counteract the effects of the pepperoni? Was it something else I had eaten on Friday? I can't remember what else I had for lunch on Friday. Alas, I will probably never know what made Friday afternoon's samples so high. And this is the way it usually is with science. We carefully document and report obscure differences which are not real and neglect to report the real differences because we can't explain them.

Arthur L. Babson
1973

APPENDIX IV: DEAD AS A DODO (1994)

Tibbles the Cat

Tibbles the cat may very well be the only animal in the history of the world that singlehandedly brought about the extinction of an entire species. Extinctions have always been part of the ebb and flow of evolution. However, the pace of extinctions has been frighteningly accelerated since the arrival of man and his fellow travelers.

New species come about through isolation of a population, and nowhere is this isolation more complete than on islands. Most of the flightless birds that have evolved in various parts of the world have done so on islands devoid of natural predators where flight was an unnecessary luxury and a waste of energy. Some of the flightless birds that have disappeared in historical times have been huge. The largest of several moas of New Zealand stood twelve feet tall and weighed five hundred pounds. They are all gone. The moas were dwarfed by the elephant bird of Madagascar who weighed a thousand pounds. To the east of Madagascar on the island of Mauritius the fabled dodo was doomed by the arrival of man with his pigs and rats in the 16th century.

At the opposite end of the scale was the diminutive Stephens Island wren, the only flightless songbird in the world. Stephens Island is the outermost of a group of islands in Cook Strait between North and South Islands, New Zealand, and was the only home to the wren, a golden-brown sprite that emerged from its hole at dusk and scurried about in search of insects. Tibbles belonged to the lighthouse keeper. In 1984 he discovered the wren and for several months dutifully delivered specimens to his owner. One day he stopped. There were no more.

Martha, 1885-1914

Martha died at 1:00 PM, September 1, 1914 at the Cincinnati zoo. She was twenty nine years old and the last passenger pigeon in the world. Only a century earlier there were an estimated five billion passenger pigeons in North America. They were far more numerous than any other species of bird that ever existed, with a population that approached that of all other land birds in the United States combined. Audubon in 1813 described a single flock in Kentucky that he calculated contained over a billion birds. How could a population of such incredible proportion be decimated in a mere century, a blink of time on an evolutionary scale? It is difficult to believe. But the passenger pigeon was tasty and its habits of migrating and roosting in massive flocks and nesting in huge colonies made it easy prey to market hunters who slaughtered them and sold them by the barrel.

The last wild passenger pigeon was shot in 1900. Martha’s stuffed body can still be viewed at the National Museum in Washington, a poignant reminder of the consequences of unbridled greed.

William F. Cody, 1846-1917

The greatest concentration of large mammals the world has ever seen is not the several million wildebeest and zebra that annually migrate between the Serengeti and Masai Mara, the most spectacular display of large herbivores still extant. It was the incredible herds of American bison that roamed the Great Plains. Estimated at fifty million the bison represented an order of magnitude more biomass than the passenger pigeon. By the beginning of the 19th century their numbers had been reduced to an estimated thirty million and to about twenty million by 1870 when the country went on a killing spree. The bison, or buffalo as it is commonly known, was seen as a competitor to domestic cattle, The army saw the destruction of the great herds of buffalo as a means of finally subduing the recalcitrant Plains Indians who depended on the buffalo for their livelihood. In a short nineteen years the twenty million were reduced to a remnant of a thousand individuals scattered in zoos and in private hands. Buffalo Bill personally killed over 4,200 in eighteen months.

Unlike the passenger pigeon, however, the buffalo bred well in captivity, The Bronx Zoo had acquired one of the largest herds, and these animals became the founders of many other private herds across America. These and several free-ranging herds now number about 200,000 animals. The largest is in Yellowstone National Park, about 4,000 head or somewhat fewer than Buffalo Bill dispatched in a few months.

The return of the buffalo, albeit in a mockery of their former numbers, suggests that hopefully we can learn from our past mistakes and can live in harmony with the natural world and respect our fellow creatures as did the Plains Indians.

Arthur L. Babson
December 1994

APPENDIX V: THE GENESIS OF 'GENESIS' (1996)

"It ain't necessarily so." - Ira Gershwin

The book of Genesis describes the history of the world as revealed by God to Moses. However, Moses didn't write it down; paper wasn't invented yet. The stories in Genesis were orally transmitted for many generations until scribes ultimately recorded God's words, and these have been recopied, translated and reinterpreted ever since.

The first five books of the Old Testament starting with Genesis, also called the five books of Moses, are to this day hand written in ancient Hebrew on long scrolls which are the sacred Torah of Jewish synagogues. Many devout Jews and Christians accept the stories in Genesis as the absolute truth, divinely revealed. Being neither, I am less than convinced. In defense of those ancient scribes who actually wrote the book, it must be remembered that scientific knowledge of the world at the time was essentially nonexistent.

The unquestioning acceptance of the Bible by so many people in light of current scientific knowledge is difficult to understand. If religious beliefs were entirely innocuous it would be inconsequential. But they are not. Furthermore, I believe some Bible stories have been responsible for considerable mischief. The story of Noah and the Ark is a good example.

God decided that His main creation, man, had not turned out well at all. Men were misbehaving in every way imaginable. His solution was to drown everyone and start all over. This was an ill-conceived plan. He could have smitten everyone He was unhappy with - turned them into pillars of salt or whatever. He was good at that sort of thing. To avoid having to create everything from scratch He found one righteous man, Noah, who with his wife and three sons and their wives would be the seed stock to fulfill the function previously provided by Adam and Eve. Of course, He couldn't drown everyone in the world without simultaneously drowning all the animals in the world. It would seem a poor choice of means of retribution for man's transgressions to have to kill all the innocent animals at the same time. At the very least it is unseemly that the Creator would squander the genetic diversity built up over almost two thousand years. But, except as noted below, He smote them all.

God instructed Noah to collect a male and female representative of every species of terrestrial animal and stow them all, with sufficient food for a year's voyage, aboard an ark that God commanded Noah to build. The specified design was a three decked vessel 300 cubits long, 50 cubits wide and 30 cubits high. Simple arithmetic reveals that the ark would be 450,000 cubic cubits which is a lot of gopher-wood. And he probably didn't just have it delivered from the local lumber yard. We can only assume that Noah was an expert shipwright or at the least a pretty good carpenter. Of course, he already had 600 years to hone his skills. Certainly the ark was not the first floatable vessel ever built. When the waters of the deluge started to raise the sea level and flood their homes, why didn't the fishermen of the time just provision and climb into their own boats and ride out the storm? Of course, this would have foiled God's plan, but I mention it only as an additional illustration of how the plan was faulted.

As large as the ark was it had to be extremely crowded. While two of most species would suffice, God instructed Noah to load seven pairs of all birds and "clean" beasts. ("Clean" meant that it was OK for Noah to eat them. In addition, Noah needed an extra pair of all clean beasts to use as a burnt offering to God at the end of the flood). Besides all the birds, mammals, reptiles and amphibians, space would be required for all the terrestrial invertebrates including the millions of species of insects. It has been estimated that there are a million species of beetles alone. (The Creator had an inordinate fondness for beetles). Many of these animals have very specialized diets. Giant pandas eat only bamboo and koalas eat only eucalyptus, neither of which grew in Noah's neighborhood. Collecting food for all these animals would have been a task equal to the building of the ark. (Of course, this would not have been a problem with the dung beetles).

More problematic is the question of how all these animals found their way to the ark within the seven days God allowed Noah to collect them. Most of the birds could fly. (Not, however, the Stephens Island wren, a flightless bird confined to a small island in New Zealand that was exterminated in the late nineteenth century by Tibbles, the lighthouse keeper's cat. This may be the only time since Noah ate the last two unicorns that the extinction of an entire species was effected by the predation of single animal). It would have been a long swim for a jaguarundi from South America or a Tasmanian devil. Land snails from Florida and the Bahamas? Forget it!

It rained for forty days and forty nights, after which the entire world was inundated. "Fifteen cubits upward did the waters prevail; and the mountains were covered" (Genesis 7:20). Now either the law of gravity was suspended or the water ran down hill. In either case fifteen cubits is not enough to submerge most trees, no problem for squirrels and birds. I believe the implication is that the water did seek its own level and cover all the land, as Mount Ararat was the first landfall.[11] At almost 17,000 feet Ararat is a formidable mountain, taller by far than anything in the contiguous United States or Europe. But it is a mile short of the highest Andes and two miles short of many peaks in the Himalayas. Of course Noah and his contemporaries wouldn't have known about the Andes or Himalayas, but God should have. He made them. Where did all this water come from? Where did it all go when the deluge receded?

The ark was afloat for seven months before it finally came to rest on Mount Ararat, but Noah and his entourage didn't disembark for another five months. Prior to this a dove returned with an olive branch signaling the uncovering of dry land. How the olive tree or any land plants survived an inundation of a year is unclear. Noah and his family were instructed to go forth, multiply, and replenish the earth, a real challenge as the surface of the earth would have been totally devastated by the flood. God must have been contrite when He realized the extent of the damage, as He made a covenant with Noah. He promised not to do it again. To reassure Noah, God created the rainbow as a token of his sincerity. Big deal!

[11] A colleague who is a Jewish scholar believes the correct interpretation of this passage is that the highest mountains were covered by 15 cubits of water. This would require considerably more additional water than exists today in all the oceans of the world.

It is hard to believe that anyone really accepts this tale as factual. Yet people have seriously searched for the remains of the Ark on Mount Ararat. Why should it bother me that anyone accepts this fanciful story as true history? I am troubled because people who can abandon reason in the blind acceptance of authority, biblical or otherwise, can murder their children and follow leaders like David Karesch and Jim Jones into oblivion. Fundamentalists, be they Christian, Jew or Moslem, scare me. The true believer is a dangerous person. He can strap thirty pounds of plastic explosive and nails around his waist and detonate himself on a crowded bus in the full knowledge that he will be assured a place in heaven for his selfless and noble act. The most brutal (and Godless) conflicts in history have been when both opposing groups knew that God was on their side.

The religious right as exemplified by people like Pat Buchanan (an avowed creationist) is becoming a powerful political force in this country and a serious threat to freedom and enlightenment. Many school districts are required to provide equal time for teaching creation science (an oxymoron if there ever was one) and organic evolution as equally supportable theories of the history of life. Many teachers are so intimidated they choose not to teach evolution at all. The Scopes monkey trial could happen again. Can book burnings and witch hunts be far behind.

The creation stories in Genesis have wrought havoc for millennia. The inferior position of women throughout history is a result of God's having created Adam first and Eve almost as an afterthought and only from one of Adam's ribs. (I recall as a child being told that to this day men have one less rib than women). We are not even told the names of Noah's wife or daughters-in-law, despite their being the mothers of all people. I was appalled at a wedding I attended not long ago in a fundamentalist Baptist church when the minister continually referred to the bride as "the lesser vessel." "...and thy desire shall be to thy husband, and he shall rule over thee." - Genesis 3:16.

Snakes, which I happen to like, have gotten a bad rap since the devil disguised himself as a serpent in the Garden of Eden. I believe most people's irrational fear of snakes can be traced to this biblical tale. The inferior status of all animals and the justification for overpopulation is clearly set down. "And God blessed them [Adam and Eve], and God said unto them, Be fruitful, and multiply, and replenish the earth, and subdue it: and have dominion over the fish of the sea, and over the fowl of the air, and over every living thing that moveth upon the earth." - Genesis 1:28.

But back to the patriarch, Noah. After the flood, Noah became a grower of grapes, got into the sauce, and fell down not only drunk but naked. He was discovered in this state by his son, Ham, who summoned Shem and Japeth to salvage their father's honor. These two backed into the room to avoid seeing Noah's nakedness and covered him with a garment. Noah was so mad at Ham for seeing the old reprobate's embarrassing condition, he cursed Ham's son, Canaan, that he should become a servant to Shem and Japheth, a kind of retribution usually reserved to God himself. One has to wonder at the deplorable state of human affairs if this inebriated and vindictive man was the best God could find to father the human race. God works

in mysterious ways. "Such is the human race. Often it does seem such a pity that Noah and his party did not miss the boat." - Mark Twain

Arthur L. Babson
October, 1996

APPENDIX VI: CALCULATING THE CIRCUMFERENCE OF THE EARTH WHILE ENJOYING A RUM PUNCH ON THE DECK OF MY HOUSE IN VIRGIN GORDA (1999)

'The ship was cheer'd, the harbour clear'd,
Merrily did we drop
Below the kirk, below the hill,
Below the lighthouse top.

The Rime of the Ancient Mariner
Samuel Taylor Coleridge

Christopher Columbus named Virgin Gorda on his second voyage to the New World. It is said that the island's profile with Gorda peak a prominent feature reminded him of a reclining maiden. I think he had been at sea too long.

Conventional wisdom holds that Columbus was an intrepid explorer who risked life and limb by sailing west to get east, a maneuver that would only work if the world was round. If it were flat, as many presumably believed, he would sail off the edge and there would be no Columbus Day to celebrate. A flat earth required something underneath to support it: a tortoise, an elephant. They both need something to stand on. And so it goes, *ad infinitum.* A round earth, on the other hand, has an elegant solution to the problem of the bottom: halfway down and you're going up.

Of course, Columbus and every intelligent person of the time realized that the world was round. All the observable heavenly bodies were round. Long before Columbus, early Greek scholars knew the earth was round. In the fourth century B.C. Pythagoras taught that the earth rotates on its axis once a day. Aristotle (384-322 B.C.) cited physical evidence for a round earth: lunar eclipses always projected a round shadow of the earth on the moon.

Eratosthenes (276-196 B.C.) even calculated quite accurately the circumference of the earth, albeit his method of calculation was exceedingly crude. Eratosthenes, who was chief of the library at Alexandria, heard of a well in Syene (modern Aswan) where the sun's reflection could be seen at noon on June 21. He surmised that the sun was directly overhead at that moment. He believed that Syene was due south of Alexandria. He measured the length of the shadow cast by an obelisk in Alexandria at the moment there was no shadow in Syene and by knowing the height of the obelisk he computed the angle of the sun to be $7^{o}\,12'$ or about one fiftieth of a circle. To calculate the distance from Alexandria to Syene he learned that it took camels 50 days to make the trip. Camels traveled about 10 miles a day making the distance between the two cities 500 miles and the circumference of the earth fifty times that or 25,000 miles.

A century later Posidonius (130-51 B.C.) used a similar approach. He measured the elevation of the star Canopus at two points and calculated the distance between the points from the sailing time and speed of ships instead of camels. Posidonius reached a similar conclusion of

about 25,000 miles for the circumference of the earth. For obscure reasons Strabo (64 B.C.-A.D. 20) reduced this figure to 18,000 miles. Ptolemy (100-170) supported Strabo's value, and because of Ptolemy's stature this distance would go unchallenged for fifteen centuries. The smaller circumference, of course, made the Orient seem far closer to Europe than it actually is. Columbus used Ptolemy's figure to argue his case before the king and queen of Spain, and it also gave him the confidence to attempt a western sailing to India. Almost thirty years later Magellan also believed in a smaller world. Had he any conception of the vastness of the Pacific Ocean he might have reconsidered his attempt at a circumnavigation.

Despite the fact that over two thousand years previously, Aristarchos of Samos maintained that the earth not only turned on its axis but also described a yearly journey around the sun, causing the cycling of the seasons, Aristotle and all those following him pronounced an earth-centered universe. A contemporary of Columbus, Nicolaus Copernicus (1473-1543), published in 1543 his theory of a sun-centered solar system. Written much earlier, the book was withheld by Copernicus for fear of reprisals from the Roman Catholic Church. His fears were validated a century later when Galileo (1564-1642), despite convincing scientific evidence supporting the Copernican view, was forced by the Inquisition in 1633 to publicly renounce this doctrine. Galileo was imprisoned in house arrest for the remainder of his life.

The enormous size of the earth makes it difficult for an ordinary observer on the ground to see the curvature implicit in a round geometry. Not so with mariners like Columbus. They could see objects beyond the horizon but only the tops of these objects. Columbus' lookout was perched in the crow's nest on top of the mast not so he could see better but because he could see farther. The lighthouse was built not by the kirk but at the top of the hill where the Ancient Mariner could see it from a greater distance.

Anyone who has sailed to Anegada knows firsthand the benefit of height in observing distant lands. Unlike the rest of the British Virgin Islands that are all mountainous, Anegada is a flat coral island that rises only a few feet above sea level. You can't see it until you are almost on top of it, accounting for the numerous wrecks that litter the surrounding reefs.

From the deck of my house I can look down Sir Francis Drake Channel and see most of the British Virgin Islands. The name of this major seaway, originally known as Freebooters Gangway, was changed to honor the famous British explorer, slaver, and pirate, nicknamed the Dragon by his enemies, who used it frequently to prey on Spanish and Portuguese ships. In November, 1595, Drake and his fleet of 27 ships moored in North Sound on Virgin Gorda for a week in preparation for a successful attack on a fleet of Spanish treasure ships laid up in St. John harbor for repairs.

Tortola, the largest of the British Virgin Islands, has been called the sailing capital of the world for the number of bare-bones and captained yachts it offers. If, in fact, the world were flat, from my vantage point, about 80 feet above sea level, a ship would have to be over 80 feet tall for its mast to intersect the horizon at any viewing distance. In the near distance I can look down on boats of all sizes. However, several miles away, even the smallest boats appear partly

above the horizon. This observation, which could hardly be unique, is only consistent with a curved ocean surface.

The farthest island I can see is St. John in the U.S. Virgin Islands about 20 miles distant. The southeastern tip of St. John appears from my vantage point to be two small islands, the intervening low-lying land being well below the horizon. On the other hand, Collision Point on Virgin Gorda and the Dogs, all about 3 miles distant, are clearly nearer than the horizon as I can see open ocean beyond them. The shoreline of Salt Island, about 11 miles away, appears to line up precisely with the horizon. Thus my sight line can be considered tangent to the earth's surface at 11 miles. A line drawn from the horizon and perpendicular to the line between my viewing point and the center of the earth would intersect the latter at a point about 80 feet below the shoreline making a right triangle, the small side of which would be 160 feet or about 0.03 miles, and the hypotenuse 11 miles. A similar right triangle (equal angles) can be described where the smaller side is 11 miles and the hypotenuse is the radius of the earth. The ratio of the two similar sides is equal according to the following equation:

$$\text{Earth Radius}/11 \text{ miles} = 11 \text{ miles}/0.03 \text{ miles}$$

Solving this equation gives a radius of the earth of about 4,000 miles and a circumference of 25,000 miles.

"Susan, do I have time for another rum punch before dinner?"

Arthur L. Babson
Turtle Bay Villa, Virgin Gorda
March 1999

Note added September 2002 – A panel of physicists named Eratosthenes' calculation of the circumference of the earth one of the ten most beautiful experiments in the history of science. The ten experiments were listed in the September 2002 issue of *Physics World*.

APPENDIX VII: THE BRAVE NEW WORLD OF GUNS FOR EVERYONE (2000)

"First-Grader Shoots Classmate to Death"
Headline, *Newark Star Ledger* March 1, 2000

"Guns don't kill people. People kill people."
National Rifle Association Mantra

In 1998 two boys, 11 and 13, opened fire at a middle school in Jonesboro, Arkansas, killing four girls and a teacher. From West Paducah, Kentucky, to Springfield, Oregon, to Littleton, Colorado, school children are killing other school children with guns. The Flint, Michigan, grade school shooting was unusual only in the ages of the shooter and the victim, both six years old. The *Star Ledger* article went on to say, 'Although the school, which has 450 students from kindergarten through grade four, has security guards, it does not have metal detectors." You can bet that metal detectors will be installed in this Flint, Michigan, grade school, because our society will go to whatever lengths that are required and will spare no expense to ensure the safety of our children.

It wasn't always this way.

When I was a child all the boys had cap pistols, and we shot each other with gay abandon. You can't buy cap pistols today. They are too dangerous. You can't buy, at least in New Jersey, firecrackers and other fireworks that made the Fourth of July one of my favorite holidays, a close second after Christmas. Today if you want to see fireworks you have to battle crowds of people at a town sponsored display.

We not only shot off our own fireworks on the Fourth, we made our own explosives during the rest of the year. The ingredients for black gunpowder, sodium nitrate, charcoal and sulfur, were readily available at the local pharmacy. My favorite bomb was made from a spent CO_2 cartridge used for making home-brewed seltzer water, which I filled with black powder and detonated electrically. Another favorite was made with an empty .22 caliber long rifle cartridge casing which I filled with black powder plus the head of a Blue Tip match and crimped over. These we would place on the trolley tracks on Bloomfield Avenue where the approaching trolley would crush and explode them.

If school children today run the risk of being shot at school they at least arrive at school in the relative safety of those giant, yellow behemoths that are the bane of every motorist. Recently I was driving down South Road in the middle of the day when I noticed a school bus parked about a quarter of a mile away with its yellow lights flashing. I approached cautiously and was about to pass it when all of a sudden the yellow lights turned to red, the large, octagonal stop sign jumped out of the side of the bus, and a long rail swung out from the front to dissuade anyone from foolishly trying to cross the street in front of the bus. I jammed on my brakes just as a man came racing out of the adjacent house and boarded the bus. After what seemed an

interminable length of time he emerged carrying a small child and what appeared to be a car seat.

When I was in grammar school we walked the mile or so to school without benefit of sidewalks. Later I rode the bus to Montclair to attend school. Otto's bus company owned one bus and Otto was the driver. It didn't have any flashing lights or stop signs. Otto made no attempt to control the daily rough-housing that went on in the back of the bus. The bus was in less than tip-top condition. I recall one morning racing down Clairmont Avenue in Montclair totally out of control. When the bus finally stopped and we all got off we saw the rear wheel sticking out about a foot. The axle had broken. We walked the rest of the way to school.

Small children are not allowed to ride in the front seat of a car. They must be strapped into contraptions reminiscent of electric chairs which themselves are strapped into the back seat of the car. How children survive long trips under such inhumane conditions is a mystery. When I was a small boy the chauffeur of a friend's father would let me sit on his lap and drive the car. Of course, I wasn't actually driving, as I couldn't reach the pedals. I couldn't even see out of the windshield without the benefit of the added elevation provided by his lap, but I steered the car and did it quite well. When I got somewhat older the great thrill was to ride standing on the running board. Half a dozen of us would pile on grabbing anything available to hold onto. It was great fun.

Our usual mode of transportation was a bicycle which we rode everywhere. Kids today also ride bikes but they better not get caught riding without a helmet. Bike helmets hadn't been invented when I was a kid. A few years ago a child was killed on a bike on Route 24 in Mendham Township. The Town Fathers' reaction to this tragedy was to require construction of a bike path through the woods and everyone's front yard parallel to my street, Old Mill Road. Never mind that the path would go from nowhere to nowhere, since both ends of Old Mill Road were in Chester Township where there wasn't a strident call for bike paths. Our neighbors, in an unusual display of unanimity, rose up and defeated the proposal. (In 29 years I have never seen a child riding a bike on Old Mill Road.)

We have more than enough laws on the books to protect children from imagined hazards. You cannot discard a refrigerator without physically removing the door to prevent a child from crawling inside and shutting the door behind him. (Did this happen once or was it twice in the history of refrigerators?) Of course, today's refrigerators have magnetic gaskets instead of positive latches. Any kid worth his salt could push the door open and crawl out. Have you seen the ugly deck railings that are now standard in New Jersey? There can be no more than 4 inches between uprights to prevent a (very) small child from squeezing between them. Of course, all swimming pools must be fenced in. You cannot buy toys that have pieces small enough for a child to swallow or possibly choke on. I would like to take a couple of aspirin for the arthritis in my hand, but I can't remove the child-proof closure. The list goes on.

