

**In Burt Guttman**  
**Interviewed by Stephen Beck**  
**The Evergreen State College oral history project**  
**July 17, 2017**

**FINAL**

**Beck:** This is Stephen Beck. I'm interviewing Burt Guttman. It's July 17, 2017, and we're sitting in the living room of Burt's house.

Burt, I'd like to start out just by asking you a few things about your early life. Where were you born, and what was it like growing up in your family?

**Guttman:** I was born in Minneapolis, Minnesota in 1936. My family was really very interesting. The interesting things about my family are that they actually did kind of a preparation for me to be at Evergreen. Evergreen was the place that really suited me because of the way I grew up.

My father was a Russian Jewish immigrant. He came over in 1905. He had wanted to be an engineer, but apparently he had some kind of eye trouble. I gather that he tried to get in at Ellis Island, and couldn't because of his eye trouble, and he went into Canada. He then came illegally into the United States, into Minneapolis, some years later. But he wasn't able to become an engineer.

He married my mother in 1918, and they almost immediately had my two older brothers, my brother Lester and my brother Norm. Lester told me about how they used to go walking. My father was interested in science, and interested in the world. They would go walking at night, and my father would tell them about the stars, about astronomy, and so on. That got them interested in science.

My brother Lester went to the University of Minnesota from 1936 to 1940, and he graduated in chemistry. In 1940, he went off to the University of California at Berkeley. He worked with a guy named Kenneth Pitzer, to get his Ph.D. in physical chemistry. He did some interesting studies on the entropies of certain materials. And 1943, when he graduated, was, of course, in the middle of the war, and Robert Oppenheimer was there. Oppenheimer was looking for bright, young scientists to go to Los Alamos to try to develop an atomic bomb. That's what Lester did for the remainder of the wartime.

My parents would ask him, "What are you doing?" And he would say, "Oh, I'm pushing a cart," or something like that.

He was there when the first bomb went off, July 16, 1945. He was there in the desert. Oh, I should go and get a little sample of the fused glass from the desert that he sent home. It's a little family treasure. So Lester was an influence in becoming interested in science.

My brother Norm, two years later, '38 to '42, also went to the University of Minnesota. At that time, there were two interesting guys on the University of Minnesota faculty. One of them was B. F. Skinner, the psychologist. He had written his first book, and he was interested in promoting the idea of behaviorism. The other one, in the philosophy department, was a young man named Herbert Feigl. Feigl was one of the younger members of the Vienna Circle, that is, the philosophers of science who started what was called logical empiricism, or logical positivism.

So there was a group of young guys, including Norm, who gathered around both Skinner and Feigl. They were interested in psychology, and some of them remained on the faculty of psychology at Minnesota, but they were interested in psychology and philosophy; doing psychology from a philosophical point of view.

So I had that as a background. Here were my two brothers, whom I love and admire, who were, in many ways, like fathers to me.

**Beck:** They're significantly older, aren't they?

**Guttman:** They were much older. They were diapering me [laughing] and acting rather like fathers. But they were a considerable influence on me. I don't know exactly how, but I grew up with that influence about philosophy—well, about science and philosophy.

But the other great thing was that I was in Minneapolis, on the north side of Minneapolis, and it was just a wonderful place to grow up. I just had a natural instinct to loving nature, loving the outdoors, loving wild things, and I was allowed to run free. There were empty lots around, and I was a little kid. The weeds were about as tall as I was. I would go in there and look at all the bugs and the butterflies and everything, and I became just interested in nature. So that's the kind of background that I had, an interest in serious science.

**Beck:** You went to the University of Minnesota, too, didn't you?

**Guttman:** Yeah, I went to the University of Minnesota. The other thing I should mention is that my fourth-grade teacher was Miss Bergquist, and Miss Bergquist was interested in geology. She had a brother who was a geologist, and she got all of her classes interested in learning about rocks and minerals and fossils. And collecting them.

Well, I had sort of a natural bent toward collecting. And so Miss Bergquist got me educated in basic geology, with a collection of rocks and minerals that I started there, that I still have. And incidentally, that collection of rocks and minerals is being donated to Evergreen.

**Beck:** Oh, good.

**Guttman:** So I had all those influences. I went through public schools, getting very interested in biology, and evolution. There were some major textbooks I used to carry around. I shouldn't call them textbooks, but books like Ernst Mayr's *Systematics and the Origin of Species* and books of that kind. Serious books about evolution and taxonomy.

I went to the University of Minnesota in 1954 to '58, and I guess I didn't do anything special. Oh, that's right. I knew that I wanted to major in zoology. There was no biology department, but I knew I wanted to major in zoology. And at some point, I guess it must have been during my second year, I got a job at the zoology department. Grover Stephens was the assistant professor who was in charge of labs for the general zoology courses, and he needed kind of a lab aide, lab assistant. He wanted to set up displays of some kind of zoological things for students, and so I got hired to do that. That was fun. It was a wonderful way for me to earn money and do zoological stuff.

