SCIENCE HISTORY INSTITUTE

MARY-DELL CHILTON

Life Sciences Foundation

Transcript of an Interview
Conducted by

Brian Dick

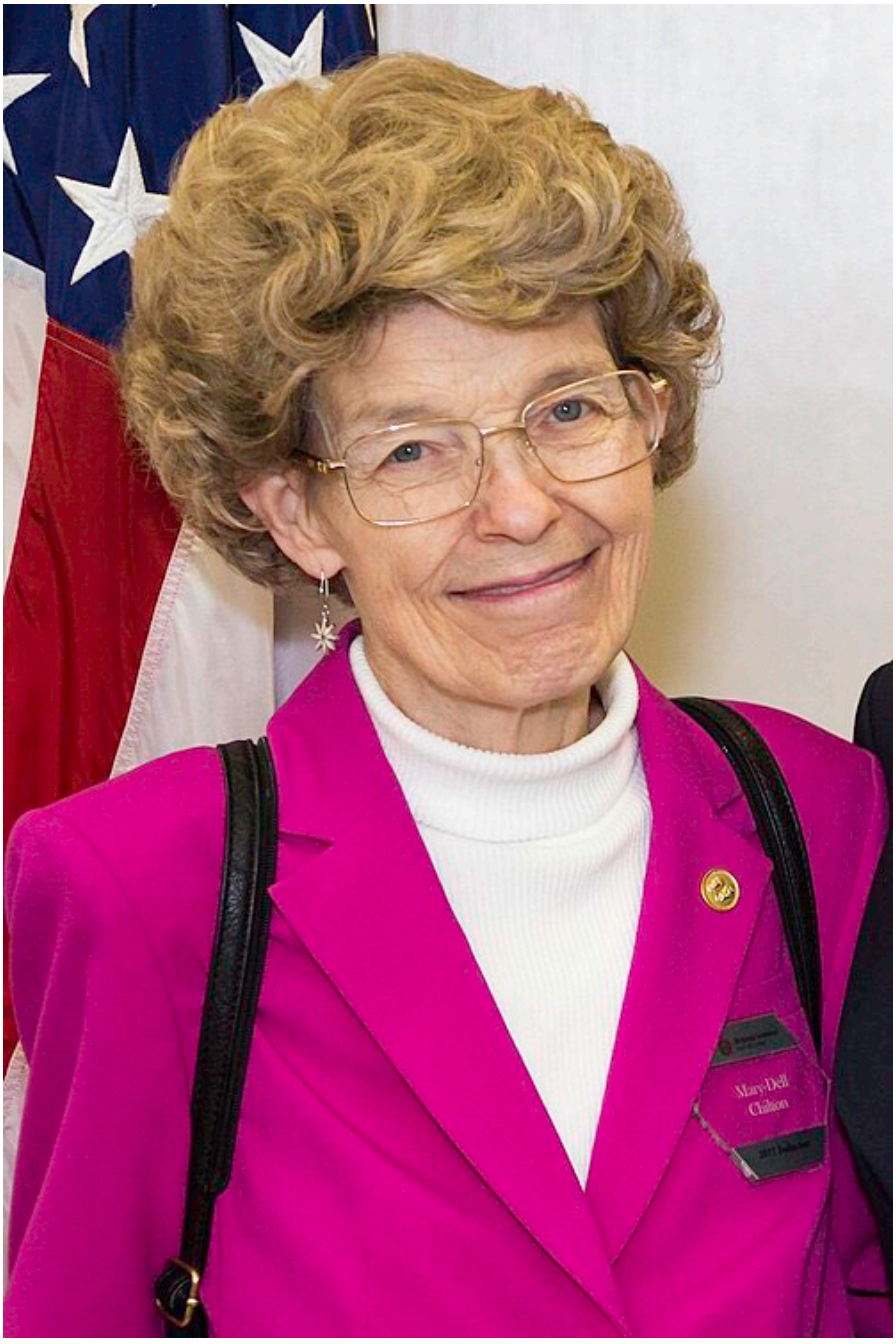
at

Syngenta Biotechnology Inc.
Durham, North Carolina

on

29 August 2013

(With Subsequent Corrections and Additions)



Photograph of Mary-Dell Chilton at the US Department of Agriculture, May 12, 2015

Mary-Dell Chilton

CHEMICAL HERITAGE FOUNDATION
Center for Oral History
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MARY-DELL CHILTON

1939	Born in Indianapolis, Indiana, on 2 February
2026	Died in Carrboro, North Carolina on 24 June

Education

1960	BS, University of Illinois at Urbana-Champaign, Chemistry
1967	PhD, University of Illinois at Urbana-Champaign, Chemistry

Professional Experience

University of Washington

1967-1970	Public Health Service Postdoctoral Fellow
1970-1971	Lecturer
1971-1979	Research Associate

Washington University, St. Louis

1979-1983	Associate Professor, Biology

Ciba-Geigy Corporation

1983-1993	Various positions

Syngenta Biotechnology, Inc.

1993-2018	Various positions

Honors

1985	National Academy of Sciences
1986	Rank Prize in Nutrition
1986	David Gottlieb Medal, University of Illinois
1987	Hendricks Medal, American Chemical Society
2000	John Scott Award, City of Philadelphia
2002	Benjamin Franklin Medal in Life Sciences, Franklin Institute, Philadelphia
2011	CSAA Presidential Award, Crop Science Society of America
2013	Triangle Business Journal Lifetime Achievement Award
2013	Order of the Long Leaf Pine Award

2013	Raleigh News & Observer Tar Heel of the Year
2013	World Food Prize
2015	USDA Hall of Heroes Inductee
2016	National Academy of Inventors Inductee
2018	World Changer Award, Research Triangle Park Rotary Club
2023	National Medal of Technology and Innovation

INTERVIEWEE

Mary-Dell Chilton was born in 1939 in Indianapolis, Indiana but grew up with her maternal grandparents in Southern Pines, North Carolina. She went to Catholic school where she excelled academically. After she became a teenager, Chilton moved back in with her family. She continued to do well in school, particularly in mathematics and science. When she was seventeen years old, she built a Cassegrain telescope as part of a science talent search run by Westinghouse. After graduating from high school, she attended the University of Illinois, initially intending to become a physics major but quickly transitioned to chemistry. Chilton stayed at Illinois for graduate school and took a course that changed her life and career, "The Chemical Basis of Biological Specificity," taught by luminaries in molecular biology. The course sparked her lifelong interest in biotechnology. Midway through her graduate career, she moved to the University of Washington with her thesis advisor, Benjamin Hall, and earned her PhD from Illinois *in absentia*. She became a postdoctoral fellow at the University of Washington and later a temporary lecturer after her second son was born. While teaching, Chilton learned about the unusual claims surrounding *Agrobacterium* from one of her students, Thomas C. Currier, and learned Eugene W. "Gene" Nester was working on a grant application to study crown gall disease. She wrote the grant application and started a new position at Washington when the grant was funded. In that new role, Chilton began studying *Agrobacterium* and discusses in detail the discoveries the lab made in the interview. She talks about doing brute force experiments with everyone in the lab over the weekend to learn more about *Agrobacterium*.

In 1979, Chilton left Washington to become an associate professor of biology at Washington University in St. Louis due in part to some conflict with Gene Nester. Once at Washington University, Chilton began focusing on the disarming of the tumor-inducing plasmid, which led to success with transgenic plants in 1983. She talks about attending the crown gall conferences, competition with other scientists and labs, and the Miami Winter Symposium. In 1983, Chilton moved from academia to industry when she took a position at Ciba-Geigy focused on getting Bt to work in plants and transforming corn and soybeans. Chilton moved up in the company and transitioned to more of an administrative role, although she preferred doing experiments—the work of a scientist. She got her wish in the 1990s when Ciba-Geigy became Syngenta, and she became "a molecular biologist again," in her words, with a focus on big plasmids. As transgenic plants began to hit the market in the 1990s, Chilton talks about the GMO controversy. She concludes the interview by talking about her more recent work like gene stacking, the World Food Prize, and her desire for transgenic technology to be embraced.

INTERVIEWER

Brian Dick received his PhD in sociology from the University of California, Davis. Before coming to the Institute he was a research associate at the Life Sciences Foundation. His research interests include the history of agricultural biotechnology, the emergence of the biotech industry, and the Human Genome Project.

ABOUT THIS TRANSCRIPT

Staff of the Life Sciences Foundation conducted this interview, which became a part of our collections upon the merger of the Chemical Heritage Foundation and the Life Sciences Foundation into the Science History Institute in 2018. The Center for Oral History at the Science History Institute edited and formatted this transcript to match our style guide, but, as noted, Science History Institute staff members did not conduct the interview.

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INTERVIEWER: **Brian Dick**

ALSO PRESENT: **Steven Goldsmith**

LOCATION: **Syngenta Biotechnology Inc.**
Durham, North Carolina

DATE: **29 August 2013**

DICK: Today is Thursday, August 29, 2013. We're here in the office of Mary-Dell Chilton at Syngenta in Durham, North Carolina. So, just to start off here, if you could maybe tell us when you were born, where you were born, in 1939, and also some of your family background, your parents, any siblings, things like that.

CHILTON: Well, as you said, I was born in 1939 in Indianapolis, Indiana. My dad was an insurance salesman, so we did a lot of moving around. The family did a lot of moving around. I can't remember a whole lot of that. The first thing I remember I was given to my grandparents, who lived in Southern Pines, North Carolina, and the exact reason I was given to my grandparents is lost in the mists of history.

But I think my mother was having some problems. I had a brother just a year-and-a-half older than I am. He was—let's face it—he was a hellion, and he was very aggressive toward me, so it may have been to some extent for my own safety and comfort. It may be that my mother wanted me to have the experience growing up that she had. She grew up in Southern Pines, North Carolina. I don't know. I really don't know what the reasons are, and maybe there were many, and maybe they were complex.

But, anyway, so I was given to my grandparents, and that's the first home that I can remember. Big stucco house with a upstairs with three bedrooms and an attic that I liked to explore and a big garage. I liked to jump off the roof with my playmates. Made my grandmother [Henrietta Dell Hayes] very angry. [laughter]

DICK: About how old were you when you moved to your grandparents?

CHILTON: I must've been three or four. It was certainly a year before I started going to kindergarten. And at my . . . my grandfather [Claude L. Hayes]—I should tell more about my grandparents since they were the ones who raised me—my grandfather owned the bookstore in Southern Pines, and he owned a building, a big double building. And my grandmother had the

store next to his storefront. She sold dresses. She had Mrs. Hayes' Shop. And she sold the first ready-made hats that were sold in that time. She went to New York on buying trips. One time she brought me back a beautiful little blue velveteen dress with a lace collar, and they had a photograph made of me in that dress. I remember it. I even remember the itchy dress, but I loved it; it was so beautiful. [laughter]

Anyway, so my grandmother was a businesswoman, and my grandfather was supposedly a businessman, although the family history has it that she was the real brains of both of those businesses, which made money. There's a photograph of the fiftith year anniversary of his bookstore, and it has my . . . . I'm in the middle of the picture with my legs all wrapped around each other because it was so cold, and I was trying to stay warm. [laughter]

And my grandmother and grandfather were in that picture. My mother is in it and a few other more distant relatives. He gave away—this was just after the war years—and he gave away nylon stockings, one package to each lady who came into the store. I think you didn't even have to buy anything; you just got one for coming in. That was a big deal. Stockings were very expensive.

DICK: And then that was a way to sort of bring in customers and . . . ?

CHILTON: Yeah, it was just, "Thank you for your fifty years of business," is what it was, I guess. So it was a very festive occasion.

DICK: And did you . . . he had a bookstore, so I'm . . . . I think that you had this opportunity to be surrounded by books. <**T: 05 min**> Were you in there going through the books reading?

CHILTON: Well, there are two answers to that. [laughter] One is I was in heaven because I could borrow any comic book that came into the store just so I kept it clean and put it back within a day or two. So—excuse me—I certainly exercised that privilege. [laughter] But the other thing is my grandfather knew a fair amount about children's books, and so all of the classics he brought home and read to me. I was read to a lot as a little child. And I became an avid reader myself. I read every horse story that the library had. Albert Payson Terhune's dog stories. Yeah. And I got into the Nancy Drew mysteries and the Hardy Boys mysteries and all that. So, I'm a good reader; to this day, I enjoy reading.

