CHEMICAL HERITAGE FOUNDATION

ROLF DESSAUER

Transcript of an Interview
Conducted by

Sarah L. Hunter-Lascoskie and Hilary L. Domush

at

Dessauer's Home
Wilmington, Delaware

on

7-8 August and 1 October 2012

(With Subsequent Corrections and Additions)

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

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ROLF DESSAUER

1926 Born in Nuremberg, Germany on 3 November

Education

1948 B.A., Chemistry, University of Chicago
1949 M.S., Chemistry, University of Chicago
1952 Ph.D., Chemistry, University of Wisconsin, Madison

Professional Experience

E. I. DuPont de Nemours and Company, Wilmington, Delaware
1952-1960 Research Chemist, Organic Chemicals Department
1960-1969 Senior Research Chemist, Organic Chemicals Department
1969-1978 Research Associate, Organic Chemicals Department
1978-1986 Research Associate, Photo Product Department, Photo and Electronics Department
1986 Senior Research Associate, Imaging System Department

DX Imaging, Lionville, Pennsylvania
1987-1991 Subsystem Manager/Senior Research Associate

1991-2013 Consultant in Photoimaging Technology

Honors

2001 DuPont's Pedersen Award for Development of Hexaarylbiimidazole Chemistry
2001 DuPont's Plambeck Award for Contribution to Photopolymer Technology

ABSTRACT

Rolf Dessauer was born in Nuremberg, Germany, one of two sons of a physician and a housewife. His family fled to the United States after *Kristallnacht,* eventually settling in Flushing, New York. After service in the U.S. Army, Dessauer received his bachelor's and master's degrees from the University of Chicago, and a Ph.D. in organic chemistry from the University of Wisconsin.

Dessauer began his career at E. I. du Pont de Nemours & Company, working at the Jackson Laboratory on dyes and UV-screening agents. He developed dyes for Alcoa's anodized aluminum, discovered a way to color Teflon, and taught dye chemistry to employees at DuPont's Ducilo plant in Buenos Aires. Although his inventions often met with resistance, his work on UV-screening agents was a commercial success.

Reassigned to DuPont's Experimental Station, Dessauer began work on photochromic materials, leading to imidazole derivatives, which formed stable colored free radicals on exposure to light. When mixed with leucodyes, these hexaarylbiimidazoles (nicknamed HABIs) produced colors. The project was called ultraviolet imaging (UVI) and it initially generated interdepartmental enthusiasm. By exposing a coated paper to ultraviolet light—or visible then UV light—one could form negative- or positive-mode images. Paper was coated on both sides; the product thus produced was named Dylux 503 proof paper and it found wide acceptance in the printing industry from 1969 to 2010. Additionally, HABIs were found to be useful in photoinitiating polymerization, leading to successful color proofing systems and a family of photoresists; under certain conditions, coatings containing HABIs promoted changes in adhesion.

Dessauer's job then was to find other uses for HABIs. He developed photodecoration for leather and for furniture, early bar code labels, color cathode ray tubes, and an identification system for patients' specimens at Georgetown University. Again, Dessauer had to fight for support for his projects. There are now at least eighteen hundred U.S. patents involving the HABI family, most of the first four hundred of them granted to DuPont.

Dessauer had been at DuPont for thirty-five years when DuPont and Xerox formed a new company, DX Imaging (DXI), to market newly invented photopolymer electrography. Dessauer left DuPont to work at DXI, but the company was closed down after three years. He became a consultant for DuPont, Xerox and a number of other companies, including Hewlett Packard, and this work resulted in another patent. Since then Dessauer has also written histories of his work—notably, *Photochemistry, history and commercial applications of hexaarylbiimidazoles: all about HABIs*, published by Elsevier—and he is writing an e-book about color. He also plans to write an entry for Wikipedia. Additionally, with his friend, the late Thomas Gravell, Dessauer made a study of watermarks of early postage stamps and documents, printing on Dylux 503. He and his wife, a long-time friend from Germany, visit Philadelphia often, exploring restaurants and theaters. He keeps up with the biimidazole literature and is still thinking about the unsolved problem of tackiness. He has contributed records of his work to the Hagley Museum and Library.

Dessauer bemoans the current lack of long-sightedness at DuPont; to him, the company seeks to commercialize products rapidly or lose interest in the technology. He points out that commercialization of Dylux technology took about eight years, and he feels that there were many projects that would have succeeded commercially with just a little more time and some

support. He says that the policy of moving managers around frequently meant that people who did not fully understand the field made important policy decisions. Furthermore, important experience is being forgotten or discounted because of rapid technological changes. He does, however, name a number of "heroes" from DuPont, many of them friends for life. Dessauer believes that his main contribution is to have kept alive this chemistry, fighting for support in often-hostile environments. He points out that he was lucky to have good health, enough resources to get by, and a personality that led to a large network. Despite all his complaints, he had fun finding uses for dyes. Asked if he would do it all again, he says yes; in fact, he would go back to work right now, if he could.

INTERVIEWERS

Sarah L. Hunter-Lascoskie earned a BA in history at the University of Pennsylvania and an MA in public history at Temple University. Her research has focused on the ways in which historical narratives are created, shaped, and presented to diverse groups. Before Sarah joined CHF, she was the Peregrine Arts Samuel S. Fels research intern and Hidden City project coordinator. Sarah worked both in the Center for Oral History and the Institute for Research at CHF and led projects that connected oral history and public history, producing a number of online exhibits that used oral histories, archival collections, and other materials. She also contributed to CHF's *Periodic Tabloid* and *Distillations*.

Hilary L. Domush was a Program Associate in the Center for Oral History at CHF from 2007-2015. Previously, she earned a BS in chemistry from Bates College in Lewiston, Maine in 2003. She then completed an MS in chemistry and an MA in history of science both from the University of Wisconsin-Madison. Her graduate work in the history of science focused on early nineteenth-century chemistry in the city of Edinburgh, while her work in the chemistry was in a total synthesis laboratory. At CHF, she worked on projects such as the Pew Biomedical Scholars, Women in Chemistry, Atmospheric Science, and Catalysis.

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INTERVIEWERS: **Sarah L. Hunter-Lascoskie**
Hilary L. Domush

LOCATION: **Dessauer's Home**
Wilmington, Delaware

DATE: **7 August 2012**

HUNTER-LASCOSKIE: Okay. Today is August 7th, 2012. I'm Sarah Hunter-Lascoskie. I'm here with Hilary Domush. We are in Wilmington, Delaware, interviewing Rolf Dessauer. As we mentioned, typically we start at the very beginning. We know that you were born in 1926 in Germany, but it's a period in Germany I'm not quite familiar with. So I'm curious what it was like, you know, being a child and growing up in Germany at this time.

DESSAUER: Well, my beginning: I was born in Nürnberg [Nuremberg], Germany. My father [Adolf Dessauer] was a physician. He had a practice in a relatively blue-collar section of Nürnberg, right near where Siemens [Siemens-Schuckertwerke] had a very big factory. I think it was a fairly ordinary life. My mother [Lilli (Schloss) Dessauer] was also born in Nürnberg. My father, however, was born in a little village, near Schweinfurt, [Germany]. When he was nine years old, he left home and moved to Schweinfurt and stayed in the home of a rabbi, so he could go to high school, because they had no high school [or] *Gymnasium* in the little town [where] he was born. So my father had lived by himself since he was nine years old. It was hard [to] ever to shake […] him up sufficiently to tell him that he wasn't right about something, because he [had] made all the decisions. […]

Well, anyway, we had a comfortable life. We were reasonably well off. Everything was hunky-dory until [Adolf] Hitler came to power in 1933. It became progressively more difficult for people who were Jewish, or were of Jewish descent. […] The Nazis were sort of clever. They said, "Oh, anybody who served in the First World War," as my father did for four years, "wouldn't have to worry about anything." But a few years later, that fell by the wayside. So […] by about 1935, my parents decided they would want to leave Germany.

Now my father had gone to, what in German was called *Humanistisches Gymnasium*, which meant [he studied] Latin, Greek, French, and Italian, but not English, so at age fifty, he started with English. And it was necessary, because in order to become licensed as a physician in that time in New York state, you had to pass a language exam before you [could take the Medical State Boards]. […]

[recording paused]

DOMUSH: So before…we were talking about your father learning English at fifty, which is a big undertaking. But I was curious, was there a large Jewish population in Nürnberg? Was it…?

DESSAUER: [Yes.] I have quite a lot of literature about Nürnberg, and they have an interesting website, so you can really research it very well.[1] It's partly in English, surprisingly. Nürnberg, at the time I left, [had] a population of about four hundred ten thousand, and oddly enough, Jews were not permitted to live in Nürnberg until 1858. Interesting.

But there were about nine thousand Jews living there after World War I. By the time Hitler came to power, there were only six thousand. Apparently three thousand, including some relatives of mine, [had] left because of economic limitations. Then there were another three thousand that left between 1933 and 1939. Then that left three thousand, and the remainder, some managed to get out […] until 1941. Then, of course, quite a few, including my grandmother, went to [concentration camps].

[…] Now there's a small Jewish population there. I'm not that interested in it because of <**T: 05 min**> the Jewishness of the thing; I'm interested because it's people. Actually I had found out that there was a historian, [Gerhard Jochem], whom the city of Nürnberg had hired to study the Jewish persecution there. […] He had written a book about the victims, and it's a detailed catalogue of everybody who was deported and killed, with birthdays and pictures.[2] And I [wrote to him that] it would be interesting to have a book about the people who escaped—what happened to them? I got a very brief comment back: "Start writing."

[…] Actually, [my wife] Angela [R. Dessauer] and I visited him a few years ago. I donated […] a fairly complete ancestral study that someone in my family had made [that showed that I had relatives that lived in Germany over two hundred years ago]. I look at this website now and then, to see if there's anything interesting and new. Of course, one of the questions that has always occurred to me [is], why is there not a good history of the Jewish immigration […] to the United States? I once met […] a professor of sociology at Harvard [University]. He had a similar background as I did. I asked him why. He said they were all too busy working, they never had time to write, the people who came over here. So I started writing, and I have [in] my computer, I think it's about a hundred odd pages of my early history.

But anyway…. So, well, the difficulty at the time in 1935 […] was, even if you wanted to leave, you had to go someplace, and there were not necessarily open doors for people to come to. In the United States at the time, the immigration policy was that you had to have an affidavit

[1] "Nürnberg's Victims of Shoah," last updated 21 August 2009, accessed at http://www.jewishgen.org/yizkor/nuremberg/nuremberg.html.
[2] Gerhard Jochem, *Gedenkbuch für die Nürnberger Opfer der Schoa* (Nuremberg: Nuremberg City Archives, 1998).

from somebody who [was] willing to donate money if you needed it, in case you became indigent or so. Of course the United States was still in [a] Depression, so there were relatively few people with the resources to assume additional burdens.

Well, we had a number of relatives, very remote relatives, and […] my parents managed to find an affidavit for my brother [Heinz Dessauer], who was seven years older [than I]. When he was eighteen, he came to the United States. In 1938, after *Kristallnacht*, [nearly] all our furniture got sort of [smashed] into small pieces, [and] my father decided that if he would get an affidavit, he would go. Since my mother and I had no affidavits, [we were left] behind with the expectation that my father might either establish himself quickly, or that he would find somebody else to give us [affidavits].

So it turned out my father came to the United States in December, 1938. My mother and I got our affidavits in April, and we came to the United States and landed here on June 2, 1939. Interestingly enough, the man who gave us, my mother and myself, an affidavit was […] named Bernard Gimbel. Do you remember Gimbels Department Stores? He was the owner of [these] so we felt a small obligation to buy things at Gimbels at a later time. Well, we were…

DOMUSH: Was he in the habit of giving affidavits? Or did he know you?

DESSAUER: […] No, we had a very—well, a remote, like four levels away—relative who knew him, and apparently pleaded with him, and so it happened.

Now physicians, in a way, had an advantage, because medicine is pretty much the same all over the world, whereas lawyers had a disadvantage because people who were lawyers in Germany had no way of reestablishing themselves as lawyers in the United States without going through lengthy law school and so on. So, I mean, betting on my father being able to establish <**T: 10 min**> himself, was, you know, was a fairly sure thing. […] My father, at the time he came over, was fifty-three years old, and he […] didn't necessarily want to live in New York, but by 1939, there were only four states left where foreign-born, noncitizen physicians could even get a license. I mean, instead of now, where half the physicians you meet are foreign-born. But then, that was the not the case. The four states were Ohio, Illinois, Massachusetts, and New York. So we stayed in New York. It took a year for my father to pass the English proficiency exam. Then it took him another year and a half or so before he passed his medical state board exam. So by May 1942, things were fairly good for my parents.

Now war broke out in 1941, and my brother was drafted in 1942. My parents, after a fairly lengthy search, found a section in Queens, New York—Flushing—where they seemed to need a physician. My father became a physician in [Flushing], and had a very successful practice for a number of years. When he finally started setting up practice, he was fifty-seven years old. When people asked me when I was going to retire, I said, "Well, my father only started working when he was fifty-seven. How in the world can I retire until about ninety?" So my father practiced medicine until 1960. He died in 1966. My brother went into military

intelligence, and the same group that—there were a number of other German refugees, like Henry Kissinger, who [were], I think, in the same outfit for a while.

[…] [I went] to junior high school in Manhattan [New York], Junior High School 164, which was on the edge of Harlem [New York]. It was a school where the student makeup was probably 50 percent black, about 30 percent Puerto Rican, and about 20 percent Caucasian…

DOMUSH: Now how was your English? I…

DESSAUER: Well, oh yes…

DOMUSH: I assumed that you didn't know any when you…

DESSAUER: That's a good question. I should have mentioned that. My father, like a lot of Jewish physicians, had to stop practicing in 1936, so he had not much to do except wait for his affidavit. He started to study English with ferocious intensity. Every day he had a different tutor. Every day he would go for English lessons. Naturally, some of these I had to go to, [too]. I had only two tutors, Mr. Fabian, who was a Berlitz School teacher and Mrs. Rodgers, who was an American lady who lived in Nürnberg [and taught American English]. So I went.

[…] By that time I was in […] high school, [the Jewish *Realschule* in Fürth, about 15 kilometers from Nürnberg]. We had English. Of course, everybody knew that they were going to need it, so […] [all students] learned English with enthusiasm. The same group of kids was together for a number of years. I still remember the name of most of the boys in my class, which is sort of interesting. I found a picture of our class on the Internet.[3]

DOMUSH: Oh, wow.

DESSAUER: […] I was a better than average student, I […] liked history. I didn't like [biology], because my father claimed he knew a lot about all of bones and everything, and whenever he started quizzing me before [he] let me go out to play, I was up against my father's superior knowledge. But I learned English. <**T: 15 min**> When we came [to the United States], we made a very dedicated effort to learn English. I mean, we practically did not speak German

[3] "Group portrait of students at the Juedische Realschule in Fuerth with their teacher, Benno Heinemann," 1936. United States Holocaust Memorial Museum, #09125. Courtesy of Paul Stiefel. Copyright of United States Holocaust Memorial Museum. Dessauer discovered the photograph through the Alemannia Judaica website "Fürth (Mittelfranken): Texte/Berichte zur jüdischen Geschichte der Stadt," accessed at http://www.alemannia-judaica.de/fuerth_juedische_schule.htm on 20 September 2013.

at home. My mother actually had some English in school in Germany. My brother, of course, had also.

[…] In those days, of course, there was no television, so my father went every afternoon to a movie. […] He could go to a movie for ten cents. He decided that was the fastest way to learn English. [He] went to matinees every day of the week and learned English. That was until he started practicing, and then […] it turned out he spoke enough English to get along quite well.