We live in a paradoxical society where children can shoot each other with guns but cannot play with Lincoln Logs or many other educational toys. All because a bunch of right wing nuts led by the NRA believe that the Second Amendment of the Constitution would be in

jeopardy if any restriction on gun ownership is under consideration. That venerable document states as follows: “A well regulated militia, being essential to the preservation of a free society, the right of the people to own and keep arms shall not be abridged.” You never hear the NRA refer to the basis of this right: to provide for a “well regulated militia.” Any nimrod should be allowed to own any number of weapons including semi-automated assault rifles.

It wasn’t long after a new neighbor moved in next door to us that he started disturbing the peace of our quiet neighborhood by shooting off all kinds of guns at what appeared to be random targets. My wife, Susan, called the police and was told he shouldn’t discharge a firearm within 450 feet of a house. I determined that a 450 radius from our house encompassed his entire property. So, on a quiet Sunday after he unleashed a fusillade of shots we called the cops again. The officer who responded informed us that the first cop was in error. The 450 foot rule only took effect during hunting season when at least there is some justification for firing a gun, but only shot guns or muzzle-loaders with limited range. The rest of the year there is no restriction on discharging any kind of firearm on your own property. God bless the Second Amendment!

Last night I heard on the news that Wayne LaPierre, executive vice president of the National Rifle Association, said that President Clinton, “…needs a certain level of violence in this country. He’s willing to accept a certain level of killing to further his political agenda.”[Tough gun control legislation]. This is indeed a brave new world.

Arthur L. Babson
March 14, 2000

APPENDIX VIII: HISTORY OF DPC INSTRUMENT SYSTEMS DIVISION

Remarks of Arthur L. Babson at the Dedication of the new
Facility in Flanders, NJ on May 24, 2002

In the mid 1980's I had a good idea: to separate solid and liquid phases in an immunoassay by rapidly spinning a specially designed tube on its vertical axis. It was a good idea because it would enable the automation of these assays. I wrote up and submitted a patent application with the intention of licensing or selling the patent rights. At the time I was helping a colleague, John Underwood, with a number of consulting projects. I showed my invention to John and asked him who he thought would be a good customer for it. John immediately saw the value in the technology and said, "Art, you don't want to sell this. You want to start a company." I didn't want to start another company. I already had Babson Research Laboratories that was doing very well. Fortunately, John was persistent.

During a consultant visit to London Diagnostics, a start-up company in Minnesota, he introduced me to Art Kydd, an early stage venture capitalist who had invested in London. On a subsequent visit in early 1987 I had dinner with Art and a few of his wealthy investors in a restaurant in Minneapolis. On the restaurant table I demonstrated the principal of separation by axial centrifugation using a battery powered wooden model of a tube spinner and hand fabricated tubes. On the basis of this demonstration and further investigation of my background, Art and his friends put up the seed capital and Babson Technologies was incorporated on March 18, 1987 with myself as President, Chairman, CEO, and Treasurer and John and Art as Board Members.

We changed the name to Pegasus Technologies on June 19, 1987, because I wanted the employees to think they were working for their company, not mine. Soon thereafter the company with one employee, me, moved into one second floor room of the unused Williamson School building in Chester, New Jersey. I wasn't entirely alone. My wife, Susan, set up a studio in a corner of the room to practice her artwork. There was plenty of room. Susan wasn't a freeloader but a big help in getting me started. She set up the initial accounting system and painted the whole room. Of course, she painted it all different faux marbles, a technique she was perfecting.

Finding my first employee proved to be difficult. I had a number of headhunters looking for a mechanical engineer, as every instrument system starts with mechanical design. I rejected all of the candidates until I found Tom Palmieri. Tom was reluctant, because I only had two second-hand desks and a homemade drafting table. He finally decided to join the venture and became my first employee on March 15, 1988. As most of you know, Tom is still directing the mechanical engineering department and doing a great job.

Doug Olson joined us on April 18 as head of reagent development, Arthur Ross, an electrical engineer, started May 9, and we were off and running. We contracted with Vic Huebner to do the software.

With venture capital funding every advance of money is tied to the successful completion of milestones, and we had a milestone to have a functioning breadboard system by the end of the year. We were only seven employees, but on December 31, 1988, Doug was able to produce an acceptable dose response curve for digoxin. The breadboard proved the validity of the approach and pointed the way to a more robust design.

By September, 1988, we had also started development of an automated chemistry analyzer that could do total, HDL and LDL cholesterol and triglycerides using modified IMMULITE assay tubes and axial centrifugation to do the HDL separation. We called the system the Cardiac Risk Profiler. The rationale for this project was to bring in through a corporate partner some much needed cash flow to support the larger immunoassay project. Cholesterol, particularly so-called "good" and "bad" cholesterol, HDL and LDL respectively, was all the rage, and it seemed the timing couldn't have been better.

On February 10, 1989, we visited Becton Dickinson and demonstrated our prototype batch analyzer that could produce twenty profiles from twenty serum samples in thirty minutes. They were impressed, but they convinced us that the market for lipid profiling was the physician office laboratory. They didn't want an instrument that produced twenty profiles in thirty minutes. They wanted an instrument that did one profile in twelve minutes, the average time the patient was with the doctor, and used whole blood rather than plasma or serum.

Three and a half months later we returned to Becton Dickinson with a working breadboard that did just what they wanted. The operator transferred about 0.2mL of whole blood to one chamber of a small, reagent filled cuvette array, put the disposable device in the instrument, closed the door, and pressed the start button. Twelve minutes later the entire lipid profile for the sample was printed out along with an interpretation of the data. All steps in the analysis including the centrifugal separation of the blood cells and the HDL were performed automatically. Results were calculated from simultaneous assays of cholesterol and triglyceride standards packaged in the disposable. The instrument never needed calibration and could be run by a high school dropout, even a physician. We built three engineering prototypes, one of which is in my office if anyone would like to see it.

The manufacturing cost proved to be too high for B-D's method of distribution, but other companies were interested. Unipath in California was on the verge of closing a deal when the CLIA '88 (Clinical Laboratory Improvement Act) regulations were finally promulgated. These proposed regulations were so onerous they threatened to put physicians out of the laboratory business. Unipath did an about face, and we were never able to find another partner.

In early 1989, it became apparent that we would require serious funding. Art Kydd's fund was almost exhausted, so we started looking for additional venture capital. Every time we would be visited by venture capitalists we had to send the men downstairs to the Nifty Thrifty Shop to buy a tie for a quarter. Venture capitalists like to present themselves as risk takers. However, they like to share the risk. The lead investor was supposed to have completed its due diligence by April, but the deal was delayed by several months while we searched for additional investors and watched our committed capital disappear. By the time we had a final contract we

were out of funds and the venture firms were able to dictate the terms. One was to replace me as CEO. In March 1990 we brought in Bob Fennel as president. Bob handled the business, and I handled the science, which was fine with me.

In May 1990 we changed the name of the company again, because another diagnostic company had used the name Pegasus. We had a contest where all the employees submitted about 75 proposed names. One was IMMULITE, which we decided to reserve for the instrument. Another was Ceres. That evolved to Cirrus, which wasn't even on the list, and after our lawyers approved it we became Cirrus Diagnostics, Inc.

In the meanwhile the immunoassay instrument development proceeded at an encouraging pace. In July 1990 we introduced IMMULITE and Cirrus at the International Congress of Clinical Chemistry in San Francisco. We lucked out with an incredible location: a ten-foot island at the intersection of the two main isles in Muscone Center flanked by Abbott and Technicon. This was a real coup, because no one had even heard of Cirrus. The instrument we showed was one of three engineering prototypes that wasn't even fully functional. It did have a very expensive custom cover which made it look finished.

The following year at the AACC meeting we showed one of the twelve production prototypes we had assembled in the schoolhouse. By this time we had taken over the entire second floor of the school and part of the first floor. There was no elevator in the building and everything had to be carried up and down the stairs. The IMMULITE weighed close to 200 pounds. The biggest challenge was a 400 pound refrigerated centrifuge that we hauled up a plywood ramp with a block and tackle. Tom was pushing, and if the rope had broken he would have been history.

During this time we were actively seeking corporate partners. Unfortunately, many of the logical partners already had their own instrument development programs underway. I had contacted Sigi Ziering in March 1991 to see if DPC would be interested in joint reagent development for IMMULITE. He declined saying DPC was working on its own automated system, Millenia. We contacted Sigi later in the year, and this time he showed interest in Cirrus as a potential acquisition. He and Marilyn visited us on October 28, 1991. On January 20, 1992 Howie Wilson, Samantha DePriest and Alice Bragg visited us. Alice brought several hundred characterized serum samples with her and spent two days running about 1000 assays on IMMULITE while Samantha visited our beta sites and Howie inspected our reagent manufacturing facility in Roxbury. Alice's results were very good, and DPC signed a letter of intent to acquire Cirrus on March 10. On April 13 Sigi and Sid Aroesty accompanied by Nico Arnold from The Netherlands, Andrew Moore from the UK, Willi Tillmann and Peter Pusch from Germany and Harald Skafte from Sweden descended on us. The international folks all gave their enthusiastic approval. On May 28, 1992, we were acquired by DPC for $28 million, and we became DPC Cirrus, Inc.

Sid took over as president of both DPC and DPC Cirrus. In December, 1992, thirty-seven of us moved out of the schoolhouse and into brand new rented facilities in Randolph, New Jersey. Each year we expanded the space occupied in the Randolph facility. We started

development of IMMULITE 2000 in 1993. Doug took over as president in October, 1994, when we were 82 employees. IMMULITE 2000 was introduced in 1997. Last December we moved into this beautiful building and changed our name once again to DPC Instrument Systems Division. We currently have 377 employees; over ten times the number when we moved out of the schoolhouse. Out of little acorns big oak trees grow.

In the ten years we have been part of DPC, sales of the company have almost tripled. More significantly, the IMMULITE products now account for over 80% of those sales. It is an incredible success story for which we can all be justifiably proud.

APPENDIX IX: LAWYERS LOVE LIABILITY LAWSUITS (2002)

In the United States when something bad happens to somebody, it is somebody else's fault, preferably somebody rich. We live in the most litigious society in the world, with more personal injury lawyers per capita than any other country. These lawyers are most attracted to product liability cases because the defendants are usually rich, the juries overly generous, particularly with punitive damages, and the lawyer gets one third of any judgment. The record punitive-damage award in a single case until this year was $4.8 billion against General Motors for a car fire that badly burned six people. The award was subsequently reduced by a California judge to a mere $1.1 billion. In October 2002 a Los Angeles jury awarded $28 billion to a woman who accused Phillip Morris Inc, of luring her into a lifelong smoking habit with fraudulent advertising and marketing

The favorite cases of personal liability lawyers are class-action suits representing thousands of clients where the payoff can be incredible. There was an attempt, known as the Castano litigation, to sue the tobacco companies on behalf of all the smokers in the United States. Federal courts refused to hear it as a class-action. In a successful class-action lawsuit in April, 2000 a Florida jury awarded three plaintiffs representing an estimated half million sick Florida smokers a total of $12.7 million compensatory damages. The same six-person jury subsequently awarded the entire class punitive damages of $145 billion. And this was after the $246 billion settlement the industry had made with several states' Attorneys General to recover costs spent by the states in treating smoking-related ills. Stanley M. Rosenblatt, the Florida lawyer representing the class, had previously won a class action brought against tobacco companies by flight attendants made ill by second-hand smoke. Rosenblatt collected $49 million in fees for this case while the flight attendants are still waiting to collect a cent. Commenting on the Florida tobacco case, Robert Scheer, a columnist for the *Los Angeles Times*, wrote, "In this case the lead attorneys stand to make billions; even the losing lawyers will end up filthy rich and the class-action plaintiffs will be dead or too sick to get much solace from the loot. The real message is to go to law school, because it confirms once again that lawyers are the main beneficiaries of human misery."

Of course, the real message sent by the Florida and California juries is to punish the tobacco companies for selling defective products and covering up the fact. However, the punitive damages awarded to thousands of nameless smokers is absurd. Everyone knows smoking is bad for you. Everyone knows it is addictive. Everyone knows the tobacco companies have lied about the dangers of smoking. If smokers have chosen to believe them rather than the surgeon general, that's their problem. Smokers should be the last group to profit at the expense of the tobacco companies. They chose to smoke with full knowledge of the risks therein. It would make more sense to distribute any punitive award to the class of nonsmokers who have for too long had to put up with the annoyance and stink of smokers, the established danger of second-hand smoke, and the additional taxes and medical insurance premiums resulting from self-inflicted damage to smokers.

Tragedy can often bring out the best in people, but it can also bring out the greed. The firemen who rushed into the Twin Towers of the World Trade Center after the horrific terrorist attack on September 11, 2001, are true heroes. The families of the victims are not. Despite the promise of about $1.5 million per family from the federal Victim Compensation Fund, at least 950 relatives of victims of the attack have filed lawsuits against the Port Authority of New York and New Jersey, the owner of the Twin Towers.

A tragic fire in a Seton Hall dormitory on January 19, 2000, resulted in three deaths and half a dozen serious injuries. Within 90 days there were twenty notices of intent to sue with individual claims ranging from $10 million to $50 million. Potential named defendants besides the college and its president included the Archdiocese of Newark, the Village of South Orange, Essex County, and the state of New Jersey. Willie Sutton wasn't the only one to go where the money was.

Refreshingly, some liability lawsuits are unsuccessful. On August 24, 2000, a jury in Elizabeth, New Jersey, threw out the claims of a woman who was involved in an accident three years previously, that gases escaping from a deployed air bag had injured her vocal cords. In the accident on Route 46 in West Patterson one car crossed the median and struck Edith Krauss' Mercedes head on. A second vehicle rear-ended her car and subsequently crashed into the driver's side where she was sitting. Krauss and her husband walked away from the mangled heap that had once been her $30,000 car. Instead of thanking Mercedes-Benz for providing the safety features that certainly saved her life, Krauss sued the company for a defective air bag claiming inhalation of sodium azide used to inflate the bag had burned her vocal cords.

While the jury found that Krauss' vocal cord injuries were not caused by chemicals from the air bag and the device was not defective, they did conclude that Mercedes-Benz should have provided warnings about the possible hazards of air bags. While air bags have saved hundreds of thousands of lives since they were first installed in 1970, perhaps a warning such as the following might make car owners more comfortable.

> "WARNING!! This car is equipped with air bags on both the driver's and passenger's side, each of which contains a small bomb. When a collision occurs a sensor activates the air bag's inflation system that involves instantaneously reacting sodium azide with potassium nitrate to cause an explosion. Hot nitrogen gas causes the bag to inflate at a speed of 200mph. Sodium azide is very toxic and may be fatal if inhaled, swallowed or absorbed through skin."

Manufacturers are so wary of frivolous product liability lawsuits they often go to ridiculous extremes in cautioning users. A plastic bottle of seltzer water has the following prominently displayed: "WARNING! Contents under pressure. Cap may blow off causing eye or other serious injury. Point away from face and people, especially while opening." How the cap can blow off when restrained by screw threads is a mystery. Some time ago my wife bought me a tool that contained the following "SAFETY RULES". Can you guess what it is?

WARNING:
FAILURE TO FOLLOW THESE RULES MAY RESULT IN SERIOUS PERSONAL INJURY

GENERAL SAFETY RULES. Operate [the tool] under good visibility and daylight conditions only. Work calmly and concentrate on your job. Always hold [the tool] firmly with both hands.

DON'T USE IN A DANGEROUS ENVIRONMENT. Don't use the tool on damp or wet decks, or in rain, sleet or snow. Under these conditions the operator may slip and fall, and injury may result.

KEEP CHILDREN AND VISITORS AWAY. All children and visitors should be kept a safe distance from the work area. [The tool] is not a toy; do not allow children to play with it.

DON'T OVERREACH. Keep proper footing and balance at all times.

DON'T FORCE TOOL. It will do the job better and be safer if excessive force is not used during operation. Do not pull tool towards the operator's face.

KEEP AWAY FROM GLASS. Do not allow any part of tool to contact any glass surface. Use extreme care to keep tool from contact with windows, patio doors, French doors, or other glass surfaces. Contact with a glass surface may result in glass breakage and the danger of broken and/or falling glass; serious injury may result.

KEEP AWAY FROM ELECTRICITY. Do not allow the tool to contact and electrical wires or devices; serious electric shock and personal injury may result.

DRESS PROPERLY. Do not wear loose clothing or jewelry. Non-skid footwear is recommended.

DRUGS, ALCOHOL, MEDICATION. Do not operate the tool while under the influence of drugs, alcohol, or any medication.

No, it wasn't a chain saw or even a weed whacker. It was a pole with a blunt, rounded prong at one end to remove debris from between the boards of wooden decks. If I had read the warnings before unwrapping the tool I might have sent it back. However, I doubt if the Clear The Deck Tool Company has suffered too many product liability lawsuits.

Arthur L. Babson
October 11, 2002

APPENDIX X: INTELLIGENT DESIGNER UNMASKED (2006)

In what can only be described as a eureka moment, I became aware of the identity of the Intelligent Designer. (The original eureka moment, you will recall, is reputed to the Greek mathematician, Archimedes. Hiero challenged him to devise a means of discerning if an artisan Hiero had commissioned to build a crown of gold had cheated by blending in a quantity of much less expensive silver. Archimedes realized while floating in his bathtub that he could make the distinction by weighing the crown both in air and while submerged in water. He jumped out of the tub and went running naked through the streets of Syracuse shouting, "Eureka, eureka!" (Of course, running naked in public was not all unusual at that time.)

Unlike their forerunner Creationists, who unabashedly identify God as the Creator and thereby made it a simple matter for the courts to rule against any equal time for Creationism with Darwinian evolution in school science curricula, Intelligent Design advocates disclaim any specific designer thereby hopefully avoiding any religious connotation that would preclude their "theory" being taught as an alternative to evolution. So far, the courts, at least in Dover, Pennsylvania, have not been persuaded.

The basis for Intelligent Design is the observation that all living things, especially humans, are exceedingly complex, so much so that they couldn't possibly arisen by chance within any conceivable time frame and certainly not within the 6000 or so years the ID-ers believe encompass the history of the world. A complex design such as a watch found on a beach (a favorite ID example) implies a designer. (Actually, a watch is not a very complex mechanism consisting only of a two-part escapement, essentially a governor, a few gears and a coil spring.) Another favorite example of the Intelligent Designers is the blood coagulation mechanism, a complex cascade of mostly proteolytic enzymes triggered by the release of tissue thromboplastin at the site of injury, each enzyme activating a subsequent enzyme and finally resulting in the conversion of prothrombin to thrombin, the enzyme that converts soluble fibrinogen to insoluble fibrin strands, the basis of the blood clot. This is the mechanism that keeps us from bleeding to death when we cut ourselves shaving. It is, indeed, a complex scheme that can go awry in any number of ways to the detriment of the individual. Hemophiliacs, for example, lack a single factor in the scheme. On the other hand, ask any stroke victim if they think the blood coagulation cascade is a good invention.

The problem with ID-ers is that they equate complexity with superior design while exactly the opposite is true. I have been involved with the design of instrumentation all of my life and have learned that simpler is always better. No designer in his right mind would come up with the blood coagulation cascade.

Which brings me back to my opening statement about the identity of the Intelligent Designer: It is Rube Goldberg (Ruben Lucius Goldberg 1883-1970), the Pulitzer Prize-winning cartoonist who immortalized devices that were exceedingly complex and performed very simple tasks in a very indirect and convoluted way. A Rube Goldberg machine has been defined in *Webster's Third New International Dictionary* as, "accomplishing by extremely complex

roundabout means what actually or seemingly could be done simply." Certainly, blood coagulation epitomizes a Rube Goldberg machine.

It has not escaped my observation that another entity might be encrypted in the name, "Goldberg", however the possible significance of this does escape me.

Arthur L. Babson
March 3, 2006

APPENDIX XI: REMEMBRANCES (1995, UPDATED IN 2007)

FOR BETSY AND JIM

The Babson brothers liked to write. Uncle Stan has published three books, "Diversions in a. Busy Life," "Where Sands are Pink," and "Bonefishing." Uncle Art has two, "Birds of Princeton" and "Modern Wilderness." Uncle Sid has "Tahiti Holiday." These books in my library are wonderful reminders of the men who produced them. Uncle Cliff didn't write, but I have two of his exquisite watercolors depicting wildlife in Hatfield Swamp to remember him by. From my father not a single word and very few photographs. And I am the poorer for it.

I have tremendous admiration for anyone who writes a book. It seems like a prodigious task. I could never do it, although I had aspirations at one time. I started to write an account of Susan and my building our house and during our fish collecting days we talked about writing the definitive work on fresh water non-game fish of the Northeastern United States. The first project fizzled out after a few chapters and the second never got started. Which is not to say that I have written nothing of merit. My technical publications number about sixty five, and taken together equal the volume of a modest book. But they were written over a period of thirty eight years. I have bound most of them together and send them to you with these memoirs. I don't expect you to read them, but I thought you might like to have them. While I could never write a book I do enjoy writing and have also enclosed a number of unpublished essays that I hope you will enjoy reading. Several I never finished, but perhaps someday I might.

I was destined to become a scientist. I can't imagine any intellectual endeavor more stimulating or satisfying than doing science. A lot of people would find experimental science frustrating, as most of the experiments one does don't come out right, i.e., the way we expect them. Of course, if we knew in advance the results of an experiment it would be pointless to do the experiment at all. Someone once said that when you do an experiment you are asking a question of Nature and Nature always gives you the right answer. But often we don't know the question that we asked. Deciphering the question is the thrill and satisfaction of experimental science.

Of course, the biggest thrill comes at rare moments, if at all, when one has a profound insight that no one ever had before that can alter the way-future science is done. My own field of biochemistry provides many examples: Bruce Merrifield who invented solid-phase peptide synthesis that revolutionized polypeptide chemistry; Rosalyn Yallow who with Solomon Berson developed the technique of radioimmunoassay that, by increasing the sensitivity of analyses several orders of magnitude, revolutionized clinical chemistry and changed my life; James Watson who with Frances Crick divined the structure of DNA and created the sciences of molecular biology and genetic engineering. I mention these three Nobel laureates because I have had the privilege of meeting them all. (Jim Watson is a Director of my company, DPC). My own contributions have been considerably less momentous but no less thrilling. The seemingly simple concept, for example, of separating a solid from a liquid phase within a vessel, a necessary step in most immunoassays, by rapidly spinning the vessel on its

longitudinal axis has spawned a multimillion dollar company that now employs a hundred people in New Jersey and probably many more than that in California and around the world.

While science is my livelihood and intellectual sustenance nature is my life and spiritual salvation, I am above all a naturalist. I can't imaging living in a city surrounded by hoards of people and noise. As a child I absorbed the animal stories in the books by Ernest Seaton Thompson. My heros were Roy Chapman Andrews who discovered dinosaur eggs in the Gobi Desert, Carl Akeley who collected African mammals for the Museum of Natural History in New York, and especially Martin Johnson, the first cinematographer to record the wild animals of Africa. I longed to be like Martin Johnson. I made Doris read Osa Johnson's book, "I Married Adventure" and it almost scared her out of marrying me. Little did I realize in those days how completely my dreams of Africa would eventually be fulfilled.