Okay, so I went to Catholic school. We weren't Catholics. My grandfather was a member of the Episcopal church, in fact, but the Catholic school was supposedly a better school academically. So I went to this kindergarten starting at age four; they let me in, kind of, early.

With my birthday in mid-year, I was between ages for the grade . . . between grades for the age or something. So I was entering first grade at age five, which would be pretty early now.

I guess I did okay in school. I went to that Catholic school for grades one through four. My grandfather died along about there, and my mother . . . . I was still left to live with my grandmother to keep her company, I guess. And my mother insisted that I needed to go to public school, so they switched me over. And they gave me a bunch of standardized tests, and they wrote the results on my report card. And it would show what the subject was—mathematics or whatever—and then it would show this grade, this month, and that grade, this month my level where I had tested.

And it was astonishing as a rising fifth grader, I was testing at nineth, tenth, eleventh grade in some of the subjects. And that was the first inkling they had that I was bright. [laughter] I had the feeling in Catholic school that I was struggling and wasn't really very good at things. But maybe part of what I wasn't very good at was the litany and some of the praying. Things like that. I don't know.

DICK: Well, as I say, was religion influential at all at this time? Were you attending church, things like that? Or was it, kind of, just something in an environment that you were just around?

CHILTON: They drummed a lot of Catholicism into us as Catholic school kids. And I guess I believed it. I don't know. I was a little kid. [laughter] I wasn't being very critical. My grandfather took me to Sunday school at the Episcopal church. I was a choir girl and the rest of that. And I don't know I thought a great deal about it one way or the other. I just thought that's what you did, so I did it.

DICK: So you're testing really well at these various subject. What are the subjects that you're really enjoying that you're doing well at? Or are you enjoying the subjects and stuff?

CHILTON: I loved school. I loved my teacher. We had one teacher for all subjects in that era. I guess they still do as youngsters that age, don't they? No, I don't remember anything outstanding that I liked particularly. I liked it all. I liked the teacher, and I worked hard for her. And I liked to draw—horses. And I loved horses. I was crazy about horses. And so we struck a deal. My grandfather wanted me to take piano lessons; I wanted riding lessons. So the deal was that if I would take the piano lessons and practice, then I could have a riding lesson every week. So that was fine. [laughter]

And I kept on with the riding lessons, but my piano teacher gave up on me after about a year-and-a-half, <**T: 10 min**> I think. [laughter] I wouldn't really practice enough to suit her. So, let's see, things went along like that. Now I was living with my grandmother, going to public school.

DICK: How was that change? You know, moving from a Catholic school to a public school?

CHILTON: I didn't have to wear dresses anymore; that's one main change. [laughter] Catholic school, you had to wear a dress. Not a uniform, but a dress. So I could wear pants. I liked that. I was a tomboy. I liked to race around. I played with the boy next door; he was a year or two older than I was. And I'm trying to remember. Somewhere, I guess, at about the time I became a teenager, my mother decided that I should go back and live with the family. I think my brother by that time had become a little more civil, although he still would snarl at me and say, "I'm gonna beat you to a bloody pulp," and I believed him. But anyway, seventh grade I was back living with my family. My grandmother kept my dog. Since my mother already had a dog, I couldn't bring my dog home, so . . .

DICK: And this is back in Indianapolis that you . . . ?

CHILTON: No, they were living in Elgin, Illinois by then. As I said, my father moved around a bit. And by this time, he is becoming an executive in the Chicago office of his insurance company. It was the Continental Casualty Company, Continental Insurance Company Group. They were the . . . you might remember—you might not—that there were machines in airports with a picture of a Continental soldier on them that sold flight insurance. That was his company.

Anyway, eventually, to jump ahead in the story, my father became the president of the company in New York and then chairman of the board. I think that he was chairman of the board after that. So he rose very high. But all that was yet to come. So we were not well-off. My father bought his first house in Elgin, and it had four finished rooms and an unfinished attic. It had two bedrooms and a living room and a kitchen, that was . . . and a basement, an unfinished basement. So my bedroom was . . . . I slept with my mother. My mother and dad had twin beds, so I slept with my mother, and I had one drawer of her dresser for my [underwear] and two or three hangers in her closet so that was my space. Not much. And I remember when we got our first black and white television set sometime around that period.

Let's see. Going back to school. I went to a junior high school nearby, and again, I loved it. I loved school. I loved all my teachers and did rather well. I think I got good grades. I got A's. And I don't have a clear memory of what those grades were. I didn't keep so much track of them until I got into high school. And at that point I can remember that I was a straight-A student, so probably I was before, but I don't remember exactly.

DICK: Were you still doing riding lessons, or were there other . . . ?

CHILTON: No horses, no. As I said, my father was not prosperous. He became a wealthy man, I guess, middling wealthy. But, no, at that time we . . . my mother actually took a job as an agent for hearing aids. My grandmother was deaf; I didn't tell you that. My grandmother was

deaf, so she was very familiar with the problems of deaf people. My grandmother had a hearing aid. It was more or less the size of your tape recorder or a little bigger than that.

And she had a battery pack that was a big as my whole hand. [laughter] <**T: 15 min**> She wore it in a little satin pouch made for it on her leg strapped around her waist, I guess. And it really didn't work terribly well. It worked; I mean, she could hear, but she had to look at you to figure out what you were saying. Anyway, so my mother was interested in that, and she went to work for Sonotone, actually the company that made my grandmother's hearing aid. And she had the agency there and fitted people with hearing aids and sold them and so on. And I can remember whenever she'd sell an instrument, we'd have steak dinner at our house. [laughter]

So that shows you the level of prosperity in the household. We always had good food on the table. That was never a problem. But in the way of clothing, I had two little corduroy suits, and I would wear one, and then my mother would wash it and hang it out to dry. And it would freeze in the winter. Freeze solid. [laughter] But ice sublimes incredibly, so it would dry out there even while frozen. And then I'd wear the other one for a week and then switch back and forth. So one was green—this color—and one was bright red. So that was my outfits in junior high school.

DICK: Were you involved in any clubs, activities, things like that in middle school and into high school?

CHILTON: I'm not remembering anything. We played volleyball at lunchtime, at lunch hour, but that's the only thing extracurricular that I can remember from junior high. In high school, yeah, I got involved in a number of different things. We had a calculus club, for instance. Our mathematics teacher was wonderful. He had each year some kind of after-school activity. And when I was a senior, he had this calculus club. He started from the very beginning. Have you studied calculus?

DICK: To a certain extent, yes.

CHILTON: Yeah. So he started from the very, very elementary and made us not just know how to use it, but understand why it worked the way it did, how it worked. What was inside it? So I knew more calculus coming out of his calculus club than kids know from studying it in college. I guess kids study it in high school now, but back then, that was . . .

DICK: Pretty rare?

CHILTON: So we went to the state math contest and won. And I went to the state Latin contest, won at that. I don't remember any other contests.

DICK: I know that they . . . . I've read that when you were seventeen years old, you built a telescope for . . .

CHILTON: I did, yeah. So that was for the science talent search that was run by Westinghouse back in that time. You had to write up a project. You had to do a project and then write it up and submit the paper. So, yeah, I got interested in telescope making, which means you start with a disc of glass, and you turn that into a telescope mirror by grinding out the curve. You have to grind one piece of glass over on the other. And then you use finer and finer grades of carborundum, so you get a finer and finer finish on it. But the last step, you have to polish it, and for that you use jeweler's rouge, which, rather than grinding, it has sharp, sharp edges, and it shears away the glass. So you actually polish the thing to within a fraction of a wavelength of light.

And I worked with the optical shop at the Adler Planetarium in Chicago, [Illinois]. There were a bunch of wacky kids like me making telescope mirrors there. And the director of the planetarium was interested in us and set up a Foucault testing device. This is a French guy's name, and he devised a very nice . . . a nice little machine for testing your . . . the curvature of your telescope [. . .] it has to be not a sphere, which is the natural thing you tend to get when you grind, but it has to be corrected to be a parabola. The parabola, the <**T: 20 min**> property of the parabola is that parallel light waves coming in will be bent to a perfect focus on one point. With a sphere, if you put light out from this point through the mirror, it will go back to the same place. That's what a sphere is perfect at. So, it goes back to the same focus. But you need a parabola to take light from infinity, from the star. That was that.

So, I learned all about that, and I made . . . it was a Cassegrain telescope, which was not the simple kind that everybody else makes, but a more complex thing with one primary mirror that's a parabola, and it has a secondary mirror that's a . . . . I'm sorry, my brain has turned spongy, and it won't keep the names of things. [laughter] What do you call this thing with two foci? Hyperbola.

DICK: Concavely or . . . ?

CHILTON: It's a convex mirror, yeah. Is it hyperbolic? It's a hyperbola is what I'm searching for, yeah. There's a cone of rays coming from the . . . it comes from the infinity in, and it's going as though to a focus here. But you put this hyperbolic thing in here, and that is curved in such a way that it takes this cone—what would've been this cone. There's one focus of the hyperbola. And it has another focus over here. So it'll take that cone that was converging as though here and make it converge back there. So you get more amplification on your telescope.

So you can have a telescope that is physically this long, and it is making an image as though it were . . .

DICK: Much longer.

CHILTON: Much longer, yeah. So . . .

DICK: So you're doing the mathematics, astronomy. There's, sort of, physics. Even technical craft . . .

CHILTON: Exactly, yeah.

DICK: . . . knowledge. Were you at the planetarium quite often?

CHILTON: I went in on Sundays. I worked on Saturday to make the money to buy the train ticket to go work on the mirror on Sunday.

DICK: Where were you working?

CHILTON: The Retzel Jewelry Store in . . . we lived in Hinsdale, Illinois at that time. That's . . . my high school years were in Hinsdale. Junior high, Elgin, Illinois. And then we moved to Hinsdale. By now, dad is more prosperous. They built a house. I had a real bedroom with my own closet, my own chest of drawers. And I had a bureau in my bedroom, which was filled with water halfway to make it nice and heavy, so that you could grind your mirror on it. So that's where I worked on my telescope mirror when I wasn't working in the planetarium.

DICK: It sounds like astronomy was a big interest at this time?

CHILTON: It's not astronomy, okay? It only sounds like astronomy. It's actually telescope making was the interest. I didn't take too much interest in astronomy—using the thing. It was making it.

DICK: I see.

CHILTON: Understanding how it works and understanding how you needed to manipulate it to get it to come out right.

DICK: It's more the device rather than how it's applied.

CHILTON: Yeah, so when I graduated, I got all these scholarships, but my dad was too prosperous for me to get any stipend. So my dad announced to me that I was not going to go to a private school that was good in optics and astronomy and all that; I was going to go to the University of Illinois. He'd paid taxes in this damn state of Illinois, and I was gonna go to the University of Illinois. So I did. And I started out as a physics major, and I presented myself to Mr. Peter Paul in 101 Observatory and asked for permission—on the strength of my telescope-building experience—permission to take Astronomy 101, which you weren't allowed to take till you were a sophomore, you see. And he wouldn't let me in. He said, "Well, you don't know any calculus, for example. You need calculus." And I said, "Yes, I do." And I think he didn't believe me. So he didn't let me in, and I said, "To hell with that," and I never took any astronomy. [laughter]

DICK: Well, why do you think he was so, I guess, unsupportive or . . . ?

CHILTON: I don't know. I looked like some prideful, cocky kid to him probably. <**T: 25 min**> I don't know. I don't know why he did that.

DICK: What got . . . so you started off as a physics major. How did you, sort of, get in having that initial decision? Was that based on interests you developed from high school?

CHILTON: Well, it was the closest thing to the optics. It looked like what I . . . where I would like. But it turned out it wasn't. I fell asleep in physics lectures. Partly I had a decent high school physics course, and we kinda . . . we knew it. And partly I think the real thing is that you couldn't see to the edge of things. You couldn't see anything that we didn't know already. We already knew the whole universe, and they weren't teaching us about quantum phenomena or anything interesting like that, and it was just Newtonian physics, and it was a bore. [laughter]

And I was taking chemistry because you had to. And I found that that was very interesting. I could see some unknowns in chemistry, so I liked that better, and I switched my major. And it was not until after I graduated that I really got interested in biology. So I was a chemistry major. And my training is chemistry, and my PhD is actually in chemistry. So I think that's not a bad thing to have a degree in one of the fundamental sciences rather than a degree in biotechnology. What are . . . ? What does somebody know if they know biotechnology? They

don't know anything. They don't know how to solve the problem because they don't know any of the fundamentals of whatever it is.