Well, I started talking about my junior high school. It was interesting. The class that I was in was 7A-7. Then, there were eight [parallel] 7A classes; 7A-7 was pretty close to the bottom of the [group]. We were half black. I think mainly there were Puerto Ricans and German and Austrian refugees thrown in. We had one "American" boy named Howard Isaacs. He was held up as our role model. But, we learned. And that class did fantastically well, because they all—I mean, the European kids all—had parents who insisted that they study and work hard. Both […] the Puerto Rican and black kids responded, and after one year together, we were […] broken up, and skipped [grades], and so on, so I finished my junior high school [courses] in a year and a half instead of two years. Then I went to George Washington High School for a little bit. I think one of my contemporaries [at J. H. S. 164 and George Washington High School] was Alan Greenspan, but I didn't know him. […]

Well anyway, we moved to Flushing in September 1942. My father and mother rented a house, and what used to be a dining room became […] my father's office. The living room became a waiting room for the patients. We had a kitchen downstairs and bedrooms upstairs. It was relatively modest.

I went to Flushing High School. One of my first friends there was a boy named Ed [Edward J.] Heubel, whose claim to fame was that his parents had bought him an *Encyclopedia Britannica*. It was, in those days, an investment.

DOMUSH: Yeah.

DESSAUER: Ed and I became really good friends until he died last year [2011]. […] [We still see his widow].

Well, Flushing had been the site of the 1939, and also later on, the 1964, World's Fairs. It was not a particularly exciting place to live, but I got along. I had two teachers who were outstanding, a mathematics teacher [Lillian Lieber], who [is] even in Wikipedia, and an English teacher [Louis Gray], who was superb. I took chemistry and history, and other subjects. I did quite nicely, graduating […] [from] high school in three and a half years, and when I graduated, I [received awards] in chemistry and mathematics and history. The history…

DOMUSH: Did you like the sciences more than the humanities? I mean you said you got…

DESSAUER: I liked the…

DOMUSH: …the award in history as well. And you certainly seem to have an interest in history now.

DESSAUER: […] Well, I found history came easily because I like to read. I was a tiny little kid. I mean looking <**T: 20 min**> at me now, it's hard to believe that, but I was small and not very athletic and determined to do something academic. I liked chemistry a lot. We had two very good chemistry teachers. Again, one of them [Leonard Fliedner] later became principal of […] Stuyvesant High School, which was one of the good science high schools in New York. [...] It had an interesting system in the first chemistry course. Depending on how you did on your exams, you sat further and further away from your teacher. It was obvious who was the smartest kid. I don't know, but that system seemed to work, and I studied chemistry with a passion. I even had a chemistry set at home.

My father, on two occasions, [threatened] to throw me out [of the house because of] some chemical experiments. I didn't want to blow anything up, but I remember when I electrolyzed saltwater and came up with a test tube filled with chlorine gas, and said, "Look what I've got." He was not enthusiastic.

The other great scientific experiment that almost terminated my scientific career was [when] *Life* magazine had some story about how to do some science experiments at home. One of them was to dissolve wooden matchsticks in carbon disulfide, then drop it into water and it comes out as a fiber. So I went to the neighborhood drugstore, and I asked them for some carbon disulfide. [The druggist] said, "Be sure to tell your father that—"

I said, "Oh, I know. Carbon disulfide is very bad for inhaling, and it's very flammable."

So he said, "Okay, as long as you know that. Tell your father."

My father had a little laboratory, because in those days physicians would do their own blood and urine analyses. He had a little petri dish. He dumped a little carbon disulfide in it. My father was an endless chain-smoker. So he always had a [cigarette] lighter [at hand], and he'd stand with the lighter and the carbon disulfide. I said, "Don't do that! Don't do that!" All of a sudden this thing caught fire and it turned into sulfur dioxide, and my father practically choked.

I said, "I told you not…"

"Get that out of here!" Well, he said, "Either you go out with this and bring it back, or you find a new place to live." [laughter]

I said, "I told you not to burn it."

But to the end of [his days], my father was always interested in what happens when something burns. So anyway, so I had chemistry, physics, and math and such in high school, and did quite well in them.

Well, the big problem was college. I had a—oh, incidentally, are we going too detailed, or is this all right?

DOMUSH: Oh, this is going great…

HUNTER-LASCOSKIE: Yeah. Detailed is great.

DESSAUER: Okay. I just don't want to bore you…

DOMUSH: No.

HUNTER-LASCOSKIE: No.

DOMUSH: No, no, not at all.

DESSAUER: Okay. Well, the problem came up [of] where to go to college. We lived relatively close to Queens College in New York, but at that time, [it was] not a particularly highly regarded school. My father, having grown up in the German university system, felt that all universities and colleges were the same, because in Germany you could transfer from A to B to C and back to A without being penalized or anything, although I think that obviously some departments were better in some fields than others. But it was simply not the way it is in the United States where […] Harvard was one thing, and Queens College probably was not on the same level. My idea was [that] I ought to go someplace which was as good as I could get in.

Well, it was the problem of money now. […] In 1944, when I was looking for colleges, most college tuitions were about four hundred dollars per year. It wasn't a lot, but four hundred dollars then was not four hundred dollars now. Well, my parents, my father had only been working for a year or two. He said, "We don't have the money to—" I mean, <**T: 25 min**> "I can give you some money, but—so you better get a scholarship."

Well, the difficulty was [that] on the scholarship [application] you had to list your father's income. My father's income in 1943 was about fourteen thousand dollars. Now the President of the United States only got twenty-five thousand in those years, so he had a pretty high income, which of course could predetermine that I wasn't going to get a financial scholarship, so I was sort of stuck. Well, in March of 1944, the U.S. War Department announced a college program, where [young men] of seventeen could enlist in the military, be sent to college until they turned eighteen, and then go into some military operation, like the infantry. But if conditions were right, they said, "We would let you go back to college and let you finish your career." So that sounded quite interesting. The program was called Army Specialized Training Program, ASTP. For those of us who were seventeen, it was called ASTRP, which was Army Specialized Training Reserve Program. We got no pay, but we had uniforms. So that sounded like that was, perhaps for me, the best thing. They had actually promised us, the people who enlisted in it, to [go to] colleges or universities relatively close to where they lived. A friend of mine, who had gone in earlier, went to Princeton [University], so I thought well, that's a great idea. I'll go to Princeton. [I should say there was also a similar program for Navy, for the Navy college program, which were called the V-5 and V-12 Navy College Training Programs.]

I went [to 480 Lexington Avenue in Manhattan to] enlist in the U.S. Army. […] Well, I get down there and they [said], "Are you a citizen?"

I said, "No, I'm not a citizen."

"Well, […] you can only enlist if you're a citizen."

I said, "But it doesn't say so. It says you have to be a citizen to enlist in the Navy, but it doesn't say anything about enlisting in the Army."

"Well, we have a rule. You [cannot] enlist."

I was really furious. It was the first day I had ever missed school. I wrote a scathing letter to the commanding general of the Second Service Command that was—at that time—was New York, Delaware, and New Jersey. I got a reply about two weeks later saying, "Come down, and we will go through the operation […] to study your case on its merits." […] I was an enemy alien, because I was born in Germany, but I was stateless. We were, after all, stateless for the same reason the United States was at war. But it didn't seem to […] make any sense.

DOMUSH: And your brother at this point was already—he was serving in the Army?

DESSAUER: My brother was serving in the Army. […] I don't know if he didn't want to go, but, I mean, he was not as enthusiastic about it as I was. So I thought, "Well, all right. They would make a special case because of me."

Lo and behold, I got another letter back saying, "We appreciate your willingness to serve, but our regulations say you have to be a citizen, and we can't change that." Well now I had been in the United States just five years while all this occurred. My father was a citizen, because they expedited citizenship for physicians, and theoretically, I should have become a citizen derivatively from my father. In other words, at that time if your father was a citizen […] you could be a citizen. But here I was not a citizen. So I wrote letters, and I wrote a letter to the Secretary of War, […] [Henry L.] Stimson at the time. I wrote to the Attorney General, […] Francis Biddle. I wrote to somebody [else], the head lawyer [James Ullio, of] the United States Army. I got two very nice letters back saying they have their rules.

But I [received] a letter from Attorney General Biddle. He said, "We […] admire your dedication and we will make you a citizen on the 29th of June." The 30th was the day when the enlistment period stopped <**T: 30 min**>. So I thought that was pretty neat, [and] on the 29th of June, my father and I went to Brooklyn, New York, to the Eastern [District] Court and I was sworn in as a citizen. Oddly enough, they asked you questions about it, and the question they asked me: "What was the first state to join the Union?" I had never been to Delaware…

DOMUSH: But did you know the answer?

DESSAUER: [Yes]. So I said I was thankful to…. I weighed 93 pounds, and [one] had to weigh 97 pounds to be accepted, but [I could make that up with] orange juice, and I got into the Army. I got my Army Serial Number. I called up my mother and I said, "I'm Private Dessauer reporting for duty."

My mother nearly fainted. She had encouraged me all the time, because she'd thought, "Well, you're so puny they wouldn't take you." But I was in.

So I sat around for a month. Then […] I was [told] to report to Lexington, Virginia, to attend Virginia Military Institute [on August 4th], which I did not think was exactly where I had intended to go, because I knew they were parading and marching up and down. Actually, I got to respect them. I had a good—a fairly good time there, and [I] had good chemistry teachers. I spent six months there, got very good grades, aced everything that had to do with chemistry. So I figured, well, that's good, because if I want to go somewhere else, I would ultimately be able to show what I had done.

Well, after the VMI period was over in January 1945, I went to the infantry in Camp Blanding, Florida, [for Infantry Basic Training]. Then when it was time to reassign me, the Army decided I should go to Japanese Interpreter School. I said, "Why?"

They said, "You have a talent for languages. Look how well you speak English."

I said, "I speak German even better."

Well, "We need people to study Japanese." Well, I had really wanted to go to Germany. I think I had a few scores to settle. I was worried that if I could ever speak Japanese, I could never get out of the Army, because in 1945 it looked like we were in for a long, long war with the Japanese.

[…] I could talk and talk and talk. [The Army] decided I could study electrical engineering instead. I spent six months in [State College, Pennsylvania] […] and studied electrical engineering, which I disliked intensely. But the war was over, and so the question [was], what would happen next? Well, at that point, the Army offered us an opportunity to reenlist for a finite, a definite, period, so [one] could predict at the end of that time you could be out of the service and could enroll in a university on such and such a date.

So that sounded attractive, and most of my fellow students, soldiers all—we all understood, and we dutifully were sent to [Camp Crowder,] Missouri, to a military camp where they didn't know what to do with us. For about two months, our daily routine was to play bridge from 8:00 a.m. to 12:00 p.m., have lunch, play bridge from 1:00 p.m. to 5:00 p.m., and go bowling in the evening for exercise.

DOMUSH: Wow.

DESSAUER: I really learned how to play bridge. Of course, in between, I did a fair bit of reading. As a matter of fact, I probably got as much as I could take of bridge. But anyway, we had prisoners of war who were doing the cooking and doing the cleaning, make our beds—it was a wonderful experience until some idiot complained. Then we wound up being back into Infantry Basic, and then, things were not so good.

Well, fortunately, what happened afterwards was that the Army decided to close that camp. We were all sent to Fort Monmouth, New Jersey. Now that, to me, was great, because it was close to New York. At that time in New York there was something called "99 Park Avenue," where uniformed servicemen could get theater tickets for free <**T: 35 min**>. I had always enjoyed going to the theater, so I had a year of free theater twice a week, or three times a week depending on how ambitious we were. And…

HUNTER-LASCOSKIE: Wow, this sounds like a very idyllic Army experience…

DOMUSH: Yeah.

DESSAUER: Well, I tried to make the best out of the situation. It was very nice. In Fort Monmouth, they put me in the Signal Corps School. At that time, telephone switchboards—

military telephone switchboards—were manual. So you had operators to push buttons. You know, stick plugs into holes and so on. […] I finally wound up teaching in that school until I got out of the service. Well, what was important, I was to figure out what to do post-Fort Monmouth, and…

DOMUSH: Now can I ask real quickly…Where was your brother at this time? Was he…

DESSAUER: Oh, my brother? My brother was in Supreme Headquarters Allied Expeditionary Force, which was the [organization] that [Dwight D.] Eisenhower commanded. He was in Military Intelligence. So, […] career-wise, he […] did better than I did in the Army, wound up as a captain. He worked in the end for what became the OSS, the Office of Strategic Services. Later on, after he got out, actually they offered him a job. He probably would have been in the CIA [Central Intelligence Agency], if he had pursued it, but he decided he didn't want to do that.

My brother unfortunately did not have an opportunity to go to college here. I mean, he went to night school, and when he got out of the service in 1947, he was twenty-eight years old, so he decided he'd better get going and do something for a living, instead of going back to school. Of course, when the war with Japan was over, suddenly a lot of military guys got the GI Bill [of Rights], and could go to colleges.[4] The question was, was it easy to get into colleges? Not only was it easy to get in, [but] would you have housing for it? The [universities were] fairly generous in letting people in, but the housing was a tremendous challenge.

Well, my friend Ed Heubel, [of whom I spoke] earlier, had gotten into Yale [University] after high school. One time I visited him while I was still in the military. I was quite impressed with Yale University, so I applied to Yale. My mother had some relatives who lived in Chicago [Illinois], and I figured well, if I could get in to the University of Chicago, I might be able to stay with them, if I couldn't [find] any other accommodations.

The University of Chicago had an excellent chemistry department then, where the remnants of the Argonne National Laboratory and the atomic bomb were. So I thought, well, that wouldn't be bad, if I could get in there. So I applied to the University of Chicago but—they sent me an application—but they didn't send me a catalogue. Well, everybody said, "Don't waste any time."

So I sent in the application [but] I didn't realize that [the organization] of the University of Chicago was totally different from any other university, because they took youngsters out of high school after two years, and sent them to their college. So if you went [as a freshman to their college], you were actually a junior year in [a conventional] high school. So after four

[4] The GI Bill is formally titled the Servicemen's Readjustment Act of 1944, Pub. L. No. 78-346, 58 Stat. 284 (1944).

years of university/college, you went to a division. Then [you worked on] a master's degree, [or] a Ph.D. depending on how long you stayed there.

So they [wanted] to know [which] department I applied for, [and] I said, "I applied for the chemistry department." I received an invitation to come to—to [take] some tests. One was a fairly straightforward intelligence test, I guess, which was administered to me by one of my [former] high school teachers. Then there was a two- or three-day-long entrance exam, which was held at <**T: 40 min**> Hunter College in New York. I got an Army furlough for that. It was something like sixteen hundred multiple-choice questions, and it covered everything. The philosophy at the University at that time was, well, we can't really ask people about factual things, because they've forgotten those, but they could ask you how you reason in chemistry or in math, and so on.

To my great surprise, I [was] accepted [by] the University of Chicago in the [Division of Physical Sciences, which] was like a [junior] in a normal college. So at Yale, they only let [me enter] as an upper freshman, so that didn't appeal to me. […] In March, 1947, I embarked for Chicago by train, and […] I took some chemistry courses. They had a very liberal policy at the time for people with advanced standing. They gave me credit for everything I had studied at VMI and Penn State. My language problem was minor, because my German was still quite fluent at the time.

Then it came to [the question], what are you going to do for coursework? Well, my advisor then was a man named George [W.] Wheland, who was quite an eminent chemist in his day. [His son actually works] for [E. I.] DuPont [de Nemours and Company] now, still. He couldn't figure out what to do with me. He said, "Why don't I give you a bachelor's degree, and then you get a master's degree later." So I [received] my bachelor's degree in five quarters at Chicago.

DOMUSH: Wow.

DESSAUER: Then I stayed on for a master's degree. I left Chicago with a bachelor's […] and a master's degree in 1949. I must say, I did not enjoy the University of Chicago very much. I thought there were enormous pressures. I mean they admitted people and flunked [them] out, and it was like in one door, and then a bigger door for people going out. Housing was initially very poor. Competition was fierce. I mean—it was at that time that a lot of young people wanted to go to medical school, and they'd audited a course once or twice before they took it for credit. So it became very difficult, really. My problem also was that sometimes I had two chemistry courses, which had labs at the same time, because of my hurried, advanced schedule. Anyway, I survived, and that became…

DOMUSH: Was it any difficulty for you being so far away from New York, and so far away from your family?