Stephens had a research program, doing research on invertebrate zoology. And every summer, he went out to his own lab at Woods Hole, Massachusetts. The Marine Biological Lab at Woods Hole, Massachusetts is really a great international center for biology. Many scientists, many biologists, a lot of very well-known people, go out there every year, and there are courses. Stephens always taught a course in invertebrate zoology, along with some other zoologists. It's a course that I took at the end of my senior year. But he was out there, and he would teach the course, along with some other guys, and also do research. Interesting stuff.

He hired me as a research assistant, along with some of the graduate students from Minnesota who went along. And that was just a wonderful experience. I started to meet people who were obviously all interested in biology, but really doing it in a very serious way. I would hear lectures: Every Friday night, they would have lectures by really very famous, well-known biologists. It was a very stimulating place to be, a really wonderful place to be. Part of the research we were doing was research with fiddler crabs—*Uca*. These are crabs that turn color, dark and light, in a 24-hour cycle. Stephens was interested in the cyclical phenomenon. So I helped him do that kind of stuff.

So that was all my early influence in biology.

Oh, there's something else I've got to bring in. Our family just happens to have these intricate, intimate connections with an important part of the world of science. I said that Norm had worked with Skinner until 1943, when he went into the Army. For about a year after he graduated in '42, he worked with Skinner on what sounds like a crazy project. The idea was that you would have guided missiles, and you would have a pigeon in the missile. The pigeon would be able to see a screen that would show what

the missile was aimed at, and somehow or another, the pigeon would be trained to peck at the screen, and guide the missile into the heart of the cities. Kind of a crazy project.

Norm went into the Army after that, but when he came out of the Army, he went into the Signal Corps, relearned all kinds of electronic stuff that would be good for his research. By that time, Skinner had moved to Indiana University, so Norm went there to do his graduate work. Actually, Skinner moved on to Harvard at about the same time, but Norm did his Ph. D. work at Indiana University.

Now, completely aside, molecular biology was beginning in a very odd way. There was a young physicist named Max Delbrück, and he became interested in doing biology in a much better way. He was a very bright physicist, and he wanted to start doing biology in a way that was consistent with really good, modern science, modern physics. He came to Caltech in the late '30s. He met a young guy named Emery Ellis there. Ellis was working with bacterial viruses—bacteriophage. People had been playing around with bacterial viruses for quite a long time, and a lot of the work was very sloppy. Of course, Max couldn't put up with that kind of thing. But he and Ellis worked out really good ways of working with bacteriophage, so they started to do experiments with phage.

Around the same time, there was another young guy, an Italian microbiologist named Salvador Luria. Luria found out about phage, and also started to do work with phage. He also came to the United States, and Luria and Delbrück did a certain amount of work together. They were both at Vanderbilt University. That was the beginning of molecular biology.

Luria then got a job at Indiana University. His wife, Zella, was a graduate student in psychology, and Norm was a graduate student in psychology, so in that way, Norm met the Lurias. That didn't mean anything to me until quite a few years later. But our family had this connection with Luria, who started doing important work, beginning work, in molecular biology, that phage became one of the major tools in molecular biology.

Meanwhile, Norm, after he got his degree at Indiana, got a job on the faculty of Duke University, and that's where he was for the rest of his academic life, for the rest of his life. One summer, when I was about 16, I went down to spend a little while with Norm and his wife, Ronnie. We drove back to Minneapolis and stopped off in Chicago.

Oh, I haven't said anything about Lester. After the atomic bomb project, Lester got a job at the University of Chicago. He was working in the Institute for the Study of Metals, doing physical chemistry work on metals. So, as I say, we stopped off in Chicago for one night. Norm and Ronnie had one kid at that time. They went to a hotel; I stayed with Lester. The next day, after we had breakfast, Lester took me to a lab of a guy named Aaron Novick.

Novick had another kind of really interesting connection in the world of science. He was an organic chemist, and I don't really know what Aaron was doing during the war, but it was some kind of war-related work in science. I don't think he was really on the atomic bomb project, but something related to it. One of the physicists who was involved in the atomic bomb project was Leo Szilard. Szilard was a really interesting character.

Anyway, when the war ended, the atomic bomb was done, we had defeated Japan and the global war was all over, a lot of the physical scientists, the physicists and chemists, who had worked on things like the atomic bomb project, who had been involved in the war, wanted to turn away from death. They wanted to turn toward life, so it was natural for a lot of them to decide to start learning about biology, doing some kind of serious biology. And I think they had kind of a general attitude that biologists were not really doing experiments very well. Biology was kind of a sloppy science.