So I write these memoirs for you so that you will have something to remember me by. But I also write them for me. I have enjoyed the process of dredging up old memories, some good, some not so good. These experiences have molded my character for better or worse.

The least understood of the brain's functions is memory. Even the most vivid memories can be modified over time to conform to what we would like to have happened or feel should have happened. To the best of my knowledge these memoirs are true. At least they are true for me. This is what I remember.

Those who believe they are in control of their destiny are deluding themselves. The many choices we must make almost daily do influence the direction of our lives, but far more significant, in my judgement, are events of happenstance that push us willy-nilly one way or the other. Sometimes seemingly trivial incidents can have profound consequences and alter the course of our lives dramatically. I have tried in the following account to record some of the experiences that influenced mine.

APPENDIX XI: REMEMBRANCES (1995, UPDATED IN 2007)

I was born March 3, 1927 in Orange, New Jersey, but I don't remember it. In fact I remember very little of my early life. I do know that I was supposed to have been a girl. They even had a name picked out for me. Nancy. In 1929 we moved to a new home in Essex Fells. The McMillans moved into a new house next door at the same time. My parents knew them well. Harold McMillan was Mother's brother Del's business partner. Bob and Dave McMillan were the same age as Norton and me. Duncan was about three years younger. We were all good friends and played together as children. Duncan tagged along as there were no kids his age in the neighborhood. I liked him the best because he was smaller than me. Few kids were.

Becker Farm in Roseland, now known only by a road of that name, produced wonderful raw, unhomogenized milk with a cream layer so thick you could eat it with a spoon. It was delivered fresh to our door daily. My mother would leave a note for the milkman listing the requirements for the day which she would place with the washed empty bottles in a small compartment built into the house with a door opening into the kitchen and another door opening onto the back porch. I was the only one who could squeeze through the opening. It was my favorite way of getting into the house.

Becker's used horse-drawn wagons to deliver the milk, which were replaced by sleighs in the winter when the streets were snow covered, a not infrequent occurrence since snowplows were rare. One of the most pleasant sounds I can remember was the clippity-clop of the milkman's horses' hooves on the road as I lay snuggled in my mother's bed, where I would often retreat in the middle of the night to escape the demons I knew were hovering in the shadows of my dark room.

My bedroom was the smallest in our house and was in the cold, northeast corner. Norton had a large sunny room on the southeast corner. I liked my little bedroom because I was allowed to do pretty much what I wanted there. I remember hammering staples in the plaster walls to allow me to control the light switch on the wall with strings from my bed so that I could turn the light on or off without getting out of bed. Mostly what I liked was that I could crawl out the window onto the garage roof and climb down a rose trellis to the back yard. I would sneak out this way at all hours of the day and night.

I started kindergarten at the age of four, a year before I should have. This was a terrible mistake that had a profound effect on my psychological development. Naturally shy to start with, I was the youngest and smallest child throughout grade school. Poor at athletics, I was always the last one remaining in choosing up sides for any game. I hated Field Day when everyone was obliged to compete in field and track events. To make matters worse I was very late in reaching adolescence, not attaining anything approaching my present height until a senior in high school. All of this combined to stunt my social development and contributed to my introverted nature, which is still a handicap when I find myself in unfamiliar social situations. On the other hand, it might have made me a better scientist. One never knows. Why my parents made me go to school so young I'll also never know. Either they thought I was exceptionally smart (which I wasn't) or they wanted to get rid of me.

Our family physician was Dr. Butler who had a small private clinic in his house in Caldwell. I rarely visited the clinic because Dr. Butler would normally come to the house, black bag in hand, a far more civilized way of administering to the sick than is practiced by the medical profession today. On the other hand, medical practice was a lot more primitive in those days. Only twice in my life have I had general anesthesia, both times in Dr. Butler's clinic. Once when he set a badly broken arm that I suffered on falling out of a tree, and the other time when he extracted my tonsils and adenoids. The procedure was simple and cheap. While his nurse physically restrained me, Dr. Butler held a cone containing gauze dampened with ether over my nose and mouth until I passed out, a procedure not unlike that which I would apply to laboratory rats in graduate school. To this day I am nauseated by the smell of ethyl ether.

I usually enjoyed being sick as it kept me out of school, which I hated, and my mother would spoil me rotten. She would read to me for hours and make me treats like orangeade and chicken soup. I didn't, however, enjoy the whooping cough, the only serious childhood disease I remember contracting. In those days when a child came down with a contagious disease the house was quarantined with signs posted on the doors prohibiting access to any but the immediate family. I particularly enjoyed Norton getting the mumps and measles because I too was quarantined and kept out of school and I wasn't even sick.

One of the earliest experiences I remember was my baptism by the Reverend Harold Onderdonk of St. Peter's Episcopal Church in Essex Fells. Fortunately this was a private ceremony, or my parents would have suffered severe embarrassment. When I saw the Reverend dip his hand into a bowl of liquid and reach for me I took off without hesitation. However, I was soon apprehended between two pews and dragged back kicking and screaming for the completion of the anointment. I believe this experience cemented in my mind a disdain for organized religion that has sustained me all of my life.

I did continue to attend St. Peter's at the urging of my parents but never with any conviction. I sang in the boys' choir where the members were paid according to their ability: 75 cents a month for class A, the best, 50 and 25 cents for classes B and C respectively and nothing for probation. I remember one proud day when the choir master was so disgusted with the group he made each person sing individually and was demoting about half the members on the spot, most of them to probation. When my turn carne he beamed with delight and promoted me from class C to class B despite the lack of service time normally required at the lower class before one was eligible for promotion.

One of the boys demoted to probation was my best friend, Larry Soule. Sometime later a new music teacher in grammar school decided that all of the students had to individually audition for the class glee club. The only ones who didn't make it were Donny Rosenquest, who couldn't sing a note, and me. I was chagrined and believe to this day that the teacher got me confused with Larry who had tried out just prior to me.

My father didn't like Larry (he disapproved of all of my friends) and would openly belittle him. One day he offered him twenty five cents to go home. Larry declined, explaining

that his father had paid him twenty five cents to come over to our house.

My father grew up in a home with five boys and no girls, which probably accounted for the fierce competitiveness and lack of sensitivity he shared with his brothers. My mother, on the other hand, had three sisters and only one brother and was a much gentler person than my father. Fortunately for Norton and me she was the usual disciplinarian who would administer the occasional spankings that I'm sure we deserved. However, when the offence was sufficiently serious my father would be called on to provide the corporal punishment with a leather slipper which really hurt. More often than not just the threat of the slipper was enough to bring us into line.

My father was an unmerciful tease, often to the point of cruelty, and he was very stingy with compliments. He would frequently reduce my mother to tears. He was fond of saying that the only thing I could do better than him was that I could crawl through a smaller hole.

The third floor of our house in Essex Fells had a large playroom that we were allowed to mess up. We had a wonderful set of wooden blocks that I never tired of playing with. Lincoln logs, erector sets and Lionel trains provided countless hours of constructive play that I'm sure stimulated my creative instincts. We had a set of iron molds for lead soldiers which we hand painted and used in great mock battles in the playroom.

In the ping pong room over the garage we had more realistic battles with souvenir guns and swords my father brought home from World War I. This was always great fun and no one ever got hurt.

Essex Fells was a great place in which to grow up. You could get anywhere on a bike and there was always something to do. My favorite games were "kick the can" and "hares and hounds". Kick the can was a wonderful game that any number could play and being small was not a disadvantage. The only equipment required was a tin can. After designating someone as "it", someone else would kick the can and everyone else but "it" would run and hide. "It" had to retrieve the can and replace it in its designated spot before looking for the other players. On spotting and vocally identifying a player "it" would run back and kick the can and the game would resume with the spotted person now being "it". The strategy for the players was to hide as close to the can as possible. If the spotted person managed to get to the can and kick it before "it" could, he avoided becoming the new "it". Furthermore, any player could run in and kick the can at any time requiring the person who was "it" to retrieve the can as before. Therefore, the strategy of "it" was to stay as close to the can as possible and still find a hidden player. I recall a game when Fritz Reindel became so frustrated with everyone kicking the can on him that he chose to stand on the can and try to spot a player. I carefully snuck up and kicked the can right out from under him.

Hares and hounds was also a game any number could play and physical stature was inconsequential. The only equipment required was chalk. The hares would take off a short time in advance leaving a trail of chalk arrows for the hounds to try to follow. False trails were expected and the more tortuous the true route the better. The object was to leave a trail that was

difficult but not impossible to follow. There were no restrictions but most of the time the route was through the woods.

Each year I would venture farther into the extensive woods behind our house until I knew every inch of them. While I was always interested in the outdoors and the natural world, my particular fondness for birds can be traced to an unusual incident. Walking home from school one winter day I noticed a small gray bird sitting in the snow. I picked it up and carried it home, but before I got home it died. I paged through the pictures in a volume of *Audubon's Birds of America* which Norton and I had given to our parents for Christmas until I identified the tufted titmouse. After that I used to spend hours going through the book until I had memorized every bird in it. From then on I kept lists each year of the birds I identified in the field. During the spring migration in high school I would go bird watching every day. One day I spotted a hooded warbler which was a real coup as none of my naturalist uncles had ever seen one.

My father was more into wildflowers. He was very competitive and made a game out of everything. Each spring our family had a contest to see who could find the first of several common wildflowers. I remember Norton beat me unmercifully because I found the first violet when I stayed home from school one day feigning sickness.

My father was 42 years old when I was born. I always resented the fact that he was so much older than me, because I was too young to join in many of the fun activities he enjoyed. Principal among these was hunting which he and my uncles did every fall. One Christmas morning when I was 14 or 15 I was disappointed because I had obviously received fewer presents than Norton. Then my father remembered a present he had stashed away and overlooked. He came down stairs with an oblong box that I immediately realized contained a gun. It was one of the most exciting moments of my life. It was a beauty; a double barrel, 20 gauge Ithaca shotgun. I couldn't wait for an opportunity to shoot something.

I got up early one morning the next spring and quietly walked out the back of the house with loaded gun. I had barely reached the edge of the property when a Cooper's hawk flew by. I dropped it with a single shot. My first kill. Cooper's hawks are now rare and protected as are all raptors, but back then they were all vermin.

Harvey Roberson and I ran a trap line in the Troy Meadows during the early years of World War II. Every day after school we rode our bikes in all kinds of winter weather the several miles to the meadows to check the traps. We didn't catch many muskrats, but the pelts commanded very high prices in those days. One day we came upon the remains of a half eaten black duck. We set a trap on it and covered it with leaves. The next day we were confronted by a very angry great horned owl which Harvey quickly dispatched with a club. I was experimenting with taxidermy at the time and Fritz and I skinned and mounted the owl.

The great horned owl became a successful crow decoy. We would take it out into the woods or meadows and raise it on a tall pole where it would be visible for some distance. After building blinds of brush to conceal ourselves, we would try to call in crows with our crow calls.

Crows hate all owls and will harass any found during the daylight hours. One day Harvey and I were trying to call in crows from separate blinds near the owl decoy. A marsh hawk (now known as the northern harrier) spotted the owl and made a bee line for it from a considerable distance away. Harvey and I both fired simultaneously and argued for years over who actually killed the hawk. I did a creditable job of mounting him with wings and tail outstretched, and displayed him in a prominent place for many years.

I hunted sporadically for many years with little success. In 1953 while engaged in a postdoctoral fellowship at the University of Iowa I shot what I'm sure was the first legal pheasant of the season and my last. I had surveyed the bird habitats around Iowa City for days preceding the season and had positioned myself on the first day of the season at the top of a grassy ravine where I had spotted birds previously. At the sound of the noon whistle which signaled the opening of the season I rose and began slowly walking down the ravine. I hadn't gone ten feet when the first cock pheasant exploded into the air in front of me. I shot him and he fell into high grass off to my left. At the moment of my shot a second cock rose and flew off to the right. While I could easily have taken him too I was concerned that I might lose the first in the high grass and also was reluctant to get my limit of two birds so early. After retrieving the dead bird I continued to hunt the rest of the day flushing numerous hens but never getting another shot at a cock.

But that was over forty years ago. I still have my shotgun but haven't fired it since that day in Iowa and have no desire to.

When I was growing up there was always at least one new house under construction nearby which would become a source of lumber for construction projects. A favorite project was tree houses, and we built many of varying complexity. Another one was jitneys. Everyone that could find an old set of baby carriage wheels built a jitney. I had the fastest one on Oldchester Road. Larry Soule's older brother, Jack, almost crashed corning down Stuart Road in Larry's jitney because Larry had wound the steering ropes the wrong way; turning the steering wheel to the right made the jitney turn left and vice versa.

One memorable jitney was a group project. Most jitneys were built to hold the driver only. This one accommodated three or four kids with a place to stand on the back holding onto a horizontal bar. It was, in fact, a fire engine and was conceived because one of the neighborhood kids came into possession of a pump-type fire extinguisher. Several of us would wait at the top of the hill on Oldchester Road while others would light a small fire in the vacant lot on the corner of Felis Road. When the fire was the right size a shout would bring the jitney careening down the hill and we would all jump off and put out the fire. This game ended after one fire got too big to extinguish and the real fire department had to be called.

In the fall we would gather acorns for acorn fights. A well thrown acorn can hurt, so we made wooden shields to ward them off. Some of these were crude, but I remember an elaborately decorated one I used for years. I copied Audubon's bald eagle from the book. This was surmounted by a banner proclaiming, "Nemo me Impune Lacessit", stolen from Edgar Allen Poe's "The Cask of Amontillado", which roughly translates into, "No one attacks me

without punishment". Audubon's eagle had its foot raised on a dead catfish. This didn't seem appropriate for a shield, so I substituted a cowering mouse. I was really proud of that shield.

In the winter, acorn fights would give way to snowball fights. Most of the time was spent in building two opposing forts out of snow about·ten yards apart. Between the swords, fists, acorns, and snowballs we spent an inordinate amount of time fighting.

Next to Christmas my favorite holiday was the Fourth of July. There were no restrictions on the purchase of fireworks in those days and we bought everything we could: Roman candles, sky rockets, pinwheels, cones, as well as all kinds of explosive devices including several sizes of fire crackers, cherry bombs and torpedoes. Torpedoes were round balls that exploded on impact when thrown at a hard surface. One day I was sick in bed and Mother was reading to me. Norton and I were having a gentle game of catch with a torpedo when he tossed it too vigorously. It hit me on the head and exploded. I was unscathed, but Mother suffered a shrapnel wound in the hand.

Firecrackers were favorites, and we would dream up innovative ways to set them off to produce unusual effects. Buried in an acorn and launched with a sling shot they made fine aerial bombs when the wick didn't get blown out. A large firecracker imbedded in an apple made a terrible mess. Later I would learn to make my own explosives.

While we had chemistry sets as children it wasn't until I took chemistry in high school, my very favorite course, that we started to assemble a real chemistry lab in the basement and I learned the basics of making bombs. Blue tip matches, which ignited when struck on any hard surface, figured prominently. We had previously discovered many delightful uses for blue tip matches. We had little toy cannons with a spring activated plunger that would launch a projectile ten feet or more. When the projectile was a blue tip match inserted head down it would ignite when the plunger was pulled. We would build little log cabins or forts out of twigs and try to shoot each other's building with incendiary missiles. A little kerosene poured on the cabins helped speed the game.

Bouillon cubes came in little tin cans about three quarters of an inch in diameter and four inches long. We would fill the cans with blue tip matches and hold the capped cans with tongs in a fire. When sufficiently heated the cap would blow off violently followed by a long tongue of flame.

One of my favorite bombs was made by half filling a .22 long cartridge casing with gunpowder, adding the head of a blue tip match and firmly crimping the open end by folding it over itself in a vise. These would explode with an extremely loud report if hit with a hammer. Our favorite place to put them was on the trolley tracks on Bloomfield Avenue.

Hand grenades were made by mounting a roofing nail to the cap of a shotgun shell by supporting it with a cork. Cardboard fins glued to the body of the shell would guarantee that it would strike the nail when thrown onto a hard surface and explode. I remember tossing one out the open window in high school one spring day onto the sidewalk below. I wasn't caught, and

my stock with my schoolmates went up measurably.

My most powerful bomb was made with an empty canister of the kind used to make seltzer water which I filled with black gunpowder after knocking out the seal. These were detonated electrically. I cut all but a single strand of braided copper cord and twisted the two single strands together, shorting them out, and shoved them into the canister. This, of course, required a very long cord because it had to be plugged into the house to set off the bomb. I had managed to splice together numerous electric cords retrieved from various defunct electrical appliances until I had a cord over a hundred feet long.

Essex Fells was about as Waspish as you could get. I knew no blacks that were not servants and few Catholics. The Goodmans were the only Jewish family in town. Burton was in my class, and he was ostracized unmercifully. He was also the dullest kid in class, which didn't help his social standing. I remember feeling sorry for him, but not enough to offer the hand of friendship. I'm sure my parents were anti-Semitic, especially my father. I recall his consternation when Virginia Kirkus, my mother's roommate at Vassar and founder of the renowned *Kirkus Reviews* whom we called, Aunt Virginia, married Frank Glick. I don't recall ever hearing about the plight of the Jews in Europe during the War.

Because of the Depression we didn't go on many vacations, and those we did were often low budget affairs. The Warriners from Essex Fells had a big house in Montrose, Pennsylvania with a cottage by the lake in which we stayed once or twice. Several times we rented the Spencer Miller cottage in Montauk, Long Island that didn't even have plumbing. We had to lug five gallon jugs of water from town. Uncle Durand and Aunt Marie had a farm in Vermont where we were frequent house guests. During one vacation in Montauk a nor'easter blew cold rain the entire week. Our parents spent most of the time teaching us how to play contract bridge. I took to it enthusiastically and became reasonably proficient. Norton didn't.

One summer when I was very young we rented with the Stanley Babsons a house in Mantoloking on the Jersey shore. My father and Uncle Stanley commuted down by train for the weekends. We used to play a game on the beach we called roly-poly. Each player scooped out a depression in the sand and whoever was "it" would roll a tennis ball toward the grouped depressions. If the ball ended up in your hole you had to retrieve and throw it at another player all of whom were by now fleeing in all directions. If you managed to hit someone he was then "it". If you missed you were "it". After any player had accumulated three "its", he or she was obliged to stand bent over while each of the other players had a free shot at the loser's backside with the tennis ball. One time my cousin Mollie excused herself after losing and went into the house and stuffed a magazine down her shorts to soften the blows.

One day we rented a small sailboat to cruise around Barnegat Bay. Just as I was stepping from the dock to the boat a wind shift caused the boom to swing abruptly knocking me into the water. I couldn't swim and went straight to the bottom. I remember looking up at the bottom of the boat through the clear water and remembering my mother's instructions to kick my legs if ever I found myself in such a situation. My father jumped into the water with all his clothes on to save me, but I was already on the way up. He ruined his expensive wrist watch.

Another year when I was about twelve, we chartered a forty-five foot ketch named the Malolo, which came with a very grumpy and unsociable captain. The Stanley Babsons sailed it from Newport, Rhode Island to Gloucester, Massachusetts while we drove up in their car and sailed it back. We spent several days in Gloucester which is the place our family originally settled in America. James Babson, the family founder, sailed from Weymouth, England, with his mother Isabella in April 1637. James built a cooperage on Cape Ann where he made barrels that is now an historical landmark. Every five years the extended family has a reunion at the James Babson Museum, which is a lot of fun.

The trip on the Malolo was not much fun for me. My cousin, Johnny Baird, wheedled his way into our vacation through his mother, Aunt Dotti, who thought it would be a wonderful opportunity for him to expand his considerable knowledge of boats. The captain gave Johnny all the interesting assignments when we were under sail, which was infrequent as the winds were almost nonexistent. Johnny and I didn't like each other, and he went out of his way to inflict pain on me when we were sleeping on deck each night and out of sight of the rest of the family. At another time in Essex Fells I had the distinct pleasure of opening up a sizable gash on Johnny's head with a well-thrown rock.

Our parents attempted to provide Norton and me with all the advantages. In my case this often meant too much too soon; piano lessons when I was too young to appreciate them; dancing lessons when I didn't like girls; horseback riding lessons when I was too small to control such a large animal. I recall once with Norton and an instructor approaching a tree leaning at a sharp angle across the bridle path. Norton and the instructor directed their mounts around the obstacle. My horse decided just to walk under it. The tree trunk caught me in the belly and, when I ran out of horse, I went crashing to the ground. Another time in Vermont I was placed on a giant beast with stirrups totally out of reach and turned loose with Norton and our cousin Charles. After a bit they took off on a brisk canter and were soon out of sight. My horse refused even to trot. Eventually I had to dismount and lead him back to the barn.

In later years I did learn to ride reasonably well as a member of the Montclair Mounted Troop. One year our troop was chosen from several units of the Junior Cavalry of America to march in Madison Square Garden during a national horse show. I delight in telling people that I once performed in Madison Square Garden.

While we grew up during the Depression we were not poor. We were not rich either, and my parents early on taught us the value of money. Norton and I had to earn our spending money and we did it in a number of innovative ways in addition to the usual grass cutting, snow shoveling and leaf raking. We set up a soft drink stand on the long fifth hole of the country club and sold drinks at a premium when we weren't caddying. Every Sunday I worked the traps that launched clay targets for the Essex Fells Gun Club. Every day I stoked the coal furnace and carried out the ashes for the Eaglesons, who lived across the street.

The advent of World War II eased the Depression and brought new opportunities for money-making schemes. Gas rationing limited most people to one gallon a week. We didn't

have mail delivery at that time, so I developed a thriving business delivering on my bike every day the mail to many households in town. Nobody drove to Deer Lake because of the gas rationing. On weekends friends and I would ride our bikes to Deer Lake where we had a tent and would have the whole place to ourselves.

My father was a charter member of the Deer Lake Club which was started in the 1930's and at least one of our immediate family has been a member ever since. Soon thereafter he bought a second hand canoe which I still have. Susan and I use it on the Black River every year and took it to the Wading River in south Jersey on our honeymoon.

One day before the war we were returning from Deer Lake with my mother's sister, Marie, and her husband, Durand. We stopped at a roadside stand which advertised raspberries at ten cents for one box and twenty five cents for two. Uncle Durand tried to explain to the farmer that this pricing was nonsensical. The response was, "Sure, we realize the price for one box is twelve and a half cents, but we let it go for ten." Determined to teach the ignorant farmer a lesson in economics he proposed buying one box for ten cents, driving around the block and buying the second box also for ten cents, thereby getting two boxes for twenty cents. The farmer laughed at this obvious attempt at city folk trickery and said he would charge fifteen cents for the second box. No amount of logic would dissuade the farmer of the error in his thinking. The irony of this story is that Uncle Durand ended up buying only one box when he really wanted two and would have gladly paid the very reasonable price of twenty five cents. It was a question of principle.

My proudest moment at Deer Lake occurred one day when my daughter, Betsy, was about four years old. My father had challenged her to swim about ten yards and had offered an inducement of five dollars. I carried her out the prescribed distance from shore. At my spur of the moment suggestion as I released her to return to shore she turned around and swam about twice that distance in the opposite direction to the raft. I'll never forget how pleased she was sitting there laughing and smiling with all the big people.

When I graduated from high school I was just seventeen with almost a year to put in before I was eligible for the armed services. I had no idea what I wanted to do with my life, so I applied for admission to Cornell in the college of electrical engineering. I didn't know the difference between an electrical engineer and an electrician. I also applied for the Army Specialized Training Reserve Program (ASTRP) that was available for seventeen year olds as an early entrance to the ASTP, the source of most of the new commissioned officers. I was accepted by both Cornell and the ASTRP and chose the latter because it was free and being an officer seemed like a good idea at the time. Thus I began my college life at Rutgers University as a cadet in the very regimented ASTRP.