DESSAUER: No. My mother's [cousins, and] my mother's aunt and uncle, were somewhat supportive of me, and took me out to dinner and so on. Then I really didn't have much time because we were studying all the time. I don't remember a single date that I [had] at Chicago. It was just study, study, study, study and more studying. I learned a few things. I can't complain. But I don't think it was a very healthy atmosphere.

I had a friend, who later on became a physician, [whose family] took pity on me; [they] lived [in] Rockford, Illinois, not too far from Chicago. So they invited me for a weekend to—a Memorial Day weekend in […] 1948—to visit them. Then we drove up to Madison, Wisconsin, from there to look at the University of Wisconsin [Madison]. I said, "I know where I want to go." It was a nice warm day, and there were all these coeds sunning themselves in Lake Mendota. After Chicago, that just seemed to be heaven.

HUNTER-LASCOSKIE: Now, I'm curious. You mentioned earlier that perhaps your father was disappointed that you didn't kind of follow in his footsteps. I'm curious: as you're going through this—you know, your military, the education—what's your father's perspective? Did he say anything about what you should be doing? Was there just, you know, general encouragement?

DESSAUER: Physicians beget physicians, I guess <**T: 45 min**>. When it came to "What do you want to do for a career?" I mean, I was interested in science. At that time, I tried thinking about what my father did, which was, I'm sure, financially rewarding. I don't think it was very scientific. I mean, those were the days when there really wasn't much medical insurance. People would always call late at night, or on weekends when they were really sick. My father would have emergency after emergency.

You know, it was a strain on him, and I don't think that was what I wanted to do. I mean, I wanted to work in a laboratory and invent things, or do something useful and not deal with sick people who were grouchy and unpleasant, and infectious, all of those things. As a matter of fact, I think I really hurt my father when I said, "Well, I think dentistry is more scientific, because you're dealing with something that you can do something about." That was another one of our not-so-good conversations. Then […] he finally accepted that maybe chemistry was a good career for me. I think—when I finally saw some success in my college [studies] in Chicago, he was content that I was not to be a physician. So I have not had any regrets since then.

But anyway, I found my interest in science suffered a little in Chicago, because I really felt it was very hard to maintain good grades. […] Also, I felt the University of Chicago had relatively—in my opinion—few people who were really good teaching professors. We had […] Frank [H.] Westheimer, who was inspiring. I really liked him. Then there […] was Morris [S.]

Kharasch, but by and large, the teaching was done by assistants. Professors were off consulting on important things, and the students were pretty much left to their own devices.

I think competition is healthy up to a point, but not over, not beyond. A good illustration was one [that] got me into trouble. Professor Wheland, who was my advisor, taught an advanced chemistry course. So when the first session started, first quarter, he said it was very simple: we had three exams. Your grade is the lower average of the two best exams. The exams are marked on a curve: ten, twenty, forty, twenty, ten. So it behooves you to study. All questions were multiple-choice. Well, I can't possibly imagine how organic chemistry can be taught multiple-choice, but he did.

Well, the first semester, the class was pretty [large]; I got my A. The second semester the class had shrunk a lot. I got a B. The third semester was really hard going. There were only about twelve of us left in the class. So one day, I went to his office, and I said, "I have a question." I said, "If you took a dozen monkeys and gave them a pencil and paper, and give them your exams […], 10 percent of the monkeys would get As, and 10 percent would get Fs. Do you think that the A-monkeys would know more chemistry than, the F-monkeys?"

He threw me out of his office.

DOMUSH: I'm not too surprised.

DESSAUER: Well anyway, my career in Chicago came to a semi-glorious end with a master's degree. They pretty well had at that time a policy that unless you were really exceptional, and I wasn't, they wanted you to go to somewhere else for your Ph.D. degree, and…

DOMUSH: And you were probably fine with wanting to go. You were probably okay with wanting to go somewhere else…

DESSAUER: Oh, yes. […] I applied to [the University of] Minnesota and to Wisconsin, Wisconsin because I had rather liked my pre-visit there, and Minnesota because my friend Ed Heubel, who was by that time a political science major, <**T: 50 min**> had also gone to Minnesota. I thought it would be nice to have somebody with whom I was friends. I found I'd hardly made any friends at Chicago. I mean it was—everybody was competing against each other.

I [was] accepted [by] the University of Wisconsin and thought that was great. In September of 1949, I arrived in Madison and began my career there. Now, in Madison at that time, the big problem was housing. They did not have enough facilities for undergraduates and they surely didn't have anything for graduate students. Actually I wound up in an Air Force

base, Camp Truax [Truax Field Air National Guard Base]. I don't know if you remember that at all. That was about [six] miles from Madison.

DOMUSH: Oh, wow.

DESSAUER: So we had buses that would take you there in the morning into Madison and in the evening out to Truax. I spent a year doing that. Then the remaining two years, I actually had an apartment with a couple of graduate students. I found the atmosphere in Madison was totally different from Chicago. […] You could talk to the professors. There weren't quite as many [Chinese graduate] students who were marking your papers, trying to explain chemistry in poor English. It was a nicer atmosphere, and the campus, of course, was much, much nicer. I've been back to Chicago a couple of times, and actually, fifty, sixty years later, it looks quite nice, but at the time it wasn't.

So anyway that gets me up to 1949. Can we stop for a second?

DOMUSH: Yeah. Let's take a break for a second.

[recording paused]

HUNTER-LASCOSKIE: Okay. We're back on.

DESSAUER: My brother had a friend…

DOMUSH: Sorry, you said this was about 1940…

DESSAUER: 1939, 1940. My brother had a friend whose aunt was what would now be called a do-gooder. She was a lady of considerable wealth, who really meant to do well. Her name was Lottie [Charlotte] Abrams. She belonged to the Ethical Cultural Society in New York [New York Society for Ethical Culture]. Have you ever heard of the Ethical Cultural...?

DOMUSH: No.

DESSAUER: Ethical Cultural Society is—I think it's probably a heavily Jewish group that has an excellent lower school on Central Park West on [64th Street] and an excellent high school in

Riverdale in the Bronx [New York]. They had programs to help refugee children. I was a refugee child then. One of the things they had was an art class. Every Friday afternoon for the better part of a year, I would go down to the Ethical Cultural Society art class, and there was a reward for good attendance. They managed to get us memberships in the Museum of Modern Art.

Actually in my case, they [arranged to send] a bunch of refugee children [on vacation] in Vermont. It turned out there was, at that time, a lady named Dorothy Canfield Fisher, who was one of the founders of the Book of the Month Club, and she was very well off. She lived in Arlington, Vermont, and organized some of her neighbors to take in refugee children. She went to the company that published Hitler's *Mein Kampf*, and asked them for the money.[5] They said, well, the money was blocked. So that financed my summer of 1940 in Vermont. I spent two months with a congregational minister who was terribly poor, but he was a very nice man. I mean he took us, took me and another boy, not because of the love affair of teaching children, but because he got fifteen dollars every other week for each of us. So we were housed in Vermont.

But what was particularly good for me <**T: 55 min**> was that Dorothy Fisher invited me one time. She said—well, she had a couple of adult guests who would like to meet us children. There was a professor from an English university, who was also a refugee. He and his wife took a liking to me, and […] asked me, "What do you like to do?"

Well, "I like to read." The parsonage had a nice little library.

So they said, "What are you reading?" I said—well, at that time I was reading, I think, [Emil Ludwig's] *Napoleon*, which was—at that time had been a best seller.[6] He said, "Well, I tell you what. You read a book a week, and come over for coffee and cake, and then we'll discuss it." So for about six weeks, I had my private book review sessions with a professor, and that was great. I mean, you know, these people had time for me. My parents didn't have time. My mother was working. My father was working, or was studying. So it was sort of pretty nice. I failed to mention, my mother worked first as a cleaning woman, and then worked in a factory until my father started working again.

But anyway, the other good thing was that there was a local painter. He took an interest in me, and took me painting. When I came back from Vermont, here I had my membership in the Museum of Modern Art. I thought, "Oh, boy. This is great." The Museum of Modern Art in those days on Saturday afternoon very often showed old movies. It was cheaper for me to go to the old movies at the MoMA than to go to the neighborhood movie house, so I would walk

[5] Adolf Hitler, *Mein Kampf: Complete and Unabridged, Fully Annotated*. Translated by Alvin Johnson. (New York: Reynal & Hitchcock, 1940). Houghton Mifflin Company leased its rights to Hitler's manifesto to Reynal & Hitchcock from 1939 to 1942. Reynal & Hitchcock then donated profits from the book to the Children's Crusade for Children, an organization led by Dorothy Canfield Fisher that enabled American youth to provide aid to European refugee children.

[6] Emil Ludwig, *Napoleon* (New York: Boni and Liveright, 1926).

down there on Saturdays or Sundays from Washington Heights [Manhattan], which was at 159th Street, where we lived, to 53rd Street where the Museum of Modern Art was. This pass never expired. […] I never had to buy one, [but] then got a pass [in 1952 when I came to DuPont].

Well, this had two interesting [consequences]. One was that I got very interested in modern art, and then at one point I bought a Hopper—Edward Hopper—watercolor, which I sold for an outrageous price, and instead bought this painting here, which was commissioned by NASA [National Aeronautics and Space Administration], and it's a space painting.[7] I'm still a member of the Museum of Modern Art, and I still like it, although I think it's too crowded now when you go there.

Anyway, I think we…. Back to Wisconsin

DOMUSH: Right.

DESSAUER: Well at that time…

DOMUSH: Sorry, let me ask one question real quickly before we get to Wisconsin. I know when you were in Chicago it was very busy. It was very competitive. Did you get to do any of the—you know, did you ever get to go to the museums, or anything like that?

DESSAUER: I went a couple of times to the Rosenwald Museum—the science museum—[Museum of Science and Industry], and I went to the Art Institute [of Chicago]. But […] I was limited by finances, transportation, and time. And generally, on occasions, my parents would pay for me, to take me back to New York. So I really think I lived a very monastic life in Chicago. In my last quarter there, I lived at International House, which was very much nicer. At that time, I almost enjoyed it, but I just found the whole experience too stressful.

I've been to—oh, I went to my fiftieth reunion. Obviously, once you go to a reunion they send someone to liberate you from your assets. They were very disappointed in me, because I said, "No, I [didn't] really like it." I said, "I'm going to donate some money, that's not the issue. But I found it was not, in my opinion, an experience conducive to developing a young person's scientific interest," and so on. It may be totally different now, but I didn't like it. I mean it was a very prestigious place, and […] <**T: 60 min**> you know, you could rub elbows with people who were Nobel Prize winners and all that kind of thing. But they weren't terribly interested in us, they were interested in other Nobel Prize winners. I would say that knowing what I do now, I probably would have done the same thing, but I'd probably feel the same way afterwards.

[7] Yeffe Kimball, *Asteroid*, 1958. Acrylic.

So anyway, we were in Madison. At that time the procedure was to give you a placement exam. I must say I was overconfident. I didn't do too well on the placement exam, which meant I had to take it again. The next time, I did very much better. With certain animosities—one of the professors at Wisconsin was, a fairly famous one, was named Sam [Samuel M.] McElvain. And McElvain had written a book on identifying organic compounds, which, you know, [was for] an analytical course.[8] I had taken an analytical course in Chicago, where they use a different book. The book they used was Shriner and Fuson, who were from the University of Illinois.[9] So obviously, you came with that [to] Wisconsin, you had to take the same course again, using McElvain's book. I thought that was childish.

It was an easy course to have, and I think I finished all of the lab work in two weeks and wondered where—when he came to the lab, and said, "Where's Dessauer?" I was off reading somewhere. They said, "Well, what happened to your unknowns?"

I said, "They're all done."

He said, "How come?"

I didn't want to admit it. [By chance], he gave me the same unknowns that I had in Chicago! Anyway, that was asked. But in general I thought the professors were quite nice.

I had—well, and then after about six months, we're supposed to select a major professor to do your graduate work. I met up with a [professor] named Al [Alfred L.] Wilds, who was a steroid chemist. He had been famous for being one of a group of three chemists who had made the first aromatic steroid, equilenin, [by a] total synthesis.[10] Nineteen forty-nine was sort of when cortisone and all these drugs became very popular. So there was a lot of money for research at the universities in that kind of chemistry. Al Wilds had a group of about fifteen people who were working on a total synthesis of steroids. […] Another professor, named William [S.] Johnson, [had a group] working in a similar area. […] [Somebody], who would make the first total synthesis of a nonaromatic steroid, […] was R.B. [Robert Burns] Woodward, who was one of the greatest chemists that the United States ever produced. We were not quite as good as he and his hundreds of students and postdocs who were in there doing everything in their field of synthetic organic chemistry there. As a matter of fact, I stumbled on—reading about Woodward the other day. He was quite a remarkable guy.

Well, I would say the pace at Wisconsin was considerably more relaxed. Some of the people who taught—did you know Aaron [J.] Ihde at all?

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[8] S. McElvain, *The Characterization of Organic Compounds* (New York: Macmillan Publishing, 1945).
[9] R. L. Shriner and R. C. Fuson, The *Systematic Identification of Organic* Compounds (New York: John Wiley, 1935).
[10] W. E. Bachmann, W. Cole, A. L. Wilds, "The Total Synthesis of the Sex Hormone Equilenin and Its Stereoisomers," *Journal of the American Chemical Society* 62 (1940): 824.

DOMUSH: I know the name, that's all.

DESSAUER: He taught history of chemistry, and I had him, and I had Ed [Edward L.] King, and Gene [Eugene] van Tamelen. I became pretty good friends with […] van Tamelen, who was a young professor, who ultimately left for Stanford [University], and […] who died about two years ago. In his first year, van Tamelen sort of hobnobbed with all of the graduate students, because <**T: 65 min**> they were more like him than he was like the senior staff. Then in his second year there, he had gotten married, [to an] attractive young woman named Mary [Houtman van Tamelen], who was an ardent bridge player. She and I played an enormous amount of bridge while Gene could work in the laboratory. […] On one occasion, he complimented me and said, "You know, you were very important in my success as a chemist, because Mary always wanted to play bridge, and she'd just as soon let me work in the lab." So we stayed pretty good friends for a long time, but I guess we've lost touch [later].

My research was—I don't think it was anything extraordinary. It was studying reactions to build up the ring system. The problem with steroid chemistry is that if you do a total synthesis, it's probably somewhere on there with fifteen to thirty steps. Since each step does not go in one hundred percent yield, you have to start with many pounds of whatever is first, or second, or third compound, so that you have a few milligrams at the very end. So we were studying reactions and optimizing conditions that would lead to the desired higher quantity—significant quantities of materials. I don't think I made any major mistakes or major advances, but I managed to have a very good relationship with Al Wilds.

I think the most significant thing that happened in Madison when I was there was—they had a student union, which showed good movies. One time they showed a movie—I remember it was *Rembrandt*, with Charles Laughton.[11] I wanted to see that, and I thought, well, I could go after lunchtime.

I [came] back from the movie, and the whole chemistry department [had] fire trucks and everything parked outside of it. Somebody came and said, "Oh, God, we're glad you're alive."

I said, "What's the matter?"

They said, "Oh, don't you know?" They said, "Well, there was a big explosion and it was in the lab that you and [Robert C.] Doban normally have lunch at."

DOMUSH: Oh, no.

[11] *Rembrandt*, directed by Alexander Korda. (London: London Film Productions, 1936).