Anyway, Leo Szilard wanted to turn to biology. He got hold of Novick and asked him if he'd like to do some cooperative research. Both of them got positions at the University of Chicago. I don't remember exactly how that happened. At the same time, just after the war, Max Delbrück, who started to do phage biology, went to Cold Spring Harbor, which is on the north shore of Long Island. It was long a kind of home of classical genetics; a number of classical geneticists had worked there.

Max started to teach a short course, maybe a two-week course, in phage biology, teaching people how to work with phage. A number of physicists and chemists who were interested in turning to biology went and took Max's course, because in a short time, you could learn basic methods for doing work with phage, and start doing really good, really well-done, well-defined experiments. Novick and Szilard took Max's course, and went back to Chicago, and started to do work—actually, I don't know how much work, but they did some work with phage. They also did general genetic work with bacteria. There had been another strain developing, of people starting to do bacterial genetics, learning about bacteria in genetic ways. Szilard and Novick invented a device called the chemostat, which is a device that allows you to grow a culture of bacteria endlessly, continuously, in exponential phase, and do experiments on those bacteria.

I was saying that I'd gone to visit Norm and Ronnie when I was 16, and I stayed with Lester that one night. In the morning, Les took me over to Aaron Novick's lab, and introduced me to Novick. Because I had been interested in things like watching birds, and sloppy collecting of shells and so on, I think he wanted to show me what real biology was all about, so he introduced me to Novick. We spent a few minutes talking to him. He didn't impress me very much. He had this warm room. He'd keep a room at 37 degrees for the chemostat, so you could keep on growing your cultures of bacteria that you

were experimenting with. It didn't really impress me very much. But that was something in the background.

So I'm in Minnesota, and I've had this contact with Grover Stephens, going out to Woods Hole. One of the faculty at Minnesota was Nelson Spratt, who was an embryologist. I took his embryology course. Embryology, in general, seemed like it was a pretty good field of biology to maybe go into. I graduated in 1958, and about the same time, Spratt had a graduate student named Mel Steinberg. Mel had just gotten a job on the faculty of Johns Hopkins. And Mel apparently thought I was a bright guy, a worthwhile guy, and he convinced me to go to Johns Hopkins for my graduate work. There was an embryologist named Margaret there that I could work with.

So I decided to do that. I got a Woodrow Wilson Fellowship for my first year of graduate work, and I went off to the university, to Johns Hopkins. At Johns Hopkins, I was just one of about 20 new graduate students. I did all kinds of basic stuff, learning biochemistry for the first time, and similar things. That was in the biology department.

Right next door there was a biophysics department, and there was a guy named Charlie Thomas, who was about the only biophysicist there. Anyway, that's what they called him. Charlie also was a molecular biologist. This was 1958 to '59. Charlie had a weekly seminar, and I started to go to Charlie's seminar, and started to learn a little bit about what was being called molecular biology, which was growing up at that time.

At Christmastime that year, I went back to Minneapolis, for Christmas – for Chanukah actually! [laughs] and came back. Norm was on sabbatical at Wisconsin, so I went through Wisconsin—on the train, I guess—then back to Chicago, and then took the train from Chicago back to Baltimore. It was a delightful trip, because of a beautiful young woman, who was one of my fellow graduate students at Hopkins, and she was coming back from Iowa, so I was able to spend some time with her. If I had been properly mature for my age, and if I had had my senses, I would have courted her and married her. Anyway, she was a delightful companion on the trip back.

I had the latest issue of *Science* with me. There was a little notice in *Science* that Aaron Novick, who was formerly of the University of Chicago, had gone to the University of Oregon to start a new Institute of Molecular Biology. That sounded interesting.

Living as I had at Minneapolis, I hardly really knew about life. I was living at home. I didn't have much of a social life. I had good friends, but I wasn't becoming a really independent, adult person. So my one year at Hopkins was really important for my social development, my personal development. I lived with a group of guys. All together, we lived in one of the houses there. That was important to me,

but I really needed to be off being more of an independent person in some way. So the idea of going all the way out to Oregon kind of intrigued me.

I wrote to Novick and reminded him that we had met—that Lester had introduced us—and asked him about coming to Oregon. He wrote back and said he vaguely remembered me, and told me it was possible that I could become a graduate student out there. I think that he contacted Charlie Thomas, and found out whether I was okay or not, and I guess Charlie said, “Yeah, he’s okay.” So I said, “Yeah, I’d really like to come out there.” This was early ’59. “I would really like to come out and become a graduate student there.” Aaron immediately wrote back and gave me orders of what I should do.