I may be the only person who was kicked out of Rutgers and went back to get a Ph.D. The academic program was easy and I was on the Dean's list with little effort when I was summarily separated from the program. One of my good friends and I decided to sleep in one afternoon and cut a chemistry lab. We were confident that we were familiar enough with the sergeant's inspection schedule of the dorms that he would not discover us. What we didn't know

was that very day a new sergeant was assigned to our unit, and he caught us sacked out.

Within an hour we were really sacked and I found myself hitch hiking home with my tail between my legs dreading the inevitable confrontation with my parents.

They were not pleased. My father's final command was, "You have two weeks to find a job." I managed to find a job at Cullen's Photography store in Montclair. My job was helping with the printing in the back room. It was mostly a manual process and only black and white prints and enlargements were made. Mostly I fed the washed prints onto a belt conveyer to the heated and highly polished print drying drum. Glossy prints were fed with the photo surface facing the drum and matt prints were the reverse. After drying the prints had to be sorted by a code number printed on the back, the negatives cut between exposures and the two combined in an envelope and identified with the customer's name. It was fun to look at people's private photographs, and once I was privileged to observe what would have been considered pornographic at the time.

However, the job was quite menial, and I shortly got a better job through the help of my father. A good friend of his, Paul MacKinney, was president of a company in Bellville, New Jersey, called American Dyewood. They needed help in the research lab and Mr. MacKinney agreed to take me on. It was the first of several times my father's connections would help to advance my career or bail me out of trouble.

The Friday before the Monday I was scheduled to start my new job I was worried about how to tell Mr. Cullen about it. As this was the first full time job I had ever had I had no experience in quitting. Mr. Cullen had to leave the store for some time that day and he instructed me to watch the showroom. To keep me busy he told me to dust all the bottles of developer, hypo and such on the shelves. I considered this a meaningless endeavor and shortly gave it up. Cullen returned and after checking and discovering dusty bottles on the shelf he berated me and sent me to the back room. While I was chagrined at his criticism it gave me the opening I sought to tell him about my new job. About five o'clock I entered the showroom and addressed him as follows, "Mr. Cullen, I guess I should tell you that I won't be working for you anymore." Without hesitation he glanced at his watch and said, "You beat me by half an hour." I never even got to tell him about my new job.

American Dyewood manufactured sundry dyes and chemicals. The company occupied a number of dilapidated wooden buildings. Each dye was made in a different building to avoid cross contamination as the synthetic processes were poorly contained. In the wintertime when the ground was snow covered it was very colorful with different colored foot prints leading from each building.

My boss, Wally Peck, was Director of Research and he lived in Caldwell. A gentle and pleasant man, he would pick me up at the corner of Ryerson and Bloomfield Avenues each morning and drop me off in the afternoon for the half-mile walk home. Research consisted of half a dozen chemists working in a separate building. I remember one old chap who appeared to sit all day watching a boiling pot of brown wax. I think he was making shoe polish. One day the pot got too hot and burst into flames. He tried to carry the flaming pot outside but he spilled the

contents along the way leaving a burning trail in a building that it would be an understatement to call a fire trap. Fortunately a fire extinguisher was handy to extinguish the flames.

My job was mainly involved in analyzing the quality and purity of guanidine hydrochloride, one of the company's experimental products. I learned many valuable skills including gravimetric and volumetric analytical techniques. I thoroughly enjoyed my job and glorified in the association with real chemists. Later, while being interviewed about special skills after my induction into the Army, I told the interviewer I had worked as a chemist in a research laboratory. He scoffed at this and refused to include the information in my records.

I received my Greetings letter from President Roosevelt on March 4, 1945, dated March 2, the day before my eighteenth birthday when I became eligible for the military draft, directing me to report for service to Fort Dix. My father at that time was commuting every week to Washington, D.C., where he had a job as an expediter (I never understood what he did), the war having temporarily put him out of business as a General Electric home appliance dealer. G.E. was manufacturing war materiel, not refrigerators. I was anxious to get into the fray. All my friends were already in the service. Unlike the Vietnamese War, WW II had united the country with a single purpose: to defeat the Axis powers. The issues were black and white. We were the good guys, the Germans and the Japanese were the bad guys. And this was true.

My father had an apartment in Washington and he commuted by train, coming home on weekends. He rode down with me and shook my hand when I got off the train at Trenton to report to Fort Dix with the admonition to be a good soldier. Once more I would disappoint him.

The Army and I did not get along. Since early childhood I had been by nature a nonconformist and a rebel against authority and regimentation. These were not the qualities the Army admired in their recruits, and I frequently found myself in trouble. Once I made the mistake of suggesting to a training sergeant a better way to do something. While a mild rebuke might have been appropriate, I was instead subjected to a withering diatribe of scorn and contempt. But I did my best to conform and learn as much as I could about the business of surviving and killing the enemy. As it turned out I saw no action unless I include a brief skirmish with an Armenian at the Ayameke Paradise over the affections of a girl named Tomiko.

I spent the summer of 1945 in Camp Blanding in north central Florida, a God-forsaken place of sand, insects and unimaginable heat. Basic training was not supposed to be fun. In a short fourteen weeks we had to be transformed from soft civilians to hardened killing machines, and we were.

Never having previously been south of Mantoloking, New Jersey, not counting a brief visit to Washington, D.C., it was my first introduction to the semitropics and I was thrilled by the new wildlife. My mother had sent me a pair of opera glasses, and every Sunday morning that I could I would venture into the bush to search for new birds. I had also fashioned a butterfly net out of cheese cloth and coat hanger that I kept hidden in a hollow tree near camp. I managed to collect several species new to me that I secreted in a cigar box. These activities were, of course,

carried out without the knowledge of my fellow recruits. If they had found out about my strange behavior I would have been a laughing stock.

My two best friends in basic training were Paul Basette, a twenty-nine-year-old Catholic from Troy, New York, and Arthur Bitterman, an eighteen-year-old Jew from Brooklyn. After the war the three of us would have a reunion at Paul's house in Troy. Every Saturday night when we weren't on field maneuvers we would go to the beer garden to drink beer, eat chocolate (a great combination if you haven't tried it), and write letters. I kept up a sizable correspondence with my friends in the various armed services as well as my family and the girls at home. My mother saved my letters home and a rereading of them has assured the accuracy of the recounting of this period in my life. Following are excerpts of some letters to my mother and father from basic training.

May 6, 1945

"Yesterday we had our first formal rifle inspection. I had spent about two hours cleaning it Friday night and I thought it was perfect. The lieutenant came to my rifle and said, "Dirty front sight." I asked him what was the matter and he pointed out a speck of dirt about the size of a fly's turd. So I spent Saturday afternoon cutting grass with my bayonet."

June 3, 1945

"One night last week I was on guard duty. The first thing in the morning the lieutenant came around to inspect my rifle. I hadn't cleaned it since the afternoon before, so naturally it was full of sand and dust. When he saw it he blew up and gave me a week of extra detail. Keeping your rifle clean is supposed to come before eating or sleeping. On top of the extra work, my rifle was restricted to the company area for three days and I was tied to it with a tent rope. I had to eat, sleep and even shower with it."

June 13, 1945

"I am enclosing the bond that cost me three days of detail. I bought it last pay day. They said they'd have the bond the next day. But they didn't have them 'til last Friday. It happened that I was on table waiting that night and by the time I got to the orderly room the lieutenant had gone. He gave me K.P. on Sunday, table-waiting on Monday, and latrine duty on Tuesday. As a matter of fact today is the first day I haven't had guard duty or detail in thirteen days."

July 6, 1945

"I suppose the Fourth of July was a holiday for you. Well it wasn't any holiday for me. We got up at 4:00 AM. As the rosy fingers of dawn were brightening up the eastern sky we were on the road to the field firing machine gun range. As the sun made its appearance on the horizon we were already rolling in the sand on the firing line. Most of the company finished work at 8:00 PM, but I had to clean guns and didn't get a chance to pitch my tent until it was dark and had started to rain. I said, "To hell with it," and crawled into a tent with two other guys."

July 31, 1945

"Sunday night at 7:00 we pulled out for bivouac. At about 12:00 we arrived here by a

nice lake. Did you ever walk 15 miles carrying 60 pounds of equipment? Well, don't try it. I don't know how you'd make out, but it damn near killed me. Only one guy in our company dropped out against twenty in another company. The last two miles we marched through deep sand. I thought my legs would drop from me any minute. One guy in our squad upon arriving at the bivouac area collapsed without taking off his pack or unslinging his rifle. I made a beeline for the lake, and after drinking about half of it, soaked my feet in the other half."

August 3, 1945

"Yesterday was one of the toughest days of training we've had. We started out early in the morning after only three hours of sleep and went over to the artillery range. The problem was very interesting. We had to take an enemy position about a mile away, in front of which were two outpost lines of defense. When we got to about 300 yards from the first enemy line the air force came through. First, two B-25's came in low and dropped a load of anti-personnel parachute bombs. Then four P-51's strafed the area. When the planes had left, the machine guns opened up on the first line of defense. When the machine guns ceased firing the three rifle platoons of the company moved up on a 600 yard skirmish line. While we were moving up a battery of 105mm howitzers gave us artillery support by dropping shells about 150 yards in front of us. All the while charges of explosives were set off all around us to simulate enemy fire. After we assaulted the first line we took up fire on the second while the machine guns moved up. After we had taken the second line a terrific barrage was laid down upon the enemy stronghold. Then we closed in for the final assault firing from the hip and shoulder and bayoneting dummies as we came to them. After the assault we consolidated and took up defensive positions on the other side of the enemy's previous position. Two fellows were hit by shrapnel.

"That afternoon we ran a problem in which we knocked out pill boxes. Each rifle squad had a bazooka team. The problem was four miles long about half of which we had to double time. About 9:00 that night after we had cleaned our rifles, B.A.R.'s, machine guns and mortars we started back to the bivouac area. We had only gone a little ways when it started to rain cats and dogs. It was so dark that the only time you could see the man in front of you was when the lightning flashed. Needless to say, we got soaking wet."

Paul Bessette was rather small but spunky. One night after too many beers he challenged me to put on the boxing gloves to settle a dispute the nature of which I no longer recall. Before I could manage a single punch he landed a solid blow to my forehead that sent me sprawling. End of contest with no hard feelings.

Paul managed to get his wife pregnant with their second child during his furlough after basic training, and while the three of us were to go through camps Gordon and Adair together, he was excused from further duty along with all married men with two or more children just before we shipped overseas.

While few recruits were killed or seriously injured during basic training, on one occasion I came close to being a casualty. The field problem involved taking a village built for the training exercise. I was instructed to toss a hand grenade through the window of a log hut. I

quickly ducked around the side of the hut and hit the ground assuming the wall was solid. As I hit the ground I glanced up momentarily to see before me a wide open doorway and my grenade sputtering not fifteen feet away. It exploded and luckily I escaped the resulting hail of shrapnel unscathed.

V-E Day came on May 8 while I was in Camp Blanding, but there was still the Japanese to deal with. After a much welcomed furlough on the completion of basic training I was sent to Camp Gordon in Georgia for five weeks of advanced infantry training. While we learned to use all types of weapons including hand and rifle grenades, mortars, bazookas and machine guns, the basic infantry piece was the M-1 Garand rifle. This was a superb weapon that was effective at several hundred yards and was designed to be totally disassembled without tools for cleaning and servicing in the field. I could rapidly take it apart and put it back together blindfolded and was sufficiently proficient in its use to be awarded a marksman medal. Overseas I was destined to serve in three different field artillery battalions and never saw an M-1 in a year in Japan. Instead I was issued a much smaller carbine that I had only fired once in basic training.

After Camp Gordon we travelled by very slow troop train to Camp Adair in Oregon and thence to Fort Lewis, Washington, a staging center for shipment to the Pacific. The train took over five days to make the journey, and the only time I got enough to eat was when I was on K.P. Despite the food I thoroughly enjoyed the journey. I managed to see a lot of the country, and I was thrilled to view my first snow-capped mountains as we travelled through Montana, Idaho, and Washington before turning south to Portland.

Bitterman and I managed a weekend trip to Portland from Camp Adair to visit Gorham and Ruth Babson who drove us out to see Uncle Sid and Aunt Grace at the foot of Mount Hood. This was a particularly enjoyable experience for me because in 1905 my father and his brother Sid had cleared the land with teams of horses, built the house, and created the apple orchard that the family always referred to as, “The Ranch.”

V-J Day came on August 15 while I was at Camp Gordon signaling the end of the war so the chances of being killed were remote. Strangely, the end of hostilities diminished my limited enthusiasm for the whole endeavor. However, my army career was just beginning.

About two thousand of us shipped out of Tacoma, Washington, aboard the USS Admiral Coontz, a 600-foot-long troop ship, on October 23. I wasn’t on board an hour before I was put in the kitchen, where we peeled 2,400 pounds of potatoes, enough to sink any boat I had been on before. We proceeded south into the teeth of a gale. The big ship rolled continuously and most of the troops got sick. The decks were awash with vomit.

Bunks were two by six foot pipe frames with canvas stretched between the pipes, and they were four deep. There wasn’t room for everyone to stand by their bunks at the same time. Mine was a topmost bunk and was situated in the stern next to one of the twin screws that propelled the ship. The ship rolled so badly that part of the screw would come out of the water at the extreme end off a roll and the blades would pound with a fearful racket and vibration that would appear to threaten the integrity of the hull. Sleep was difficult.

After several days the storm abated and for the rest of the trip the seas were relatively calm. Ours was the first troop ship to cross the Pacific unaccompanied by a protective convoy. While there was no danger of being torpedoed there were errant mines floating in the ocean that posed a threat, and constant vigilance by lookouts was necessary. One day the ship hove to when a mine was sighted bobbing in the waves a couple of hundred yards away. When small arms fire failed to explode the device the attack escalated to include a 40mm anti-aircraft gun. After a half hour of sporadic fire the ship moved up to dangerously close range and sank the mine. We spotted three more mines during the remainder of the trip.

We were about two weeks making the crossing and there was little to do. I read six books and all the magazines I could find. The greatest pleasure was watching the effortless aerial maneuvers of the black-footed albatrosses that followed us most of the way. After a brief stop at Nagasaki, where it was discovered we weren't supposed to be, we docked at Nagoya on November 10.

The next day most of the troops disembarked in three groups at 6:00, 8:00 and 10:00 AM to join the 33rd Division in Kobe. About two hundred of us didn't leave the ship until 6:00 PM. We were only told that we had been selected for special assignments. After about a week in a replacement depot two other men and I were transferred to a luxury hotel in Osaka where we ate in a fancy dining room waited on by Japanese girls in fancy kimonos. Things were definitely looking up. A few days later we were taken to the 98th Division headquarters for our special assignment: to try out for the 98th Division band! My induction records included my having played second clarinet in the Montclair High School football band, but I hadn't touched the misery stick in two years. Needless to say I didn't pass the audition. That night I was unceremoniously dumped at the 923rd Field Artillery Battalion in Nara.

After a number of odd duties the first month the commanding officer called me into his office a few days after Christmas and I recall the conversation verbatim.

"Babson, do you know how to cook?" "No Sir."

"Would you like to be a cook?" "No sir."

"Report to the kitchen at 05:00 tomorrow morning. You're a cook."

Since I was friendly with the mess sergeant I managed to get assigned to officers' mess where there were only about thirty men to cook for and the food was much better than that which the enlisted men got. We put in a fourteen hour day but we only had to work every other day. On days off I would sleep late, pick up my pass around 4:00 and hop the train for the Ayameki Paradise to drink beer and dance with my girl Tomiko. The battalion was at about half strength because the guys with 51 or more service points had gone home. Consequently the few privates left were pulling incredible hours of K.P., guard duty and other noxious chores. This was my best time in the occupation and I enjoyed it. After a couple of weeks the other cook went home and was replaced with a novice like me. I became First Cook.

The officers' mess was in a Japanese house which was a tinder box. One day the grill got too hot and the grease burst into flames soon catching the paper-thin wooden wall. The other

cook grabbed a Pyrene fire extinguisher and started pumping carbon tetrachloride onto the hot grill. The fumes, which probably included phosgene, were overpowering and I quickly exited through a window. The other cook stood his ground and extinguished the fire. This would not be my last experience with fire.

In January, 1946, the 923rd was disbanded and those of us who remained moved to the 367th Field Artillery on the other side of Nara. I lost my job as cook and was assigned to the battalion switchboard. I started on the night shift when there was little activity to give me time to get used to the equipment. On my first night on the job calls essentially stopped at about 10:00 PM so I turned on the night alarm, a buzzer that was triggered by a call, and went to sleep on a cot in the office provided for that purpose. About 3:30 AM I was abruptly awakened by an incredibly loud siren. I looked out the window to see headquarters barracks blazing away. Of course, the night alarm was buzzing ineffectually and most of the thirty lines had calls waiting for me. I eventually cleared them and called and notified the list of people and groups posted by the switchboard. Last on the list was the Nara fire department. No one answered so I had to dispatch a jeep to go wake them up. When the fire truck finally arrived there was no water pressure.

It was a spectacular fire, especially when the 50 gallon fuel drums stored next to the building started exploding. The building burned to the ground and all the equipment therein was lost, but fortunately no personnel. Early the next morning an officer who identified himself as the fire warden came in to the switchboard office to see me. It turned out he was as scared as I was, because it was also his first day on that assignment. Nothing eventually bad happened to either of us.

On February 15 we were transferred to the 89th Field Artillery stationed in Nagoya. Unlike Nara which had been untouched by the war, Nagoya had been bombed into rubble, and only a few reinforced concrete buildings remained. Most of the time I pulled guard duty or military police assignments. The schedule was four hours on, eight hours off so half the duty was at night. Four hours is an incredibly long time to stand around doing nothing. I tried to devise ways to alleviate the boredom as illustrated by the following excerpt from a letter home. I was pretty cocky then even for an eighteen year old.

May 26, 1946

"Whenever I get guard duty I try to get assigned to the ammunition dump, which is a large factory building in a far corner of camp. It is an ideal setting for a murder mystery. It is filled with cat walks, large ovens, and machines that throw grotesque shadows on the floor where the ammo is stored in pits. There is always water dripping, rats running around the rafters, and other weird noises which add to the spookiness of the place. A lot of nervous guys refuse to stand guard there. Only last week a guard fired three times because he "heard a noise". Some officers won't go near the place at night because not a few have been shot at by trigger happy guards. Myself, I am perfectly at ease there. Everyone knows that I do not walk my post in the prescribed manner and many an O.D. [Officer of the Day] has tried to catch me napping. I am too foxy for them. The secret to my success is in the little device known as the booby trap. I have collected from damaged motors in the building a collection of copper wire varying in

thickness from threadlike wire barely visible in broad daylight to heavy duty wire. Before I retire with a good book in my easy chair situated between two rows of ovens not visible from any entrance to the building, I have the place so wired up that the craftiest of O.D.'s could not get in without first unconsciously announcing his presence.

"I could go on all day enumerating the ingenious devices that I have rigged up. Instead I will give an example of what would have happened last night had the O.D. come snooping around. Coming up the road he would trip the first wire, stretched across the road some distance from the building and leading into a window, dropping a large steel lamp shade onto the concrete floor from a height of seven feet. I would leisurely mark my place in my book and move into the shadows to await his coming. Stepping through the open doorway onto a large metal sheet which makes quite a racket he would trip the second wire. This releases a heavy iron plate which comes crashing down on the open end of a steel drum. About six feet into the building he hits the third and fourth wires. Simultaneously an iron chain swings down banging into a drain pipe and a wire spool comes bumping down a flight of stairs making a terrific racket. If he had tried to sneak in one of the two back doors he would have encountered similar obstacles. A lot of guys have tried to copy my ideas, but most of them don't have the brains to devise sensitive hair triggers and I never give away my trade secrets.

"Last night the lights were out in the ammo dump. I had set up all my booby traps and was standing about fifteen yards from the door with my back toward it when something stepped on the metal sheet and tripped the wire at the door. I whirled around but could see nothing because it was so dark. I called out, "Halt. Who goes there?" but there was no reply. I hollered again, unlocked my carbine and was about to cut loose when a little black dog trotted up to me. He kept tripping wires all night long and just about drove me nuts."

One night in the ammo dump it was bitter cold. I could not get warm. In desperation I found an empty can and took it into the toilet. I ripped out a few pages of the book I had been trying to read and lit a small fire inside the can to conceal the flame. I soon realized that my fire was blazing more than it should have been. Apparently the can contained something very flammable and my little fire was out of control. Lighting a fire in an ammo dump is not a good idea and I raced out to find a bucket of sand provided for just such an emergency and was able to put out the fire. I was scared but I was also warm.

Not all M.P. duty was objectionable as indicated by the following excerpt.

March 30, 1946

"Last night I was on geisha guard and will be on again tonight. There are four yoshiwara districts in this city. They are off limits to G.I's but are kept open for the Japs. There are some 460 girls in this particular area and only two of us to guard it. It is really quite the place. The streets are lined with small open front houses containing anywhere from one to four girls. The girls, dressed in multicolored kimonos, sit just inside their respective houses and try to entice passersby to enter. Needless to say, it required much will power on my part to stay out of trouble."

The most enjoyable guard duty I pulled in Nagoya was in a tank farm on an island in Nagoya harbor we called the fuel dump. It was full of birds and very peaceful. It should have been trouble-free duty, but I found a way to screw up.

May 1, 1946

"I am now at the fuel dump for the fifth consecutive day. I hope I am on tomorrow as we are having a command inspection by General Mullins. We have three posts at the fuel dump. There are telephones at posts one and three but not at post two. Should the O.D. come around post one immediately notifies post three of the intrusion, but post two is usually caught unawares. The other day I found an old Japanese telephone which didn't work but the ringing circuit was in order. I found a spool of wire and we strung a well-concealed line from post three to post two, attached it to the bell and hid it in the ceiling. Using a little hand generator to supply the current, post three can now communicate with post two using a code of rings.

"I wrote that last line about an hour ago. Since then I pulled about the biggest boner I have ever pulled. The guard on post three shot a duck about fifty yards off shore. The wind was blowing the duck further away. Seven Japanese fishermen offered to launch a boat and retrieve the duck. When they got the boat in the water they asked me if I would go along and I said I would. By this time the duck had drifted about three hundred yards, but we finally rowed out and got it. When I got back to shore the other guards were not there. They had my rifle. I gave each of the men a cigarette and proceeded to walk up the beach duck in hand. I rounded the corner of a building and almost bumped into the O.D. and the sergeant of the guard. Switching the duck to my left hand I saluted smartly. I didn't offer any excuses because none would hold water. Fortunately he didn't report us."

In June the 89th was transferred back to Nara, where we spent most of our time training in the field. I was in the wire crew whose responsibility was to set up a field communication network. This consisted of a six-line switchboard with telephone lines running to the gun battery, battery command, the motor pool, battalion headquarters, and the forward observation post which was a mile or more in front of the four 105mm howitzers. Each of the three batteries laid down their own network either by hand carrying a spool of wire or playing it off the back of the truck from large spools. One man would walk behind the truck pushing the wire off to the side of the road. The several separate lines would often get hopelessly tangled. At the end of the field exercise we would have to pick up all the wire. If you found it tangled with another line guess whose wire got cut? The last group to retrieve their wire would find it in a dozen pieces which would all have to be spliced back together before it was rewound on the spools.