DESSAUER: It turned out my fellow student, Bob Doban, who was a very good friend for a long time, had an explosion. It [probably was] the—up to that point—the biggest explosion in the history of the chemistry department. You know, trying to make a sodium amalgam and, of course, in the bottle—the way they made it in those days was, we rolled sodium and mercury in a large glass jar, and then cooled the reaction off in ice buckets, and…

DOMUSH: This doesn't sound very safe.

DESSAUER: No. The ice bucket […] became a flat sheet of steel. Anyway, Doban was laid up at the hospital for six weeks. He decided at that point, he wanted to go into management and not be a bench chemist for too long. He finally came to DuPont. We came to DuPont at the same time. He became a management type and ultimately did quite well; perhaps the explosion gave him some orientation. Well anyway, I…

DOMUSH: Now did you have any explosions? I know not on that scale, obviously, but any…

DESSAUER: No. I was fairly careful. The one thing which was sort of interesting to me…we made a compound which one had to isolate. It seemed to be light-sensitive, and that was my first connection with light-sensitive materials. It was a nice solid when you filtered it off, and then a few minutes later, or hour later, it became an oil. We decided, well, that was something you had to work on at night. So I spent a lot of time at night. But we always played bridge in the lab at night with Mary van Tamelen, so it wasn't so bad.

So anyway, I would say that, in general, my chemical <**T: 70 min**> career at Wisconsin was fairly safe. I know—I met a number of people with whom I remained friends for a long time. [I shared] an apartment with three chemists on Langdon Street, which was owned by a man named Rolf Darbo, who, because we had the same first name, always treated me remarkably [nicely] for a landlord in a university. My three roommates [Bob (Robert J.) Highet, Roy (E.) Starn, and Edgar (W.) Warnhoff] all came to Wisconsin at the same time, but they were a little more leisurely about getting their degree. I spent three years, and they spent four.

I knew that my major professor, Al Wilds, was a man who hated to see his students leave. I mean, […] when you're a graduate student, first year you're worthless, second year you're a little better, the third year you're pretty good, and the fourth year, if you leave, he's lost all those skilled hands, and I knew that, so about a half a year before I thought I ought to get out, I said, "My parents [have] very generously offered to let me go on a vacation, but I have to make reservations." I was going to go to Bermuda with a friend. So I said, "Do you think I could be out of here by September 1st, [1952]?"

He said, "No problem, unless you run into some roadblocks with your thesis I can't imagine."

But I said, "Good, then I'll make reservations."

He said, "Sure, go right ahead."

Well, about July, I started disappearing from the lab, and went to the library and worked on my thesis. I walked into Al, and he said, "Rolf, haven't seen you in the lab much."

I said, "Well, I'm working on my thesis."

He said, "Are you finished?"

Well, I said, "Remember we had this conversation about me going to Bermuda?"

"Oh, yeah, everybody should have a vacation. Did I say you would be finished?"

"Yes, you did and I made my reservations."

He said, "Well, I guess we'll have to arrange it for you to have your oral exams before then."

I said, "Fine."

So I departed from Madison on—sometime in August, late August, I think August 20th, 1952. [On my] interview trip, I had visited the Jackson Lab of DuPont [DuPont Jackson Laboratory]. My interview trip was in March of 1952. They were working on dyes at that location, and that was something I really was interested in. So I accepted the job offer, and I think I appeared in Wilmington and was paid somewhere around the order of about four hundred sixty dollars a month; that was the going wage for Ph.D. entering chemists at DuPont.

It was interesting, I mentioned my friend, Bob Doban, [who] had the big explosion. He had also decided to come to DuPont. DuPont at that time had a discriminatory policy. If you were married, they moved you. If you were single, they didn't, so I joined the Doban household, and we moved together. I lived in a rented room for six months until I had a car. Then moved into an apartment in Kynlyn [Wilmington]. Anyway, my career at DuPont had started.

DOMUSH: Now, before you applied to DuPont, did you apply to other companies as well?

DESSAUER: Well, the way it worked at that time was that, at a university like Wisconsin, companies would come and [post] a sign saying, "The following companies will be in on such and such a day." You'd meet with one of their headhunters or whatever you want to call them. Then they would tell you how wonderful their company was. They would try to see if you […]

[wanted] to be invited on an interview trip. Well it was sort of standard that you'd get invited, if you wanted, to three or four companies. If you were really a prospective genius or something, you might get more. My friend, Bob Doban, despite his explosion, I think was invited to companies all over the country. I was invited to Union Carbide [Corporation] in Tonawanda [NewYork], which was the Linde Division <**T: 75 min**> then. I was invited to American Cyanamid [Company], and Sinclair Oil [Corporation], and DuPont [at] several locations. I also visited [Pfizer Inc.]. I didn't get a job offer from the pharmaceutical companies, and I really didn't want to live in Tonawanda, New York. I didn't want to live wherever Sinclair was, not too far from Chicago. DuPont…

DOMUSH: Did any of the oil companies down in Texas send representatives up?

DESSAUER: Not to me. No.

DOMUSH: Yeah.

DESSAUER: I, actually, I felt at that time, Jackson Lab was a very impressive organization, [for] what they were doing in many different fields. The man who was responsible for me to be hired [and I didn't] get along that well after I was hired. His name was Madison Hunt. He said, "Boy, I've never seen anybody's eyes sparkle like yours when it came to synthetic chemistry." I would say I had a good interview with DuPont in Wilmington.

They also had me go down to Richmond, Virginia, to visit the Spruance Plant. That was not a big success, mostly because the flight from New York to Richmond was cancelled. I had to catch a midnight train. I arrived in Richmond at something like 4:30 in the morning, and they were going to pick me up at 7:00 a.m. for breakfast. I was sort of tired that day, but I don't think I would have wanted to work in Richmond then anyway.

So I was very happy with the Jackson Lab. I knew at least one friend from my days at Wisconsin, and he was there a year before I was, so I knew somebody [there]. My first assignment was to develop a yellow or red pigment with some of the properties of copper phthalocyanine, which I must say nobody has invented yet, but [it] was an interesting program.

The unfortunate thing was that the man who became my first supervisor there had done some experiments and made this compound, which had the wrong color, but it was a ring. Copper phthalocyanine—I mean, you may or may not know—is a heterocyclic, polycyclic compound, which apparently is very flat and forms very stable light-fast, heat-fast, everything-else-fast pigments. The man who had done the most work to elucidate the structure was […] [Reginald Patrick] Linstead, who was one of the great organic chemists in England in the 1920s, 1930s, and 1940s. He was still working in the 1950s in the field.

[…] I started at Jackson Lab in September. In those days, […] there weren't that many technical journals, so if you were an ambitious young chemist, you subscribed to your own copy of the *Journal of the American Chemical Society*, the *Journal of Organic Chemistry*, and *the Journal of the Chemical Society*. So when I got my *Journal of the Chemical Society* for December, on page 5000 was exactly what we were doing.[12]

DOMUSH: Oh, no.

DESSAUER: Only, he [Linstead] was doing it better. […] So that sort of put a kibosh on that problem, pretty much. It was sort of interesting. I always loved that chemistry. It was interesting. I had lots of ideas. I think one of my last [patent] applications, […] when I was consulting, […] about ten years ago when I was consulting for Hewlett Packard [Company], was still with copper phthalocyanine.[13] So they don't go away.

The question was, what to do with Rolf <**T: 80 min**> Dessauer now that he's no longer working on copper-phthalocyanine-like materials? I mean, I really, in a way, faulted my leadership not being up-to-date more on what was going on. As a matter of fact, surprisingly, two doors down from me, was Charlie [Charles J.] Pedersen who was an organic chemist who worked for DuPont; he was a research associate. Later on, I discovered that Charlie had done much of what Linstead had done, but had never published it. […] Charlie was, it turned out, a very good chemist, and […] he won a Nobel Prize [Chemistry, 1987], that confirms that to some extent. I said, "Charlie, why didn't you tell anybody that?" "I couldn't tell anybody I have something like that. They don't let you work on anything else. It gets boring."

So he worked on crown ethers afterwards, [and] did better with that.[14] He had done some interesting work, and I think we could have pulled something out of it, but I didn't know until after the Linstead publication that Charlie Pedersen had been very active in that field, and had done wonderful things. Anyway…

DOMUSH: Was he a nice guy to have right down the hall?

DESSAUER: [Yes], he was. I mean there were a couple of elderly men. There was another man named Viktor Weinmayr who was an Austrian who was also very—sort of a mentor type.

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[12] J. A. Elvidge and R. P. Linstead, "Heterocyclic imines. Part I. Imino-derivatives of isoindoline and their reaction with primary bases," *Journal of the Chemical Society* (1952): 5000.

[13] R. Dessauer. Phthalocyanine precursors in infrared sensitive compositions. U.S. Patent Application No. 20050053864, filed 5 September 2003 and published 10 March 2005.

[14] C. J. Pedersen , "Cyclic polyethers and their complexes with metal salts," *Journal of the American Chemical Society* 89 (1967): 7017–7036; C. J. Pedersen , "Macrocyclic Polyethers:Dibenzo-18-Crown-6 Polyether and Dicyclohexyl-18-Crown-6 Polyether," *Organic Syntheses* 52 (1972): 66-74.

He would like to help you. More importantly, help you avoid making the mistakes he did. In those days, we would get research reviews, like, every three or four months. The management would size you up—whether you're worth keeping, or promoting or firing, or God knows what. After my first review, Viktor put his arm around and said, "Rolf, you know what you should do? You're a good chemist. Learn how to give speeches."

DOMUSH: Well, and as we were talking before we started the interview, I mean…

DESSAUER: Pardon?

DOMUSH: As we were saying before we started the interview, the ability to speak well in front of others has a huge effect on…

DESSAUER: Oh, I think that in those days the ability to speak and to have good visual charts was probably far more important than substance of what you talked about, and…

HUNTER-LASCOSKIE: Did you start giving these talks early on while you were at DuPont?

DESSAUER: Well, in those days I was pretty—well, for a number of years, confined to work in the laboratory. The only talks you gave then were either in your group or [in] your research review. My becoming more involved outside DuPont, where I did give talks, didn't begin for another few years. But anyway, there were nice people in Jackson Lab, and it had a genial atmosphere, and it had things wrong with it. I mean there were a number of things that could have been improved.

Anyway, so by that time DuPont started working on a synthetic elastomer for tires, and it was a fairly sizeable effort. The name of the product was to be Adiprene. Adiprene […] was a polyurethane, and one of the components of it was butanediol. It was decided that in order to make Adiprene at a reasonable price, one had to make butanediol for a much more reasonable price.

[…] Madison Hunt, who was the man who I think I owed my job to, was head of a task force to investigate ways of making butanediol. [He] found some work that had been done at the old—it was then called the Plastics Department, [later] Polychemicals. They had found a way of oxidizing butane to make into <**T: 85 min**> maleic [anhydride]. Then one would reduce

that to butanediol. […] Herb [Herbert F.] McShane, Jr. had found a way of taking butyrolactone, which was one step from maleic anhydride, and reducing that to butanediol.[15]

So we needed a good way of making butanediol. I was told I was going to be the man who would make butanediol in one step from butane. My experience of working with dyes—I'm sorry, with gases—was very, very limited. I think I had used a gas in something in Wisconsin, but it was—I had no idea how to proceed. I was told "Well, you go and see a bunch of engineers and have them build you a reactor. And somebody will give you some catalysts."

I said, "Oh, good, I would like to work on catalysts." […] Homer [B.] Adkins, who was the world's expert on hydrogenation, [had been a professor at Wisconsin just before I began my graduate studies there]. I thought that would be an interesting thing to spend a career on, but I didn't know how to—where to start.

Well, Hunt, too, didn't know much about that either. He said, "You go to the glass shop and tell them to build you a reactor. They can do that fast."

So then I went to an engineer and I said, "How do I do that in a glass?"

So he said, "What does Hunt want you to do?" He said—well— he'd get a Wood's metal bath, which is sort of a bucket with something called Wood's metal, which melts when you heat it. You immerse your glass vessel in it. I remember the engineer, whose name was Joe Quattlebaum, said, "Rolf, you're going to blow us all up."

I said, "Okay. We cannot use glass." So we used copper pipes. So they built me a bunch of reactors and I spent months getting this set up in my hood trying to pump through these reactors butane gas, and [oxidize] it, and making anything that was other than what I put in.

Well at that time, DuPont [had what was] called the [Chemical Department]. The [Chemical Department, renamed the Central Research Department in 1957], was sort of the shining bastion of synthetic chemistry and inventive new chemistry. Of course, they had made catalysts for everything. All of a sudden my laboratory bench was filled with bottle after bottle of catalysts from the [Chemical Department]. They would bring over thirty or forty catalysts a week. I mean it would take me—I could maybe evaluate one, two catalysts a day and I wanted to make my own catalysts. Yeah. I was told, "You better get those from [Chemical Department] first." Well the whole thing was a disaster. We couldn't even oxidize the butane along the line of the previous Polychemicals Department report.

So finally, Hunt went down to Texas where the inventor of this process was and talked to him. He agreed he would come to Wilmington, and make the catalyst for us. So we had a

[15] H. F. McShane, Jr. and R. B. Ramsey, Jr. Hydrogenation of Maleic Anhdride. U.S. Patent No. 2,772,292, filed on 13 July 1953 and issued 27 November 1956.

couple of […] five-gallon, round-bottom flasks set up, and all of the chemicals laid out, and he mixed them, and he said, "That's not the way it worked back home." So it turned out that catalyst study was a total disaster. Then it turned out my friend, Herb McShane, who had reduced maleic anhydride to butyrolactone, or maybe it was butyrylactone to butanediol, the catalyst he used cost more than the end product. And [the catalyst was] consumed as it went on. So this whole thing was a total disaster. There was one point—and one more thing that was important in this whole episode. Do you know what a fluid bed is?

DOMUSH: No.

DESSAUER: Fluid bed catalyst? Well, a fluid bed is when you […] treated solid particles as though they are liquid. So we made very tiny, maybe almost like nanochemistry now, but you <**T: 90 min**> made very, very finely ground catalysts. They were ground so [small] that if you put them in a metal tube, or any kind of tube, you blew air from the bottom and it acts just as if you have liquid rather than solid particles. So we had built a very fancy device which was to be used with a catalyst that either I was going to invent, or had been invented elsewhere. [We never obtained the yields that had been reported].

By that time we had a group of maybe twenty people working on this, it was no small operation. We had this enormous tube, […] two-stories high, and had thermocouples all along so you could measure temperature. [We] bubbled up gas and isolated a product. Well one fine day, something went wrong with the thermocouples. The stuff in the tube melted in the middle, and it became unusable. Well they terminated the project because the company didn't want to spend another one hundred twenty-five thousand dollars and six months to build it. The chemist under whose watch this occurred was […] Howard [C.] Smith, a very nice man. We were good friends. But he got promoted after that from one thing to the other. So one of the things I learned [was that] if you had failures, it doesn't kill you. Howard has been dead for five or ten years now, unfortunately.

Anyway, the butane project got killed. [We concluded that the engineer who had done the early work had not adequately calibrated his equipment!] It turned out Adiprene never became a rubber tire and it was subsequently referred to as our tireless effort. Well, I had, in my quest to evaluate all these catalysts, I'd gotten on Hunt's nerves because, first of all, I didn't get started until I had my equipment calibrated. Then I asked for more technicians, and normally in those days, you had one-half a technician per chemist. […] I don't think it was my job to stand in the hood changing temperature [of the catalyst reactors]. I figured you have a technician to do that. My job was to learn how to make good catalysts. Anyway, I did not cover myself with glory on a project that was inglorious, to put it mildly.

So the question was, what comes next? I was in the General Organic Division, and I guess I got either demoted or transferred to another division then, sort of like a baseball team. You take two guys from here, and give you one guy from there. I was assigned to a project called Developed Dyes. This was really interesting and was…

DOMUSH: Now this is still at Jackson Lab?