I told you that people had started to take Max Delbrück’s course in 1946-47. Max taught that course in phage biology for a couple of years, and then he turned it over to other people. But the course continued. Every summer, there would be a phage biology course at Cold Spring Harbor. And bacterial genetics was developing. Actually, one of the people at Johns Hopkins, Philip Hartman, was one of the early bacterial geneticists, so there was also a Cold Spring Harbor course in bacterial genetics. And Aaron, being in the heart of molecular biology, talked to the people who were teaching these courses, and tried to get me into them, but they were already filled. But he got me in as a so-called “auditor.”

The course on phage biology was being taught by Frank Stahl and George Streisinger, two prominent, well-known, important people in phage biology. Aaron’s order was, “You’re ordered to go to Cold Spring Harbor for the summer and take these two courses.” The same way that I had been at Woods Hole, I got into being there at Cold Spring Harbor, being in many ways about as closely tied to modern molecular biology as anybody could be.

Frank Stahl was already becoming well known for an important experiment that he and Matt Meselson had done, essentially showing that DNA replication had to take place the way it took place. Frank became one of the faculty members at Oregon, and then, about a year or so later, George also became a faculty member there. I was in the middle of all this stuff. It was—oh, this was still back at Woods Hole. After I had graduated from Minnesota, after my senior year, I went back. I worked again for Grover Stephens in his lab, and I also took the invertebrate zoology course he was teaching.

During that summer of 1958, I was back there at Woods Hole. There was all this socializing, and parties and stuff. I remember one guy came along with a little nametag that said “J. Watson, Harvard.” And like a stupid wise guy, I said, “Oh, Harvard? What do you do at Harvard?” He said, “I’m a biochemist.” I wasn’t much interested. But a couple of the graduate students from Harvard came over to me [laughing] and said, “Oh, you wonder what Jim Watson does at Harvard?”

Oh! I suddenly realized that was Jim Watson, of Watson and Crick. It turned out somebody had gotten me into contact with some people who liked to go out birding, or birdwatching, as we called it in those days. I was supposed to go out birdwatching with them the next day, and I did. Jim Watson came along, so here I was, birdwatching with Jim Watson and other people. As I say, both in the laboratory and socially, a young integral part of modern molecular biology as it was developing at that time.

**Beck:** You were still quite young at that point, wasn't it?

**Guttman:** Yeah, I graduated at 21, 22, somewhere in there.

**Beck:** And the science itself was still quite young, too.

**Guttman:** Oh, the science itself was really very young. There was hardly anybody being called a molecular biologist.

Anyway, I went out to Oregon. It was a new life; it was an independent life. I was all by myself. I guess I was a little bit lonely for a while, but I made good friends. There were wonderful people there at the institute. I started to do research, and eventually, about '61 or so, people were wondering, exactly how were proteins made, and how did genes work? We were working on some ideas.

Aaron was very much a part of the whole thing. It was almost a ritual—to be one of the really “in” people in molecular biology, you had to spend some time in Paris at the Pasteur Institute, with biologists Francois Jacob and Jacques Monod particularly. Jacob and Monod were working with *E. coli*, the bacteria, and working out basic ideas about how genes are controlled. Aaron was working on the same general kind of thing, a system for enzymes involved with lactose metabolism. He had mutants that were defective in various things, and had strains that were making large amounts of the enzymes, and so on, and so I fell into doing that kind of stuff.

But he was constantly in contact with all of the European scientists, like Monod and Jacob. There was a young guy who'd come from South Africa, Sydney Brenner, and Brenner and Jacob put their heads together, thinking about gene regulation.

They said, “We think that the information is actually carried to the ribosome.” Ribosomes are where proteins are made. “We think the information is actually carried to the ribosomes by a special kind of RNA that we call messenger RNA. And we're going to do these experiments – this summer, we're going to Caltech and work with Matt Meselson to try to show this.”

Indeed, they did show it. They discovered messenger RNA, and the general idea of messenger RNA. And Aaron and I had exactly the right system for investigating that, so I worked on this. We were able to show—I was able to show—that when a new gene is turned on, the genes for lactose



metabolism, you get a new messenger RNA. That was my Ph.D. thesis, and it was good. It was a little bit of important work in the development of molecular biology.

**Beck:** Your piece of the edifice.

**Guttman:** Yeah. At the same time, I was interested in biology as a whole. Actually, there was one rather key event. I was having coffee in our coffee room one day, and Aaron and Frank walked in. Aaron was enthusiastic about the ideas of . . . hold on a second, it'll come back to me. I'm having trouble with people's names these days.

Anyway, the general idea that this one fellow had promoted was that an organism can be defined very simply. An organism is a structure that reproduce itself, and mutate. They were talking about this idea, and I heard this. It really struck me—my goodness, how simple—that the fundamental thing that makes an organism what it is, is that it's really something that can reproduce itself, but it can mutate. So therefore, that produces selection and evolution, and that's the fundamental story of what biology is all about. That was one thing that I heard.