One day I cut a bamboo tree about twenty feet long and rigged it with a pulley at one end and a hinge at the other that I could fasten to the side of the truck. We turned the wire spool sideways and fed the wire through the pulley like a fishing pole. With a rope tied to the middle of the pole to raise it when necessary I was able to lay wire well off the road where it wouldn't get tangled at a speed of about 20mph instead of 3mph and with one less person. Our captain was overheard telling another officer what a clever idea it was but he never said a word to me.

I got out of the army in time to spend Thanksgiving at home and entered Cornell in the

spring term of 1947. By this time I was pretty sure I didn't want to be an electrical engineer but, since that was the school that had accepted me in 1945, that was where I had to start. If there was any question about my curriculum choice, Gauss' Law convinced me and I transferred to zoology the following year.

My first term at Cornell was fun. I couldn't take many of the regular electrical engineering courses because I had not taken the prerequisite fall term courses. Consequently I had a lot of electives like ornithology, free hand drawing and foundry. Despite this and my switching to zoology in the spring of 1948 I was able to graduate in three and a half years by taking a heavy course load and going to summer school all three years.

Zoology was more fun than school is supposed to be. The field trips were a lark, and they pointed out for me the best places to see birds or find snakes and salamanders. I kept boxes and a terrarium in my room with as many as fourteen snakes at one time. The salamanders in my terrarium were mostly transients as the snakes would eat them. My favorite snake was a three-foot milk snake that, being a constrictor, would stay curled around my arm quite contentedly. One day I approached Jackson, the black custodian in the dorm, with the milk snake on my arm held behind my back and suddenly reached out saying, "Put 'er there, Jackson". I grabbed his outstretched hand and held on. His eyes bugged out and he struggled to free his hand, and after doing so backed away with his eyes glued on the snake repeating like a mantra, "Oh shit, oh shit, oh shit." He then proceeded to disappear for the entire day.

Unbeknownst to me at the time, another student in a room down the hall would lock his door and stuff newspapers under it to seal himself in prior to going to bed in case any of my snakes got loose.

One of my best friends at Cornell, Bill Rowan, fell in love with a co-ed, Jeanette West, and they planned an August, 1949, wedding in Jeanette's home town of Alva, Oklahoma. Bill asked me to be his best man, so I met him in Scranton after summer school and we drove to Oklahoma in his car, camping on the way. After the wedding I shipped all my good clothes home and set out hitchhiking for the Rocky Mountains.

It took me three days to reach Estes Park. On the way I caught a single ride with a family with two small children all the way from the panhandle of Texas to Boulder, Colorado. I had been sitting in the hot sun for hours trying to get a ride. The family had passed me, but I looked so dejected that they turned around to come back for me. During a stop for supper the most violent hailstorm I have ever witnessed smashed every window in the restaurant and left dents in the trim of parked cars and piles of ice a foot deep in the gutters. It was only dumb luck that I wasn't caught out in the storm.

We reached Boulder about midnight. The family offered to put me up for the night, but I declined and bedded down in a nearby field. The next day I eagerly climbed the first mountain I came to. The following excerpt from a letter home will provide a taste of my mountaineering experience:

August 23, 1949

"I got into a bit of trouble coming off that mountain the day before yesterday. Instead of coming down the way I went up I followed what I thought was a blazed trail. I thought it was kind of rough, but it wasn't until the trail went off the edge of a cliff before I realized that what I thought were blazes was a disease of the pine trees. I got into some pretty tight spots and had a few harrowing slips before I finally made it to the bottom.

"I camped that night by a stream where I shaved and took a much needed bath in frigid water. Yesterday morning I hitched up to Estes Park, where I stocked up on food and a trail map. I got up to the Continental Divide late yesterday afternoon, where I got a big kick out of rolling snowballs in August. I camped by a beautiful little glacial lake.

"This morning was the first time I've been cold all summer. The wind was blowing down the glacier and across the lake and it went right through me. About eight o'clock coming down the trail I saw what was either a small cougar or a large bobcat. I dropped my pack and chased after it but only caught another fleeting glimpse before it disappeared into the brush."

After several days I hitchhiked home, picking up a few memorable rides. The last was for two days in the cab of an old school bus that some guy had purchased in New York state and driven to Milwaukee, where the back of the bus had been removed and a crane mounted on the bed. He was driving it back to New York non-stop. It was a slow trip. Every few miles I had to fill the leaky radiator from a 50-gallon drum we had strapped onto the side of the cab.

He let me off in Denville, where he turned north. It was a Sunday morning, and I looked so disreputable that no one would pick me up for the last fifteen miles home. In desperation I called my mother to come for me. I overheard my father, who had not seen me since June say, "There are buses that run from Denville aren't there?"

I met my first wife, Doris Lelong, at a Young People's Fellowship meeting at St. Peter's church when I was sixteen. The Lelongs had recently moved from the next town, Verona, to the Conovers' house on Rensselaer Road right between Joyce Watter and Nancy Wiggin and across the street from Ralph Tyson, all classmates of mine, Ralph being one of my best friends. My very best friend, Larry Soule, was at the same meeting, and we both took an immediate shine to Doris. We walked her home after the meeting and soon began a friendly competition for her affections. Larry eventually gained the upper hand, but Doris and I remained close friends. She stayed in Verona High School for her senior year while I went to Montclair High, but we rode the same school bus and sat together each day until she got off the bus in Verona. In those days the school bus was strictly segregated by choice - boys in the back, girls in the front. I would be subjected to considerable derision for saving a seat for Doris in no-man's-land in the middle of the bus on the way home from school.

Toward the end of my senior year in high school I had my only other serious girl friend, Fran Miller, a schoolmate from Montclair. Every Saturday night Fran and I would go to a local night club and drink rum cokes (an awful concoction, but we didn't know any better) and dance. On the way home we would park and neck. I used up the greater part of my mother's wartime

gas ration, one gallon a week, on my dates with Fran. She was a great dancer and necker, but I soon realized she could do little else and I became bored with the relationship.

Doris entered Simmons College in Massachusetts that fall and I continued to correspond with her regularly. She gave me an 8x10 photograph of herself and I kept it close to my bunk throughout my army career. When I returned home from the service she was the first person I looked up. When I asked her to go out she informed me that she was going steady with Ralph Tyson. That romance eventually foundered over the bridge table. Ralph was a shark and Doris was a social player. His criticism could be laced with biting sarcasm, and Doris decided she didn't need that.

We continued to keep in touch during college years. In my junior year at Cornell I invited her up for Spring Weekend. I didn't know it at the time but she was considering a marriage proposal from Paul Ostergaard when she received my invitation. I didn't even know she had a boyfriend in Massachusetts. Had I not written she might have married him. At any event, Doris accepted my invitation and turned Paul down. He eventually married Jackie McKnight, Doris' roommate and maid of honor at our wedding.

Several more weekends at Cornell cemented our relationship and one morning, while watching the sunrise in one of the many state parks around Ithaca after having stayed up all night, we decided to marry.

Doris had the good sense to realize that her parents were sufficiently old fashioned to not accept being excluded from participating in this decision, and she insisted that I request her father's permission. Although I liked Arthur Lelong, approaching him to ask for his daughter's hand in marriage was a daunting proposition. It was with considerable trepidation that I raised the subject one night after Doris had retired early as planned. His only concern was how I expected to support his daughter with a bachelor's degree in zoology. We were to have many spirited discussions on this issue, which was finally resolved when I was accepted for graduate study in biochemistry at Rutgers.

Doris and I were married on December 23, 1950, during the Christmas break at school. We spent our honeymoon in the Poconos at Splitrock Lodge skiing, a bad idea. She took a nasty spill, breaking her ski poles and goggles and suffering a bruise on her hip the size of a dessert plate that was black and blue for a month. Release bindings, of course, had not been invented yet.

At dinner our first night at the lodge we were seated with a young couple named Norm and Trudy Larson. We both played at projecting the image of old married couples until about dessert one of us asked 1 he other how long they had been married. It turned out that they too had been married the previous day. Furthermore, they both taught school, shop and home economics, at Metuchen High school, a block from where we were to' move into the second floor of a house on William Street and live for two and a half years.

Doris was a good sport and put up with my outdoor activities, albeit with less than my

enthusiasm. In the summer of 1952 when she was pregnant with Betsy we spent four days and three nights canoeing down the Passaic River. Her father helped put us in above the Great Swamp and picked us up in Singac where the river crosses Route 23. During the entire trip we hardly saw a soul except where the river crossed or ran next to a road. Even through Summit and Chatham a strip of woods along the river was sufficient to screen us from view. We slept on the ground without a tent and cooked over an open fire. It was a great experience even after breaking both paddles trying to avoid almost certain destruction by a broken dam below Chatham. Much of the river was familiar to me having spent many hours canoeing through Hatfield Swamp, the locale for much of Uncle Art's book, Modern Wilderness. Unfortunately, Doris became covered with poison ivy.

That same summer we were the first occupants of a lone fishing camp on the shore of Moose Pond in the Adirondack Mountains recently purchased from an old codger by Uncle Collie. The cabin was filthy and we spent the entire first day cleaning it out. For a week we never laid eyes on a single person. We canoed on the lake and hiked in the woods and had a wonderful time. I tried in vain on several occasions to locate the source of an intricate bird song that I had never heard before. The bird eluded me every time even though I appeared to approach it closely.

Some years later at a meeting of the New York Academy of Sciences I attended a lecture by Dr. Peter Paul Kellog, one of my ornithology professors at Cornell who had pioneered the recording of bird songs in the field. He played the song of the winter wren, a bird I had seen often during migrations but never heard. It was my elusive Moose Pond sprite! I was thrilled. I would later use Kellog's recording of the winter wren in a "Moment of Fellowship" at the Morristown Unitarian Fellowship. Played at normal speed it is a five-second series of complex high trills that almost sounds squeaky. Slowed down to half, quarter and finally one eighth speed the song became incredibly musical, at first thrush-like and finally eerily haunting. My message to the congregation was that there is much wonder and beauty in the natural world that our limited senses cannot fully appreciate except through the use of technology to expand our senses.

When I graduated from Cornell in 1950 I still intended to become a zoologist. I had taken biochemistry in my senior year and I found this subject also fascinating. At any rate there was no escaping graduate school. I applied in zoology at Cornell and Princeton and in both zoology and biochemistry at Rutgers. The only school that accepted me was Rutgers and only in biochemistry. So that was how I became a biochemist instead of a zoologist.

The master's program was only a year, so in June of 1951 with a M.S. degree and a new wife to support I went looking for a job. I found a laboratory position at Squibb, which I verbally accepted. Before I started, however, Jim Allison, the Director of the Bureau of Biological Research at Rutgers, offered me a $5000 fellowship to stay on for my Ph.D. Out of this I would receive a salary of $2000 a year. This was an offer I could not refuse as most Ph.D. candidates were on teaching assistantships which paid $1400 a year and required one third of their time. I accepted without hesitation (and without consulting Doris), even though I had not intended to get more than a master's degree, and informed Squibb that I would not be joining

them.

I was the first McCollum Fellow at Rutgers. This was made possible by a gift from Arthur McCollum, the inventor of Flako Pie Crust. Because I wasn't required to spend any of my time teaching or assisting professors with their research I was able to satisfy the Ph.D. requirements in only two years. Doris was working at the telephone company, and we actually saved money in graduate school. Of course we lived very modestly; we didn't even own a car until shortly before Betsy was born.

In April 1953 I flew to Chicago, my first plane ride ever, to attend the annual meeting of the Federation of American Societies for Experimental Biology. My main reason for going to the FASEB or Federation Meeting as it was usually known, was to sign up for the placement service that the society sponsored. I was to receive my PhD degree in June and I needed a job. Jobs were not plentiful. I ended up with a postdoctoral fellowship at the University of Iowa that paid $4,000 a year.

Around the first of July Doris, Betsy and I left New Jersey in the old Chevy my father insisted on selling me the previous year for $600, the trade-in value the dealer offered him when he purchased a new car. It was a long, hot trip and we arrived in Iowa City in the middle of a terrible storm that blew down most of the few trees available to be blown down. Iowa, like all the prairie states, is essentially treeless save for those planted by the settlers.

We moved into a student housing project not far from the campus; concrete floors, corrugated iron walls and roof with tiny windows so high Doris couldn't see out. Our second floor apartment in Metuchen was luxurious in comparison.

Bob and Maxine Whiteside, who would become our best· friends, had a daughter, Marsha, a couple of years older than Betsy and a younger son. They lived two units away, so to save on baby sitters we wired an intercom between the two houses so that we could visit each other and monitor the kids over the intercom. When it rained the noise of the rain on the roof was deafening, especially over the intercom, the volume of which was turned to maximum to detect the slightest peep from a small child.

Bob was a resident in oral surgery. However, the free dental services he provided were more than offset by the experimental procedures he subjected me to. He pulled an infected molar, which I'm still not convinced was necessary, and subsequently transplanted a tooth he had previously extracted from a co-ed. The girl's tooth took hold and for months was the best tooth in my mouth. After about a year, however, it was rejected as was predicted by my dentist in Morris Plains.

My boss at the Radiation Research Laboratory was Ted Winnick, a biochemistry professor in the medical school. His wife, Ruth, also worked in the labs as did Reinhold and Ruth Bennisch. I would find out that academe is a lot more tolerant of these relationships than is industry. This was my first exposure to radioisotopes, and the work was a logical extension of my doctoral dissertation on protein metabolism in tumor-bearing rats. It involved the use of

amino acids labeled with ^{14}C and was primitive in retrospect.

Iowa was a depressing place to live. The weather was abominable; winters were bitterly cold and summers frightfully hot and the transition from one to the other seemed to occur overnight. It was so dusty from so much of the land given over to corn that it would on occasion rain mud, the initial drops picking up enough soil particles in their descent that they would leave dirty splotches on an otherwise clean car.

Ted Winnick sparked my interest in mineralogy, an interest that had remained latent since I studied geology at Cornell. Iowa is not a Mecca for minerals but I spent many weekends scouring gravel pits for flint and jasper. One day I read a letter in Mineralogy magazine that mentioned a concentration of geodes in a gravel layer in the Skunk River in southern Iowa. Geodes are prized by rock hounds. They are formed by the slow deposition of minerals into a cavity in the bedrock from saturated solutions in ground water slowly percolating through the rock, the cavity having previously been formed by dissolution of the bedrock by the very same ground water of a somewhat different composition many eons previously. If this process is interrupted prior to the cavity being completely filled with deposited minerals, and if the surrounding bedrock is subsequently uplifted and eroded away, and if the mineral deposited in the cavity is more resistant to erosion than the surrounding bedrock, what was the cavity is now a hollow rock which when broken or cut open reveals a sparkling array of beautiful crystals of the deposited mineral. Oftentimes more than one type of crystal can be formed within a single geode if the composition of the ground water changed sufficiently during the formation of the geode.

Armed with no more information than the brief reference in the letter I located the Skunk River on a map of Iowa and set out one Saturday to find geodes. At every juncture of road and river I got out and explored up and down the river examining the exposed banks for the gravel layer. Late in the afternoon and defeated I crossed the river one last time on the way home. I glanced upstream and noticed a small rock island a couple of hundred yards away that another foot of water would have completely submerged. Could this be the elusive gravel layer? I turned around and parked as close to the island as possible. Grabbing my geologist's pick I wrestled through dense riparian woods and water almost to my waist to reach the island. I hammered away at one rock after another without success. Then I noticed a rough and roundish rock in the shallow water. It felt lighter than it should and when struck with the hammer split open to reveal two hemispheres covered with brilliant quartz crystals! Knowing now what to look for I found many geodes. Most were completely filled with quartz and I left these. I gathered as many as I could carry and headed back to the car and home, a happy hunter. I returned to the rocky island several times before leaving Iowa. These geodes are still the cornerstone of my rock collection.

Before we moved to Iowa my father arranged for me to meet with Ulrich Solmssen at his home in Essex Fells. He was then vice president for research at Warner-Chilcott Laboratories in Morris Plains, New Jersey. In August of 1954 he called me up and offered me a job at the stupendous salary of $6,200 a year to be a Senior Scientist working for George Phillips, the Director of Biochemistry, whom I had never met. This was a 38% increase over my

salary at the time and I accepted. I felt guilty about leaving Ted Winnick shortly after starting a second fellowship year but not enough to stay.

Doris, Betsy and I drove back across the country and took up temporary residence in my parents' home in Essex Fells. This normally would have been a terrible idea, but they were scheduled to take an extended trip around the world so most of the time we had the house to ourselves. My big salary persuaded us to both start another addition to the family (Jim was born nine months later) and buy our first home. We found a small development only five minutes from work called Idlewilde Country Club Estates. The estates were only one third of an acre and the only country club in evidence was a small, mud bottom swimming hole generously called Sylvan Pool which we would be allowed to join. But there were many families with small children and it would turn out to be a great neighborhood for raising children.

The house we chose was under construction and would not be finished until December. I persuaded the builder to knock $1,500 off the $21,750 price tag by leaving the upstairs with just the rough framing and eliminating the porch. Over the next couple of years I would put in two bedrooms and a bath and build a much better porch than he had planned. These projects gave me a taste for house construction that would inspire me fifteen years later to attempt to build an entire house of considerably greater size and complexity.

While Doris and I had a traditional marriage ceremony in the Verona Methodist Church, we did not attend any church for the first few years of our marriage, a concession to me that I didn't fully appreciate at the time. However, when our children were little she insisted that they be exposed to Sunday school. About this time, 1955, Marshall Deutsch, a colleague of mine at Warner-Lambert, was agitating for the establishment of a Unitarian Fellowship in the Morristown area. I knew nothing of Unitarianism at the time and became intrigued with the idea of a religion with no creed or prescribed set of beliefs that welcomed all free thinkers. We became founders of the Morristown Unitarian Fellowship, and I was elected its third president in 1958. My inaugural sermon was a plea for conservation and population control, not popular subjects at the time. I quote the last paragraph of that address:

"It lies within man's power to hold his own numbers to a level that will insure the maximum spiritual and material benefits for this and future generations. This is a challenge and an opportunity never before presented to any species on earth. It is up to us to use our God-given intelligence and meet this challenge. The greatest moral responsibility mankind can assume is to provide for the welfare of future generations. When we continue to indulge in irresponsible fertility, eradicate entire species of living organisms, and depredate our natural resources which cannot be replaced by any amount of technological ingenuity, we are forsaking that responsibility."

Around President's Day in 1959, riding home from the Fellowship I asked Betsy, just turned six, who was our first president. She responded, "George Washington." When I asked her who was president today, she said, "You are."

I did my best to promote an appreciation for nature amongst the Fellowship members.

Every year around May 15 I led what became known as the annual "Giant Bird Walk", a several hour trek through the Great Swamp and Troy Meadows starting at 5:00 AM. These became very popular and attracted a large group. Jean Baker and I took the Fellowship teenagers on weekly natural history excursions in the spring, including a canoe trip down the Passaic River, that were very successful.

Early one Sunday morning Chris Street and Bob Hoen showed up at our door carrying a long 2"x12" plank. Chris while walking his dog had earlier come upon a deer trapped in a deep pit above Mountain Way School. Their plan was to use the plank as a ramp up which the deer could escape. Somewhat skeptical of this scheme I picked up a coil of rope and joined them. The deer was obviously exhausted from its frantic and futile efforts to jump out of a concrete pit about 12-14 feet in all dimensions which I assume was a cistern at one time. Its muzzle was bloody from repeated violent contact with the walls. Chris and Bob lowered the plank which even at about a 30 degree angle came only half way up the vertical wall.

I made a noose out of the rope and repeatedly failed on attempts to lasso the animal. Frustrated, I was about to climb down into the pit on the rope and tie up the deer for hoisting out, an exceedingly foolish action, as even in her exhausted condition she could have cut me to ribbons with her sharp hoofs. Then I got the idea of dropping a noose on the floor of the pit and snaring the animals' legs as she stepped into it. She assiduously avoided the noose as long as she could see me holding the rope. I then backed off out of sight and sent Chris and Bob to the other side of the pit. Upon their signal I jerked the rope as hard as I could. on the very first attempt I caught both hind legs cleanly above the hocks. The three of us quickly hoisted her out upside down. Immediately Chris and Bob took off, facing the prospect of a very large animal lying on the ground but representing the prospect of sudden violent activity. But she lay there quietly while I untied her feet and then stood up and ran off into the woods.

This would not be the only time I would save a helpless animal from certain death. Arriving home to our house on Old Mill Road one winter evening years later I heard the yelping of a dog that had broken through the ice on our pond. I quickly donned my hip boots and broke a path through the ice to the center of the pond to grab the exhausted dog as she was about to go under. Another night we were attracted to frantic barking and found a large black dog completely mired in mud up to its chest in a slough about 100 yards from the house. Again armed with hip boots while Susan held a flashlight on the scene I waded in and had to extract the dog's legs one at a time before I could carry him to safety. Of course, there were Snippy and Snappy, the baby raccoons that fell out of the den and were ignored by their mother. But that is another story.

Jim Chilcott purchased the Maltine Company that manufactured, among other things, the elixir of the same name, moved the company from Brooklyn to Morris Plains, New Jersey and changed the name to Chilcott Laboratories. In the early 1950's he merged with the William R. Warner Company and it became Warner-Chilcott Laboratories. About the time of my arrival a larger pharmaceutical company, Lambert Pharmacal, merged with Warner-Chilcott and the parent corporation became Warner-Lambert Pharmaceutical Company, dropping the Pharmaceutical name in the early 1970's.

While the main business of all these companies was pharmaceuticals, each had acquired one or more laboratory products that top management didn't understand. Among these were such diverse products as Evans Blue dye, a sterile solution for the in vivo estimation of blood volume; Ketodase, a glucuronidase solution used in urinary steroid determinations; Inulin, another sterile in vivo product used to measure kidney glomerular filtration rate; and simplastin, a thromboplastin preparation used to determine prothrombin times. Responsibility for marketing this motley array of products was given to Raphael Cohen with the creation of the Laboratory Supply Division of Warner-Chilcott, the progenitor of General Diagnostics. Mainly with products I developed, General Diagnostics would become a leader in the emerging diagnostics industry.

At that time clinical chemists prepared almost all their reagents themselves, a practice that is forbidden by the Food and Drug Administration (FDA) today. The concept of quality control, well established in pharmaceutical companies, was unheard of in the clinical laboratory. There was little uniformity in procedures between laboratories and most physicians looked upon laboratory tests as merely confirmation of a tentative diagnosis. My first project was the development of a lyophilized normal human serum with known analyte values that could be used for comparing day-to- day and lab-to-lab results, a procedure now mandated by the FDA but unheard of in 1954.

The day I arrived, George Phillips presented me with an assistant, Sylvia Malament, a woman with a foul mouth and terrible temper that couldn't get along with anyone. She hated Jane Lenahan who shared a lab with us. George went so far as to commission the construction of a plastic divider to separate the common refrigerator into Jane's side and ours. Sylvia and I got along splendidly. I also got along well with George mainly because he pretty much left me alone. The main problem I would have with him was his practice of unjustifiably attaching his name to scientific publications of mine, a right he assumed as head of the department.

Every couple of weeks Sylvia and I would visit all the local hospital blood banks collecting the outdated units of blood that would otherwise have been discarded. This became our source of raw material for the control serum. We devised procedures for adjusting the levels of constituents of clinical interest to any desired level. The first commercial normal human serum control, Versatol, was introduced in 1956. This was soon followed by whole series of Versatol products with abnormal levels of various analytes including enzymes. It was a very successful product line that dominated a rapidly growing market.