DESSAUER: In Jackson Lab, [yes]. We're now up to about 1954. The problem with dyeing is that, in those days, the major fiber, of course, was cotton. When you dye things, you're trying to make sure that the dye sticks to the fiber. There are several ways of doing it. You either make the dye reactive, which is now a way that it's done. Or it was a way of solubilizing the dye, impregnating the fabric, then converting the dye—the soluble form of the dye—back into the dye. That is, among other things, vat dyeing and so on. So people were inevitably looking at new ways of dyeing things.

In Jackson Lab [we] had developed a system of making copper phthalocyanine, which was a pigment, into a soluble version, which […] [one] could use to impregnate cotton, then [treat with reducing agents and convert] into copper phthalocyanine.[16] [This yielded] beautiful blue and green colors; […] <**T: 95 min**> however, [there was no] yellow or red to go with it. […] You cannot go into the dye business with just one color. Of course, this was something […] pretty close to what Linstead had done in England, when he sort of scooped us on what we were doing, but at that time I didn't even know that in Jackson Lab they had developed these phthalocyanines colors for blues and greens.

Anyway, my assignment now was to come up with something that could be used under similar conditions, developed either thermally or with a reducing agent, and so on, preferably thermally. This had been the baby that a very talented [chemist], Herman [E.] Schroeder, who was a director, [had proposed]. Well, he was not a director when he had proposed it, but became a director. […] Herman Schroeder was a product of Harvard, and a very creative man. He had proposed this program, and […] I was the eighth or ninth chemist who had worked on it. It was really interesting. One of the things that had been developed was that you'd mix tetracarboxy napthalene with ortho-phenylenediamine. […] Together, [the components] are soluble. You impregnate the fabric with that. Then you heat it up and get a nice red piece of fabric with a very, very light-fast dye. So that was interesting chemistry. I really enjoyed it.

I had a new supervisor named Mason [Hayek]. [Mason], I didn't know until later, was colorblind, which was not exactly what you want for a dye chemist. But anyway, I was sort of stuck with having to report to him. Well, Mason was a nice person. We went to lunch together and we discussed the world of art and politics and so on, everything was fine. Then my opportunity to give a research review came up. So Mason said, "You know this program has gone on for a very long time. We're not commercial, and this can't go on forever."

I said, "Well, what do we do?"

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[16] R. A. Brooks, J. G. Burt, B. F. Skiles and M. S. Whelen, "Chemistry of Copper Phthalocyanine Precursos," *Journal of Organic Chemistry* 24 (1959): 383-5.

He said, “Well, I want you to go out and find out whether this is ever going to be commercial.”

So I said, “Where do I go?”

He said, “Well, first stop is Tech Lab.” Tech Lab [Technical Laboratory], as I mentioned earlier, was in the Chambers Works and it was a place [where] the company evaluated [dyes] and competitive products. So I went over there with my samples of impregnated fabrics. Then, the method we used to develop the color was very [pedestrian]. We had [steam irons], and we took the fabric and [ironed it]. Well, the problem with this was that no matter how hard you try, it’s not uniform. So the first thing my friends in Tech Lab said, “It’ll never sell, because it’s not uniform.”

I said, “What do you have [to do] to make it uniform?”

They said, “Well, it [should] have heating systems that provide a uniform surface area. But if you do that, that’s expensive, and the dye industry is not going to spend that money.” Then there were a couple of other things.

Anyway, the conclusion was: this was not going to be very commercial. So I said to Mason <**T: 100 min**>, “Well, it’s not very good news.”

He said, “Well, we’ve got to tell it as it is.” So I [gave] my research review, and [got up to] tell everybody that what they’d been working on for years was not going to be a commercial success. Well, there wasn’t much applause.

So I sat down, and got back to my lab, and […] Mel [Melvin A.] Perkins, who was [Mason’s] boss, called me to his office. He said, “What do you want to work on now?”

I said, “Oh, […] I have some ideas on how to do things in this field.”

He said, “Well, you killed that project. You know, you killed it.”

I said, “Did I really?”

He said, “Yeah. I think that’s pretty clear that the project—they’re not very happy with you.”

So I said, “Well, I told Mason—Mason told me—I rehearsed my talk […]”

He said, “Well, let’s call in Mason.” So he called in Mason. […] Perkins was an elderly man, he was close to retirement. He said to Mason, “Did you rehearse Rolf?”

He said, “Yeah. Yes, I did.”

He said, “Did you tell him to kill it?”

“Oh, no, I didn’t tell him to kill that.”

I said, “What do you mean?”

He said, “Well, it was Rolf’s conclusion.”

I said, “Well, my conclusion, I checked it with you, and it apparently wasn’t a good idea to conclude this.” Well I was close to tears. I mean, I said, “You know I was set up.”

Anyway […] Perkins said, “You know we hired you as a synthetic chemist, not as a market evaluator. Didn’t you do anything good?”

I said, “[Yes]. I have a notebook full of stuff that I did, which I was going to talk about, but Mason said it’s most important to establish the commercializeability of this project.”

So he said, “Let me see your notebooks.” So he took my notebooks, and he called me up a day later and said, “I wish you’d talked about that. We would be in this business still.”

So [I was] still very unhappy, and I got a phone call from the laboratory director: “Well, I think we’re going to put you on probation. We’re going to see what develops. But you’ve got to remember that there are people who have worked in this field for years, and they don’t want to be told by somebody who is a new chemist what they should have supported or killed.”

So I said, “Well, I told you, I had shown Mel Perkins my notebooks, and he thought I was very good. I rest my case. I can’t help it.” I mean, I was devastated.

But they gave me another project, and I didn’t have that many interesting projects. Then I was transferred to another group, also in dyes. […] In that group, we were developing dyes for the new fibers, Orlon and Dacron. That was sort of interesting. Orlon had been designed as a fabric for a variety of uses, but had not been dyeable with very bright colors. People were wondering why it couldn’t be dyed with bright colors. So one of my friends, who was also from the University of Chicago, named Bill [William R.] Remington had made some inventions by which Orlon became dyeable with very bright colors. It had previously been [considered a failure because it could not be dyed in bright colors […] like rhodamine and triarylmethane dyes and so on. All of a sudden, Orlon became popular in blankets and in socks, and sweaters because [one] could dye it with these beautiful bright colors.

So the next <**T: 105 min**> question was, why can’t we do the same thing with Dacron? So we started working with the Textile Fibers Department. They were trying to modify Dacron to make it dyeable with basic dyes. The very same dyes that worked with Orlon were absolutely worthless on Dacron. It was a different polymer, and there were all sorts of photochemical

reasons why it wouldn't work, but we didn't know that. […] [We] had a pretty good relationship [with Textile Fibers Department].

Then one of the chemists at DuPont's Kinston [North Carolina] Laboratory, […] named Sidney [B.] Maerov had used some materials, which were called UV absorbers. UV absorbers were chemicals that absorbed light and converted it to heat, and so protected fabrics, and plastics, and so on. We found out that this might be a way to improve the quality of our dyeings, to take these very [poorly] light-fast materials and make them light-fast. Then, since I was a synthetic chemist, they suggested I work on that aspect of it. That was my real introduction to photochemistry.

At that point, my reputation was somewhat better established. We invented some UV absorbers that were patentable.[17] I organized a symposium on light—fabric protection, which attracted about sixty or seventy people from all over DuPont to Jackson Lab. I was having a good time. I found out that the […] Plastics Department and Film Department would use my UV absorbers, [and] later on, what was F&F, Fabrics and Finishes [Department], used our UV absorbers. We found that we had made something that was in an Eastman [Kodak Company] patent, but Eastman had not patented properly, so our material became commercial.[18] It was known as Rylex D, and it was my first commercial product.

Well…

DOMUSH: So but Eastman actually—they had submitted a patent, and done so incorrectly.

DESSAUER: [Yes]. They had limited themselves to the wrong structure.

DOMUSH: Oh, wow.

DESSAUER: It was sort of funny. We had a patent chemist who saw through the patent, […] so it was really [exciting], because it got me out of dealing [solely] with textile fiber people, and everybody was interested in UV absorbers. We could tailor them to certain applications.

There was one thing that happened on the way, which, again, provoked a certain amount of controversy. In those days our reaction vessels for making dyes were little glass jars, sort of like that, […] [rounded at the bottom], with stirrers. […] [They were] closed at the top, and you

[17] J. B. Armitage, R. Dessauer, and A. M. Hyson. 2-Hydroxy-4-n-dodecyloxybenzophenone. U.S. Patent 3,006,959, filed 22 September 1958, and issued 31 Oct 1961.
[18] G. R. Lappin and J. W. Tamblyn. Polyolefin compositions containing 2-hydroxy-4-alkoxybenzophenones. U.S. Patent 2,861,053, filed 7 November 1957, and issued 18 November 1958.

could heat [these] with [heating mantles] and so on. And our stirrers were made out of Teflon. […]

Well, I suddenly discovered with the dyes I was making, the Teflon [stirrers became] colored. Now Teflon had a reputation of being stain-free. Here I was [making] red Teflon, blue Teflon, yellow Teflon. I thought that was really neat. I called up the people who made Teflon, and they were very interested. So while it wasn't a full-time thing—but my leader, I think it was Howard Smith at the time, said, "Rolf, you better write a report about this. It's very interesting."

Well, the report had been over to tech lab, who were not overly friendly to us, and they probably remembered the experience earlier. There were some derogatory comments like, "Crazy, mixed-up kid. What is he going to dye next?" Well somebody sent me a report that had gone up the—from up to down—and everybody <**T: 110 min**> had made a more derogatory comment about my great invention.

So I took the report and I stapled three pieces of colored Teflon to the report and sent it back to all of the people. Back then—we didn't have color copiers. […] I said, "Well, just because Tech Lab can't do it, doesn't mean it can't be done." That didn't get over too big. I kind of had quite a controversy with the director of Tech Lab, which was ultimately resolved by him sort of semi-apologizing. Later on we became reasonably good friends. But the colored Teflon was another one of my great experiences.

DOMUSH: Did anything come of that? The colored Teflon?

DESSAUER: No, no, [it] didn't. That's unfortunate. One thing I learned at DuPont is when you invent something new that people hadn't expected, it's an enormous fight to push it on. I mean, if somebody says, "Let's make nylon cheaper," that's easy. But you come up with something that is totally different, as you'll see from my Dylux days—we've got a long way to go.

So now the next step was—we got to my then leader, Howard Smith, sort of recognized this. One of my enthusiasms was to try to talk other people into doing things with dyes and UV absorbers. He said, "Well, I think you have a new project now. It's called New Dye Uses."

I said, "What does that mean?"

He said, "It's whatever you want it to mean." Howard was a nice guy, and really bright.

So I said, "Does it mean I have to use new dyes or find new things to do with them?"

He said, "Whatever you want to do, just do it."

So one of the first things that happened was anodized aluminum; [it] was becoming more useful. Anodized aluminum had been dyed with [three dyes]. One was a turquoise […] dye, and a black dye. But if you wanted a red, or a green, or a really good blue that wasn't so possible.

In those days, DuPont had what were called development conferences. [We would] invite a company to come in with a host of technical problems. They'd meet [downtown] at the DuPont de Nemours Building. We'd have a nice lunch, and breakfast and dinner sometimes. They'd bring in some technical people and they'd say, "We would like to make red-colored anodized aluminum," or something else. I became one of the people from Orchem [Organic Chemicals Department] who would attend these and see if we had anything that would meet their requirements. Alcoa [Inc.] impressed us as being someone with whom we could work.

We developed a program of […] developing dyes for anodized aluminums. I got a couple of patents out of that.[19] Some of these patents went to Alcoa. I [had] a very nice relationship. They found out that […] I was [good and I was conscientious]. When I promised people something, they would get it.

I remember I had one experience where we were dealing with a company in New York called Sinclair and Valentine [Ink Company LP]. They had been interested in developing [colored] anodized aluminum, and printing [on] it. They had found one dye that DuPont had commercially available, and they wanted more, but they wanted it in a pure form. They asked how long it would take. So somebody said, "Well, it'll take a month or so." They gave me the project.

Well, it took us three or four days to do what they wanted. In those days, I would spend weekends in New York because my parents were still alive, so I figured I'd take off early from Jackson Lab in New Jersey, [and drop off the dye] at Sinclair and Valentine. They'd be happy to have their dye. Well that coincided with an ACS [American Chemical Society] meeting in San Francisco [California], and I went from New York to San Francisco. I didn't show up in Wilmington for two weeks.

I came back, all hell breaks loose: "Dessauer! What have you done now?"

I said, "What have I done now?"

[One of the sales directors] said, "Yeah, we got some money from Sinclair and Valentine. We don't know what it's for." They were so impressed with the speed with which we delivered that they sent DuPont a check and nobody <**T: 115 min**> knew what to do with it.

[19] R. Dessauer. Process of coloring anodized aluminum. U.S. Patent 3,019,143, filed 21 October 1959, and issued 30 January 1962; R. Dessauer. Coloring of oxide-coated aluminum. U.S. Patent 3,108,026, filed 10 October 1960, and issued 22 October 1963; R. A. Brooks and R. Dessauer. Process for coloring anodized aluminum. U.S. Patent 3,110,616, filed 5 April 1961, and issued 12 November 1963

So I said, "Well, you know, I was told to be customer-friendly." Well, not that customer-friendly." So I got into that hot water.

Then I had another situation where my friend, Ed Heubel had gotten a professorship at the University of Buenos Aires. […]

DOMUSH: And he was in political science, you said.

DESSAUER: He was a political scientist. He and his wife liked Buenos Aires [Argentina], and they invited me to come down and visit them. Well, for some reason or other, you had to tell DuPont [where] you were going on any foreign trips or something like that. I [received] a phone call from somebody in the medical department, "Would you take some […] vitamins to DuPont of Argentina—for […] President [Jorge Luis] Aguilar [of] Ducilo [Sociedad Anonomia Industrial y Comercial]?"

I said, "Sure, be glad to." So I flew down. I [called up Aguilar at] DuPont, and I said I had some ascorbic acids and some other things.

So he said, "Well, why don't we have lunch?"

I thought, "That's nice. Thank you."

So there's a luncheon of the Rotary Club of Buenos Aires [at] a very nice hotel. So he said to me, "Well, is there anything I can do for you?"

I said, "Well, I'd like to see the plant."

He said, "You want to see the plant on your vacation?" I said [yes]. He [said], "Okay. We'll go to the plant." So we went. He introduced me to people there saying; "This crazy American wants to visit our factory on his vacation."

So I—anyway, he [took] me to the plant and showed me around. Well, they have a problem with dyes. So I said, "Well, okay. Well, gee whiz, I know how to solve that problem." One of their problems was they were making a different kind of nylon, and sometimes it came off [discolored], and not as white as it ought to be. So they wanted to dye combs and things like that. I knew exactly what dyes to use. So I said, "Oh, that's fine."

They said, "Well, would you like to come and teach us about dye chemistry?"

I said, "Sure. So for two days they picked me up in a limo, and drove me out to [the] Ducilo Plant. I gave some lectures on dye chemistry, and everybody was very nice, they invited me to meals. And they asked me if I could stay longer. So I sent a telegram back to Wilmington. I said, "I'm very busy teaching dye chemistry at Ducilo."

HUNTER-LASCOSKIE: On your vacation.

DESSAUER: On my vacation. My then-boss was reasonably tolerant. Everybody thought it was great, except the dye sales people said, "If they want to know something about dyes, we have people down there."

I said, "Well apparently they're not very available, because why would they invite me to come?" Anyway, again a black mark somewhere. Anyway, my enthusiasm always ran high for these things that were a little unusual. Well, anyway, that was 1957.

Well, I'm making fairly good progress with dye chemistry, and finding new things to do with them. We made new dyes. We sold them to Alcoa. I was having a fairly nice time and my reputation was all right. I wasn't worried about getting fired anymore, and…

DOMUSH: Now at this point, are you moving along and getting promoted or are you—because you're talking about your reputation, and I'm not exactly sure.