I was just generally interested in biology. And even though I was happy to be so deeply involved in molecular biology, so deeply involved in research, the idea of teaching— well, two things that go together: teaching, and also writing about the science. I was changing . . . I'm trying to figure out the best way to say this . . . but up until that time, I had kind of envisioned myself as being primarily a researcher. I knew that the academic life, to me, was essentially what Lester and Norm had done. Lester wasn't involved in any teaching, but he was involved in research, at the University of Chicago. Norm was doing teaching and research, and that, to me, was sort of the ideal academic life.

But being involved with research, I think a part of me was discovering that I wasn't as enthusiastic about the research as I had thought I would be. But I was really getting enthusiastic about the idea of teaching and writing about my understanding of biology. I got a post-doctoral fellowship. I moved to Caltech and worked with phage biologist named Bob Edgar. I didn't do great research with Bob Edgar, because I was really becoming much more interested in the idea of writing a book about what biology is all about. It certainly made Aaron look at me with less enthusiasm, let's say. I did all my work with him, but when it came to what I was doing afterward, whether I was doing serious research in Bob Edgar's lab, I think it took me down a notch in his eyes.

But it was really becoming an important idea to me; that I would . . . I had all kinds of ideas that other people hadn't expressed about what biology is all about. So I went to Caltech, I worked with Bob Edgar. I got onto working with certain interesting phage mutants, but the research was not the main thing. I was just really getting much more enthusiastic about the writing.

I worked with Edgar, doing my research, for two years, and had to get a job. Meanwhile, I had gotten married while at Caltech. That's where Erica was born, in Pasadena. The one job that came along was at the University of Kentucky. In the Medical School, there was a department of cell biology, which was headed by a guy named Dick Schweet, who had become pretty well known for doing research on protein synthesis. There weren't any other jobs coming along, and this was a pretty good offer for the time, so I went to Kentucky.

I set up a lab, and I continued the kind of research that I'd started with Edgar, but the important things were becoming the writing and the teaching. We were teaching medical students, so I did some of that. I didn't make Schweet very happy at times, because I was trying to teach the medical students some radical ideas about how certain biological systems worked. But in any case, I did that kind of work.

**Beck:** You published a book while you were there, right?

**Guttman:** Yeah, I got my first book done shortly before I left there. Dick Schweet died—he got killed in a plane accident—so they got a new head of the department in. And over in the Arts College, there was a department of microbiology. It was not a very good department. They had kind of dumb old farts running the thing. There was one young guy who was a molecular biologist, microbial geneticist. He was a guy that I had some respect for.

Somehow, they kind of half-joined the department of cell biology in the Medical School with the department of microbiology over in the Arts College, so I was allowed to teach a general biology course for two years. That was great for me, because I was really able to start really developing my ideas about how biology ought to be organized, and how it ought to be taught, and I think I made friends among the students.

Kentucky wasn't much interested in teaching. They were more interested, like so many big universities, with having their faculty members doing research and becoming prominently known for their wonderful research. But I was allowed to do some teaching there, and I was teaching in a way that really spoke to the students, and the students appreciated that. I still have somewhere some nice notes they wrote me about appreciating the teaching that I was doing.

It became clear that I wasn't going to be able to stay at the University of Kentucky. That was early, about 1971. Luckily, another notice appeared—probably in *Science*—that this college called The Evergreen State College was opening up, and they were looking for faculty. And I really needed a job [laughing] for the next year, and I applied. The main connection I had was with Don Humphrey.

**Beck:** Did you know Don before Evergreen at all?

**Guttman:** No, I didn't know anybody there. One of the secretaries, whom I met after I came here, was one of the people who was looking through all the applications that had been sent into this new place. She said there was something in what I had written, something that I had sent in, that got to her, and she felt that I sounded like the kind of person that would be good for Evergreen. So she got me on the shortlist of people who were being considered more strongly, and Don Humphrey apparently liked it.

**Beck:** Do you remember who the secretary was?

**Guttman:** I don't remember. I came out—that's right, there was a possibility of a job, oh, at one of the little colleges down in California, so I came out on a trip. I went down there and gave a seminar, and then I came up here and met people. We all liked one another. I can't remember exactly when that was, but at least it was at a time when Evergreen was hiring. I think they hired 41 new people. That was for the second year of teaching.

**Beck:** Do you remember any of the people you met when you came up for that trip?

**Guttman:** Yeah. I met Betty Ruth Estes, and I met Betty Kutter, and Don. Byron Youtz. Those are the main people I can remember. But it just seemed like an ideal place, it seemed like the right place. I wanted a place that appreciated good teaching. I guess I was still thinking about doing more writing about biology. It seemed like a place that would appreciate that. It was just a compatible place, so I was really happy when Don said I was on the shortlist of the last 11 people to be hired. Then, finally, they indeed hired me. So we traveled all the way up to Washington, and here I am. [laughing] That was a wonderful time.