While working on Versatol-E, our elevated serum enzyme control, I became impressed with the complexity of most enzyme analyses. Enzymes are catalysts and are usually measured by determining their effect on the rate of a biochemical reaction. Serum enzymes can be very specific indicators of organ damage. I developed simple, patented procedures for many of them: alkaline phosphatase (bone and liver), acid phosphatase (prostate), aspartate aminotransferase (heart), lactate dehydrogenase (heart and liver) and amylase (pancreas). Ten different reagent kits that allowed even the most unsophisticated laboratory to perform these assays reliably was my most significant contribution to the science of clinical chemistry and was the contribution

for which I am best known.

When I started working in clinical chemistry every test in the hospital laboratory was run manually; automation did not exist. Then in 1954 Leonard Skeggs invented a technique which was introduced in 1957 as the AutoAnalyzer. This simple device, initially disdained by clinical chemists, would rapidly revolutionize the hospital laboratory. Skeggs had a difficult time peddling his invention. All the major companies he approached turned him down, including, I would only later learn as I had not been consulted at the time, Warner-Lambert. Skeggs finally persuaded Technicon, a small laboratory instrument company in Tarrytown, New York, to take it on. In 1969, Jack Whitehead, the president of Technicon, would become an instant billionaire when he sold 5% of the company in a public offering of stock that immediately was bid up to a value of $57 million.

By 1964 Warner-Lambert management finally realized that laboratory automation was the wave of the future and acquired a small company in Richmond, California, called Research Specialties which had developed a system called the Robot Chemist. Unlike the continuous flow approach of the AutoAnalyzer that would enjoy incredible success and dominate the automated chemistry analyzer market for many years, the Robot Chemist was a discrete sample analyzer. As such it was the progenitor of all automated chemistry analyzers in existence today, long after the demise of continuous flow. Had Warner-Lambert not totally misunderstood and mismanaged the business they might have dominated the laboratory market instead of failing miserably.

Instead of making the Richmond operation part of General Diagnostics and giving the responsibility to Ray Cohen, Bob Clark made it a separate division of Warner-Chilcott Laboratories. Ray tried to persuade me to move to California to be in charge of reagent development for the system, but I refused. He then sent Steve Koziol out to be his man on site. Every six weeks I would fly to San Francisco to spend a few days with Steve and the engineers reviewing the progress on the instrument redesign and the reagent development effort. This was duty that I thoroughly enjoyed. San Francisco at the time was an exciting city where I felt totally safe walking around at any time of night. I would walk from the hotel to the Powell Street cable car, ride to top of the hill and walk down to Grant Avenue where I explored Chinatown and its environs.

One of my favorite haunts was a watering hole called the Bocce Ball. I would sit at a table sipping Venetian coffee and listen to some very professional opera singers on a stage. Now I am by no means an opera buff. I once sat through an entire performance of *Cavalleria Rusticana* and, except for the intermezzo, it was dreadful. However, at the Bocce Ball they only sang requests and favorite arias. One of the performers was an attractive contralto of whom I would always request, “Mon coeur s’ouvre a ta voix” from Saint-Saens’ *Samson and Delilah*. I would sit there listening to this incredibly beautiful music with tears streaming down my face.

The Robot Chemist had excellent temperature control and a double-beam spectrophotometer. I wanted to promote it as an enzyme analyzer rather than a general chemistry analyzer, as this was an area where we easily outshone the AutoAnalyzer, but

management wanted to go head to head with Technicon. But we could not compete with the AutoAnalyzer's ability to perform multiple assays simultaneously merely by splitting the flowing sample stream. Management of the division was in chaos. Ray Cohen threatened to resign if Bob Clark didn't give him full responsibility for the instrument. Clark surprised him and accepted his resignation. Ray subsequently failed in several ventures, became impoverished, and died a broken and forgotten man. Chris Matthews, the president of the division, left, and responsibility for the instrument manufacture was transferred to American Optical, probably the most old-fashioned and conservative division of Warner-Lambert. This was the death knell for the Robot Chemist, a tragedy in retrospect as today automated analytical systems constitute almost the entire in vitro diagnostics business.

In 1967 I was made Director of Diagnostics Research, a new department formed from Jane Lenahan's and my groups in Biochemistry and Don Kronish's group in Microbiology. The same year Susan got a master's degree in biochemistry from West Virginia University and took a position in the Pharmacology Department at Warner-Lambert. She developed a serious allergy to the rabbits she was working with and transferred to Diagnostics Research in 1968 to work for Bob Megraw.

In early 1970 Bob was having marital problems and started chasing Susan around the lab. To salvage a potentially bad situation I made her my personal assistant. We soon developed a productive scientific collaboration as well as a growing personal relationship. In the meanwhile Susan's marriage to David Tenney collapsed, never having had a solid foundation.

My marriage to Doris had not become really bad but it was not very exciting. Still I had no thoughts of terminating it when my affair with Susan started. But here was a young, intelligent, attractive woman who shared my interests and loved animals and nature and me. I saw the brass ring going by and I reached out and grabbed it.

Telling Doris that I was leaving her was the most difficult thing I have ever had to do. Telling my mother was the next most difficult. A week or two later, October, 1970, I moved into the small house that Susan rented on Old Mill Road in Mendham Township. We were married on May 15, 1971 by Clark Olson, the minister of the Unitarian Fellowship, in a small ceremony in our front yard which we had written ourselves but not shared each other's part until the actual ceremony.

After the guests left Susan and I cleaned up, put the canoe on top of the Ford station wagon, and headed for Brigantine National Wildlife Refuge. We spent our wedding night in a heavy rain sleeping in the back of the station wagon. When we awoke a cat who had somehow managed to get in through the partially open rear window was asleep on the cooler above our heads. We saw a lot of birds and had a wonderful canoe trip down the Wading River which was swollen with spring rain. The water was warm for May and we swam naked in the clear flood waters. It was a wonderful honeymoon.

Susan and I wanted our own home. The year before her divorce she and David purchased six acres of woodlands adjacent to their rented property. We surveyed all the

available vacant lots in the area and none compared in quality or price to the lot next door. We managed to purchase it back from David (Susan took nothing in the divorce settlement) for only a modest increase in what they paid for it. The first thing we did was hire a backhoe to dig out a spring-fed marshy area and convert it into a pond. Our house would overlook the pond.

One of my dreams had always been to build my own house. I had added a porch and finished two rooms and a bath in our house on overlook Trail. I had built a deck onto our house on Junard Drive. Those projects were nothing compared to what Susan and I would undertake. One evening while we were lying in the hammock I asked her with great trepidation If she would consider building our house ourselves. I was overcome with joy when she enthusiastically supported the idea. We would not only build it, we would design it.

That year I went through reams of graph paper trying out one design after another. What finally emerged was a split level slab house with a five sided, cathedral ceiling, living room-dining room-kitchen. When I showed these plans to the Mendham Township building inspector, a retired printer, he scoffed at them and said I had to have an architect draw them. (He was wrong about such a requirement, but I didn't know it at the time.) So I approached Bill Halsey, an architect acquaintance, who was dumbfounded when I presented him with not only a complete set of plans and elevations but also a scale model of the house. But Bill was very helpful in the selection of windows, siding and other aesthetic details.

I bought a chain saw and in the fall of 1972 we started clearing the trees for the house lot as well as the 600 foot long driveway. This was back-breaking work but fun. For the next three years we would do little else except work on the house. Certain jobs such as excavation, well drilling, installation of the septic system, we contracted out. We also hired Joe Herman, a Hungarian stonemason who didn't speak English, to do the masonry, Bob Alpaugh to do the plumbing and Wayne Nolting to put up the sheet rock. Almost everything else we did ourselves. We did all the rough and finished carpentry, all the wiring, all the painting. It was a real adventure, an experience I would not have missed but one I would never do again.

While building the house we allowed ourselves few diversions. One was seining for fish. For many years I had kept at home and in the lab at Warner-Lambert fresh water aquaria which I stocked with native fish caught with a seine in local brooks and ponds. Susan took to this activity with unbounded enthusiasm. Frequent restocking of the tanks was required owing to the high mortality rate of the fish in captivity. You never knew what you were going to catch. We had black-nose and long-nose dace, common and golden shiners, common and chub suckers, johnny darters, mudminnows. It was lots of fun.

Another activity we indulged ourselves in while building the house was canoeing on the Black River. One spring day we swiped half a dozen eggs from a wood duck nesting box. We hatched them in an incubator and kept them in a box in the house. When they got too big for the house I built a cage in the back yard with a pond of two-by-fours lined with polyethylene. They loved this little pond and would swim under water from one side to the other. When they were almost full grown we released them on the real pond. By this time we were down to two females and a male we named Woody. After they learned to fly they would disappear during the day.

But in the evening when we were working on the house they would fly in, circle once or twice and drop like stones into the pond. It was a great sight. On the first day of hunting season they flew out in the morning and failed to return.

Of all the wildlife that we got to know intimately raccoons would become our favorites. After we were in the new house Susan started putting food outside the bedroom door in the evening to attract them. At first they were very skitterish and would dash off if we made any noise. Eventually they became more tame and would take a proffered dinner round from our hand and even come a few feet into the room to get it. None of this prepared us for Missy and Rascal.

One Saturday morning we were awakened by two small raccoons at the bedroom door. We opened the door and they walked right in. We fed them raisins and played with them until we had to leave for a noon wedding and I reluctantly put them outside. When we returned they were still there. After more raisins and play they abruptly fell asleep on the floor, an instantaneous transition from frolicking action to lifeless lumps. I arranged a wood duck nesting box on the wood pile outside the bedroom and stuffed their inert bodies inside. We named them Missy and Rascal.

Over the next several weeks I would enlarge the hole in the nesting box several times to accommodate their growing bodies. Before long they abandoned the box altogether and more or less assumed the nocturnal habits of the other raccoons. Every evening they would show up to be fed and played with. They would search us out usually by climbing up to the deck off the dining room. If they returned late at night they would signal their presence by climbing to the top of the bedroom screen door and chirring. What we regarded at first as a cute antic began to wear thin after Missy learned she could chew her way through the screen and gain access to the supply of dinner rounds we kept in a large plastic bucket. Often we would be awakened at night by the sound of Missy in the bedroom trying to pry the lid off the bucket. Or we would be eating dinner and Missy would appear at the dining room table. I was replacing door screens weekly.

Rascal disappeared after the first winter but Missy stayed around for two years bring her babies in the late spring. She was a beautiful animal and a delightful companion.

One morning in the spring of 1984 we were awakened by the sound of crying raccoons. There was a den tree about 75 feet from the house. We went outside to find two tiny raccoons clinging to the tree outside the den hole while the mother perched in a crotch several feet higher in the tree. While we watched one of the babies fell about 30 feet to the ground. The second one soon followed. Both appeared unhurt so I put them in an open box under the tree where the mother could retrieve them if she was so inclined. We both had to leave for work and in the afternoon they were still in the box. We had no choice but to take them in.

Unlike Missy and Rascal these tiny babies were not weaned. We fed them commercial bitch's milk through a baby bottle several times a day. If neither of us was going to be home that day one of us would take the raccoons to work. One day I took them to Ortho [Diagnostics] where I was consulting. I had one of the directors write a package pass which was required to

carry anything out of the building. The guard looked at the pass which said, “Two raccoons” and never cracked a smile.

We were determined to acclimate Snippy and Snappy to an independent existence and turned them loose as soon as we felt they could make it on their own.

At the time we started building our house I had been promoted to Director of Diagnostics Research and Development. Jane Lenahan and her group had been transferred to marketing in 1968 and for several years we had no research effort in blood coagulation products. Marketing finally realized they were losing market share as a result of no new products in this area. I put Susan in charge of coagulation research and, with no prior experience, she soon became the most productive group leader in the department. This productivity, wrongly attributed to me, only exacerbated the petty jealousies that were building in the department against her.

When I was promoted to Vice President for Research and Development in 1976, then president, George Masters, wisely insisted Susan not report directly to me. Soon thereafter she transferred to a corporate position and eventually quit to work for MetPath.

The highlight of my General Diagnostics career was in 1975 when I was awarded by The American Association for Clinical Chemistry the Gerulat Award for outstanding contributions to clinical chemistry. This award was especially gratifying because when I joined the AACC in the late 1950’s there was strong opposition to even allowing membership to industrial scientists. In terms of number of new products that made it to the marketplace I was probably the most productive scientist in all of Warner-Lambert. As late as 1980 and despite numerous product and company acquisitions, products developed by me or under my immediate direction accounted for over 80% of General Diagnostics’ sales. I had sixty scientific publications and twenty five patents assigned to Warner-Lambert. I also had management responsibility for over fifty people that were about as productive as I had previously been with five. Top management decided that while I was unquestionably a good scientist I was a lousy manager. Since I had had a lot more fun and satisfaction as a group leader than as a vice president they were probably right. Without my prior knowledge they hired a senior vice president above me to manage the worldwide diagnostics research efforts which then included Nuclear Medical Laboratories in Texas and a research group in Germany. I couldn’t believe the company I had devoted twenty five years of my life to could treat me so shabbily.

Bob Schiff was the kind of individual to whom any perceptive person would take an instantaneous dislike. One of my key managers, Ken Sumner, quit almost immediately. While I was considered by many to be the father of General Diagnostics, I soon realized this was the end for me. There would be no gala retirement party that I had often imagined. My distrust of Schiff would be vindicated when within two years of his coming he and all the people he brought with him from Roche would be summarily fired, escorted out of the building, when he was caught lying about research data that showed a high profile cancer test that was under development was not going to work. In 1985 the entire Diagnostics Division was sold by Warner-Lambert to a Dutch firm, Organon Teknika, and moved by them to North Carolina.

I did not quit abruptly like Ken but carefully planned my exit. George offered me severance if after a few months of trying to work with Schiff I chose to leave. I knew then the exact day I would quit: the day the severance would carry me beyond my fifty-fifth birthday when I would be vested in the Warner-Lambert medical plan.

I briefly considered and rejected the idea of looking for a position in another company. Warner-Lambert had become unbearably political in recent years. I had no reason to believe any other large company would be any different. The idea of being my own boss was very appealing. I made the decision I should have made ten years earlier: to start Babson Research Laboratories. On January 2, 1981, I went to work in the lab I had constructed and provisioned mostly with discarded but working instruments and chemicals from Warner-Lambert. I had the luxury of my severance to tide me over until I became self sufficient.

Back in 1978 when Susan and I were both gainfully employed, me as a vice president at Warner-Lambert and she as Marketing Manager of Research and Veterinary Services at MetPath, our accountant told us we had to have a tax shelter. A second home would do nicely. We had spent two lovely weeks in Virgin Gorda at the Biras Creek Resort that year and decided on the Caribbean for a second home. Susan did a lot of research at the library and we narrowed down our choice to the BVI and St. Martin. She had written to the Governor of the BVI and he passed her letter on to Tom Smith who was trying to develop an isolated section of Virgin Gorda. Tom invited us to visit as his guest.

With my brother Norton and his wife Lynn we spent a few days in St. Martin and a few days in Virgin Gorda. It was no contest. on our next visit to Tom we put a deposit on a lot with a private beach and a magnificent view down Sir Francis Drake Channel with islands on both sides.

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We couldn't appreciate the view at the time because the vegetation was so thick you couldn't get onto the property. Tom had a couple of his workers hack a path with machetes up from the beach to a fairly level ledge which looked like a good place to site a house. I designed a narrow house that would fit into the hillside. Tom's crew of men would build it.

When Peter Weichers, Tom's new partner, started clearing the driveway with a bulldozer he discovered a much more level and suitable house site higher up the hill. However, because of the location of some magnificent trees, my house design was inappropriate for this site. Peter was anxious to get on with the job and so were we. Susan called Bill Halsey and told him to get on the next plane to Virgin Gorda, we would pay for the trip. Bill arrived the next day and within two days he and I designed the house that we would build.

Unfortunately, when only the foundation for the cistern had been completed Tom got sick with the cancer that would ultimately kill him and all of his projects came to a halt. Nothing was done for over a year and Tom sold his entire property to a group from Connecticut. This was in 1981. Susan and I had both quit our jobs and we needed a tax shelter like we needed a hole in the head. Susan was trying to sell real estate for Jack Turpin at a time when real estate

wasn't selling at all. She had met and immediately liked Sue Woods, another Turpin agent. She asked Sue if she and Jack, neither of whom I had yet met, would be interested in becoming our partners in the Virgin Gorda house. We brought them down for a site visit and they immediately fell in love with the place as we had. Sue and Jack not only became our partners but also our best friends.

The Woods were great practical jokers and got us going many times. But they were never able to top one that we pulled on them. Sue had given us a cloth parrot that we hung in our entrance at home. One day Susan delivered our Virgin Gorda house key to a man in Mendham. Instead of writing her a check for the week's rental he peeled off fourteen one hundred dollar bills. We decided this was a good opportunity to turn the tables on Jack and Sue. I had a tiny zip-lock bag with foreign writing on it that just fit the neatly folded bills. We called them about eight o'clock and said we wanted to come over. On the way we concocted a scheme in which we would play good cop-bad cop. Our plan was to accuse them of planting the money in the parrot.

After going around and around about the parrot and the trick they allegedly played on us, but without identifying it, it became apparent, of course, that they didn't know what we were talking about. At that point I said to Susan, "Let's go home. There's no trick." However, she insisted that we tell them what happened, so I related how while pruning the fichus tree I noticed a tear in a seam of the parrot with a corner of plastic protruding. Closer inspection revealed the money bag which I now reluctantly produced for Jack and Sue. Sue got all excited because her sister had purchased an identical parrot. She immediately called Joan to have her check her parrot for hidden money.

Susan then said that since Sue had bought the parrot we should share our ill-gotten gain with them. I said no way, and Susan and I argued over the justification or lack thereof for sharing the money. Eventually I relented and then we argued about how large a share should be given to the Woods. They, of course, were extremely embarrassed by the whole affair. While they obviously would have liked some of the money they couldn't in good conscience advance their position. After about an hour of this charade we told them the truth and gave them their $700 share. They haven't tried a practical joke on us since.

One of my first mistakes at Babson Research Labs was "Litter Sitter", an ammonia detection paper strip that would signal by a color change when cat litter required changing. I tried to market this by mail order under another company name which I had registered, Professional Pet Products. It wasn't that Litter Sitter didn't work, it wasn't really necessary and a large waste of my time and energy.

You would think this fiasco would have convinced me to stick to a market I knew, the clinical laboratory, but it didn't. I was attracted to forensic chemistry because it was also outside the jurisdiction of the FDA. I developed an excellent field test for seminal fluid which was favorably evaluated by the Morris County Rape Squad, but I was unable to break through the ingrained procedures that characterized the forensic market.

I also spent a great deal of time trying to develop a paper strip test for salivary alcohol. Although I was unable to achieve the necessary degree of quantitation with the paper strip assay, I was successful in getting a $50,000 SBIR grant (Small Business Innovation Research) to develop an instrument for quantitating salivary alcohol collected on a filter paper strip. This project was successfully completed with the help of an electrical engineer I knew at Ortho [Diagnostics] who provided the electronics and software to run the instrument. While I never pursued this beyond the Phase I SBIR grant, an alcohol test would later become my most lucrative product.

Consulting was another source of income and, while not as much fun as product research, also provided needed contacts. I had sent out fliers announcing my availability. In March 1982 Charlie Gallenaugh, president of the Clay Adams Division of Becton Dickinson, called me up to ask if I could help them with some reagent problems. Clay Adams sold chemistry kits to the physician's office laboratory market. Norm Kleinman who had worked for me at Warner-Lambert left in 1967 to be in charge of reagent development for Clay Adams. He adapted most of my reagents to the Accustat, the Clay Adams analyzer, purchasing bulk reagents from Warner-Lambert. When these became no longer available Charlie came to me for help.

I consulted for Clay Adams on a monthly basis for the next five years. In addition I produced and sold to Becton Dickinson a number of bulk chemistry products which they repackaged for the Accustat. Among these were reagents for albumin, alkaline phosphatase, aspartate aminotransferase and bilirubin. I developed under contract to them reagents for potassium and urea. I developed a temperature sensitive dye solution that changed color with temperature change for an immunoassay instrument they never marketed.

My most successful product at General Diagnostics was Blood Gas Control, an ampouled solution with known values of pH and partial pressures of oxygen and carbon dioxide which was used for verifying the performance of blood gas analyzers. In its second year on the market it sold six million dollars and a number of competitors emerged to challenge our exclusive position. Because of the evanescent nature of blood gasses all these products were single use ampoules. At Babson Research Labs I developed and patented a multiple use container that consisted of a squeezable foil tube terminating in a male Luer fitting which incorporated a small duck bill valve to prevent gas exchange with the atmosphere. While all the ampouled products had a gas phase in equilibrium with the solution and therefore required temperature equilibration prior to use, my product had no gas phase and was always ready to use.

A number of companies were interested in licensing my product and evaluated it with their control solutions. All eventually rejected it for one reason or the other. A small private label company in Massachusetts called Bionostics heard of my product and Tom Kelly, a member of their board whom I had known when he was at Instrumentation Laboratories, called me and I went up to visit them. They also eventually declined to market my product, but as a result of our interaction they offered me an opportunity to make a modest investment in their S corporation and asked me to become a member of their board of directors, both of which I did. I

have been on the board of Bionostics since 1986 and every year receive in dividends more than my original investment. They manufacture most of the ampouled blood gas products sold in the U.S. including Warner-Lambert's original Blood Gas Control, the rights to which were acquired from Organon Teknika in 1994.

I spent the better part of a year developing a non-isotopic immunoassay for prostatic acid phosphatase (PAP), the only marker available for prostate cancer prior to the discovery of PSA. I still believe my test is the most sensitive and robust PAP method ever developed. Several companies were interested in my product. I favored Fisher Scientific, a division of Allied Chemical, because they were close by in Orangeburg, New York, and I had worked with them on my blood gas product. Also, they willingly paid me $10,000 for an exclusive option to evaluate it.

By August 1982 we had agreed on a contract. Fisher's attorney was doing a final review prior to signing when they hired a new vice president of technology from Abbott. He killed the deal.

With the demise of the Fisher deal I was back to square one. However, I soon had both American Dade in Miami and Ortho [Diagnostics] Diagnostics, a division of Johnson & Johnson vying for rights to the reagent system. While Dade offered me a higher royalty I elected to go with Ortho [Diagnostics] because they agreed to a higher up-front payment, and by this time I was consulting regularly for Ortho [Diagnostics]. When I had made my presentation to Ortho [Diagnostics] on the PAP test, Phil Oringher, the vice president of research and development, was so impressed he not only wanted my product he wanted to hire me as a consultant two days a week to help them with their immunoassay program. I ended up consulting one or two days a week for five years, first in immunoassay and then in blood coagulation after the automated immunoassay instrument project was scrapped.

I realized minimal royalties from the sale of my PAP kit because Ortho [Diagnostics] botched the marketing effort. (Dade would have done a better job.) In 1984 Ortho [Diagnostics] decided to discontinue the product, so all rights were returned to me. I decided to manufacture the reagents myself. With that in mind I managed to purchase from Ortho [Diagnostics] for $100 several thousand dollars worth of antibodies and chemicals which they had accumulated specifically for this product.

My target account was MetPath, the largest clinical laboratory in the world at the time. Besides the huge volume of testing they did MetPath was in New Jersey and the FDA only has jurisdiction over interstate sales. My presentation to MetPath on the PAP test was attended by a room full of technical and marketing people including Ray Gambino, Vice President of Scientific Affairs. Ray had been a consultant to General Diagnostics and a friend. He convinced them to evaluate my product and their evaluation convinced them to buy it. They sent me a standing order for 7,000 tests every other month at a negotiated price of $.85/test.