I have a different way of looking at things than many of my colleagues. I think about things quantitatively, and I think about the mechanism and so on. And I'm better than some people—than many people—at reasoning through what's going on when it isn't working. So I'm a great big problem solver, I think. In all modesty.

DICK: I do think it's certainly a skill that's useful and difficult to have. What type . . . any particular things in chemistry that you were focused on during your undergrad years? Was it more general chemistry?

CHILTON: No, it was organic chemistry. I worked on . . . after seeing photodimers—whatever they are—they have nice crystalline compounds with very elaborate, complex structures. But it was just . . . it was fun. It was interesting. Not useful. Totally useless, I think. But interesting. And the chemistry department really encouraged its graduates to go elsewhere for graduate school, but I married a man who was already in graduate school there, so they decided to accept me just, kind of, provisionally as a graduate student and see where it goes. And they started that year with cumulative exams rather than written exams for the PhD. So, we took one exam per month, and I think each month a different professor would prepare the exam. And the rule was that you had to pass—what was it?

You had to pass eight before you . . . . I think there were four per year, and you had to pass eight before you failed twelve. But you had three years in which you had to pass eight of these. It's some number like that—similar to that. I don't remember precisely. So I . . . and the first one was free; it didn't count. So they'd give you one freebie just to get your feet wet. So I failed my first one, and then I passed eight in a row. And they weren't expecting that. [laughter] They decided that I'd better stay for a PhD. So I did that.

DICK: Now, sort of this decision to go to graduate school . . . were you thinking as an undergrad that graduate school would be something that you were interested in?

CHILTON: Oh, yeah. It was never any question, of course. What I was gonna do with it, I don't think I clearly had a <**T: 30 min**> vision of that. I guess I was thinking I'd be a professor there of something or other. But the real thing is—I don't know—I was just doing what interested me and not being very practical-minded about where it was all going. I'm afraid I've been kind of like that throughout my career. I'm rather near-sighted. I do what's interesting at the moment without thinking, without planning too far ahead. But anyway . . .

DICK: But that's where you find surprising and unexpected things. Now your husband's name was Scott?

CHILTON: That was my first husband. No, Scott comes later. So, let's see. In the course of doing coursework for this PhD degree, they insisted that you have two minors, and I took one minor in biochemistry. The major was organic chemistry, so the minor was biochemistry. And I took a course that changed my life and career. It was called "The Chemical Basis of Biological Specificity." And this course was taught by professors that we now know as luminaries in molecular biology. Benjamin Hall, [Sol] Spiegelman, [Masayasu] Nomura were all faculty members at Illinois at that time. And they taught a course that they told where we were on figuring out the genetic code and how you went about figuring out what it was, what were the first experiments to determine that, and how do we know what the sequence of the protein is, and what are the problems associated with determining that, and so on.

And I learned about bacterial transformation where you can take pure DNA and mix it with bacteria, and they can actually take it up and incorporate it into your chromosomal DNA. And I thought, "That's phenomenal." So I was really fascinated by what . . . the seeming rings of the DNA molecule. So I think that fascination has . . . would describe everything that I've done since that time. DNA is . . . somebody accused me one time "Mary-Dell, anything that DNA does, you think is interesting," and that's the truth. So . . .

DICK: And then, just to give it some time here, so you graduated with your BS in chemistry in 1960. And so this is the early sixties that this is . . .

CHILTON: Sixty–two, '63, I was taking this course. So I went to [Noboru] Sueoka, who was another one of the luminaries who taught that course. I went direct to Professor Sueoka and asked him could I work in his lab on Saturdays and learn how to do this bacterial transformation? I loved that. He was working on that. And he said, "Sure, sure. Come on in." So, I did, and I worked with some of his postdocs, and we got to do these experiments. And he was the one, he said, "You should go take the course at Cold Spring Harbor," and I said, "What course?" And so, he told me about this course that they gave every summer. It lasted four weeks or six weeks or something, and they taught you all about all of these experiments. So he got me into the course, and I went and learned how to be a molecular biologist at that time.

DICK: Who was teaching the course?

CHILTON: Different people again—the luminaries of the field. A lot of different people. And I went back, and I talked to Professor Ben Hall, who was in the chemistry department but working with DNA. He was the only one. And I said, "Dr. Hall, could I come and do my degree with you?" He said, "What do you wanna work on?" And I said, "I wanna do something with

bacterial transformation." And he said, "Sure, you can do that." And so I did that. And then he said, "But, by the way, I'm leaving." [laughter] He said, "I'm going to Seattle."

I said, "Fine. Let's go." [laughter] So in the summer of '63—I think it was—we moved to Seattle, and I finished my degree at Illinois, but *in absentia*. And I got the degree not until 1967, I think. I finished it in '66, but they didn't award the degree till '67. But it took a little longer because of the move and so on.

So we moved to . . . out of the chemistry <**T: 35 min**> department into a genetics department, and that was a huge culture change. And everything was much more biological, and I learned some genetics from that, of course. And I published two papers on bacterial transformation and one more as a postdoc, which was the third one. And from there went on to do a postdoc at the University of Washington still with Brian McCarthy, who was in the microbiology department. So it's a lot of department skipping if you look at my career. And that is really the nature of . . . you know, DNA is a very interdepartmental subject. It has applications everywhere. And you have to know a lot of different stuff to move successfully with it and to do meaningful experiments. Okay, you gotta ask me some more questions; I'm getting stuck.

DICK: How was just . . . not only you're switching departments—you're moving from chemistry to genetics to microbiology. What about the change in location of . . . moving out to Seattle, [Washington], from Illinois?

CHILTON: Oh yeah, Seattle was wonderful, after Illinois. [laughter] I learned how to ski. We . . . in Ben Hall's lab, we decreed that Wednesday was Saturday, and Saturday was Wednesday. So we went skiing not on the weekend when it was crowded, but on Wednesday when we could have the slopes to ourselves. And, yeah, I got to be a pretty fair skier during that time and hiker and so on and so on.

DICK: Were you able to keep up at all with your love of horses? Has that . . . ?

CHILTON: Nope, no horses. No horses. It's sad in a way. Now I'm in a place where I could have a horse, but I have a bad back and can't ride now.

DICK: Let's see. This is around . . . moving into the early 1970s. You have your postdoc fellowship. You're a research associate in the department of microbiology at this time?

CHILTON: Yes, yeah. And in the meantime, we have shed husband number one, and I married Scott. And by now, let's see, I think it's about that time we were having our children a few years apart. And I guess after Mark, the second one, was born, I was at home looking after

him and trying to figure out what I was gonna do with my life. And Helen [R.] Whiteley in the microbiology department called me up and said, "Mary-Dell, would you come and help me teach this laboratory course?" And I said sure. I was just delighted to go over there, and it was in the process of teaching that course one of my students was interested in a paper about crown gall disease, and that was my first introduction to this *Agrobacterium* story.

And he told me that Gene [Eugene W.] Nester was planning to write a grant application to study the crown gall disease. So I went down to Gene's office and talked with him. And I said, "Look, I know how to do DNA experiments to see what's going on with this thing." Whether it was DNA transfer from *Agrobacterium* to the plant cells or not. I said, "The publications that exist on this are not right. They're not good. They're not looking forward. And I don't believe them. Not only that, but they're impossible. They're . . . they violate thermodynamics." [laughter] There were claims that tumor DNA would bind better with *Agrobacterium* DNA than *Agrobacterium* DNA itself would bind to *Agrobacterium* DNA. And that's just not possible. No way. <**T: 40 min**> Anyway.

DICK: Who was the student that . . . ?

CHILTON: Tom [Thomas C.] Currier. Let's see. Is that true? I think it was Tom. Yeah, yeah. So he was the first graduate student on this project, actually. I wrote the grant application, and because I didn't have any status, I couldn't submit it. So Gene signed it and sent it in, and we got the money, and we got the money from two sources. We could only accept one, so we did.

DICK: Who offered the money, and who did you accept?

CHILTON: American Cancer Society was one, and let's see. Who was the other one? National Science Foundation, I think, was the other one. I think that's right. And we took . . . . I think we took the American Cancer Society money because it looked more renewable than the other. So that was my salary at that time. [laughter] I got a little job.

DICK: How was it . . . I know you're having your children now. How was it, sort of, managing the home and work life, especially now as you get the grant application accepted and are teaching and so on?

CHILTON: It was hard. I decided . . . . I made . . . . I actually articulated this to myself. What I was gonna do is when I was with the kids, I was gonna be with the kids and not think about the lab, and when I was in the lab, I was not gonna think about the kids. And it was just as simple as that. And I told the boys when they were . . . when they got to an age where they could begin to understand—at five years old or so—I told them that I was the kind of mom who would go

crazy if I couldn't do my work. I loved my work, and I needed to do it. And they didn't want a crazy mom, did they? [laughter]

No, they didn't want a crazy mom. So I said, "Well, that's the price you have to pay. So I will not be with you all the time. You will go to babysitters. But I will be with you when I'm with you." So that was the deal that I made with the boys. And they seem to have turned out pretty well. One is the mayor of Carrboro, [North Carolina], and he's now running for a Senate seat that's come open in the state. So not bad. [laughter] Both of them went through law school and passed the bar exam.

DICK: Great.

CHILTON: So, yeah, they're good kids. Good kids.

DICK: Now I know Rob [Robbert Antoon] Schilperoort . . .

CHILTON: Schilperoort?

DICK: Yeah. So he's coming into the story. How did he begin to begin to recognize and become aware of this work?

CHILTON: Well, Rob Schilperoort published some of this early work that I told you was impossible. So we knew his name. And I think sometime around here, yeah, Gene Nester had a sabbatical come up, and he went and he spent his sabbatical in Schilperoort's lab, trying to learn something about crown gall and slip root and trying to understand what the hell he was doing that got these bizarre results. So he made good friends with Rob, and we got long telexes back and forth at this time. And the story of the science here is beginning to be quite interesting, competitive. So do you wanna get into that now?

DICK: Yeah, certainly. I think . . .

CHILTON: Are we finished with me?

DICK: Well, we'll continue to go back and forth, but, yeah, if we could . . . . I know there's going to be some races here, some competitions. Lots of problems to figure out. Yeah, yeah. If you could go ahead and . . .

CHILTON: Well, these will be the facts as I remember them. And different people have different memories. Mine are right. [laughter]

GOLDSMITH: Of course.

CHILTON: And theirs are wrong. To give Rob Schilperoort his due, he wrote his PhD thesis on this subject of the crown gall, and he did some of these DNA hybridization experiments. What <**T: 45 min**> you do is you bind the DNA from the tumor cell to cellulose nitrate membrane filter. It's not a Southern blot, but it works sort of like a Southern blot, except that the DNA isn't separated into bands. It's just a blob of all the DNA. And you soak that in radiolabeled *Agrobacterium* DNA, and then you wash all the unreacted *Agrobacterium* DNA away, and you count by membrane: click, click, click, click. And some counts stick to it. The control that he didn't do was to take that same DNA and put it with radiolabeled *E. coli* or radiolabeled some other . . . any other kind of DNA. If he had done that, he would've found that there's something in his tumor DNA that will bind to anything.

DICK: So, sort of, getting false positives, then?

CHILTON: Yeah, yeah. It's dirty. It's full of polysaccharide; it tends to copurify with DNA, and unless you take great pains to get rid of that, you get artifact. So that was not a good way to do the experiment. We did what you call model mixture experiments where we took tobacco leaf DNA, which you can get pretty pure. There's not a whole lot of starch in a tobacco leaf, not the way that . . . they were working with these tumor cells that grew up with tissue culture like this. [holds up object] This is callus form of growth, and the tumor cells wouldn't regenerate back to a real plant. They would just grow like that.

DICK: Just the callus?