DESSAUER: No, I was […] still a research chemist, and to a lot of people a pain in the neck, because I was always causing trouble, because everybody had to explain what I was doing…

DOMUSH: But it's the enthusiasm.

DESSAUER: [Yes]. Well, […] in 1956, the company had decided every operating department would have a laboratory at the [DuPont] Experimental Station, including the people from Jackson Lab, had to split off a group, send them to the Station, and for a lot of people that was an advantage over having to go across the Delaware Memorial Bridge twice every day. I wasn't all that keen on it, because I had a nice lab. I had a couple of very good technicians by that time. I was having a great opportunity to travel and visit people in different locations with my different dyes and so on.

DOMUSH: Now were you living in Wilmington and commuting?

DESSAUER: [Yes]. I was…I was living in Kynlyn [a suburb of Wilmington] at that time. Well, in 1958, summer of 1958, I [received] a phone call from our laboratory director—[he] was named [John Marlin] Tinker. By that time, I'd become very good friends with him <**T: 120 min**>. […] Well, his widow [Adelaide Louise Camerano] lives in Stonegates [Retirement

Community], so we're still friends. They invited me to dinner quite often. So one day he [said], "Rolf, you're going to go to the Experimental Station."

I said, "Why?"

He said, "Well, I think we need somebody who is stimulating there."

I said, "Well, I'm pretty happy here."

He said, "Well, you'll be happier there."

It turned out they had a new summer employee whose name was John [E.] Harriman. He was considered to be real bright, and maybe, if we treated him nicely, he would come to DuPont someday. So I was uprooted to work with John Harriman. We were trying to find something to do that would be interesting and stimulating. […] One of the things that […] was a big business for DuPont was called Butacite. Butacite is the interlayer for automotive glass, so your windshield doesn't shatter […]. We had somehow talked about tinting the window glass, not with pigments, but with dyes […]. It offered some real advantages. I was developing a series [of dyes], I didn't do much synthesis, [and] I liked to purify things. We got to some very pure dyes, metallo-organic dyes that were used […] for dyeing wool and nylon. But it turned out they were very light-fast in Butacite. So I got to talking to the Butacite people a lot. Then they said, "Well, you're a photochemist. Can't you make something that changes color when the sun comes out?" And that intrigued me, because nobody had anything that was terribly good. I mean, this was before the [photochromic] eyeglasses, and so on.

DOMUSH: Right.

DESSAUER: It turned out, actually, there was a lot of interest in what was called photochromism by the military, because they were all worried about [when] an atomic bomb explodes, [the flash of light would turn everybody] blind, So if you had some way of protecting your eyes, this would be good. There appeared in the *Journal of the American Chemical Society* an article about something called N-(3 pyridyl) sydnone.[20] […] A rather unusual compound, which actually turns blue in sunlight, and becomes [colorless], or slightly yellow in the absence of light. So we made some. We thought it might be something that Harriman and I could study together to develop something new.

Well, Harriman never did come to DuPont [as an employee]. He [later] became a professor of physics [at] the University of Wisconsin. Anyway, I googled him once; […] he used to be a very slim, young, handsome guy. He's not quite so slim anymore. But we had a

[20] Jack M. Tien and I. Moyer Hunsberger, "The Preparation of Substituted Hydrazines. II. 3-Pyridylhydrazine via the Phototropic N-(3-Pyridyl)-sydnone," *Journal of the American Chemical Society* 77 (1955): 6604-7.

good time together. [He] was enormously bright. I mean, I would take him to meet some of my friends, and they all thought he was a graduate student, and he was just a junior, I think.

Anyway, we got started working on this photochromic material. Then after Harriman went away, back to Wisconsin, we had another chemist work on it and nothing very exciting happened. Then the project sort of stayed on, and finally [DuPont] hired […] Larry [Lawrence A.] Cescon, who came about 1960. He found a reference in an abstract that DuPont provided us, describing a [photochromic material] developed in Japan, called imidazole derivative.[21] It wasn't really identified by its proper name yet. There was a measurement made about it. <**T: 125 min**> It turned out it sort of was interesting. So Cescon and I were talking about it one day, and I said, "Well, I've got this great technician, Bob [Robert] Jenkins. How many of these alleged photochromic materials do you want to look at?"

He said, "Let's make as many as we can." Well, it was relatively easy, because it involved getting derivatives of benzaldehyde […]. The Jackson Laboratory storeroom had dozens of different benzaldehydes we could use. So we synthesized, I don't know, thirty or forty right off hand.

It turned out that the Japanese workers who had made the first of these materials, but had not identified it, said it's an oxidation product of triphenylimidazole. Well Cescon worked on it and worked on it. We found out it was not—that it was actually a dimer, it was [a material that, when struck by light], would come apart and be colored. In the absence of light, [it] would return to [the] original state.

This sort of intrigued us, because that was the first organic photochromic material that had [a] reasonably long lifetime. What is especially interesting was you could even put these on filter paper, and shine a light on filter paper. It would be colored for quite some time before it discolored. So that meant it was a very stable free radical. Now, free radical chemistry in the United States started with […] Moses Gomberg at the University of Michigan.[22] His free radicals had to be kept inside of a glass tube because, I mean, you took hexaphenylethane, heated it, and it became triphenylmethyl radical. Then it would become hexaphenylethane again. But here [we] had some that would actually persist as a free radical. With all of the materials that Jenkins made and Cescon oxidized, we could get a whole range of colors. We could make red radicals, blue radicals, and so on. So we had something new. We could make an ink out of that. We'd print advertisements that would change color before your eyes. We could put this into windshields, and so on.

Well, this sounded really interesting. So of course, the [man] who managed to get this started [seriously] was my friend, Bill Remington, his reward for that was he was taken off the project. Another man came on, […] George Coraor, who was the nicest person I ever worked

[21] T. Hayashi, K. Maeda, S. Shida, and K. Nakada, "A New Phototropic Substance and its ESR," *Journal of Physical Chemistry* 32 (1960): 1568.
[22] Moses Gomberg, "An Instance of Trivalent Carbon: Triphenylmethyl," *Journal of the American Chemical Society* 22 (1900): 757-71.

for, and one of the most intelligent. […] Anyway, Cescon and I were then joined by […] Ernie [Ernest] Silversmith. Ernie Silversmith actually had a similar background as I did. He also came from Nürnberg, [but left DuPont and became a professor of chemistry at Morgan State University in Baltimore, Maryland].

DOMUSH: Oh.

DESSAUER: While we were […] figuring out how to make these free radicals last longer—and the problem was, while they lasted long compared to everybody else's, they didn't last long enough. The dilemma was [that] the people in the Plastics Department [wanted] this radical to be in automotive windshields. Automotive windshields see an awful lot of photons during their lifetime, so you can't have something that works for a day or a week. You need something that works forever. We couldn't make anything that would work forever.

But, nevertheless, we had a lot of help. We [worked with] people from another department, the Plastics Department, who were the Butacite folks. And they sprang loose two […] talented chemists [Carleton A. Sperati and Jean P. Paris] to work with us to provide us with solvents that would not interfere with the radical life, and so on. […] The best we ever got was [when] I once drove through the Holland Tunnel with a little glass jar of this stuff. It turned blue when I came out and […] colorless when it went in. But the best it ever did it was for about a week, and that's not quite long enough. So we were in somewhat of an unfortunate situation, that it was technically very difficult to do what we wanted to do.

Well, <**T: 130 min**> there was one fortunate thing. Cescon was probably one of the best experimental chemists that ever worked with me, so I said, "Larry, why don't we find out what's going on here, before we give up the ship?" Larry [said] all right. So what happened, we found out that the imidazolyl radical, the colored radical, was a tremendously good oxidizing agent, so [if the radical] found a hydrogen someplace, it would [become] the imidazole.

So I said, "Well, why don't we do something useful with this?" I knew of […] colorless materials that would turn color if they were oxidized. Now this goes back—do you know what spirit duplicating is, or Ditto machine? Remember the Ditto machine? Well, a Ditto machine uses a wax matrix which has a dye [e.g., crystal violet] embedded in it, and [when] you type on it, [the] wax layer breaks. In the Ditto machine, a piece of plain paper is moistened with alcohol. It is dragged over this matrix, and [it transfers] a tiny little bit of color to the white paper. This has been a big business for DuPont because we [manufactured crystal violet]. Crystal violet [was] probably the most popular dye made, [and] DuPont sold millions of dollars' worth.

[…] The weakness of spirit duplicating was that the secretaries didn't like it, because they always got purple fingers and they got their clothes dirty—purple, and so on. So if we had a colorless material in the matrix, and the alcohol contained an oxidizing agent, then it would form color on the paper, but not on the [secretary].

DOMUSH: Right, and not on your fingers.

DESSAUER: Not on fingers. So [DuPont] worked on that. They took it to a number of companies like Ditto [Corporation], and there was a reasonable amount of enthusiasm. Unfortunately, the oxidizing agent that DuPont recommended was called chloranil, which [is] tetrachloro-1,4-benzoquinone. That liberates hydrogen chloride, hydrochloric acid. So you were not getting purple fingers any more…

DOMUSH: You had different problems…

DESSAUER: …you'd get […] corroded machines. So this project was shelved again. But it turned out the man who had made these, we called, leucodyes, was a fellow named Walter Balon. Walter Balon was a self-taught chemist who was fabulously good at synthesizing things. He was working for me at the time. I said to Walter, "Do you still have any of that leucodye that you made for that Ditto project?"

He said, "Oh, yes."

I said, "What color is it?"

"It's white."

I said, "It's that stable?"

He said, "Of course."

So I took it over to Cescon and I said, "Larry, I know what we ought to do. We ought to react [this with] the [HABI]." By that time we had given it an acronym: hexaarylbiimidazole, HABI. And we mixed it in with the leucodye, added a little bit of acid, had a colorless solution, [spotted] it on a filter paper, [put] in the window and it turned deep blue. Well I ran home, and I got a flashgun. […] The same thing [with the flashgun], we had an intense blue image in seconds. I mean in milliseconds. Walter had a […] couple of other leucodyes, so we could make greens, and purples, and so on.[23]

I showed it to our new boss, George Coraor and he said, "Oh, this is terrific. What are we going to do with it?"

[23] L. Cescon and R. Dessauer. Leuco dye/Hexaarylbiimidazole image forming compositions. U.S. Patent 3,445,234, filed 13 May 1968, and issued 20 May 1969.

I said, “Well, there’s got to be something to do with it.”

He said, “Well, we really should pursue this.”

I said, “Well, this has a lot of advantages over photochromism. Some of the photochromism people use the same material over and over again. And if we formed this blue <**T: 135 min**> color, […] then we have to use it for something […].

So George said, “Well, let’s change the project.” So how are we going to do that? We had—we were going to have a research review about a week or so later.

I had been on a ski trip with […] Tim [Chilton and his wife. They invited me for dinner that week. We were talking about this, that, and the other thing. Tim had done a survey on an office copy system that photo products department had invented, but had not been successful in selling it.[24] He claimed he knew something about graphic arts. So I said, “Well, there ought to be some graphic arts application.

“Oh,” he said, “Absolutely. Absolutely.” Tim Chilton was a very handsome-looking guy. He was a company brat. His father had a very high position in the engineering department. Tim was in what was called the development department. He said, “I’ll come to your research review. I’ll sell it for you.”

So we had this research review with the director of research, [Maurice L. Ernsberger] and all of his flunkies, Cescon and I, with our flashgun, filter papers, and solution. So [into] the meeting, [in comes Chilton], […] [and] Ernsberger said, “Who are you?”

He said, “Chilton, Development Department.” Well, Chilton was already a name that everybody knew, even though it was his father’s.

“So what are you doing here?”

He said, “Rolf told me he’s got the most exciting thing he’s ever seen, and I want to help you sell it.”

“So what’s your qualification?”

[24] Dessauer adds: The PPD office copy system depended on a photopolymerizable film sandwich that contained a dye. When this film was placed over the material to be copied, light was passed through the film and where there was text, the light was absorbed, and where there was no text, light was reflected through the film again. One pass was insufficient to effectively polymerize the coating, but two passes caused polymerization. The coversheet of the sandwich was then removed, and the partially polymerized film was placed over a sheet of plain paper and heated. The dye that was in the unpolymerized areas was transferred to the plain paper, giving a right reading image. All this, in the days before there were Xerox copiers!

"Well, I know all about office copy." All right. So we go through our thing. Chilton gets up. He said, "If you guys don't make a project out of this, you're missing a golden opportunity to clean up." I mean the guy was fabulous. He didn't know what he was talking about, but he was full of enthusiasm.

So Ernsberger says, "Well, do you have time to work with us?" […]

"Yes, I think I can spend half-time with you for six months. We'll find some applications."

Well, so all of a sudden everybody was excited. In all my years at the DuPont Company, it was the only time that a research review lasted well past the allotted time. People were wondering what you could do with flashing color like that. Coraor said, "I think we ought to understand the chemistry, what's going on, before we go wild. I mean, I'll need some more manpower." So we got more manpower. [The] project was then called UVI, Ultraviolet Imaging. It grew and grew, we went from three chemists to four chemists, to five chemists. We interviewed anybody that wanted to work on that, because this was new and exciting, and [there] was not a lot of old literature, and so […] [there was not much literature to have to study and worry about].

Well, we could make fairly nice images, but we didn't know how long they would last. The other problem was we had a good way of forming color, but as long as it was in room light, it would ultimately color up. […] We knew we could stop the coloration by dipping this [paper] into the hydroquinone solution, [but] that was not too good. […] People who were not working on this program got so interested in it, that they volunteered to—on their weekends and so on—to come in and help us. I mean that lab was bustling in the evening. It was working on the weekends. It was just a wonderful enthusiastic group of folks. One of the chemists […], Phil [Philip] Manos, […] was not working on this, but he said, "I know just what you need." He made a compound for us which, when [heated], formed a reducing agent which interfered with the color forming reactions, so […] after we had an image we could heat it, and that sounded good.[25] […]

[But what ultimately was the real thriller, was the following]. By that time, I had become quite an enthusiastic <**T: 140 min**> skier. […] I had a friend named Rudy [Rudolph] Pariser, who was a neighbor at the time. Rudy, of course, is known to you from the Chemical Heritage Foundation. Rudy and I were going to organize a bus trip to the Catskills [Catskill Mountains]. We were both unmarried at the time. The snow melted so there was no bus trip. So Rudy said, "Well, all these girls have nothing to do on Saturday night, why don't we have a party?"

[25] Philip Manos. Light-sensitive composition consisting of organic color-generator, photooxidant and organic thermally activatable reducing agent progenitor, U.S. Patent 3,390,995, filed 29 April 1964, and issued 2 July 1968.

So he invited a whole bunch of young women and a couple of people from the ski club, and he apparently owed an invitation to a man named [Richard G.] Bennett. Bennett was a physicist. Bennett, his wife, and I started talking. Then Bennett wanted to know what I was doing. I told him what I was doing. He said, "You know I'm in Radiation Physics Lab. Why don't you come over and give a talk to us about what you're doing?"[26]

So on Monday, I went over and gave a talk. Al [Alexander] Maclachlan, in his [interview] said that was the most exciting thing that ever happened in the radiation physics lab […].[27] They all decided that was what they wanted to work on [HABIs]. All of a sudden, we had all of these talented people who were physical chemists, who were willing, gratuitously, to work on our project. Then, Al Maclachlan, who was particularly interested in it, started working on it [full time]. Then he came up with the idea that if you put another compound into the mix that was light sensitive, but at a different wavelength, you would [obtain] a fixation of the colored image in room light.[28] So now we had a situation. You shine UV light [on] a coated paper, and it would turn blue. You left it in room light, or shined visible light on it, and that would stabilize the image, but the blue image would remain.

DOMUSH: Wow.