MetPath did a lot of urine testing for drugs of abuse on an Olympus 5000 analyzer. They had been unable to find a satisfactory commercial source of reagents for alcohol and asked me

for help. My previous extensive work on this analyte served me in good stead and I soon had a reagent system for them to evaluate. The subsequent satisfactory evaluation resulted in a standing order for 24,000 tests per month. Between PAP and alcohol I was doing over $10,000 a month in business, almost all profit with only two products from a single customer. Happiness is a standing order!

I was far from a model child. I may not have been a great husband or father. But I was a damn good scientist. My crowning achievement was IMMULITE and Cirrus Diagnostics. However, to better the reader's understanding of the rest of this story I must digress to present a brief and oversimplified history of the development of quantitative immunoassay.

An important component of the body's defense against foreign invaders is the generation of globular proteins called antibodies by B lymphocytes which bind to and enable the destruction of invading microorganisms by other specialized white blood cells. Molecules to which antibodies bind are called antigens (antibody generators). While a foreign invader will stimulate the secretion of many different antibodies, each antibody molecule binds to only one of many molecules or parts of molecules on the microorganism's surface and each lymphocyte produces only one molecular structure of antibody. Binding induces these cells to proliferate by cloning to generate many cells producing the same antibody molecule enabling the body to mount an adequate immune response against the invading organism. The extraordinary discriminatory power of antibodies to bind with a very specific molecular structure combined with the strength of the binding reaction is what makes antibodies ideal reagents to measure specific substances within a complex mixture of similar substances like blood serum.

While the existence of antibodies has been recognized since the late nineteenth century and various crude qualitative procedures have long been used to demonstrate the presence of specific antibodies in clinical samples, it wasn't until 1959 when Rosalyn Yalow and Solomon Berson, while studying the development of antibodies to beef and pork insulin in treated diabetic patients, devised a practical means of using antibodies as analytical reagents with their invention of radioimmunoassay (RIA). This Nobel Prize winning idea was simple in concept, but it revolutionized laboratory testing procedures. A measured volume of patient's sample plus a known amount of the antigen to be measured, labeled with a radioactive isotope, is incubated with a limited amount of antibody specific for the antigen to be measured. The labeled and unlabeled antigens compete for the limited binding sites on the antibody. After physical separation of the antibody from the reaction mixture, the radioactivity therein, which is inversely proportional to the unlabeled antigen concentration from the patient's sample, is measured. Quantitation is achieved by comparing the radioactivity of the test sample with that of a series of known antigen concentrations similarly treated.

The body's immune system evolved to discriminate self from non-self. Antibodies are usually not generated against substances normally found in the body. However, by coupling a normal molecule to a foreign protein the body can be fooled to generate antibodies against the resulting complex, and some of the antibodies may be specific for the normal molecule. For example, thyroxine, the thyroid hormone common to all vertebrates, can be chemically combined with bovine serum albumin and injected into rabbits. The rabbits will recognize the

complex as foreign and generate antibodies against it, some of which will be specific for thyroxine. In this manner antibodies can be produced against almost any molecule of clinical importance.

In the early days of endocrinology, before the advent of RIA, hormones such as insulin and thyroxine could only be measured by their elicitation of a physiological response when administered to an animal, usually one from which the corresponding endocrine gland had been extirpated. When I started working at Warner-Chilcott one of their products, Proloid, which was heat denatured hog thyroglobulin, the storage form of thyroxine in the thyroid gland, was assayed by measuring the increase in metabolic rate of thyroidectomized rats housed in metabolism cages and fed known quantities of Proloid. When I was in graduate school I diagnosed my wife Doris' first pregnancy by injecting her urine into an immature rat and measuring the enlargement of the ovaries several days later versus an untreated control, a very crude assay for chorionic gonadotropin. Today these hormone assays are routine laboratory tests.

Yalow and Berson incubated their reactions for four days and separated antibody-bound and free radioactivity by paper chromate-electrophoresis, a tedious process. Advances on the general technique were rapid. Hales and Randle introduced in 1962 the use of second antibody precipitation to separate bound and free label in which an antibody from another species recognizes the reagent antibody as antigen. Kevin Catt discovered in 1967 that antibodies could be physically adsorbed onto plastic tubes. Separation of bound and free label was done simply by pouring out the tube contents after the reaction and rinsing the tube with water. The following year Miles and Hales invented the sandwich immunoassay to measure protein antigens that have multiple antigenic determinants. In this technique two different antibodies, one bound to a solid surface such a test tube and the other carrying the radioactive label are incubated with the sample to be measured. The desired antigen forms a solid phase sandwich between the two antibodies. Unlike competition RIA assays which require that antibody concentration be limiting, sandwich assays use excess concentrations of both antibodies which markedly increases the rate of the reaction and the potential sensitivity of the analysis.

RIA achieved remarkable commercial success. By 1985 over sixty companies were selling manual RIA kits to hospital laboratories to measure over one hundred different analytes. RIA was not without problems, not the least of which is the instability of the isotope. 125 Iodine, the most useful radioisotope for immunoassays, has a half life of 59.4 days. In less than two months half of it has disintegrated; in four months three quarters. Kits have a useful life of only two months and full calibration curves must be run daily to correct for the decay of the isotope. Many stable labels for immunoassay have been used and enzymes are among the most successful. They have the advantage of being biological catalysts with the potential of signal amplification of several orders of magnitude.

One more significant discovery must be described to complete this short history. As indicated previously, the body produces many different antibody molecules over different time intervals in response to an antigenic stimulus. Some of these cross react to a greater or lesser extent with related antigens. No two animals respond in exactly the same way to an antigenic

stimulus. Thus the antisera withdrawn from immunized animals can vary in specificity and affinity from animal to animal and over time.

Köhler and Milstein in 1975 were able to fuse antibody- producing spleen cells from immunized mice with mouse myeloma cells to produce hybrid cells that preserved the antibody secreting function of the lymphocyte and the immortality of the tumor cell. They devised a simple and ingenious procedure for separating and isolating single hybridoma cells and growing them in tissue culture medium to provide a virtually limitless supply of a single antibody molecule. The so-called monoclonal antibodies have provided reproducible immunoassays of incredible specificity and sensitivity. For this brilliant work Köhler and Milstein shared a Nobel Prize.

The history of laboratory tests for evaluating the status of the thyroid gland is a good example of the power of immunoassay. Thyroxine, the main hormone of the thyroid gland, contains four molecules of iodine, the sole occurrence of this element in nature. It circulates at a normal concentration of 4 to 12 µg/dL. In contrast, normal serum glucose is at a concentration of about 120 mg/dL or at least 10,000 times higher. Over 99.95% of serum thyroxine is bound to serum proteins and metabolically inactive.

The first chemical estimation of thyroid hormones was the measurement of protein-bound iodine, a complex and tedious analytical procedure in which serum proteins are precipitated, dried and incinerated to convert the organic iodine to inorganic iodide and measurement of the catalytic effect of the iodide on the reduction of yellow eerie ions to colorless cerous ions by arsenious acid.

The advent of RIA made it possible for the first time to measure thyroxine directly and many fledgling companies began their existence with RIA kits for thyroxine. (Warner-Lambert got into RIA by acquiring Nuclear Medical Laboratories). While this gave the physician a powerful tool to diagnose excess or deficiency and monitor replacement therapy it was not enough to delineate the full status of the thyroid gland.

The production of thyroxine is governed by the level of thyroid stimulating hormone (TSH) produced by the anterior pituitary gland. The level of TSH is controlled by the thyroxine concentration through a negative feedback mechanism. TSH levels are more diagnostic of thyroid status than thyroxine levels. Despite the extreme sensitivity of RIA it is only able to measure the increases in TSH found in hypothyroidism. The sandwich technique of Miles and Hales increased the sensitivity of TSH measurement an order of magnitude but still not enough to measure the extremely low levels found in hyperthyroidism. The recent development of chemiluminescent enzyme substrates provided the additional order of magnitude increase in sensitivity to accurately measure TSH in hyperthyroid patients. Cirrus was the first company to develop an automated third generation TSH assay. We can also now measure directly the metabolically active free thyroxine which is at a concentration 2,500 times less than total thyroxine or 25 million times less than glucose.

Common to almost all immunoassay techniques is the requirement to separate antibody-

bound from unbound label. This separation step has been the most difficult to automate. Prior to the founding of Cirrus in 1987 many large companies, including Squibb, Technicon, Becton Dickinson, Hybritech, and Boehringer-Mannheim tried and failed. (Hybritech was one of the darlings of the immunoassay industries, having obtained a patent on the use of two monoclonal antibodies in a sandwich assay, a patent which never should have issued, and the first FDA approved PSA test for prostate cancer. Hybritech was acquired by Eli Lilly & co. in 1986 for $350 million. Like most large pharmaceutical companies, Abbott Laboratories being a notable exception, Lilly didn't know how to manage an entrepreneurial company and Hybritech's star plummeted. In October 1995 Lilly sold it to Beckman Instruments, Inc. for one dollar.)

I actually first conceived the idea for automating immunoassay separations by spinning a vessel on its longitudinal axis in 1979 while still at Warner-Lambert. I was unsuccessful in persuading that company of the value of the idea. Several years later I still thought it was a good idea. I wrote and submitted a patent application covering the invention with the idea of licensing the technology or selling it outright. In late 1986 I asked John Underwood, for whom I was doing a fair amount of consulting at the time, who he thought might be a good customer for the technology. He immediately appreciated its value and said, "Art, you don't want to sell this. You want to start a company." Actually I didn't. Babson Research Labs was doing very well, and I was making more money than I had ever made before or have since. But John, thankfully, was persuasive.

I had met John in February 1986 through an unusual chain of associations. Shortly after I left Warner-Lambert the company acquired the diagnostics business of Pfizer including their employees. One, a woman named Hilary whom I had never met, left Warner-Lambert several years later and started to work with John, who had just established a consulting business. John needed a strong technical associate to help him in his business and asked Hilary if she knew anyone. She called Jim Carroll who used to work for me at Warner-Lambert and he suggested me. John, Hilary and I met for dinner and I agreed to help him.

During a 1986 consulting visit to London Diagnostics, a start-up immunoassay company in Minneapolis, he introduced me to Art Kydd, an early stage venture capitalist who had invested in London. On a subsequent visit in January 1987 I had dinner with Art and a few of his wealthy investors. On the restaurant table I demonstrated the principle of separation by axial centrifugation using a battery powered wooden model of a tube spinner and hand fabricated tubes. On the basis of this demonstration and further investigation of my background Art put up the seed capital that started Cirrus Diagnostics. The company was incorporated as Babson Technologies March 18,1987. In subsequent years many people, after having been shown the elegant simplicity of the IMMULITE assay tube, would remark, "I can't believe no one ever thought of that before. It's so simple." But the fact is no one did. I now have several issued patents on various aspects of this concept.

I located the company in one second floor room of the old Chester school which had been vacant for several years. For many months the company was only me except that Susan had appropriated a corner of the room for her art studio. She had been studying at the Isabel O'Neil Studio in New York for some time and had become very skilled in the art of painting

objects.

I had accumulated some second hand furniture but it hardly looked like a real enterprise. Consequently I had a lot of trouble persuading anyone to join me in the venture. I wanted a mechanical engineer to start with, and they are a pretty conservative bunch. I had several head hunters looking and they uncovered a number of potential candidates including Tom Palmieri whom I was particularly keen on. In January Tom decided he didn't want to take the chance and turned down my offer. Frustrated, I closed the operation for a month and Susan and I went on a wonderful photographic safari to Tanzania and Zambia.

In the meanwhile the head hunter kept working on Tom, and when we returned I was delighted to learn the he had reconsidered and would accept my offer. On March 15, 1988, Tom started and Pegasus Technologies, our name prior to Cirrus, became a going concern.

Doug Olson, now president of DPC Cirrus, joined us in April. I found Doug through another unusual chain of associations. John Teipel, whom I had worked with when I was consulting for Ortho [Diagnostics] Diagnostics, heard from John Underwood that we were looking for a Ph.D. immunologist. Teipel heard from another individual that Doug, whom John didn't know, might be available. I called Doug. He came to my home to interview and we established an immediate rapport.

Arthur Ross, an electrical engineer, responded to an ad in the New York Times and started in May. We contracted with Vic Huebner to do software and then had all the functional areas covered to develop an automated immunoassay analyzer.

In addition to Art Kydd's venture funds which were limited, we had a number of private individuals that invested in our first round of financing. But we had to watch our spending because the money available was limited and Art Kydd was often tardy in making advances. On five separate occasions I had to make personal loans to the company to meet the payroll. Of course, the other employees were not aware of this. It was a hand to mouth existence.

By the end of 1988 we had produced a working breadboard of the immunoassay system, which, while not working very well at least proved the validity of the approach and pointed the way to a more robust design. The patented assay tube contained a quarter inch polystyrene bead coated with antibody and an integral coaxial waste sump to capture the spun out reaction mixture and wash water. The top of the tube was sealed with foil or a rigid cap with a small central hole.

I realized the spinning tube concept could be used in other separation procedures. At that time cholesterol was becoming the rage for cardiac risk assessment particularly the high density lipoprotein (HDL) and low density lipoprotein (LDL) fractions, the so-called "good" and "bad" cholesterol respectively. HDL cholesterol determinations require a precipitation and centrifugation. LDL cholesterol is calculated by a formula based on total cholesterol, HDL cholesterol and triglycerides. I decided we should build a cardiac risk profiler that would do all these tests automatically. Such a dedicated instrument would be a much easier project than the

immunoassay system. Sold through a marketing partner it could bring in needed cash flow to support the larger project.

We designed a modified assay tube with a compartmented upper chamber to hold reagents and collect precipitates. In a short time Tom had built a prototype that would do total and HDL cholesterol and triglycerides on twenty serum samples in about 30 minutes. We showed the prototype to Becton Dickinson. They convinced us that the market for lipid profiling was not the hospital but the physician office laboratory. They weren't interested in an instrument that produced twenty profiles in 30 minutes. They wanted an instrument that did one profile in 12 minutes, the average time the patient was with the doctor, and used whole blood rather than plasma or serum.

We returned to Becton Dickinson a few months later with a prototype instrument that did just that. The operator transferred about 0.2mL of whole blood to one chamber of a small, reagent filled cuvette array, put the disposable device in the instrument, closed the door and pressed the start button. Twelve minutes later without further operator action the entire lipid profile for the sample was printed out along with an interpretation of the data. All steps in the analysis including the centrifugal separation of the blood cells and HDL were performed automatically. Results were calculated from simultaneous assays of cholesterol and triglyceride standards packaged in the disposable. The instrument never needed standardization and could be run by a high school dropout, perfect for the physician's office. Unfortunately, the estimated manufacturing cost was about $5,000 which Becton Dickinson felt was too high for their method of distribution.

We talked to other potential partners several of whom expressed interest. Unipath in California was particularly excited about the system and not concerned with the manufacturing cost. We were in final contract negotiations with them when the proposed CLIA 1 88 (Clinical Laboratory Improvement Act) regulations were finally promulgated. This law would have put the 150,000 physician's office labs under the same strict regulations as the 15,000 hospital and private clinical labs. It threatened to put physicians out of the laboratory business. Unipath did an immediate reversal and terminated negotiations.

We still have the three prototype instruments we built. We spent about a million dollars on the project, about a tenth of what we would spend on the immunoassay project. Every time the venture capitalists saw me they beat up on me for doing the project. I still think it was a good strategy. If not for the untimely proposed government regulations I believe it would have been successful and made us less dependent on the same venture capitalists that would eventually steal the company.

In early 1989 it became apparent that we would require serious funding. Several unsuccessful attempts to interest corporate partners convinced us that it was premature to pursue this route. If there wasn't sufficient perceived value to attract a corporate partner we felt a public offering might also not be successful. That left venture capital.

Venture capitalists are people, almost all men, that invest other people's money in

privately held, often early stage companies where they perceive a potential for exceptional growth. They presume to be in a position to help assure that growth by applying their management skills to the control of the company. They are interested in one thing only: making money. Lots of money. They don't talk about percentage returns, they only talk of multiples. To cover their many failures they like to think that a multiple of ten, for example, is not an unreasonable return. They don't care about helping to create technological breakthroughs or the future of the company or the people once they sell it. They aren't interested in long term investments. Get in, make a killing, get out. They are a rotten lot.

One of the companies that our venture capital firms pointed to proudly as one of their successes was Pandex which was developing an automated instrument for screening monoclonal antibodies. They realized a gain of a very high multiple when they sold the company to Baxter for $40 million. The fact that Baxter sunk another $60 million trying to make the technology work before giving up and writing the whole thing off didn't seem to bother the venture capitalists.

We started courting venture capitalists in the spring of 1989. These firms like to present themselves as risk takers. Except that they like to share the risk. The lead investor was supposed to have completed its due diligence by April, but the deal was delayed several months while we searched for additional investors and watched our committed capital disappear. We ended up with three firms located in the Midwest. The amount of documentation they required to do the deal was several inches thick. At the last moment they sprung a term sheet on me that almost queered the deal. They insisted on bringing in a CEO to replace me whom they didn't trust to continue leading the company. They insisted on hiring a marketing director which was all for show since we had nothing to market. They insisted on Art Kydd resigning from the board. Most onerous of all they wanted to dump Doug Olson and replace him with a new director of reagent development. They delayed the closing until our financial position was untenable. We either did the deal under the dictated terms or shut down the company. I refused to sacrifice Doug whom I had always considered the heir apparent. I finally worked out a compromise where Doug would remain as director of research administration and we would look for a new director of reagent development. When we signed the agreement I gave up control of the company.

Bob Fennel was brought in as CEO. He was my choice over several candidates and we got along fine. I was happy to have Bob assume the management of the company, a job which daily became more complex, and he did a good job. Unfortunately, Bob was a total failure at his main responsibility which was to attract new funding to the venture. This left us totally at the mercy of the original venture capitalists and they ended up in the last round of financing essentially stealing the company.

In the meanwhile the instrument development proceeded at an encouraging pace. In July 1990 we introduced IMMULITE at the XIV International Congress of Clinical Chemistry that was held in conjunction with the annual meetings of the American and Canadian Societies for Clinical Chemistry in San Francisco. We lucked out with an incredible location in Moscone center: an island at the intersection of the two main isles between Abbott and Technicon's

islands. This was a coup because no one had ever heard of Cirrus. The instrument we showed was one of three engineering prototypes which wasn't even fully functional. It did have a very expensive custom cover which made it look finished. No one knew that it wasn't fully functional because you weren't expected to run wet chemistry tests at the show.

The following year at the AACC meeting we showed one of the twelve preproduction prototypes we had assembled at the school house. These twelve units were committed primarily to outside evaluations. Morristown Memorial Hospital got the first one and they would become our first cash customer. By this time we had taken over the entire second floor of the school and part of the first floor. It is important to understand that the entire IMMULITE system was designed by us. Many large companies like Roche and Ortho [Diagnostics] farmed out their instrument development and design. Of course, we used many local machine shops and vendors to produce parts (we still do). But we owned the design and could change suppliers whenever we wanted.

Building complex instruments in the school without even an elevator much less a loading dock was not very practical. So we contracted with a machine shop in Mountain Lakes, New Jersey to build the next twenty five systems. This order required the commitment of one million dollars. By this time we were seriously courting corporate partners and additional venture funds. Unfortunately, most of the logical partners for us already had their own instrument development programs underway. Our burn rate was approaching $300,000 a month and we were running out of money. One of our venture firms had exhausted their fund. The other two presented us with a take it or leave it deal in November 1991 in which they in essence sold themselves 25% of the company for about a million dollars.

I had written to Sigi Ziering, the chairman of Diagnostic Products Corporation in March 1991 to see if DPC would be interested in joint reagent development for IMMULITE. We already had several tests approved by the FDA but the broader the test menu the better in an automated system. DPC had the largest menu of manual immunoassay kits of any company. I was surprised when Sigi declined saying they had their own system they were working on. What he was referring to was their Millenia system, which was a semiautomated batch analyzer that would never be successful in the marketplace.

I contacted Sigi again in December, 1991, and this time he was interested in looking at Cirrus as a potential acquisition for DPC. He and his wife Marilyn visited but were noncommittal. Shortly thereafter Sigi wanted to send several people to Cirrus and run hundreds of assays on blood samples they would supply. Bob Fennel thought this was presumptuous at such an early stage in negotiations and called Sigi on January 6 saying that this would be too much of a disruption and ended up cancelling the visit planned for the following week. Bob then left for a trip. I was appalled that a potential partner might be lost because of egos. I called Sigi back and rescheduled the visit. On January 21 Howie Wilson, Samantha de Priest and Alice Bragg arrived and spent two days with us. Alice ran her samples on the IMMULITE all day long. The results were good and she loved the instrument. On March 20 DPC signed a letter of intent to acquire us and on May 28 DPC bought Cirrus for $28 million in DPC stock.

In December, 1992 we moved into brand new facilities in Randolph, New Jersey and started manufacturing IMMULITEs. DPC now develops and manufactures the reagents in Los Angeles. We have instruments in over fifty countries and have the largest menu of tests of any system. We have recently completed our second expansion in the Randolph facility and are planning the third for next year when we will also be manufacturing the next generation system, IMMULITE 2000, the project I have been mostly involved with. As of this writing we have 110 employees and IMMULITE accounts for about $50 million a year in business for DPC. The success of this venture has exceeded my wildest expectations.

I cannot end this narrative without a few words about Africa. One of the things that attracted Susan and me to each other was our shared love of animals. To really see animals you have to go to Africa. East Africa is the only place left in the world where one can see vast herds of ungulates of many species and the predators that depend on them. The latter are totally unconcerned with tourists as long as one stays within the vehicle. We have had lions fall asleep in the shade of the car and cheetah cubs climb all over it.

We have made three trips to Africa and will probably go again. Our first trip in 1979 was to Tanzania and Zambia. We had our own guide, Mike Taylor, and private safari car for three weeks in Tanzania where we stayed in comfortable lodges in the various parks. We had purchased two Nicon FEs for the trip and took colored slides. One of the cameras jammed hopelessly after only a few days and the other malfunctioned in that the automatic aperture didn't function properly. All the pictures except those taken at full aperture were overexposed, but we didn't know this until we got home. It was a heartrending discovery.

We went to Zambia because they allowed walking safaris and we wanted this experience. We almost didn't get there. On the way to the airport in Arusha to catch the weekly flight to Dar es Salaam where we were to catch an early morning flight to Lusaka, Mike had a flat tire. While we got to the airport well in advance of the plane, the agent had sold our seats to two of his friends that were on standby. No amount of cajoling would get him to change his mind. The only way to get to Dar was to drive.

Outside of a few cities there are essentially no paved roads in Tanzania. In fact there are few roads of any kind. We left Arusha about 5:00 PM. The dirt road rapidly deteriorated to a deeply rutted mud track. All night long we drove through dark hills rarely passing another vehicle. Every hour or so we went through a town consisting typically of a couple of stores and a bar with several prostitutes hanging out. The bars never seemed to close. We arrived in Dar about 6:00 AM just in time to catch our plane after driving continuously, except for time out to fix another flat tire, for thirteen hours.