CHILTON: Yeah. That was one of the challenges of the genetic engineering was to figure out why it would grow like this and not turn back into a plant. But anyway . . . so when he made DNA from that stuff, it is full of slimy polysaccharides and things that cause these artifacts. So we made leaf DNA, which was pure, didn't have all that slime in it. And then we mixed in 1 percent, 0.1 percent, 0.01 percent of *Agrobacterium* DNA, and we measured how fast the radioactivity would bind to this. And it bound with linear kinetics. It didn't all jump on the way the tumor DNA did. And we looked at how much we could see. And it turned out you had to have—I can't remember my numbers here—but I needed to have about 1 percent or maybe it

was down to 0.1 percent. But you couldn't see really tiny trace amounts of *Agrobacterium* DNA if it were there. You couldn't see it by this method. It wasn't sensitive.

So we did that . . . we did every control, and what we found is that the method really was not powerful enough to answer the question. And so I knew about how to work with DNA in solution not stuck to filters. And to measure the renaturation kinetics of DNA. And so I designed what we called a driver DNA experiment, and it works like this. You take a low concentration of labeled *Agrobacterium* DNA, and you measure its renaturation rate. That is, you take the strands apart—Watson from Crick—and you put them at 60 or 65 degrees, depending on the soil, and they'll go back together, they'll renature. And there's a test you can do to tell what's single-stranded and what's double-stranded. So you can measure the kinetics of this. I told you that I think about things in a kinetic kind of . . . this is an example.

Okay, so the experiment is we know that if you double the DNA concentration that reaction will go four times as fast. And if you double it again, it will again go four times as fast. So it's concentration dependent. So the reasoning is this: that if the tumor DNA had copies of *Agrobacterium* DNA in it, then when we add tumor DNA to this mixture, it should renature faster. And exactly how much faster, we should be able to tell from the rate. So you do a positive control. And we did <**T: 50 min**> an experiment like that, and it said, "Absolutely not. It's not here." So we published a paper saying, "Nope, sorry. It wasn't there." And then . . .

DICK: Around what year was that? Do you recall?

CHILTON: The *PNAS* [*Proceedings of the National Academy of Sciences of the United States of America*] paper.[1] I don't remember. I can find that out for you, but I don't remember it. It's . . . if we look on my curriculum vitae, my publication list is there somewhere. It won't be there. We don't reference that.

DICK: Not in there?

CHILTON: That's really old stuff. Anyway, in Jeff [Jozef Stefaan] Schell and Marc Van Montagu's lab—that's Marc Van Montagu of [inaudible]—they found . . . looking for something else they found by accident that there were these huge circular DNAs in *Agrobacterium*. They were looking for a virus that they were working on, and they found these huge some sort of DNAs that had to be a plasmid. And we knew about small plasmids. We had looks through small plasmids in *Agrobacterium*. We knew from a bunch of biology that I have written about, and I haven't talked with you about. Do you want the whole lecture?

[1] Mary-Dell Chilton et al., "Agrobacterium tumefaciens DNA and PS8 Bacteriophage DNA Not Detected in Crown Gall Tumors," *Proceedings of the National Academy of Sciences* 71, no. 9 (1974): 3672–3676. Accessed at https://pmc.ncbi.nlm.nih.gov/articles/PMC433838/ on 25 July 2026.

DICK: Please.

CHILTON: I'll give you the paper. You can get it from there. I wrote a memoir in *Plant Physiology*.[2] Maybe you've seen a copy of that. Do you?

DICK: Let's see. I've got the . . .

CHILTON: Is that what that is?

DICK: Yes. This is the memoir, yes.

CHILTON: Okay. So that has some of the history of why we thought there had to be some kind of something transferred to plant cells. So we were trying to figure out how the crown gall works. How it is that the bugs change the way those plant cells grow forever.

DICK: Could you maybe speak about just this, sort of, phenomenon of continuous growth, and how that differed from other materials?

CHILTON: I was trying to skip over some of this for brevity, but you don't want any brevity, do you? [laughter] Okay, the deal was . . . this goes back to where the problem was when we came on the scene. The deal was that we knew that *Agrobacterium* caused these tumors, that if you wounded the plant cell and put the bacteria into the wound, that an outgrowth would occur. And we knew that the cells of this outgrowth made some strange chemistry that the plant—natural plant—didn't make. And the chemistry was named octopine because it was already a known compound in octopus, so it was called octopine. And if you used a different strain of *Agrobacterium*, you might get a nopaline tumor, or we now know a list of these opine-type compounds. They are simple. They're made from one amino acid and one ketoacid or one sugar, typically.

So they're nutritionally good stuff. And it turns out that an octopine strain of *Agrobacterium* not only makes an octopine tumor, but it has the ability to break down octopine and use it as a sole source of carbon and nitrogen. So the reasoning is that apparently the purpose of the gall from the point of view of *Agrobacterium*, the purpose is it's making a storehouse of food that it can eat, and other bacteria cannot. Nothing else can eat it.

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[2] Mary-Dell Chilton, "Agrobacterium. A Memoir," *Plant Physiology* 125, no. 1 (2001): 11. Accessed at https://academic.oup.com/plphys/article-abstract/125/1/9/6098991 on 25 July 2026.

DICK: So, no one else can be—or nothing else can be—really drawn from that as an energy source for it?

CHILTON: This is a slight oversimplification. There are in fact a few *Pseudomonas* bacteria in nature that have figured out how to eat this or maybe they knew for other reasons. So I've slightly oversimplified it, but basically the story is that it's a unique carbon source for *Agrobacterium*—carbon and nitrogen. Okay. And what we knew is that the plant cells, if you cut them off this gall or cut them out of the gall and surface sterilize them—that is, dip 'em in alcohol and set 'em on fire—and then take a sterile scalpel and cut some tissue out of the middle of this and put it on an agar medium such as I just showed you, the cells will grow under conditions where normal plant cells won't grow. Normal plant cells require some hormones—some auxin and some cytokinin in order to grow. <**T: 55 min**>

Auxin is the elongation factor; cytokinin is the cell division factor. So you gotta get bigger, and you gotta divide in order to make this gall. So *Agrobacterium* prudently has learned how to transform a cell to make something once, and at the same time make that cell grow furiously fast. So it's pretty clever when you think about it. And the thing is how did the *Agrobacterium* do that? And the speculation was must be putting something in there. Armin [C.] Braun studied this for decades at . . . in New York City. Help me out. What am I trying to say? Rockefeller [Institute]. The Rockefeller University.

DICK: Oh.

CHILTON: And he was kind of the godfather of this field long before we came into it. So he named this the tumor-inducing principle, this thing, whatever it was, that *Agrobacterium* gave to the plant cells. And the speculation . . . one would conclude that it had to be some self-replicating thing because the plant cells remembered this alteration forever. They never went back. You could grow a whole houseful of them, and every cell still had the ability to keep growing and growing. So the obvious candidate would be some kind of DNA, maybe some kind of RNA, maybe some kind of virus, maybe some kind of plasmid.

It's a short list of things that would behave like that. But then there was another camp that thought, "Well, maybe *Agrobacterium* is just flipping some kind of switch in the plant cells that takes them from quiescent to grow furiously. And in the course of doing that, all these other things happen." So there was that school of thought.

DICK: Do you know who was pursuing that approach?

CHILTON: Well, there wasn't much you could do with that approach. I mean, it's . . . you could look for things. I knew how to look for things. But it's not clear how you look for that. Anyway, I didn't really believe that. I thought probably something was transferred. But, on the other hand, I don't know. The story as it eventually began to emerge was so amazing that I could hardly believe it. But we'll get there. We'll get there. So it was this perplexing story. How . . . what is the tumor-inducing principle? That was what made us to trouble ourselves to look for this. We knew that if there was a way to put something in the plant cell that would stay there, that the possibility was we could do that too. But that's a big *if*.

DICK: What year is it? We're curious with this because I know that really the sort of rDNA possibility, that's '73, '74 where that's kind of becoming known. Insertion into nucleuses and things like that are becoming available.

CHILTON: Exactly, yeah. It was I'm thinking late sixties. Does that sound right? No, that doesn't sound right. Let's see. Fifty-six was when I graduated from high school. Sixty-seven is when I got my PhD degree. So it's seventy . . . it's early seventies is when the recombinant DNA technology was coming along. Early seventies, yes.

DICK: And were you keeping up with things that were coming out of Herbert Boyer, Stan Cohen, Paul Berg's labs?[3]

CHILTON: That's what I'm saying. That was just . . . that was research and word of mouth and beginning to be published.

DICK: And up here in '74, that Ivo Zaenen. . . ?

CHILTON: Zaenen, yeah.

DICK: Tell me about that breakthrough in Schell and Montagu's lab.

CHILTON: I was just getting to that, and that's when we jumped back.

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[3] Herbert W. Boyer, interview by Arnold Thackray, Sally Smith-Hughes, and Mark Jones in Boston, Massachusetts, and San Francisco, California, 28 March 2000, 24 April 2013, and 21 May 2013 (Philadelphia: Science History Institute, Oral History Transcript # 0193, in process); Paul Berg, interview by Deanna Day and Jacqueline Boytim at Beckman Center for Molecular and Genetic Medicine, Stanford University School of Medicine, Stanford, California, 16 February 2016 (Philadelphia: Science History Institute, Oral History Transcript # 0950, in process).

DICK: Oh, sorry, sorry.

CHILTON: That's fine. So Zaenen was the student who found this gigantic, circular thing when he was looking for a virus. And Schell and Van Montagu, I think, quickly recognized that that might be the tumor-inducing principle. So they looked in either of the <**T: 60 min**> strains of *Agrobacterium* that didn't cause galls, and they didn't see these big plasmids. So they thought that the plasmid was the cause. And the word came through the telex to our lab in Seattle that they were plasmids and a little bit about how to see them—what's . . . and there were two experiments you could do to see them. One was to lyse the bacteria gently and put it on a gradient—an alkaline sucrose gradient is how you do it—and spin it just the right amount, and you could see a band of DNA moving on the gel.

And the other experiment was a cesium chloride gradient where the DNA would band in a high concentration of cesium chloride that's separating the plasmid on the basis that it's a closed circular configuration rather than linear DNA like a chromosome. And it . . . you stuff ethidium bromide into it, and it changes the density of the surface differently than the density of the linear chromosomal fragments. So, when you separate it, there it is. So once we knew that, once we knew how to detect this, we had a lot of material on hand. Gene had had Alice Montoya, who was one of the people in his lab—she was his technician—he had had her doing KIR crosses where you mix two bacteria together in a sunflower plant, a root, and it turned out that you could actually get transfer of virulence from a virulent strain through an avirulent strain. But one didn't know what virulence was at that time.

But now we had the transfer strain and the mother strain, we could look in there, and we could see that there's a plasmid here, and there wasn't in the avirulent. And when you did the KIR cross and virulence transferred, you could see the plasmid. So we were quickly able to . . . it took a week or ten days to get all these data. So we knew that the plasmid was right. We knew that the plasmid was indeed the agent of the tumor within a week of getting that information.

DICK: Wow.

CHILTON: And so immediately we knew that we wanted to do one of these hybrid DNA experiments, the kind we had done with the whole *Agrobacterium*. We wanted to do that with this plasmid. That was what we should be looking for. So we set up to do that. One of these experiments required everybody in the lab, and the radiolabeled—what was it?—DATP [deoxyadenosine triphosphate], I think, that we bought from New England Nuclear would come on Thursday, and we would use it to make radiolabels, test DNA, whatever it was *Agrobacterium* or now the plasmid. And then we would have to work hard the entire weekend. And about Monday noon or Monday afternoon, the experiment would be finished.

But everybody in the lab worked day and night over that weekend to do one of these. You had to work fast because the probe was so hot that it would, sort of, suicide-destroy itself because of the radiation. It's not radiation that would harm us, you understand. It's radiation that . . . it breaks the backbone of the DNA molecules. And these are pretty big DNA molecules. So we did that experiment with the plasmid, and it said no, not there. And so we sat back on our haunches and decided to give up, more or less. And then I got feeling guilty because we had promised the American Cancer Society that we would do an experiment to take the plasmid apart into specific pieces and look for those one-by-one <**T: 65 min**> if we didn't see the whole plasmid in there.

And Gene said—he denies this—but I remember he said, "We don't have to do that, I mean, just because we proposed it. If in our judgment that's not a worthwhile experiment, then it's very unlikely to succeed. We can decide to put the money more productively." And I said, "Well, we really have to do that. We have to put this to rest, one way or the other. We have to do it, or we won't really be finished with this." So we did. And this time we found it. We found that two of the fragments that we separated from this tumor-inducing plasmid—as we now called it—two of the fragments were blooming in the plant cells, so all the other fragments said no and these two said yes.