**Beck:** Did you join the faculty in the fall of '71 or the fall of '72?

**Guttman:** The fall of '72.

**Beck:** What was it like when you came to Evergreen? What was your first experience of the campus? What do you remember about it then?

**Guttman:** By that time, the campus was pretty well built. The people who came in '71 came to a campus that was only partly built, so they had to have their classes in other places at first. It just seemed like a wonderful place. It was quiet. It was surrounded by evergreens. It was kind of an ideal place.

We're getting into the place where I need to start going back and seeing if I can reproduce more of what I was actually experiencing, actually talking to people. You see, the first year, quite a number of us were put on individual contracts, handling individual contract students, so I wasn't into the coordinated studies program. This is where I've really got to go back and do some reading and thinking

about it. I guess I had heard about the idea of the coordinated studies program, because I was given literature to read. I'd been given stuff that Merv Cadwallader had written, I think.

But I don't remember ever having a conversation with anybody about just why coordinated studies are so important, and exactly how we should run these things. What should be our major emphases? Maybe we'll have seminars. What should we do at the seminar? What should we discuss? How should we run these things? What do we have to be careful of? What are the things that could go wrong in a coordinated studies program? And here, you're trying to get two or three or four faculty members, with quite divergent interests and specializations, getting them together to have a conversation, and actually teach a bunch of students. People's personalities are going to clash. What should we do to all become a really united group, working together toward an end? And I can't remember conversations about that.

A few days ago, I got together with Dave Milne—we've seen Dave quite a bit lately—and I had a bit of a conversation with him about his experience in the early days. By the way, Dave is a guy you've got to interview.

**Beck:** Yeah.

**Guttman:** He said that nobody contacted him.

**Beck:** Well, I think I will contact him, actually. I think there are a couple people I'd like to talk to, but Dave is one of them.

**Guttman:** Oh, Dave is a fascinating guy.

**Beck:** Was he on the planning year, or was he first-year?

**Guttman:** First-year.

**Beck:** First teaching year.

**Guttman:** He got together with a number of guys who were teaching the first two years. They did programs in Human Ecology and Political Ecology. The faculty mix—you reminded me who they were—were people like—Don Humphrey was Dean, but there was Will Humphreys . . . names are escaping me right at the moment.

**Beck:** Wasn't Oscar [Soule], for instance? Or, he was a dean?

**Guttman:** No, Oscar wasn't a dean. But Jeff Kelly . . . I'm seeing faces, but we're right now having some difficulty . . .

**Beck:** Was Beryl Crowe there?

**Guttman:** No, he wasn't part of that. Don might have been teaching as part of that. Anyway, it was a really excellent group, and they had some really fine programs and great success. But when I told him

that I could not remember any conversations in which we had gone over these basic ideas about coordinated studies, he was really shocked at that. Because I'm sure that within the programs he was doing, they had had these conversations. What are coordinated studies all about? How should we run them? And so on and so forth.

**Beck:** Was the whole of your first year doing individual contracts?

**Guttman:** I guess it was. Mike Beug and I had offices right next to each other—Mike, of course, is a chemist—and I know we had a few students who we worked with together. I think we gave a kind of a short course, a light course, in chemistry together.

**Beck:** To your individual contract students?

**Guttman:** To our individual contract students. I'm trying to go back and remember, but I think we did that in a couple of other cases, where we had students who were doing related things, and we got them together in min-courses. That was certainly a good idea, but it wasn't like doing a coordinated studies program.

**Beck:** What's your recollection of the students? Do you have a recollection that you were working at the time, that first year? Were they up to speed, or did they seem like they really needed a lot of help?

**Guttman:** One of the really good things about the early years of the college was that, in general, we had older students, who were above typical college age. They were a delight to work with, in general. I remember being a little bit shocked at some of their language. I had come out of a quite conventional university system [laughing], where ladies and gentlemen do not speak quite that way. And Evergreen students spoke a little bit with a language I wasn't used to. Especially coming out the mouth of a rather charming, beautiful young woman, I didn't expect those words. [laughter]

But in any case, I think this was something that was carried over into the early programs I did. After that first year, I got into a couple of coordinated studies programs. The students, I believe, were, on the whole, mature. They were serious. To a large extent, they were people who had been off doing something other than going to college. I think they were quite delightful, in general, because they had come back to college knowing what they really wanted to do. They were not just flopping around in general courses, but they were coming into courses where they could discuss things of real interest to them, and they were mature enough to discuss those things in serious ways, and to do serious writing about their subjects.