Zambia was very primitive and nothing worked well. We slept in mud huts and tents. One morning the guide heard a leopard and five of us and the guide set off on foot to try and see it. The leopard disappeared, but we came upon a rhino, which we quietly approached. Alerted somehow to our presence, probably by the sound of my shutter release, he turned to face us and immediately charged. Contrary to the guide's instructions, Susan and the other three tourists turned tail and ran. I stood my ground only a few seconds longer. The guide, who was armed,

yelled and waved his arms. When only a few yards away the rhino turned and ran off into the bush. It was an experience none of us will forget. But we saw few animals while walking. It is much easier to approach them in a vehicle.

We went to Kenya in February 1988 with a diverse group of about sixteen people from all over the world. We teamed up with two Australians and a Canadian who shared our vehicle and were great company. This was a low-budget camping trip. We moved almost daily and had to pitch our own tents. Susan and I always located our tent as far away from the rest as the guides would allow. The food was lousy and the beer warm. This was the dry season and we were filthy most of the time from the dust. One morning we saw the pug marks of a leopard that had walked within six feet from the front of our tent while we slept.

After the two-week camping trip we took a train to Mombasa on the Indian Ocean, where we picked up another car and driver and toured through several large national parks on the way back to Nairobi. The most exciting part of the trip was being chased by a group of elephants that the driver managed to stay just ahead of. I had purchased a new camcorder for the trip and Susan took colored stills with a new Nikon. I recorded about eight hours of video and spent about forty hours editing it down to a single tape. Susan inadvertently erased it recording some television special we have never watched. I could have killed her.

In 1994 Susan planned a luxurious private tenting safari in all the best parks of Kenya, Tanzania and Zimbabwe for Jack and Sue Woods and us. Instead of driving long distances over dusty roads we chartered airplanes. The food was good and the beer cold. In Zimbabwe we spent five days canoeing down the Zambezi River. This was the highlight of the trip. Each day we would start off early in the morning and paddle a couple of hours. Then our guide, Dave, would cook the most incredible breakfast on the river bank. After more paddling Dave would create a very respectable lunch. During one lunch break we were visited by a large elephant who wanted to get to the river to drink. After threatening to challenge our spot he moved a little ways up the river. While we were on the river the crew would break camp and truck the gear down river so the camp was all set up when we arrived in late afternoon.

While animals including some large crocodiles could be seen all along the river, what we mostly saw were hippos, reputedly the most dangerous animal in Africa. Hippos, Cape buffalo, and elephants kill many more people than lions. Dave cautioned us to follow closely behind his canoe and never get between a hippo and deep water where they feel safe. Hippos leave the river to graze at night and spend the day in deep pools in the river. Jack was very nervous around hippos. The very first morning on the river we were proceeding single file hugging the high right bank of the river to avoid a large group of hippos in the water. Suddenly a large hippo appeared on the bank above us. Dave had missed him and was already past. I yelled to Jack and Sue who immediately started to back paddle frantically as did we. Suddenly the hippo came crashing down the bank into the water just in front of Jack and Sue's canoe, almost swamping them. Jack was in the bow and I'll never forget the look of sheer terror on his face.

The whole trip was incredible. We identified 44 mammals and 193 species of birds. We

would like to do it all over again. Jack, however, would just as soon skip the hippos.

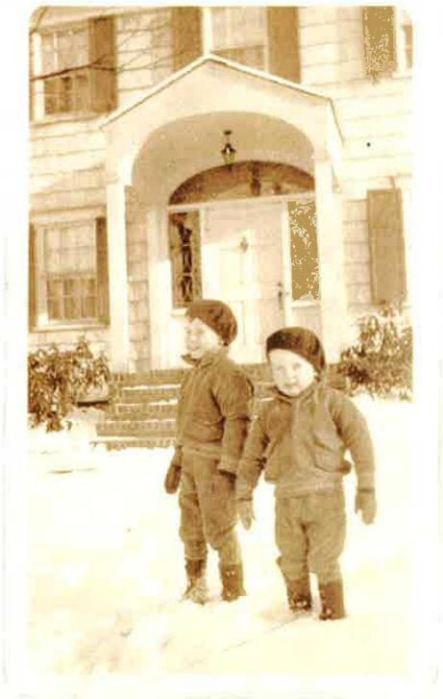
So that is my life so far. It is an interesting but useless exercise to speculate how my life might have been changed if some events, totally out of my control, had turned out differently. What if my parents had kept me out of school another year? What if I had been accepted for graduate study in zoology instead of biochemistry? What if I hadn't invited Doris to a Cornell spring weekend and she had married Paul Ostergaard? What if Arthur McCollum had not invented Flaco pie crust? What if my father had not known Ulrich Solmssen? What if Susan had not been allergic to rabbits? What if I had never met John Underwood? But the fact remains all these unrelated events did happen the way they did, and each one had a part in shaping my destiny.

Where do I go from here? I enjoy my position as Chief Scientist at DPC cirrus. I want to make sure IMMULITE 2000 is as good as it can be. But this is a full time job and I want to do other things. I would like to get involved with the Nature Conservancy, the New Jersey Conservation Foundation, or the New Jersey Audubon Society. I would like to spend more time in Virgin Gorda. I would like to have more time to play and to work on projects around the house. I would like to have time to take up a craft. I would like to have more time to read and to write. Perhaps even to write a book.

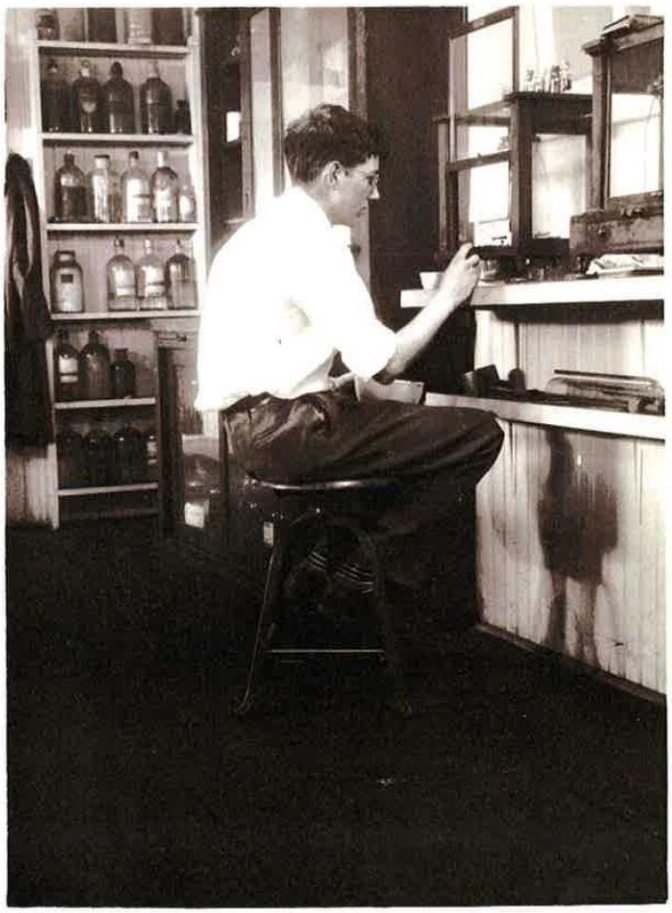
My father died in 1959. He didn't live to see me become a vice president at Warner-Lambert or win the Gerulat Award. He didn't see me build our house in Mendham and acquire a second home in Virgin Gorda. He didn't know I would found two very successful companies. Had he known these things I think he would have been proud of me.

Eagleson's dog Toby

Larry Soule and Fritz Reindel



Norton and me in front
of Oldchester Road House



Working at American Dyewood

Twenty-five feet up in a huge oak tree
Fritz took the picture from another tree about ten feet away.

Mother and me

Fran Miller

Dad and Norton

Honeymoon

Doris' and my wedding

Engagement picture

Rutgers Graduation

Overlook Trail house, Morris Plains

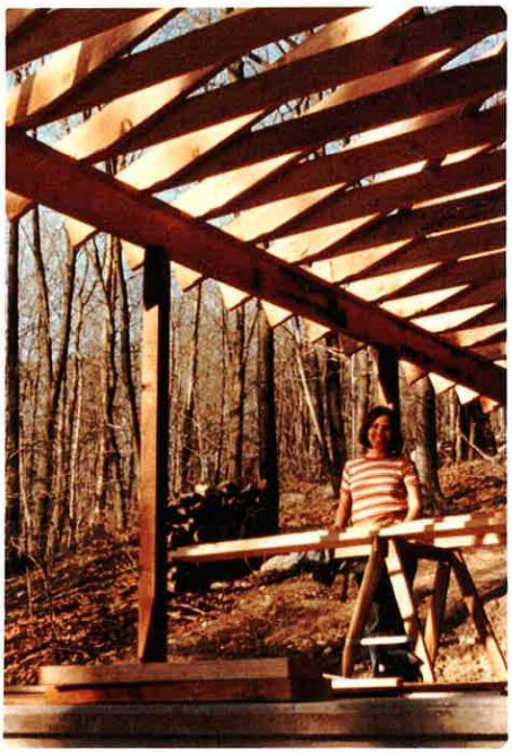
Babson brothers - Rea, Cliff, Arthur, Stanley

Unitarian Fellowship bird walk - 1966

Harbour Island, Bahamas - 1967

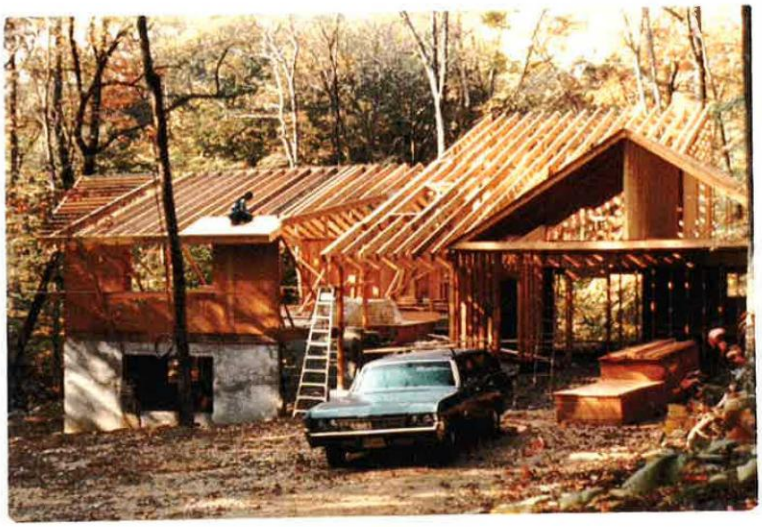
Susan's and my wedding - May 15, 1971

Sarah and Ernie



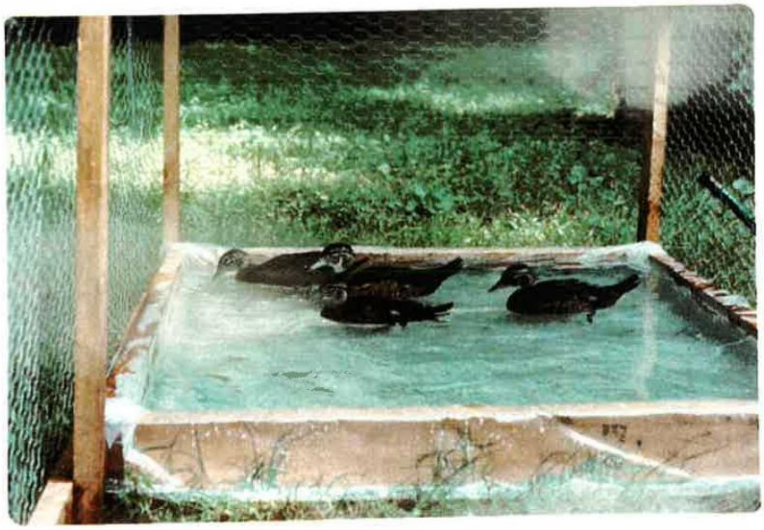
Rough framing

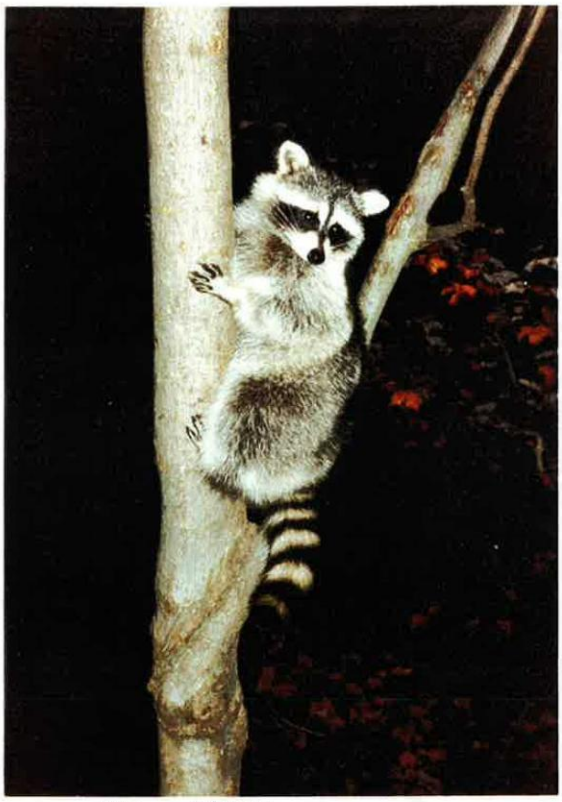
Sheathing
Fall 1993



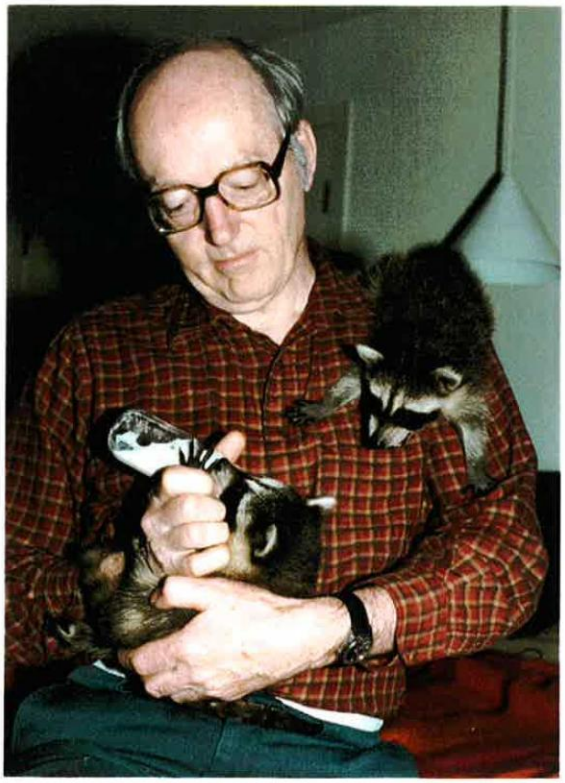
Wood ducks
in the house

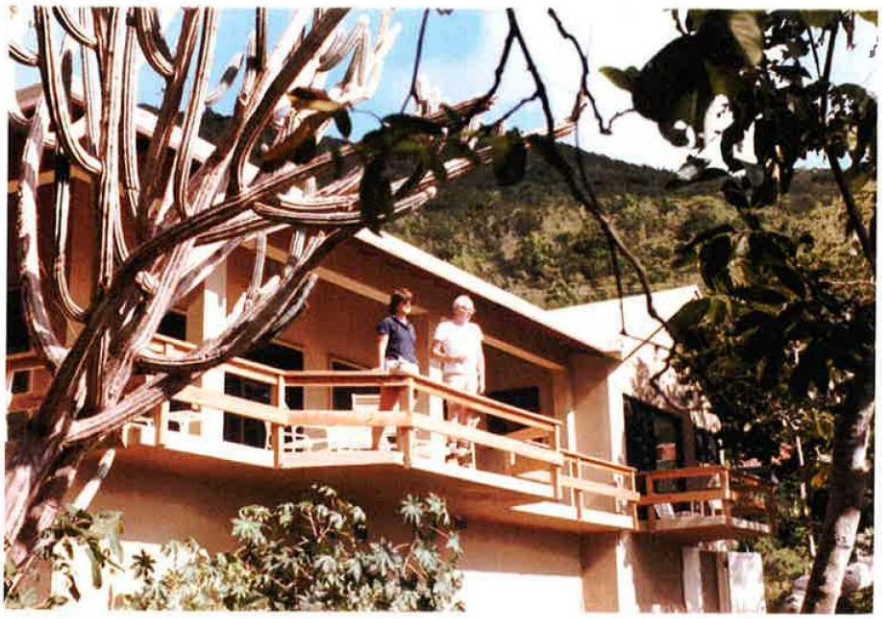
Wood ducks
on the lawn





Top - Missy
Bottom - Snippy and Snappy





Sue Woods and me at Turtle Bay Villa, Virgin Gorda

Jack and Sue Woods, their daughter Linda, her husband John Harris, their children Michael and Leigh, our God daughter

AACC meeting in San Francisco, July 1990
Me, Pradip Datta and Doug Olson

Sigi Ziering, Dotti Bernsten, Sonny Salter and me

2007 ADDENDUM TO 1995 ARTHUR L.BABSON "REMEMBRANCES"

Well it didn't happen. My retiring. I'm 80 years old and still going to work full time. I have found time to write. My essays now number 23 and are almost book-length. I have, in fact, written a book called, "Portrait of Evil, A Nail Bay Chronicle" which is now 282 pages long and still a work in progress. What I haven't had time for is doing all the fun things most people retire to do. For example, I've had to give up skiing, both downhill and cross-country. The former I gave up a dozen years ago and the latter only a couple. My arthritis has gotten worse. I was scheduled for a knee replacement on August 15, 2006, but cancelled the operation at the last minute. I decided that it didn't hurt enough to go through all the pain and aggravation. I have serious doubts that I will be able to play tennis anymore. In a very real sense I'm too old to retire. But the purpose of this addendum is to bring my memoirs up to date. So following is a brief summary of the past twelve years.

In 1997 I was named Inventor of the Year by the New Jersey Inventors Hall of Fame. This was in recognition of my invention of the IMMULITE technology. At a formal black tie dinner on February 17 at The New Jersey Institute of Technology, I and several other New Jersey inventors were honored for "extraordinary contributions to the advancement of knowledge and human welfare." It was a gala event attended by family and friends.

On June 30, 1998 I was presented the Van Slyke Award by the New York section of the American Association for Clinical Chemistry for "Outstanding contributions to the science of clinical Chemistry". This is probably the most prestigious award in clinical chemistry and is named for Donald Van Slyke, one of the pioneers of the field. Again, friends and family were invited. The dinner and ceremony were held at Bayer Diagnostics in Tarrytown, New York at the headquarters of the former Technicon Corporation, the company that started clinical laboratory automation. Bayer was one of our major competitors. So many people from the company wanted to attend that we hired a bus. I gave one of my best talks ever without notes and illustrated with historical slides illustrating my scientific achievements and the novel products I had developed.

The IMMULITE 2000 was introduced in 1998 and was an immediate success. In 2007 it continues to be our market driver. In 1998, however, the company became involved in a project that I knew was doomed to failure. For years DPC had tried to develop a market leader position in in vitro allergy testing, a market dominated by Pharmacia. DPC had an excellent menu of individual allergen tests and panels but lacked an automated analytical platform. A semi-automated system based on microtiter format was produced by Euro DPC, the company's manufacturing division in North Wales. Euro had started a project to automate the microtiter approach, but the project was foundering. We were asked to take over the project. The first decision we made was to abandon the micro titer tray format and use the IMMULITE 2000 assay cup. We maintained the requirement for a tabletop instrument rather than a console that stood on the floor. This was a mistake and I knew it was. By the time we put a foam core model together, it was huge and nobody liked it. It always amazes me that most people cannot visualize from two dimensional drawings what an object looks like in three dimensions.

I suggested that instead of developing a whole new system for allergy we add this capability to the IMMULITE 2000. This was done, and the 2000 now has captured a large segment of the automated allergy testing market. I also suggested that we modify the 2000 to accept the random access incubator developed for the allergy instrument, yet another of my inventions. This was successfully accomplished and the result was the IMMULITE 2500, which was released in 2004. The random access incubator allowed incubation times of any duration, whereas the 2000 only allowed incubation times of 30 or 60 minutes. A patent for the random access incubator was issued on February 13, 2007, my 43rd U.S. patent.

The company added more rental units in Randolph several times until there was no more space available. So we bought 22 acres in Flanders and had an 88,000 square foot custom building designed and built. We had a ground-breaking ceremony on September 27, 2000 and moved in December 2001. Because of the weather we delayed our building dedication ceremony until May 24, 2002. It was a gala affair with a huge tent erected in the front of the building and another for the luncheon in the rear. Michael Ziering, CEO and President of DPC, James Watson, Nobel Laureate and co-discoverer of the structure of DNA, and I were the featured speakers.

Shortly after we moved in we realized we were running out of space again. Both the manufacturing wing and the research wing were almost doubled in size, and we moved into an additional 73,000 square feet the end of 2005.

In July, 2006, DPC was acquired by Siemens AG for $1.86 billion in cash. It is almost certain that if DPC hadn't acquired Cirrus Diagnostics in 1992 it would not have survived. The IMMULITE product line in 2005 accounted for 92% of DPC's sales of close to half a billion dollars. By the end of 2006 Siemens had also acquired Bayer Diagnostics, one of our major competitors, and we are now engaged in consolidating the two companies into a new division called Siemens Medical Systems Diagnostics.

On February 22, 2007, we celebrated the first annual Founders Day as well as my birthday with a party in the cafeteria for the entire company. George Hayes played the bagpipes, Tom Palmieri, Bob Holt and David Stein all spoke, and I gave the following remarks:

I am pleased that this day was chosen as Founder's Day for our company. Today marks the birthday of the founder of our nation, a man I greatly admire. Fifty-one years ago I was one of the founders of the Morristown Unitarian Fellowship. In 1959, while serving as president of this fledgling organization, we celebrated Presidents' Day. To see if my daughter Betsy, just turned six, had learned anything in Sunday school that day, I asked her, "Who was our first president?" She promptly replied, "George Washington." When I asked her who was president today, she looked up at me and said, "You are."

Twenty years ago I founded Babson Technologies which became Pegasus Technologies which became Cirrus Diagnostics which became DPC Cirrus which became DPC Instrument Systems Division which is now part of Siemens Medical Systems Diagnostics. This success story can be attributed to two things: I had a good idea and was able to attract good people. And

these people attracted other good people. And good people can take a good idea and turn it into a $1.86 billion value.

But we didn't just create a successful line of laboratory instruments, we created a unique culture; a culture of innovation; a culture of excellence; a culture of caring; a culture of communication; a culture of cooperation; a culture where people look forward to coming to work; a culture where each employee can feel he is making a significant contribution.

In twenty years we have grown from one employee to over 580 and have maintained our unique culture. As we become absorbed into a giant corporation I ask each of you to strive to preserve the culture that has contributed so much to our success.

Finally, I would like to acknowledge my wife, Susan, who many of you know. Her help and advice were crucial to the success of the company in its infancy. She kept me company the many months I was working alone in the schoolhouse. They say behind every successful man is a smart woman. In my case that is very true. Thank you, Susan.

Doug Olson, who Susan used to refer to as the glue that held the company together, had a quotation from Christopher Logue framed in his office which I always liked.

"Come to the edge," he said.
"We can't, it's too high."
"Come to the edge," he said.
"We can't, we might fall."
"Come to the edge," he said.
And they came…
And he pushed…
And they flew!

Keep flying!

INDEX

C

D

E

F

G

H

N

T

U

V

W

Y

Z