DICK: Wow. Now was this . . . did this require the same sort of intense over-the-weekend, all the team?

CHILTON: Oh yeah, yeah. That's what it took, yeah.

DICK: Who . . . ? How many people were on the team doing that work?

CHILTON: How many people are in that picture? Six or seven?

DICK: Let's see: one, two, three, four, five, six, seven.

CHILTON: Yeah, that's how many.

DICK: Great. So, just for the recording, or if you wanna . . .

CHILTON: The real worker bees are these. Those guys were . . . they went out for pizza, and Milt [Milton P. Gordon] had the stopwatch. He was the timekeeper. Martin Drummond's job was making the radiolabeled DNA. And Don [Donald J.] Merlo's job was getting the DNA out

of the agarose so that we could do the experiment with it. To do that, he invented a thing that he called "the cow" for reasons that you can imagine. You put your agarose slice. You cut out your fragment from the gel, so it's a little block of agarose, and you put it in a dialysis bag and tie the ends very tightly. So you have a number of dialysis bags, each with one of the fragments in it, and you put it into an electrical field. And that will chase the DNA out of the agarose, and it'll be there in the end of the bag. And you take the bag off this cow, and you get—

DICK: So it's kind of multiple stomachs.

CHILTON: Yeah.

DICK: And just to go through the rest of the team here?

CHILTON: Let's see. I cooked the gel. I masterminded the whole layout, purification of the DNA. All the glassware that we used had to be fired in a self-cleaning oven to be sure that when we did find something, it was there and not contamination from previously unclean glassware or something. And all of us ran the columns to analyze the single-stranded or double-stranded DNA. And we had to do this with logarithmically increasing times. So we'd start with ten seconds, twenty seconds, forty seconds, and the time got farther and farther apart, but we had a lot of samples. And I didn't put any graphs in this, did I? But . . .

DICK: I don't think so. I see the timekeeper that's, sort of, getting the percentage.

CHILTON: In the beginning, yeah. The timekeeping was tight.

DICK: And then this is the—you have it labeled here—the brute force experiment.

CHILTON: Exactly. Yes.

DICK: And what . . . I kinda see here the "brute force" was just because . . . if you wanna explain why it was referred to as the brute force experiment?

CHILTON: Well, we wouldn't have thought of it as a brute force experiment except that we now do Southern blots, which are quite a bit easier, and give, in fact, more information. So . . .

DICK: So . . .

CHILTON: It's just a . . . it's a huge effort. Huge effort. And nobody does those experiments anymore.

DICK: But then that . . . because you have alternative tools that are just much easier. Now this this then was the paper that was published in 1977 in the journal *Cell*?[4]

CHILTON: I think so, yeah.

DICK: And were there any, I guess, interesting anecdotes, that <**T: 70 min**> even, say, something dropping and breaking, or you're running out and getting pizza trying to do this crazy thing over a weekend?

CHILTON: There are a couple of stories worth mentioning. One is that the people involved came to work on Tuesday morning, and we all kinda sat around and looked at each other. And even though we were working at our normal pace, we all felt like we were going backwards. We had worked so hard on the weekend that it felt like standing still when you were . . . [laughter]

And the other thing is, you know, Alice Montoya came here to work for we were Ciba-Geigy then. Yeah, she and I reminisced about this. And whenever I encountered any of the other people in that picture, we would reminisce about how that time was. It felt good to have a collaboration like that. It was . . . we've, kind of, gone through life—all of us—looking for that high again. And we've never found it. But it was . . . there was an intimacy to being a part of that machine that did a brute force experiment. So I tell that to kids that the surprise of my career maybe was how important it is to be able to collaborate like that and to be able to vest trust in your coworkers. We all had to trust each other to do that right.

DICK: Someone's not accidentally contaminating something or doing something that might not let the experiment work.

CHILTON: Yeah, you had to do things very precisely, very carefully, very much on time. And if one did slip up, report it. So, yeah, it's an act of trust. It's always an act of trust when you collaborate directly with anybody on anything. But with so many people, yeah.

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[4] Mary-Dell Chilton et al., "Stable Incorporation of Plasmid DNA into Higher Plant Cells: The Molecular Basis of Crown Gall Tumorigenesis," *Cell* 11, no. 2 (June 1977): 263-271. Accessed at https://www.sciencedirect.com/science/article/abs/pii/0092867477900435 on 27 July 2025.

DICK: How did you all celebrate? You finally get the big positive results? Did you go out for a beer or . . . ?

CHILTON: I don't remember. [laughter]

DICK: Let's see, that's . . . so, paper's published in 1977. By . . . . I definitely want to come back more to the science also, but in '79 you become an associate professor of biology. How was that shift made of becoming, I guess, a tenure-track professor as opposed to a research associate?

CHILTON: Well, I moved to Washington University in St. Louis, and the reason I moved was not because I wanted to, but Gene Nester made it necessary for me to go. He became angry with me over stuff. One thing that made him angry I think that probably kicked off the whole thing was he told me not to look for RNA in the plant cells coming from this DNA that's in there. And I said, "Oh, it's gotta be there. I mean, what do you think that DNA is doing in there? Of course, it's gonna make its RNA so it can make octopine or whatever." And so I went ahead and did the experiment anyway, and it was there, of course. And I told Martin Drummond that we could see his signal with the RNA on the Southern blots. And I told Martin, "Could you do this again and clean up the RNA by this and that method?" So it made me a nice, clean, pretty picture for publication.

I said, "If you can do that, you could be first author of the paper." And so he did, and I wrote up the paper. And I had Martin's name first and my name last, and we had Milt Gordon in there because he made the tumor DNA, or he made the tumor tissue, catalyst tissue for us. And maybe somebody else. I don't remember. And Gene Nester got wind of this that <**T: 75 min**> that paper existed, and he came to me and said, "I should be the senior author of that paper." And I said, "No, you shouldn't. You told me not to do it." I said, "I'm the senior author." He said, "You be the first author." I said, "No, Martin Drummond was promised first author if he did the experiment, and he did. And so finally I said, "Well, I will make a compromise, and I'll put your name on the paper," and I did. But that was the beginning of the end. He was really furious about that.

DICK: And putting the name on it would be because it's the work being done as lab?

CHILTON: Yeah.

DICK: Now, and why was he, I guess, not wanting to be searching for the RNA? You know, because like you said, it seems to make sense that . . .

CHILTON: I don't know. And he wanted to spend a whole bunch of money on some other project that I thought didn't make any sense. I mean, it made sense, but it wasn't gonna work, technically.

DICK: So I guess that working with limited resources, there's only so many things that can be investigated. How did you come across this position at Washington University? Were you applying widely?

CHILTON: Yeah, yeah. They weren't looking for people particularly, but I was invited there to give a mini-symposium talk, and I think they liked what they heard. And when a friend of mine, a colleague from graduate school, was a microbiology professor at WashU, and he said to me, "The chairman of the biology department told me that if you would send him a letter of application that he would invite you for an interview." And so I did. And they liked it. So that was the beginning of a happy little interview . . . interlude at Washington University.

DICK: So it seems like that you're happy with your new colleagues there at . . .

CHILTON: Yeah.

DICK: Who are some of the people that were in the department or that you were working with? I know that it's . . . you're DNA, so it's all types of different departments that . . .

CHILTON: Yeah, yeah. There were snake people and bat people, and we had all kinds of things. But the plant biology program was quite strong, and I fitted very nicely into that. Virginia Walbot was in there. Joe [Joseph E.] Verner was the head of the program, and there were several lady faculty members. Shoot. Sally [Sarah C. R.] Elgin was one, and I'm having great trouble remembering names now. Anyway, it was a very strong bunch of people. One of them told the story that they were in Washington, DC at a committee meeting, and somebody said to her, "Gee, don't you have any men in your department? Is your whole department women?" And she responded, "Well, all the strong women." [laughter]

CHILTON: Which was half true. Maybe more than half. Yeah, we had some very strong women.

DICK: That's kind of interesting, I think especially at this time, that actually having, as they remarked, so many women. What was it like, sort of, being a woman in this would've been a—in many ways—male-dominated field? […] It's—what?—the late seventies at this point?

CHILTON: I don't know. I really . . . . I'm not tuned into that. I'm not conscious of that. I mean, it was nice having female colleagues, yeah. But they were doing their thing, and I was doing mine. And my real hero was the head of the program, which was Joe Verner—a man—and the head of the department, who had hired me, also a man. So I liked him. Very nice.

DICK: I <**T: 80 min**> guess that it was more facing just unequal treatment, that different standards were . . . would be applied. That kinda stuff.

CHILTON: What I noticed moving to Washington University is that everybody was helping me and pushing me along and raising me up instead of stepping on my fingers the way they seemed to do at the University of Washington.

DICK: I see. Now were you beginning . . . ?

CHILTON: I had been a second-class citizen, maybe third-class at the University of Washington. I didn't have a tenure-track faculty position. I was nobody. So . . .

DICK: That was probably the hard part there. Did you have any graduate students when you were at the University of Washington? I know that . . .

CHILTON: It's sort of yes and no. I certainly mentored what students that we had, but they weren't my students. They were Gene Nester's students. So Francine [C.] Eden was one. Oh, yeah, and I had another renegade student come work with me. Her advisor went on sabbatical for a year, and she came over, and she was one of the early denizens in the Chilton Hilton, I guess. Her name was Daniela Sciaky and she's in that picture, actually, of the seven of us. Dani came to work on DNA, to work on the *Agrobacterium* problem with us, and she kept on, she stayed on even after her advisor came back. So they added me onto her committee at Washington State University that's where her degree was from. So I didn't have any real students of my own, no.

DICK: And so how did that . . . . I wanna get back to this if you could maybe speak to this—the Chilton Hilton? So you're moving to Washington University, so now you have your own graduate students. Is that . . . ?

CHILTON: Yeah.

DICK: How was . . . what was your mentoring style? It's from what I've read very supportive, often not necessarily what you would find among other professors. How would you interact with your graduate students? How would . . . ?

CHILTON: Well, what I would do is . . . a lot of them were postdocs rather than students, but anyway, same story either way. I would ask them what where they were going, what they wanted to learn, what kind of experiment they wanted to learn how to do and try to fit the project—choose the project so that it fit that role. How long do you wanna be here? Where do you wanna publish? Do you want an easy project or a chancy one that's more high-profile? That kinda stuff because I wanted people to get out of the experience what they wanted. So Marge [Marjori A.] Rula, now Marge Matzke who was not . . . she was not my most productive postdoc—shall I say—but was by far the most creative. And I recognized this in her. She's fantastic. She read widely, and she was full of ideas. She married another of my postdocs, Tony [Antonious J. M.] Matzke, and they went off to Austria, and she's become world-famous now. She's very much in demand. She'll probably get a Nobel. I don't know. But she was one of the first to discover co-suppression of one gene by another in transgenic stuff.

DICK: Wow.

CHILTON: Yeah, she's just amazing.

DICK: Now the Chilton Hilton, is that having graduate students and postdocs over to your house and . . . ? Or is it just the . . . ?

CHILTON: No, no. It wasn't like that. It's the homeless came and found a home there. So Daniela, who wanted to work in the lab for a time, worked with us for a time. She finally got an apartment of her own, I think, the second year or so, but she liked the little boys, the dogs, and the cats. Liked watching the medical sudsers on TV at night. But most of <**T: 85 min**> the denizens of the Chilton Hilton were French students. They were foreign students who came, kind of, penniless and had the opportunity to work and learn without too much expense of living. [William] Scott [Chilton] was a Fulbright student when he graduated from college, and I think he was invested in the idea of paying that back to the community or something. Got a great deal of out that experience himself.

DICK: And did . . . now Scott came with you. He . . . did he get a position in the microbiology department at Washington University?

CHILTON: In the biology department.

DICK: Biology.

CHILTON: It was all biology. They gave him a position. It wasn't a faculty position, but whatever it was he was a visiting professor or some other thing. Anyway, it was fine. They built him a nice little lab with a big fume hood for his machinery for his electrophoresis equipment. He was a big asset to our program actually. He worked on the opines.

DICK: Okay, so the energy source that . . . now when you go to Washington University, and this is the time where you begin to be focusing on the tumor-inducing plasmids?

CHILTON: The disarming of the Ti plasmid, yeah.