DESSAUER: On the other hand, if you went with visible light first, and then UV, you would wind up with a positive image. […] So we [coated] the sheets of [paper]. That was really [an interesting imaging system] that would not require any additive at all. [This] could all be done with light, no toner, no heating, no water, no anything. Of course, the question was, what do you do with it? But we had something that was very unique.

DOMUSH: Right.

DESSAUER: This was [1961]. By that time, we had decided photochromism was not that important anymore, but that this imaging system had merit. So of course, the logical thing to do was to go to the photo products department, who, after all, were in the imaging business for DuPont. So we went [and showed their research director, Frank K. Signaigo and some of his staff] what we had, and they were totally uninterested. They said, "First off, who wants a blue

[26] Dessauer adds: The Radiation Physics Laboratory was set up in the 1950s at the DuPont Experimental Station, to study the effect of radiation on various DuPont products. It was well equipped for making physical measurements. Most of the work originally involved electron beam and x-ray radiation. About a dozen scientists and engineers worked there.

[27] R. Dessauer, "Maclachlan608," in *Photochemistry, History and Commercial Applications of Hexaarylbiimidazoles*. DVD-ROM.

[28] A. Maclachlan, Dry silver-free photographic process. U.S. Patent 3,383,212, filed 29 April 1964, and issued 14 May 1968.

image?" Then, "It's not very sensitive. I mean, the fact that you have to hit it with intense UV light to form color is not very good. Maybe it's toxic […] what do you do with it?"

So we were sort of dismayed. I said to them, "[Maybe] it would be a photo-polymerization agent."

"Well, why don't you give us some?" We [gave] them some. Ah, it didn't work, but fortunately, the people in Orchem still had a lot of enthusiasm for it. I mean, here we were in a situation where Orchem, which was a fairly affluent department—because they made Freons, and tetraethyl lead, and all the toxic things, and dyes—had money to squander on research.

[Photo products was] a relatively impoverished department. They were trying to compete with Eastman Kodak. [They] did not have the research money that we had. Their philosophy inevitably was, well if it's so good, why doesn't Eastman Kodak have it already? I mean, I always kept on taunting the folks from Eastman [when we saw them at meetings]. I'd say, "Why […] don't you invent something like that, because it would make life easier, because [we] could compete with you?" But [they] didn't.

So we persisted. Well obviously, if you were to look at—the original concept for this <**T: 145 min**> blue dye was in the Ditto machine. So the question was, could you make a Ditto machine that would work with light rather than with an oxidizing agent? We thought, well, maybe. Do you remember the mimeograph? Mimeograph used a stencil. Then a mimeograph machine, you're actually—you're etching something, so you [can get ink] through it. So […] if you had a mimeograph machine, you could make multiple copies on our light sensitive paper, which actually worked. So we built a fancy mimeograph machine, which worked with light instead of [ink] —but nobody really wanted to have that.

So then the next question was, what in the world could you do with this? By that time, we [were] about two years on this, and people were getting very itchy, because we [were not commercialized yet. DuPont [sometimes] hired people who had broken off from other companies with the idea that maybe we'd give them a home here and get into a new business. There [was] a group of three engineers, [who] had broken off from RCA [Radio Corporation of America]. They had formed a company called Data Communications Incorporated [DCI]. […] They came to DuPont [on 22 November 1963, on the afternoon of President John F. Kennedy's assassination] with the idea of looking at materials for the Information Age, as it was called then. We showed them what we had and they said, "Oh, the obvious answer is optical printing." People anticipated that computers would print [data, and] we didn't have laser printers or anything like that. In [1963] or so, if you had a lot of printing to do, you'd do it with a teletype machine, which was fairly noisy and was limited to something like ten characters per second. They said, "Well, we could develop some kind of an optical printer for you, and you could print with light on your paper."

Well […] that seemed to be an interesting opportunity, and also was appealing because our bosses were always concerned that Photo Products would say, well, "If you're going to do the photographic thing and fall flat on your face, we told you so." But if you go into optical

printing or something, that's not some business they're in, so you could maybe carry this to fruition.

Well, this company actually did build a printer that did print optically on our paper. But somehow in between all of this, our bosses—two big bosses—weren't satisfied with the speed this was progressing. So the next best thing to do was to sack our friend, George Coraor, who was a super leader and they gave us somebody else, who was a nice enough guy, but didn't have the scope and background. This man was Bob [Robert H.] Terss.

Well, Bob Terss and the bosses then decided that the folks at DCI weren't going to be commercially viable, and they didn't want DuPont to get into the equipment business, because we were always a materials company. So the solution to this whole thing was [that] we would go and take this to a [group] of different companies and see whether they were willing to build an optical printer using our technology.

Well at that point, just about when we were ready to go out, it was—somebody asked, "Can you use a cathode ray tube to print images?" For most people, a cathode ray tube meant television. There was […] in the 1950s and 1960s [David] Sarnoff, who was president of RCA. He was pretty much the man behind color television. Well, Sarnoff said that someday your newspaper is going to be delivered out of your television set. There's going to be a home printer using a cathode ray tube. So our [leaders] thought, "[That] sounds interesting. Here you have something, can you make it go with a cathode ray tube?"

[…] People were developing very fancy cathode ray tubes for certain printing applications. These had fiber optic faceplates, phosphors that were UV emitting. Lo and behold, you could take our paper, hold it up in front <**T: 150 min**> of a [Litton Electron Tube Division] cathode ray tube, and get [an] image to form. We thought we had success. Now people didn't realize those tubes weren't exactly [what] they have at home. These were five-thousand-dollar tubes. It didn't work terribly well.

But we figured we could engineer around it, and we'd have different tubes. The image would roll—in other words—the image would roll, the paper would stay in contact. I still have some images that we made off cathode ray tubes. So it could be done, but the tubes were horribly expensive. So instead of trying to improve the chemistry, we decided to improve the cathode ray tube, which was total lunacy. In other words, we had people inventing new phosphors and all that. We would go into companies that make cathode ray tubes and have them use our phosphors. It turned out it had a couple of possibilities, but it was—I was very skeptical about the value of it.

Well, in almost all photochemistry—in all chemistry—you always want to make things better, or faster, or cheaper, or something. Well we always thought we had to have a faster imaging system. In retrospect, this is almost embarrassing, [that] it took us so long to figure out how to do it. We learned very early in our chemistry [that we could not form] the blue dye [if the coated] paper had been desiccated or dried very long, that [then] the image was very weak. So if we put in some kind of a plasticizer […] this would provide [fluidity] for the molecules to

get together. So [if] we put in plasticizer, in our [coatings], they worked fine. […] [Coatings lacking the plasticizer] didn't work so well.

One afternoon, Cescon, and a colleague, […] Bob [Robert] Cohen, and I, had a bull session. I said, "You know, I think I have finally come up with an idea how to make it faster. Why don't we take the plasticizer and polymerize it?" The polymerization was allegedly a high-speed process. Larry and Bob Cohen thought that was a great idea. They worked on it. So we, now we [have a new] coating recipe. We had the [leucodye] color [former], […] the HABI photooxidant, [and the] monomer, that could be [the plasticizer]. What we needed [was a photoinitiator to polymerize the monomer].

[Bob and Larry] put in what were then standard photoinitiators, phthenanthrenequinone, anthraquinone, and a whole bunch of [other] things. It worked a little. We polymerized and then we couldn't get color. So one day, Larry Cescon, being Larry Cescon, said, "Let's just leave one thing out. Let's [omit the photoinitiators]." What do we get now? It still worked. All of a sudden we realized that our color-forming initiator, the biimidazole, was [also] a photo-polymerization initiator.[29]

That was the beginning of something very great. As of this week [in August 2012], there are [eighteen] hundred patents in the U.S. Patent Office that mention [aromatic biimidazoles, or lophine dimers, imidazolyl dimers, tetraphenylbiimidazoles, hexaarylbiimidazoles, hexaarylbiimidazoles—all of which belong to the family now referred to as HABIs]. Of which, probably [thirteen] hundred or [fourteen] hundred […] [involve] photopolymerization initiators, and ortho-chloro-HABI, which is the […] preferred material.

HUNTER-LASCOSKIE: I think that might actually be a great place to stop.

DESSAUER: [Yes].

DOMUSH: Yeah. I think that…I think so.

DESSAUER: Yeah. Okay.

HUNTER-LASCOSKIE: Thank you…

[29] L. A. Cescon, R. L. Cohen, R. Dessauer. Process for imaging and fixing radiation sensitive compositions by sequential irradiation. U.S. Patent 3,615,435, filed 26 January 1968, and issued 26 October 1971.

[END OF AUDIO, FILE 1.1]

[END OF INTERVIEW]

INTERVIEWEE: **Rolf Dessauer**

INTERVIEWERS: **Sarah L. Hunter-Lascoskie**
Hilary L. Domush

LOCATION: **Dessauer's Home**
Wilmington, Delaware

DATE: **8 August 2012**

HUNTER-LASCOSKIE: Okay. Today is August 8, 2012. I am Sarah Hunter-Lascoskie. I'm here again with Hilary Domush, and we're, again, interviewing Rolf Dessauer. You had mentioned that you might have some interest in talking about the patent situation as related to Dylux. I think we're interested in that, too, in relation to this bigger issue of innovation. What was really interesting about hearing about how this progressed, how your career was progressing, was [that] there were obviously good things and bad things about the way in which you were able to bring this product along. I was wondering if you could kind of speak about patents and kind of that sense of how it worked for you, how innovation can work there.

DESSAUER: Okay. Well, I think one very important aspect of this technology was that we started very early to try to establish a strong patent protection. Here we had compounds that were of interest,[30] and actually, in the period from 1960 to about 1965, there were [seventeen] publications that dealt with these very materials that we thought we had invented. But everybody was measuring properties and nobody thought of doing any chemistry with them.

The difference was simply in the area of research people were interested in: how the color [of the radical] formed, and how [it was] discharged, and so on and so forth. We actually wanted to do some chemistry with these materials. Remember, we wanted to oxidize colorless forms of dyes to color, we wanted to do polymerization, and so on. So it was very important that we establish a patent [position].

At that time, searching for competitive patents was very much more difficult than it is now. Now you can go to the USPTO, the [United States] Patent [and Trademark] Office website, and you put in a couple of names [or terms], and all of a sudden it searches and it comes up tens or hundreds of […] patents that contain the term that you searched. But that was

[30] See L. A. Cescon, G. R. Coraor, R. Dessauer, E. F. Silversmith, and E. J. Urban, "Properties of triarylimidazolyl radicals and their dimers," *Journal of Organic Chemistry* 36 (1971): 2262-67; G. R. Coraor , L. A. Cescon , R. Dessauer , A. S. Deutsch , Harold Leonard Jackson , A. MacLachlan , K. Marcali , E. M. Potrafke , and R. E. Read, "Reactions of triarylimidazolyl free radicals," *Journal of Organic Chemistry* 36 (1971): 2267-72; and subsequent publications.

all [after] 1976. If you want to search any older patents, that's not so easy. [Of course, this can easily be done on line. Now, there also is Google Patents, which searches all U.S. patents.]

Anyway, this was way back in the dark days of 1960, 1961 [and] 1962. Physically, what you had to do was—you'd go to the Patent Office [in the Department of Commerce Building in Washington, D.C.] and you look into their stacks, and they'd have things fairly well organized, but you had to leaf through all of the patents, and [find] one that fit into what you were looking for. Then you'd get a copy of that and study it in detail.

Well, we found that nobody had ever applied for any patents on these materials. That was good. When we filed our first patent on biimidazole chemistry, or biimidazoles themselves, it was rejected. Most [applications are] rejected when [one applies] for a patent, it's almost routine. But the examiner said what you were doing had already been done a long time ago. Somebody had heated imidazoles and they gave off light. So that's nothing new there.

Well, I [wrote] a reply to the Patent Office. I said, "Well, this is like fireflies. If you heat them, they give off light. That's one thing. We didn't heat anything. We shined light on it, and [that] changed color." So I said, "Fireflies are not photochromic. What we were doing was more like a salamander, or an animal that changes color at will. The examiner [accepted] my argument, but he thought we needed to [show] what the actual structure was. Because of the chemical [structure] of the biimidazole molecule there [are] a number of isomers possible. […]

Well, this was a very difficult undertaking. It took us many, many years to establish. When we finally did establish it through crystallography, by that time, it was around 1970 [1968] or so. The patent only issued in 1974.[31]

So this was a remarkable situation. By 1969, we had already started selling Dylux [proof paper]. <**T: 05 min**> We had patent protection on this technology until 1991. [I invented some other HABIs, with which the patent estate could be extended to 1998.[32]]

DOMUSH: Can I ask you a quick question? You said it was about 1970 when you were able to determine structure. Were you involved in determining the structure, or was it part of the team that was working on…?

DESSAUER: Well, it's our team. The way that was done in those days was that we had to grow a single crystal. The crystallographer, in this case […] a man named, Gunther Teufer, spent about fifty thousand dollars of computer time and many years working on that. It was a

[31] L. A. Cescon. Phototropic 2,4,5-triphenylimidazolyl radicals and dimers thereof. U.S. Patent 3,784,557, filed 10 March 1967, issued 18 January 1974. This patent was a continuation of application Ser. No. 181,475, filed 21 March 1962.

[32] R. Dessauer. Dimers derived from unsymmetrical 2,4,5-triphenylimidazole compounds as photoinitiators. U.S. Patent 4,252,887, filed 14 August 1979 and issued 24 February 1981.

very difficult problem. Nowadays there's technology for doing this much more efficiently and faster. But in those days, it was a very slow process.

So I think Cescon was the one who grew the crystal, and then Teufer analyzed it. The patent, of course, issued in Larry Cescon's name, as it should have. So that was important. I think I mentioned yesterday that there are now about [eighteen] hundred patents that mention biimidazoles in them. The first four hundred were almost all DuPont's. There were a couple of patents for other companies, but they were dominated [by] our position.

I think you can't overlook, in industrial chemistry, how important it is to get patent protection […] when you have a unique system, because people always can nibble on the technology and find a way around it. Well, our technology was so solidly established that we had patent protection that was really outstanding. Of course, particularly when [you are] a company [that is] relatively slow and methodical at bringing things to the market, it's good to have patents that last as long as possible.

HUNTER-LASCOSKIE: Is patenting something management was pressing you to do, or was an internal group decision […]?

DESSAUER: Well, I mean, that was—I mean the purpose of our research was to establish patentable products. A company like DuPont has to have strong patent protection on whatever it produces, because our products routinely are very expensive. We have this enormous superstructure of marketing, research, and all the other good things, so you can charge whatever you want if you have a good product. But if it's patent-protected, and nobody had anything that was as good as what we had, so we were in clover here. Actually, I think people underestimate the importance of patents. We, fortunately, did not.

Well, I think we had sort of gotten around to 1964 yesterday. I think I mentioned that [Cescon and Cohen and I] had come up with an idea for employing the polymerization of a plasticizer to prevent color formation. This system was called [RF, rigidification fixing]. We wound up with a […] film where chemicals could react, but under some conditions we could stiffen this so that the interaction was reduced.

This created a great problem, because [Coraor] and I thought this was much better than what we had before. We thought we should push the research into a second generation product line, whereas our bosses thought […] we'd spent all this money, let's get a product out of it by the time we had developed this photopolymer fix system. Another two or three years would go by, and they [became] impatient with us.

So this caused a number of conflicts, to put it mildly. In the end, we got permission to [develop] this photopolymer type system up to a point, but then everything was more or less diverted into the <**T: 10 min**> chemistry that we called Maclachlan's chemistry of photo fix, and so on.

Well, I mentioned to you, also, yesterday that we had sent one of our engineers to Litton [Industries] in California. [He held a piece of our coated] paper in front of a cathode ray tube and it formed color. Well, this intrigued everybody very much, not realizing that it was a very expensive tube, and that we would have a long time before we could develop information recording from cathode ray tubes. However, politically, this was [technology] that did not necessarily fall into the realm of Photo Products research or markets. So our bosses said, "Well, let's see if we could do something here."