That, I think, was one of the really great things about Evergreen. After a few years, there started to be more of a push to try to get students right out of high school. I don't want to say that that was a mistake, but it changed things. Seventeen- or 18-year-old students right out of high school, I think, are

often just not ready to go to college. They don't have the maturity of various kinds—the intellectual maturity, the seriousness of purpose—to really get into what I think of as serious programs.

A coordinated studies program, I think, to be successful, has to be a program that interests the faculty. The faculty have various ideas and interests all around a central topic, and they get together, and they have a wonderful time talking to one another, and developing all of these ideas together around a central theme. If you're a 17-year-old, who's ready for general biology and English literature and something like that, you don't have the maturity to really dig into the thesis of a good coordinated studies program. I think that might be one of the problems that we're having at Evergreen right now. That's something we could talk about.

**Beck:** Yeah.

**Guttman:** I thought the students we had, particularly in the early years, were wonderful. And I really enjoyed it, in later coordinated studies programs, having students—often women, mothers, who were up in their late thirties and into their forties—who had come back to college to really talk about something serious, and educate themselves. That's always been one of the delights of the college to me.

**Beck:** Yeah. I know you want to get into the Archives and take a look at some papers, and probably we can get into more detail the next time we talk. But just in terms of your general impressions, what was going on in the early days? Were there things that seemed particularly exciting? Were there things that seemed really frustrating? Do you have any kind of general impressions?

**Guttman:** I can't give you any kind of a good answer. I don't think of things being frustrating. I guess I have just a general feeling of being part of a community with really interesting people. Have you ever seen this collection of autobiographical things, I think we called it "The Class of '72"?

**Beck:** Yes.

**Guttman:** I looked at that not too long ago, and a lot of people who were in that left after a short time. But on the whole, this was one of the most fascinating collections of people I'd ever been associated with. People, sure, mostly with academic backgrounds, with people with all kinds of very different backgrounds.

**Beck:** You were saying that there were a lot of people with really varied backgrounds.

**Guttman:** A number of them, after a short time, felt that they just didn't fit in. But I think that those who did stay, at least for a year or two, I think, really found themselves in really interesting, intellectual situations, and they found that they had important ideas to contribute to interesting programs. But I can't say a whole lot more about that.

**Beck:** Okay. We can come back to that. Are you starting to run short on time?

**Guttman:** No. I have an appointment at 2:00.

**Beck:** Okay, right. Who are some of the people who stand out from the early years as people with whom you worked or taught, and that you seemed to really learn a lot from?

**Guttman:** Don Humphrey was an important guy. And Will Humphreys. Somehow, I was able to identify Will as being a philosopher of science, who had some important things to say. I know in the second year, I did this program of Nature and Society. The third year, I did this program with Will Humphreys, and with Jack Webb and Carol Olexa. There were problems with the program, a lot of them having to do with the family problems and personalities of the people involved. I probably shouldn't go into that.

**Beck:** It's up to you.

**Guttman:** I may want to say something about that. But, to a large extent, the program finally devolved only into Will and me working together with the students. And, again, here's where I have to go back and try to recover more of what we actually worked on. But there were problems that kept the others from working with us.

That second year, I was teaching among other people with Mark Papworth. I taught with Mark a few times—at least one time later—in the Life and Health program. Mike was really quite a fascinating guy. He was an anthropologist, but he was interested in all kinds of things. Maybe that's one of the things that make Evergreen faculty appropriate and successful, being interested in all kinds of things.

**Beck:** Right.

**Guttman:** I said something about this that I recognized in myself; that I wasn't really so interested in becoming just a molecular biologist, spending my life doing research in phage biology or something like that; that my interests were much broader. So I was happy to come to a place where those broader interests were valued, and where I could pursue those broader interests. I really think, in general, Evergreen faculty are successful, are happy, when they do have broad interests.

**Beck:** But you did do some research at Evergreen. You worked in the T-4 phage lab.

**Guttman:** Yeah, I was very lucky that Betty Kutter and I were hired at the same time. Betty is a very interesting person. Quite broadly educated, I think. She's very strongly interested in phage biology and phage research, and in the application of phage to medical problems. I won't try to get into all of the phage biotic stuff, but that's been one of her major things, pushing programs—I don't mean teaching programs—pushing programs of using phage more and more as medical cures. She's been quite concentrated on that.

It was Betty's idea to start having phage meetings. It's been kind of a tradition otherwise in the larger community of molecular biologists to have meetings every year where people get together and share their research. I went along with that, but I was never very enthusiastic about it, whereas Betty was.