DICK: And what do we . . . ? How are they disarmed? What was, I guess, was the logic behind having to disarm the tumor-inducing plasmids, the T-plasmids?

CHILTON: Well, we figured that there were a bunch of genes that were known after a while. There are still genes on there that we don't know what they are. But the auxin gene and the cytokinin gene were mapped out, were beginning to be mapped out. But we disarmed the plasmid . . . . I would say we disarmed it first quite by accident. We inserted, we made an insertion into the Ti plasmid at a place that was convenient and easy to get into, so that's why we chose it. But it turned out to be the soft spot. It turned out that that inactivated the cytokinin gene, and that turned out to the be the one that you had to inactivate in plant cells to be able to determine the plant. Nobody knew that till we did that, but there it is. It worked. So just by luck, we engineered, and we published the first paper describing how to put a gene into T-DNA with transgenes.[5] And the example we chose was, in fact, the disarmed plasmid, as we discovered later.

DICK: Now without disarming it, it would just remain a callus. Is that . . . ?

[5] Jean-Pierre Hernalsteens et al., "The Agrobacterium Tumefaciens Ti Plasmid as a Host Vector System for Introducing Foreign DNA in Plant Cells," *Nature* 287, no. 5783 (October 1980): 654-656. Accessed at https://www.nature.com/articles/287654a0 on 28 July 2026.

CHILTON: It turns into a callus. Yeah, if the hormone mix in the plant cells is not natural, then the . . . it can't differentiate. But if you mess around with the hormone . . . so there's a certain range that it can tolerate and be able to regenerate. So the amount of auxin that this strain—this *Agrobacterium* strain—was producing in its tumor cells, the amount of auxin was not excessive, but the amount of cytokinin was. And if we once knocked out the cytokinin, then the auxin could be tolerated and still allow regeneration.

DICK: Now the other part of it is really, sort of, I guess, mapping out the tumor-inducing plasmid of the different regions, of, for example, the virulence region that allows it to actually make the transfer of that . . .

CHILTON: Yes, exactly. And we didn't work on that significantly. Others were doing that. There were a number of groups. I mean, the people were graduating out of Gene Nester's lab and going out and setting up islands of crown gall study here and there. There are—I don't know—four or five of them <**T: 90 min**> in the United States now that are second generation thanks . . . they're good friends. We have crown gall meetings each year, and I see all of my friends.

GOLDSMITH: Is that that picture—I'm sorry—that picture on the wall behind you? That . . .

CHILTON: These guys?

GOLDSMITH: Yeah. Is that . . . ?

CHILTON: Yeah. That's a crown gall conference, yeah.

DICK: Okay, so it's the 30th Annual Crown Gall Conference.

CHILTON: Yeah.

DICK: And that was in 2009?

CHILTON: Yes.

DICK: So are you in contact with these people that are coming out of Nester's lab that are, sort of, working on . . . they're all–?

CHILTON: Oh yeah, yeah. We're friends. I founded that conference when I moved from Seattle to St. Louis because I felt like Seattle was giant. They had a big machine, a lot of money, and we little guys out here on our own having to compete with that, we should band together and talk to each other and help each other to the extent possible. So that was my idea in starting this conference. Let the students do the presenting, and it would be good experience, and they'll have it in local regions so that people can drive a carload of students to a meeting. So it's kind of expanded from that vision over the thirty-three year . . . whatever it is years. But that was the concept, anyway.

DICK: So did it . . . ? So 1979 was the first conference. Where did you hold it?

CHILTON: It was at Northwestern University. And I think it was the Lippincotts [James A. Lippincott and Barbara B. Lippincott] were there. They were early in the field, and they have, kind of, aged out, I guess. I think they retired. But, yeah, they hosted that first meeting.

DICK: About how many people were there?

CHILTON: Twenty-five? I don't know.

DICK: Okay. That's pretty substantial.

CHILTON: Yeah. And wherever you go, of course, all the local students will come, so they'll swell the attendance.

DICK: So now they're figuring out the different components of the T-plasmid . . . the . . . it's still a matter, though, of really being able to transfer the gene successfully and then regenerate the plant that's been transformed.

CHILTON: Yeah.

DICK: So how . . . ? So this is early 1980s that you're working on this problem? And I think in '82 when you succeed?

CHILTON: You're asking me?

DICK: Yeah. I'm just making sure that I have the . . . well, I guess a bit moving on.

CHILTON: It's '83.

DICK: Eighty-three?

CHILTON: Yeah, eighty-three is the year of the Miami Winter Symposium, yeah. Yes.

DICK: Well, let's talk about, sort of, leading up to that. What were you doing in attempting to basically get these transgenic plants to work?

CHILTON: Well, lots of different things. One effort was—and this was the . . . these things were all going on in all the different labs that were . . . it was going on at Monsanto [Company], and Schell and Van Montagu were working on it at the same time, and we were to get an antibiotic resistance marker that would work in the plant, kanamycin resistance. So to get a gene that's not a plant gene to work in a plant, you need to put a plant-active promoter and terminator on the beginning and end of the coding region. And then you need to be able to transfer that somehow to the plant cell with the *Agrobacterium*. One of the problems was that the Ti plasmid is so big that to put a gene into it is extremely difficult, and one of the early discoveries that we made and Schilperoort's lab also worked on this at the same time. We were in competition.

We asked whether you could put the T-DNA—the genes to be transferred—on a separate <**T: 95 min**> little plasmid and put that in there with the helper genes with the virulence genes on the Ti plasmid. And would that still be able to move the T-DNA? And that worked. So that made everything easier. You could work with your transgenes on a little plasmid. You could add your antibiotic resistance marker—or whatever you wanted to test—into the little plasmid and then put it into *Agrobacterium* with a helper plasmid. So that's technology that we use all the time now. It's much easier. So, yeah, the mini-Ti plasmid was one thing that we were working on. And we were working on the gene promoter from the methionine synthase gene, so we could attach that, figure out where the end of it was, and attach that to an antibiotic resistance marker or whatever else we wanted to express into it. It all sounds so easy now, but yeah, it just . . . it'd never been done.

DICK: What were . . . ? I know that eventually it was a tobacco plant that was successful. Were you working with other plants also?

CHILTON: Not much. No. And even tobacco was hard enough. I collaborated with my friend Andrew [N.] Binns at Penn [University of Pennsylvania]. He was very experienced at plant cell culture, and he was the one who took our engineered *Agrobacterium* strain. I said to him, "We have a problem with this. We made this nice plasmid with a potential transgene in it, but we can't get the plant cells to grow." And so, he said, "Sure, send it out." And so we got him the *Agrobacterium* strain, and he worked with it for several weeks or a month and called me up. And he said, "Well, the reason that it didn't grow for you is that the plant cells, you gotta supplement it with cytokinin." And I said, "Aha! We hit the cytokinin gene. I'll be durned." And he called me back a few weeks or a month later, and he said, "Mary-Dell, they're making plants." And I said oh. He said yeah. And I said, "Well, Andrew, what you have is the first genetically-engineered plants." They weren't mutants. It wasn't just one plant, but the whole petri dish was full of little plants. They were all there. So that's when we knew we had it. So those plants got their picture on the cover of *Cell*. Printed backwards, as I recall.[6] [laughter] That's okay. The point still comes.

DICK: Now there's certainly competition. You have, is it Ernie [Ernest G.] Jaworski at Monsanto?

CHILTON: [Yes].

DICK: And of course, Schell and Van Montagu in Belgium. Science is very competitive field. There's often not much reward for second place. So were you guys in contact with one another? Were you, sort of, sharing or, kind of, keeping . . . maybe showing one card, but keeping the rest to your chest?

CHILTON: I certainly tried to be as open as possible. But I wasn't totally completely open with the Monsanto folks. Because I sent so many that we had a little problem with competition. When I suggested that we could do such-and-such, they would ask me, "Well, write us a proposal. How would you do it?" And so I would write it in as cryptic a way as I could without totally enabling them to do it for me. But in the case of our competition, the Schilperoort's <**T: 100 min**> bunch, they had an *Agrobacterium* strain that had the virulence genes but not the T-

[6] Kenneth A. Barton, Andrew N. Binns, Anthony J. M. Matzke, and Mary-Dell Chilton, "Regeneration of Intact Tobacco Plants Containing Full Length Copies of Genetically Engineered T-DNA, and Transmission of T-DNA to R1 Progeny," *Cell* 32, no. 4 (April 1983): 1033–1043. Accessed at https://www.sciencedirect.com/science/article/abs/pii/009286748390288X on 28 July 2026.

DNA. They had made that in the course of doing something else—just it's a natural donation mutant of the plasmid.

And that was exactly the strain that we needed in order to test the mini-Ti idea. Would it work in trans to helper genes? And they had published it ten years earlier. So I called them up or wrote to them and asked on this thing, and I had to tell them why. And they wrote back with that they had to tell me that they were working on the same thing. And I kinda think we came to some agreement. They honorably did send me the mutant strain. But we published together. There was another case of that kind of thing where I had done the same work that Jeff Schell's group had done. And we held back . . . we sat on our publication until theirs was ready, and then we published 'em together.

DICK: Now we're leading up here to the Miami Winter Symposium where all of the different groups are presenting on these results. How did the symposium get put together? How was it . . . ? What was the logistics of, sort of, deciding that this would come together?

CHILTON: I don't rightly know. I think we were invited. But I think if you felt you had a story, you could invite yourself. Somebody did invite himself onto that program. But most of us were invited speakers. I'm trying to remember who it is that was the . . . that invited himself in. It won't come back to me. But, in a way, it doesn't matter. I was not too happy with it at the time. With the competition.

DICK: What was the experience like at the symposium? It kinda seems like all this really hard work, and it seemed for many of these groups, is finally . . . it's, "We did it."

CHILTON: My perception is that all the tools were basically in hand. Each piece of the project. We hadn't put it all together in one beautiful experiment, but all this . . . each problem had been solved. So you could . . . you can see that it was going to work. We were gonna be able to make transgenic plants with efficiency. We had made the plants, and we had the progeny, but we hadn't done it efficiently. We had done it the hard way. The brute force way.

DICK: Did you share some sort of war stories with some of the other participants in terms of the difficulties that were encountered considering how tough it was?

CHILTON: You mean since, or at that time?

DICK: Or at that time.

CHILTON: I can't remember. I don't think so. I don't think so.

DICK: Well, now 1983's also the year that you made the switch from academia into the private sector. And I know that this is only the very beginning where at least in the biosciences where academics are becoming involved in private industry. There's still some, "It's not really the norm." Some people felt that it was harming the purity of science. How did you feel about making that move?

CHILTON: Well, I would say that it wasn't such early days. In fact, it seemed like most of my friends had already been hired away by industry. And symbolic of that when Ciba-Geigy decided <**T: 105 min**> to make a move into biotechnology, the only leader they found left was me—a woman, for heaven's sake. Well, it must've been a culture shock to them. [laughter]

DICK: So they sought you out?

CHILTON: Oh yeah. They sought me out.

DICK: And what was their pitch that they gave to you to . . . ?

CHILTON: Well, they said that they were going to set up a lab in North Carolina. And I like North Carolina having grown up here. And they indicated something of how big it would be for the beginning at least. And how they were looking for a leader for this. And finally, would I be interested in making myself a nominee? And I said, "Sure. Why not?" So I went home and talked with Scott about it, and he said, "Go for it." That is, we had both sets of parents near here. His parents were in Virginia, and mine were in Southern Pines. So it . . . they weren't getting any younger at that time. So it seemed like a good idea to be a little closer to them.

DICK: Was he able to find work relatively easy out here, too?

CHILTON: No, not easy. But it worked out.

DICK: You're at Ciba-Geigy Corp. I have here from 1983 to 1993? What were some of the initial products or projects that you started working on once you came aboard?

CHILTON: Well, we had to solve a number of fundamental problems. One of them was getting Bt to work in plants. Everybody was working on that. And another one was how to transform corn—maize—and how to transform soybean. You see, the problem is that Ciba-Geigy hired a tobacco expert, and they weren't in the tobacco business. And I didn't know anything about corn or soybeans. So we had a big gulf to breach if we were to get anything done. I had no idea how hard that would be. Especially transforming corn was awful.

DICK: What makes corn more difficult than, say, transforming tobacco?