The really important thing for Evergreen, I think, has been that Betty started—she already had a research grant that she brought with her, so she was able to start a research lab. I joined her after a while, being part of the research lab. That was a lab where many students were able to get in and learn what serious research in science is all about. I think that's been one of the really great things. From my point of view, that's about the most important thing Betty has contributed to the college. Very important.

**Beck:** So you started working in the phage lab after a few years. Would that have been '74. '75?

**Guttman:** I don't know exactly when I started to get into it. Boy, I'd have to go back and try to reconstruct that.

**Beck:** Okay.

**Guttman:** I brought with me things to continue to engage in research. I brought phage with me, phage stocks, phage mutants. I even brought a bunch of pipettes, tools used in that kind of research. I brought a bunch of pipettes with me from my lab at Kentucky.

Yes, I was happy to join in there, but the direction of the research was primarily Betty's direction. I think my major contribution was trying to help the students understand what the research was all about, and what they ought to be doing. I can't elaborate, not right now.

**Beck:** Fair enough. Is there anything else that you'd like to talk about with respect to just the early years in general terms? I know that we're going to come back and have a conversation a little bit later. For instance, I was thinking about, in terms of teaching, one of the things that I think you had a big part in was getting the SPLU [Self-Paced Learning Unit] Lab set up. Was that true? I know you authored several of the SPLU labs, at least.

**Guttman:** Hold on a second. First of all, we had one project going for a couple of summers. I think Fred Tabbutt was in charge of that. I think Fred had gotten a grant, an NSF grant, for us to make self-paced learning materials. I worked on that; I was delighted to do that. I made several units. I made some units in basic mathematics, basic biology, and so on. I don't know how much I actually did by way of setting up the Self-Paced Learning Lab.

**Beck:** Okay.



**Guttman:** That's interesting. I hadn't thought about that. Let me go back and think about that some more.

**Beck:** Okay. It seems as though maybe the thing to do is to pause now. Maybe we could come back and talk about some of the questions about the early years, and more specific things that you did at a later point, just because it seems as though there's some details that you'd like to get clear on, by looking through some of the papers.

But maybe just a general question, to jump ahead a little bit in time. What's your sense of how the college changed, or developed, over the first 10 years or so?

**Guttman:** Oh, my. [sighs] That's a good question. That's hard for me. The question may be tied, in part, to who was president, and how presidents changed. I'd have to go back and actually get the years.

**Beck:** But Charlie [McCann] was president until '76 or '77. Isn't that true? Charles McCann?

**Guttman:** No, I think it must have been later than that when he got replaced. But who was the first guy who replaced him?

**Beck:** And then Dan Evans became president.

**Guttman:** That's right.

**Beck:** Dan Evans was president in the late '70s.

**Guttman:** That's right. Boy, these are things I've really got to go back and think about. I can't just do it off the top of my head.

**Beck:** Right.

**Guttman:** I was one of the people who was working, sometimes pretty closely, with Dan Evans. I remember admiring him, and admiring his leadership. Because with Charlie, Charlie was a very kind of a low-key, relaxed guy. And he had this great vision, and it's wonderful that he had this great vision, but I wonder if the vision was anything much more than a general idea of what the college should be like, what's the best way to teach, the best way for people to learn? A vision of a community of scholars, who would get together in various combinations to pursue their scholarly interests, their common interests. But I don't think of Charlie as having any particular other direction for the place. When Dan became president, I don't know, I was on a DTFs of various kinds.

One of the things I've got to do. In my computer, I have a whole lot of biographical, historical stuff. I've got a list there—one sheet—of the various DTFs that I was on. But I can remember working quite closely with Dan and some other faculty on certain projects, and Dan was pushing us ahead. I think he was pushing us in certain directions where he felt that we needed, let's say, some kind of

structure, something we should actually be doing, rather than just getting together and being nice scholars. I'd have to go back and try to remember what those things were.

**Beck:** Right. He was president until, I think, it must have been about '84, somewhere in there, where he was appointed to the Senate.

**Guttman:** Yes.

**Beck:** Then he left Evergreen, and there was a period of time, I think, when Les Purce was president as an interim.

**Guttman:** Yeah.

**Beck:** And then Joe Olander came.

**Guttman:** Yes. Then Joe Olander came. And those were difficult days. Those were days when [sighs] there were a lot of conflicts between Olander and the faculty. I've got to go back and recall names, but some of us were working together, and essentially trying to keep the college straight, while Olander was doing whatever he was doing to trying to set us off in wrong directions.

**Beck:** Do you have a sense of what Olander was doing then?

**Guttman:** No, I'd have to go back and reconstruct that.

**Beck:** Okay. Well, why don't we stop for today. I'm going to end the recording. It looks like we've got about an hour and 20-some minutes here recorded. We'll just make a time to talk again in the next week or so. How does that sound?

**Guttman:** Yeah.