CHILTON: It's a bitch. I don't know. [laughter] I think a developmental biologist would probably have a word describing that. It's more like a stem cell. The tobacco plant is more like a stem cell.

DICK: I see.

CHILTON: It regenerates easily. And corn absolutely is not like that. There are some plants—potato, for instance. You cut the eye out of a potato and make a whole new plant out of it. And bananas are propagated like that. And in fact, these crops are not bred at all. They're propagated. And then, other plants are you have a seed business. And then you have hybrid seed business, in the case of corn. And the other idea was, could you make a hybrid soybean, for instance? It would be very desirable if you could hybridize soybean because then your customer would come back every year to buy seeds. And it wasn't clear to us in the beginning whether you could protect a transgenic soybean. In principle, the farmer could take his seeds—his transgenic seeds—and plant them again the next year. In principle. If you have a patent on that, then he shouldn't. Whether he would or not was not clear. And whether you could be a big mean seed company and sue <**T: 110 min**> him for doing that, that was going to be an ethical problem. So we talked about all these things. Lots of ethical problems. I was astounded, but . . . the extent to which we had to get into that kind of thing. And that before really deciding what we were going to do, what we could make.

DICK: So, with hybrid corn, it's if you replant the seeds, you don't really get the hybrid vigor. So, there's sort of a . . .

CHILTON: Right.

DICK: . . . natural sort of a thing there. But then . . .

CHILTON: Farmers are used to having to buy corn seed every year. But not soybean, no. So they feel like their rights are tramped on if you make [them] come back and buy more soybeans the next year.

DICK: And I guess it's not really, I think around . . . of course there's the major court decision in 1980 that allows individuals to patent organisms, but for plants it's even later on. I think around '85 or even '87, where that you can really begin to patent these plants and have that intellectual property protection. Was that a . . . . I guess even before that, was that a concern for Ciba-Geigy that would they be able to make their return on the investment for this research?

CHILTON: Yes. Yeah, they had plenty of concerns. I can't remember. You seem to have a date there for when permission for patenting a plant came out.

DICK: I think it's the *Ex parte Hibberd* decision—the [US] Patent Office—I think . . . believe was '85.

CHILTON: And I think up until then we did file a patent application on the technology for engineering the genetically-engineered tobacco plant while I was at the University . . . sorry, at Washington University. We did file a patent application on that. And it stood in limbo for a time until it was decided that you could even consider patenting that type of material so that slowed that down for a time. We filed that it must've been in '82 or '83. It had to be before the Miami Winter Symposium that we tried that. Or it was a year after that? Maybe it's a year after that. It was a year after the first time we talked about it, which was earlier. Yeah.

DICK: With the Bt corn, so you're working, and that's basically incorporating the protection—the Bt—into the corn gene itself in order to express the cryoproteins?

CHILTON: Protein.

DICK: Protein. And how . . . ? Was this . . . are you having to learn a lot more biology in terms of working with the different . . . ? Certainly, you're working with corn now but also just working with the Bt.

CHILTON: Bad things happen to the Bt gene when you put it into the corn plant. And I have understood at one time, and I can't remember all the details, but I think one thing was that it . . . the splicing mechanism of maize recognized part of the coding region, so it chopped it up wrong. Or failed to splice it correctly, or it was something like that. It doesn't have introns

because it's a bacterial gene, but I think the plant recognized something that it thought was introns, so it chopped up the messenger RNA. Anyway, we had to fiddle with the actual sequence. We had to make a synthetic copy of the Bt gene. Their natural gene just didn't work at all.

DICK: So this would be a, sort of, a copy DNA, cDNA?

CHILTON: No, not that kinda copy. It's . . . you have to synthesize it using different codons from the bacillus codons. You have to use plant codons. So, see, we had this wonderful thing called the genetic code, and, see, there are four ways to say lucein. There are six ways. Yeah, okay. And maybe the bacillus likes this one, and corn only likes <**T: 115 min**> that one.

DICK: I see.

CHILTON: And for each codon in the corn plant, there will be a transfer RNA that has a complement to this, so it'll be able to read that codon out of the messenger RNA. But if corn has very little or no tRNA corresponding to that particular codon, then you're sunk. So you need to change the codons so that you're using ones that corn likes and that corn has plenty of.

DICK: I see. And so, this is . . . a lot of this work's being done in the mid-1980s, late 1980s. Were there other projects beyond . . . what were you trying to transfer for the soy? Would that be herbicide resistance or tolerance?

CHILTON: I can't remember. Yeah, probably. I can't remember.

DICK: Then what were some of the other projects later on as you continued working on . . . ?

CHILTON: I don't know. I'm getting a little uncomfortable talking about the project portfolio that we had here. I don't know how much of that is . . .

DICK: Certainly. No need to—

CHILTON: . . . public information. It may be already. I don't know. We must find something more interesting more recently than that. Let's see. [laughter]

DICK: And were you . . . ? So at Ciba-Geigy, I guess here we've been discussing a lot of the science. You're a founding director and VP, so are you're taking on somewhat of an administrative, organizational role now?

CHILTON: Yeah.

DICK: How was that transition to taking on that task?

CHILTON: It was okay. It wasn't wonderful. It was . . . it looks like an important job with a lot of authority and so on and so on, but when you work in a big, Swiss-headquartered company, the truth is that you're just somebody in the chain of command. There wasn't really a lot that I could decide on my own. I had a direct line to my Swiss boss's telephone, so if anything came up, he wanted to know about it. I enjoyed the people that we were working with and the scientific problems that we were wrestling with. That part of it was fine. And I enjoyed recruiting people and that aspect of it. It was fun. But I . . . really, I don't have the heart of an administrator. I'm a scientist. That's what I really like. I like to do experiments. I like to see what happens and solve problems.

GOLDSMITH: We were talking to Vance Kramer. You said, I guess, he was one of the first people that . . .

CHILTON: Oh yeah.

GOLDSMITH: . . . came aboard. Are there others that are here from that time period?

CHILTON: And he was pretty early on, I think. […] Annick de Framond, you know her?

GOLDSMITH: Right. Not well, but . . .

CHILTON: We have to go and look at that picture of all the employees.

GOLDSMITH: At the front.

CHILTON: With the old guys in the front.

GOLDSMITH: Right. [laughter]

DICK: So, I have here in my notes—I could be off here—but that your tenure at Ciba-Geigy ended in 1993. Is that . . . ?

CHILTON: I can't remember. If it says that, I'm sure it's right.

DICK: Well, where did you go after Ciba-Geigy?

CHILTON: I didn't go anywhere.

DICK: So, then it has more to do with the mergers and acquisitions?

CHILTON: Yeah, we changed our name.

DICK: I see.

CHILTON: And I also changed my job, so I don't know which one of those is happening in that time period.

DICK: What job did you change to?

CHILTON: Well, I, sort of, became a postdoc. [laughter] They wanted John Ryals to head the group. He was a young hot shot that we had as a manager in one of the <**T: 120 min**> teams. And I was allowed to do what I wanted to do, and so it was to go back to the bench. And I worked with my then-postdoc, and she taught me all the new kits and all the new tricks that we had. So that I've slowly grown into a molecular biologist again. And there are plenty of people here who know more about other, different things than I do, but I'm pretty good at one little part of the business. I work with the big plasmids, so I'm the go-to person if you have a big plasmid that's not doing what you wanna do.

DICK: Now we're getting here into the mid-1990s, and I think it's around 1995, 1996 that the first transgenic crops become on the market, become available. What were your thoughts? Were you really beginning to see these applications actually become realities?

CHILTON: That was certainly nice to see. It was . . . the worry was that, in Europe, the technology was not really accepted, acceptable in many places. But yeah, nice to see that things are working. At last, at least.

DICK: And in Europe there is the . . . from '99 to 2003, there's the moratorium. There's a number of sort of controversies that are emerging. There's the Pusztai—if I'm pronouncing his name correctly—the potato study that there were concerns about it harming rats. There was the Losey study that thought to show that BT pollen was harming monarch butterflies. [Ignacio H.] Chapela and [David] Quist that the transgenes from Mexican maize are transferring.[7] And I know that most of those turned out to be they were incorrect, that they had problems, that the journals oftentimes would say "We shouldn't have published that." What are your thoughts as this somewhat this GMO controversy is beginning to build up at this time?

CHILTON: I don't know how to answer that. I guess one thing I thought—my mentor here had better muzzle me if I start saying bad things—one thing I thought is if transgenes did get into the landraces of maize in Mexico, what . . . where's the harm? I mean, if the gene is making them a little better, they should be glad. And if it isn't making it a little better, it won't stick around. It'll go away. I guess the concern people might have is they wanna eat food that's free from transgenes. But if they didn't look so hard, they wouldn't have noticed it. [laughter] I don't know. And then, let's see. What was the other case you brought up?

DICK: Let's see, the Pusztai rat [experiment] . . . the Losey butterfly—

CHILTON: Oh, the butterflies. I think one problem is that we always have to . . . you can't evaluate a technology in a vacuum. You have to compare this technology with what we would have otherwise—okay?—because it's not a . . . well, so if the farmer doesn't have a BT corn plant, he's gonna spray insecticide. I suspect that sprayed insecticide—no matter how you do it—has a greater likelihood of harming monarch butterflies than the occasional pollen spray that might escape into the neighborhood <**T: 125 min**> weeds. I mean, it's gonna be a rare situation

[7] Stanley W. B. Ewen and Arpad Pusztai, "Effects of Diets Containing Genetically Modified Potatoes Expressing *Galanthus nivalis* Lectin on Rat Small Intestine," *The Lancet* 354, no. 9187 (October 1999): 1353–54. Accessed at https://www.thelancet.com/journals/lancet/article/PIIS0140673698058607/abstract on 29 July 2026; John E. Losey, Linda S. Rayor, and Maureen E. Carter, "Transgenic Pollen Harms Monarch Larvae," *Nature* 399, no. 6733 (May 1999): 214. Accessed at https://doi.org/10.1038/20338 on 29 July 2026; David Quist and Ignacio H. Chapela, "Transgenic DNA Introgressed into Traditional Maize Landraces in Oaxaca, Mexico," *Nature* 414, no. 6863 (November 2001): 541–43. Accessed at https://doi.org/10.1038/35107068 on 29 July 2026.

where there'll be enough to harm a butterfly, I think. According to what I've read. I tend to read what I don't agree with unlike the rest of the world. But people act as though this is . . . as though it's this or not this, and nothing else harmful will take its place. And I think you have to look at previous and future technology and compare those with transgenes before you make an evaluation. I don't think we're gonna tie the farmer's hands, and I think he's gotta protect his crop. If he doesn't, there's gonna be a lot of hungry people here.

DICK: Well, moving onto your transition to Syngenta. So we were discussing this earlier, the series of mergers of . . . Ciba-Geigy merges with Sandoz to form Novartis. And then they decided to spin off Syngenta to focus on the agricultural biotechnology whereas the . . .

CHILTON: Agricultural business all together, wasn't it? Yeah.

DICK: And so how did . . . was it, sort of, that just seemed to be the direction to go for you once those mergers took place?

CHILTON: What do you mean—the direction to go? I mean . . .

DICK: Well, that . . .

CHILTON: . . . we just . . . we had our jobs. We kept working. They just changed the name.

DICK: So, it was very much not really much of a [. . .] it's they're changing the names; you're continuing to do your science.

CHILTON: Yeah, yeah. I guess some of the projects may've been affected, but transformation goes on, so . . . we didn't notice it so much in this bunch.

GOLDSMITH: I think there were a bunch of name changes. Different people at the helm in Switzerland. But I think for the teams that were here, I'd say it was largely unchanged. I'd say they get people come and go and some different strategies here and there, but as you say, they changed the name out there three or four times since 1983, but I don't think . . . . I mean as far as the team here goes, I don't know that a lot has changed. And I think it was . . .

CHILTON: We got new leadership, I think. But Steve [Stephen] Evola left us at that point. And who became the head then? How's your history?

GOLDSMITH: In 2000, I don't know. I've only been here five years.

CHILTON: I don't remember. […] It seems like we've had a number of heads since that time.

GOLDSMITH: Right.

CHILTON: Up until then, I remember them because I knew them.

GOLDSMITH: Yes. Right, right. So, when . . . . I don't know. I just know, going back, you had Nikhil and Roger and Roger Kimball, and then the other guy, who's now in Switzerland. I forget his name. Also, recently, but then, yeah, before that I'm . . .

CHILTON: You did pretty well. [laughter]

DICK: Well, I know in 2001, Syngenta and Myriad are mapping the genome of rice. Were you involved in this project, too, or not? What were you focusing on at this time, sort of, into the new millennium?

CHILTON: Two thousand? [Sound of papers rustling] Gotta go farther back. Let's see. Oh, we're in 2000 here. So it's targeted integration. Ah, yes. Yeah, we were using <**T: 130 min**> the int gene of lambda bacteriophage to try to get [. . .] site-directed integration into the tobacco plant, corn plant, several plants. And we're still working on this kind of thing today but not by integrase anymore. So if you ask me what I do, it is I work on targeted integration, and I work on gene stacking, delivery of transgenes, and then loop.

DICK: So the gene targeting is getting it so you more accurately sort of know where the transgene's gonna end up in the genome?

CHILTON: Exactly, yeah.

DICK: Now is . . . what are . . . . I guess getting behind the motivation before . . . behind wanting to do that. What are some of the problems with not knowing where it's gonna land in the genome, and how it's gonna be beneficial by it being able to have a more targeted approach?

CHILTON: Where there are several reasons. There's a list of reasons, but from the company's point of view, it should save money because when we put this gene—these different list of genes—into the same place every time, it seems like the regulatory process could be streamlined. We won't have to explore all the genes around that place anymore. We know what they are. Just check it out right there. And we would then know that it doesn't do anything harmful to the plant itself if we've had experience with this target site. But the other side of it is, from our point of view, that if we put the gene—the transgene—in a good place in the maize genome, for instance, we can choose a place where it's expressed well and where the plant doesn't shut it off at just the wrong moment—or any other moment.

So we get experience with a good part of the chromatin of the plant. And we know we haven't hit anything valuable to the plant. We know we're not in the middle of a gene that's important to the plant, so it should work a lot better, especially if we can do it pretty efficiently. If it's extremely inefficient, it still might be better, but the less efficient it is, then the less incentive we have to do it that way. So we have to get a decently efficient method. Not easy.

GOLDSMITH: How many genes are we getting into plants now?

CHILTON: Twenty-three.

GOLDSMITH: I guess, how hard is that compared to one? Or is it in the twenty years since you put the first one into a commercial product?

CHILTON: That's not the right question. The right question is—and I don't know the answer to that—but the question I do know the answer to is, if you put in twenty-three genes compared to two or three, how much do you impair the efficiency? It's not bad. I mean, it's—I've forgotten—15 percent or something like that, really. Could be better. But that may be the way life is. Long pieces of DNA get broken during processing, and there's certainly a lot of processing that *Agrobacterium* does to transfer it. But the history of the technology has taught us that things can really, really improve the more you refine them. I mean, you look at the efficiency of transforming maize now compared to what we were struggling against years and years ago. It's unbelievable.

DICK: And you said that the other [. . .] thing that you've been focusing on is the gene stacking?

CHILTON: Yeah, yeah.

DICK: Yeah, and tell me about the <**T: 135 min**> process there, and again, the motivation between wanting to stack genes.

CHILTON: Well, the reason that you wanna stack genes, you wanna put your transgenes all in one package, in one place. Not put in a few and then put in a few two years later and so on. The reason is if you put them in four or five different packages, the breeder will have to treat those as four or five different genes. He's gotta keep his eye on all of those. And it detracts from his ability to keep his eye on whatever else a breeder wants to keep his eye on. If we can have them all in one place, then there's just one trait from the genetic point of view. So he won't have to waste a lot of energy watching it. He can do 'em together. I lost a piece of that question. You asked something else. Oh. How do we do it? Or what is it for? Just working with big plasmids is technically a bigger challenge. As I said, some of us are good at it. I think that's my biggest one.

DICK: The photo there?

CHILTON: Here.

DICK: And what are some of the traits—I mean, if you can talk about 'em—what are some of the traits that you're working on in terms of transferring to the crops?

CHILTON: Well, a) I don't know, and b) if I did know, I probably couldn't tell you. So see you at the store. [laughter] […] I do know. I lied to you. I do know some. Not always, but yeah, some.

DICK: Have you worked on any other gene transfer methods, such as gene gun other than *Agrobacterium*?

CHILTON: Yeah, I've shot the gun. Yes. It really wrenched my soul something fierce. [laughter] *Agrobacterium* works better.

DICK: I've heard that it's sometimes . . . . I hope I'm not confusing this, but that it'll work better for dicots rather than monocots?

CHILTON: *Agrobacterium*?

DICK: Yeah.

CHILTON: Better for dicots than monocots? Who told you that? I don't think so. I did?

DICK: Well, I'll have to go back to my textbook here, and—

CHILTON: It might be out-of-date. Is it old?

DICK: Oh, it's pretty recent. I guess, one of the . . . from what I've read, one of the motivations for the gene gun was that sometimes they would have difficulties using the *Agrobacterium* as a . . .

CHILTON: I've heard that one. That's pretty old, but yeah.

DICK: But if that's something that's changed, where *Agrobacterium* to be pretty—

CHILTON: That's changing. But everything depends on what you're trying to do, and the gene gun for sure has its place. And there are some projects that you have to do that way, so . . .

DICK: How did you . . . how would you compare that to your experience? I know you're doing the *Agrobacterium* and then you did the gene gun, and were you . . . did you work with any kind of the older models or the more newer, sort of, helium gas gene guns?

CHILTON: I worked with whatever we had, which was state-of-the-art at its time. This was some years ago—maybe eight or ten years ago. I thought it was a helium gun.

DICK: Probably by that time.

CHILTON: I think so.

DICK: There's no talking . . . for example, the papaya story where they're using . . . just thinking of gunpowder, shooting it, and the smell of burning onions or whatnot as a . . . . I think you were—I don't want to keep you too long, and you're being recognized, and here we can move to some of the awards. The prestigious Benjamin Franklin medal, American Academy of Arts and Sciences, and so on. And then of course, the most recently, the World Food Prize. Tell me, I know the World Food Prize was set up by Norman [E.] Borlaug. And <**T: 140 min**> tell me about just receiving that. Did you go out to accept it?

CHILTON: It hasn't happened yet. It'll be in October.

DICK: That's right. In Iowa?

CHILTON: Yeah.

DICK: Is that . . . ?

GOLDSMITH: October 17.

DICK: Okay, I was completely blanking on it. I was . . . . I may actually go out there and try to attend. There might be an event close by, but . . .

CHILTON: Take your checkbook with you.

GOLDSMITH: It's five . . . . I believe five hundred and twenty-five dollars a person.

DICK: But as . . .

GOLDSMITH: You should go. There's a lot [on] the fourteenth. And that brochure I gave you.

DICK: Great. And then that's the World Food Prize, they put that together?

GOLDSMITH: Yes, right.

DICK: All right. Well, great. I guess here if we maybe do just a couple last more reflective questions. One is, for example, you write in your "*Agrobacterium*. A Memoir," "But perhaps the most important legacy from *Agrobacterium* has been its inspiration of confidence that formed gene integration even though DNA is sometimes delivered artificially is a perfectly natural process. My most fervent wish is that well-meaning environmental proponents will come to recognize this and embrace the technology based on it." And this you're writing in 2000.

CHILTON: That was a really good statement.

GOLDSMITH: That was a good statement, yeah.

CHILTON: Sorry. Do I still believe in it? Yes.

DICK: Well, you . . .

CHILTON: Absolutely. Absolutely. [laughter]

DICK: Well, I guess you . . . how do you feel now? We're talking so over a decade later since the controversy really began to take off.

CHILTON: I think it took off long before that. [. . .] Well, what's taken off is the technology. And now that there's more technology, there's more opposition. There's more to oppose. But I don't think the opposition has really become enhanced. I think the same people are making more noise.

DICK: Do you engage at all with if they're a debate or something like that on the GMO transgenics issue?

CHILTON: I have done a little bit of that in the past. And one of the opponents of the technology attends my church. Dean Simmons. But that's all right. We're friends. [laughter] It's just interesting. I didn't find out about him until he started writing letters to the editor of the local paper. He said, "Oh yeah. I remember you. I debated you one time." [laughter]

GOLDSMITH: What's his opposition? What's his objection?

CHILTON: [. . .] He's one of those people that don't listen to anything you say—just falls off to the other ear. Anyway, did I answer your question?

DICK: I know that there is what will . . . for example, the French researchers, [Gilles-Éric] Séralini and some of theirs, they'll come out most recently it was the rat tumor study that was shown to critics and scientists showed that . . . they were trying to argue that rats fed Bt corn—the Monsanto Bt corn—developed these enormous tumors in comparison to regular corn.[8] And of course, critics showed that they didn't have the proper controls. They were using mice that were already tumor prone. All these other issues. And it just happened in addition to Séralini putting a gag order on journalists talking to any scientist if they received the paper. So basically, none of the scientific critics couldn't point out those issues beforehand that . . .

CHILTON: These guys don't fight fair. Anyway, I'm not used by the company in debate. <**T: 145 min**> I don't know who is. Do you do that? Does Paul do that?

GOLDSMITH: Would you like to? [laughter]

CHILTON: Paul do that? I don't know.

GOLDSMITH: And certainly, we can use all the help we can get. [laughter] But that would be something, and I know we're looking at that as we've looked at your public presentations in October around the World Food Prize stuff was . . . you had talked about what do you wanna say, and so we'll talk some more about that.

DICK: No, it's kind of [. . . .] I think it was the first time the World Food Prize has gone to transgenic research that that's a . . .

CHILTON: The thing is I question—I don't know the answer to this—but I question whether the audience really has . . . is the right audience for that message. I think it's not gonna be filled with proponents of the technology. So could you inquire if we say that there? Possibly.

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[8] Gilles-Éric Séralini et al., "Long Term Toxicity of a Roundup Herbicide and a Roundup-Tolerant Genetically Modified Maize," *Food and Chemical Toxicology* 50, no. 11 (November 2012): 4221–31. Accessed at https://doi.org/10.1186/s12302-014-0014-5 on 29 July 2026.

GOLDSMITH: Yeah, no, I think it's likely to be a little mixed because you'll have people from all over the world and certainly a lot of folks from production agriculture and people who are advocating for production agriculture. But I would say probably . . . . I mean, certainly not everybody, and there's probably some skeptics will be there, and certainly a lot of skeptics will be listening and watching, for sure.

CHILTON: Anyway, interesting subject—still under consideration here.

DICK: Yeah, well, I think and this . . . kinda to wrap up our discussion here maybe just sort of reflecting on how the biotechnology—and specifically agricultural biotechnology—has changed over the years. And it's biotechnology itself relatively new, but there's mergers and acquisitions. There's changes in technology and so on. And what strikes you as—or if there . . . have there really been big changes?

CHILTON: I'm not sure what you're driving at.

DICK: Well, I guess just trying . . . just sort of looking back at the industry and your place in it and how it's evolved, how it's changed, how it's remained the same over this long view, this long—or relatively speaking—period?

CHILTON: I need help.

GOLDSMITH: What's changed, and what's stayed the same? I guess are there any . . .

DICK: Changes to—

GOLDSMITH: What's changed the most, and what's, I guess, are there things that are still constant?

CHILTON: About what? The process, or . . . ?

GOLDSMITH: About the . . . no, about the—

DICK: The industry. The science. The technology.

CHILTON: Well, the industry, I don't know. I can't speak to that. I don't know what you're after. And the technology, well, the technology has certainly gotten better and better. No question about that. I mean, we can do things now that we never dreamed of. Well, we dreamed of it, but certainly didn't know how to do it some years back. So, no, things have gotten much easier to do. They succeed much more often than they used to. But beyond that, I don't know how to address your question.

DICK: Totally fine. Are there any, sort of, final thoughts, final things that you'd like to add at all?

CHILTON: I guess especially that last sentence in the paper that you just read. That. I would like to see the technology embraced.

DICK: Great. Well, Mary-Dell Chilton and Steve Goldsmith, thank you very much for sitting down and taking the time to really share your story, share your career.

CHILTON: Glad to do it. And we get to review what you're going to write up?

DICK: Exactly.

CHILTON: I wanna make sure that it's reflective of what we said.

DICK: Exactly.

CHILTON: What we think we said. <**T: 150 min**> [laughter] What we meant.

DICK: Certainly. Great. [. . .]

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]