Well, in 1965 or so, DuPont was awash with money from having to divest itself of the General Motors [Corporation] stocks, so all of a sudden a lot of research that had previously been underfunded suddenly [received] an enormous infusion of money. What came out was [what] DuPont called Ventures. A Venture was supposed to be a small company inside a large company that had its own marketing, its own manufacturing, its own research, and its own management. There were thirty-one such ventures created in 1966. We were called the UVI Venture. We had, at that time, maybe twenty-five people working on it. [By 1971 most of the Ventures were terminated: Robert Gee, who analyzed them as a post mortem, said Dylux was one of the few that succeeded.]

As I think I told you yesterday, the optical printing—using a machine that was developed for us by Data Communications Incorporated, that didn't fly. But all of a sudden, the [possibility] of using cathode ray tubes generated a great deal of enthusiasm. My only problem was—well, I was happy that we were doing something. I was somewhat concerned that we were doing the wrong thing. I felt as long as we had possibilities of using this light-sensitive paper for proofing, why don't we push that market, which existed, instead of using cathode ray tubes to image materials.

Well, nevertheless, I was then put back into marketing. I was told [to] make this go with cathode ray tubes. Well it turned out fortunately; we made some contacts with [The] Boeing [Company] in [Kent, Washington], who were developing an information system [where] they would expose a piece of film on the faceplate of a cathode ray tube, then subsequently project that film on a large screen. In those days, cathode ray tubes for display were relatively small. The idea of having a forty-seven inch television set was something that you couldn't even dream of at that time. The military wanted to have some displays where everybody could see the same image at the same time. This was called "large wall screen display." So Boeing invited us to come out to Seattle [Washington] and demonstrate our materials. They were very enthusiastic. They begged us to help them on this. There was another program down in Florida at Apollo Research, which was [by] General Electric [Company]. They similarly wanted the image things [with] the cathode ray tube.

[One] thing that worried me was that these were relatively small markets. Here we already had twenty-five people to develop a product. It turned out, technically, what Boeing did was fantastic. They had built a situation room where it simulated the inside of a ship, and they projected film […] [that was exposed with] a cathode ray tube. It was great. But after they had accomplished that, their contract ran out, and that was the end of this program <**T: 15 min**>.

Well, we had a couple of people who were great dreamers on this program. One such man was a […] John Caris, who had worked on lasers and had been all around the DuPont Company. He decided he would call himself the Product Planner. When I told Caris [that] we had difficulties, he said, "Well, we've got enough people. We can do anything." He was very persuasive. Our whole program was directed [into] imaging things with cathode ray tubes. Unfortunately, there was no [real] market for that. It took quite a while before that was realized.

Another application that looked interesting at the time was [point of sales imaging]. Litton-Sweda had developed a point of sales system where a label that was magnetically encoded was used at the cash register to complete a transaction. The folks at Litton felt that if we could optically print on top of the magnetic layer that would make a very super label. So they came to us and we produced all sorts of label materials, which worked very satisfactorily. But then one day somebody at Litton-Sweda decided to use a typewriter to generate information on top of the label. It turned out that did not destroy the magnetic character beneath. So that project, after about a year or two, was also shelved.

So we had problems that we came up with some very esoteric ideas for our technology, but the markets weren't there.

HUNTER-LASCOSKIE: Was there any worry, I mean, internally, on your team, or maybe upper, that you were having these kinds of difficulties, where there were seemingly limitless possibilities, but they were limited in scope?

DESSAUER: Oh, yes. […] Remember, this was when computers were becoming fashionable. So the company decided they would have something called the Venture Simulator. The Venture Simulator was a device by which somebody at the DuPont Company could judge all these thirty-one ventures, how much money they were expending, how many people they had, what their competitors' status was. I mean, all sorts of things. The only thing they didn't do was [identify] a market.

So I remember we had to list all of the possible products, or product areas, where our technology could fit. I have the book still. I think there were a hundred different things we could do. We thought we could make toys using it, we could do printing and so on, advertising, all sorts of stuff. Unfortunately, answering the questions that the product—the Venture Simulator—asked required several people working full time […]. At that time for instance, 3M [Company] had an interesting product called Dry Silver, which was not competitive with what we were doing, perhaps remotely so, but not near-term. So we had to have one or two [men] just spending time trying to figure out what could 3M do with [their] technology? I mean, here we were trying to figure out what to do with [our materials]. But we had to also figure out what our competitors were doing with technology that we didn't fully understand. I mean, this whole thing was idiotic—in my opinion.

Well, we were organized into a marketing division. We…

DOMUSH: Still within the venture…

DESSAUER: Within the venture. Our Venture manager was […] Lee [Leon F.] DuMont, whom I had known for many years. He had been in Freon sales. Our marketing manager was […] Irénée ["Ernie"] DuPont May, who had also been in Freon sales. We had nobody in our [management] who had any clue as to what we were doing technically. This was somewhat embarrassing when we went out to visit other companies like IBM [International Business Machines Corporation], and RCA, who asked technical questions of our managers, who generally ducked [these by] saying, "Well, this is all subject to patents and we can't talk about it."

[…] On one occasion, I was invited to give a talk at Bell Labs [Bell Laboratories] on photochromism, and <**T: 20 min**> it was just a wonderful experience. We started at two o'clock, then we broke for dinner, and then we continued until nine o'clock in the evening. So these people were interested. It was mostly because [I] could talk technical details. Then [I] went with our Venture to present our imaging technology to Bell Labs, and they asked very logical questions, like, what colors could you make, and what is the speed, had you done this, and can you do that? Our Venture manager kept on saying, "Well, this was all subject to patents. We can't talk about it." One [engineer] went out to lunch with us, and we were done in about an hour and a half. I mean it was like being thrown out of Bell Labs.

So we had a problem that we were not well led at the time by people who were not experienced in the field, and who meant well, but we were competing, or we thought we were competing with people who were experts in their fields. […]

DOMUSH: These managers that came from Freon, how long were they with you? They didn't…I guess I'm surprised that they didn't acquire some sort of expertise in the area that they're managing.

DESSAUER: Well, you know, Ernie May was with us for about a year and a half. Lee DuMont was with us for the entire venture. I mean they had their tasks to fulfill, which was trying to get money for our project, and manpower, and so on. It was just that they did not have the perspective of where the business technically would go. […] One thing that has always bothered me about DuPont was [that] because the company [was] in so many different businesses and [because] people are supposed to grow, people were shifted from one area to another where they had no experience, and had to make managerial decisions based on inexperience—in my opinion. I think that hurt us. In a company like Eastman Kodak, say—everybody was in the photographic business at that time. So if you had something new photographically, everybody in the management had some idea of where it would go. Whereas

at DuPont, we were trying to tell a guy who was a Freon salesman what we should be doing in terms of the sensitivity of the material to certain lasers or so on. You know. It was not a good match.

[…]

DOMUSH: So who else were you talking to in terms of talking about the technical details or talking about this for sales? You mentioned Bell Labs. You mentioned that you had done some work at Boeing and with GE as part of those ventures that didn't necessarily pan out.

DESSAUER: Well, I mean, you'd usually call somebody in research in another company and say, "We have some technology that might [interest you.]" And a good case in point was [that] I had always thought we ought to be able to come up with some kind of a toy using a light sensitive material. So one day, I called Mattel [Inc.], which at that time was a very big manufacturer of toys. I said I had an interesting material that might be useful for toys and I would be going out to the West Coast a couple of days later, or weeks later. They said, "Well, if you'd like to come, send us a letter." We didn't have e-mail in those days. "When would you like to come?"

[…] One of the most remarkable things [then] happened. The following morning I [received] a phone call [at] about eleven o'clock, so it must have been eight o'clock West Coast Time, from a man who identified himself as Jack [John W.] Ryan. He said, "Rolf, I hear you have some interesting thing."

I said, "Well, I think so."

He said, "When can you come?"

I said, "Well, in a couple of weeks."

He said, "Why don't you come this afternoon?" <**T: 25 min**>

I said, "Well, we have some plans." Then, "We wanted to go visit some other companies," and so forth. […] I was going to go to […] WESCON [Western Electronic Show and Convention], which was an electronics show [in Los Angeles, California], where we thought we could meet some people who would be interested in imaging.

So he said, "What hotel are you staying in?"

I told him. He said, "That's not a very good hotel."

I said, "Well, it's hard to get a hotel room in Los Angeles when WESCON is on.

He said, "We'll get you something. I'll have my secretary call you."

So the following day, his secretary called and said Mr. Ryan was sorry, "There is no hotel available that he considers fit for you, so he'd like you to stay at his house." Well, I thought that was sort of an awkward situation, when you're trying to sell somebody something, and all of a sudden, you're his houseguest.

So I asked my then leader, Ernie DuPont May whether—what I [should] do. He said, "Oh, well, if you get free lodging, why [don't] you take it?" He almost felt his own money was involved in these transactions. So we got Mr. Ryan's address. I was then married to my second wife, Nicki [Eunice Coates]. We flew out to Los Angeles. We drove, and we got into a fabulous estate in […] Bel Air [Los Angeles, California], which is a pretty nice neighborhood. We were met by the estate manager, who guided us to our rooms. Then the estate manager said, "Mr. Ryan's secretary would like to talk to you."

So Mr. Ryan's secretary turned out to be a former Miss Sweden. She sat me down and said, "What would you like to drink?" I had a gin and tonic. She handed out some papers and said, "Please fill these out." They were intelligence tests.

I said, "You do that with all your guests?" She said, "Well, Mr. Ryan likes to know whom he's talking to." Well, later on I discovered that they had mixed me up with someone else. […]

But he said, "Let me look at it anyway." He said, "Well, you're smart enough, and you're nice enough to work at Mattel, [and] maybe you ought to consider that."

I said, "Well, I'm very happy right now at DuPont." Anyway, he turned out to be an incredible man. He had been one of the inventors of the Barbie doll. He was immensely rich, and an extraordinarily nice man. […] At that time he was separated from his wife, who lived in another part of the [mansion]. He had a [group] of students from the University of Southern California, which was nearby, [who] were running the household, cooking and cleaning, and everything. He got us tickets for a play that night. We had a really nice time.

Well, that was on a Saturday. On Monday morning, [I and an engineer] from DuPont who managed to come out, went to give a talk. It went over terrifically. We had a spray can with the Dylux solution. Ryan had me spray his shirt to let him put an image on his shirt. He said, "Look what you can do with this." I mean, it was an absolutely great thing. I went out there many times. Of course it was always a pleasure staying with someone who was so friendly and very affluent.

Then Mattel put a group of people to work on UVI, it was still—or Dylux was just in transition. After about a year and a half, I took my Venture manager out to meet—to come up with a deal. He said to Jack, "Jack, this is all very good, but tell me, what are your projections for the first, fifth, and tenth year of sales?"

Ryan said, "Lee, if it's good, it's going to last two years. In five years we'll be on other toys."

DuMont said, "Well, that's not how DuPont likes to engage in product development." So that was the end of Mattel.

Then there was this kind of thing—I thought that if we could get started with something that didn't have very stringent requirements—I mean toys were, environmentally, they were okay. But we were still not producing the highest quality consistent material, in a toy that would have been okay. So we were confronted with this kind of thing over and over again.

Well, and I must say our Venture was <**T: 30 min**> muddling along, and not doing too well. Then all of a sudden, a [new] man came into it. His name was John [Webb] de Campi. John had been a Harvard MBA who had some experience [with] photographic […] products. He made a very compelling argument how good this stuff was for proofing. We had earlier been pointed to proofing by some people in Photo Products, particularly [the technical manager of PPD Industrial Products], Larry [Lawrence B.] Friar. But the upper management at Photo Products didn't want to go into proofing [and we] couldn't go into proofing, because it was officially designated as a graphics arts application, which would fall into Photo Products realm.

Well, John de Campi did a good job. He had a very nice presence. He must have been rich somewhere because he owned six and a half Rolls Royces, old vintage cars. At one time he was president of the Rolls Royce Club of America. So we joked when people said, "Do you deliver this in a Cadillac?"

"No, we deliver our products in a Rolls Royce," which made for good jokes.

[…]

I forgot to mention that we had to give all of this a name. We were on many trips to the West Coast. We had lots of time to have bull sessions. Usually it would be two or three of us going. One day we were at a beach—at a restaurant in Laguna Beach, California. We were having cocktails, […] Ernie May and [an engineer] Ted [E. W.] James [were] with me. I said, "You know we ought to do something useful when we're drinking [cocktails]. Why don't we come up with a name for UVI?" We didn't want to call it [UVI]; DuPont trade names should not really reflect what this material is doing. UVI would be ultraviolet imaging. We wanted something that was different.

So we [talked]. I came up with a name, Dylux. We couldn't find anything particularly wrong with it. When we came back to Wilmington, we plugged that into the system by which DuPont analyzes trade names […]. We had had paints which were [called] Dulux, but nobody had come up with Dylux. I thought Dylux was sort of light: "lux," light. And "Dy" was dyes. That went through. We had a contest but it was rigged. [Ernie] said, "Well Rolf, you worked so hard on it, we ought to use whatever your name was." So I got to name Dylux. So from then on

we became the Dylux Venture. [Our first product was called Dylux 503 proof paper. It was offered as a single-side coated and double-side coated paper in a large variety of sizes.]

[Anyway, about a half-year after that, we had another impact that was very important, when Bill (William) Wartel became marketing manager. Bill Wartel was the most dynamic person I ever met at DuPont. Under Bill, things got organized. He and I had great confidence in each other. He got me promoted. In 1969 he sent me to spend three months in Europe looking for markets in Germany and England, and France, Switzerland, Sweden.[33]

It was a sheer pleasure working with that man. He had the leadership quality that made use of all of the people he had working for him. He solidly endorsed the idea that proofing would be our first market and that the other things would be peripheral. We had discovered somewhere along the line that we could coat our product, Dylux 503 paper, on both sides, which meant that you could make proofs which were two-sided, which pleased the market enormously.]

Well in 1969, in September or October was a tradeshow in Chicago called the National Association of Photographic Lithographers [National Association of Photo-Lithographers] or something like that. We introduced Dylux paper there, and it was a big hit. What was important [was that] people were enthusiastic about something that was dry, dimensionally stable, could be handled in ambient light, […] a blue color which was <**T: 35 min**> easy to correct with a black pencil, rather than a black paper that was corrected with blue pencils, so even the blue color was actually a plus.

DOMUSH: Was that something that you had anticipated, as you'd been working with it? Or was that something that…

DESSAUER: [Yes]. We had, particularly after John de Campi joined the group. I mean he knew something about proofing. I [had some] experience with graphic arts—interesting, in the

[33] Dessauer adds: In the United States, printing is mostly done in a negative mode, in Europe it is mostly positive. This means that the lithographic negative is mostly clear, and text is black. Dylux 503 paper can be used in a positive manner, but it requires first a visible light, and then a UV exposure. A product that works nicely in a positive mode is diazo paper, which requires an exposure, followed by contact with ammonia. Many printers could not see many advantages to Dylux, even though the process was dry and free of unpleasant odors. Still, there was interest among printers in England, Germany, Sweden and ultimately at a trade show in Milan [Italy] in October 1969. Importantly, it gave DuPont marketing people an opportunity to show that DuPont had something new and innovative. I reported that with some adjustments in the chemistry we could make positive prints that would be acceptable, though the Europeans seemed to want it to be black/white, not cyan/white. There was also interest in a Dylux spray, which allowed us to sensitize a variety of surfaces. I found the DuPont people, who had previously worked for the ADOX Company [ADOX Fotowerke GmbH], to be quite positive, and they were of course delighted that there was someone who could speak German. Altogether, I spent three months in Europe, and certainly enjoyed that experience.

good old days companies were much more lavish in entertaining each other. At one point in about 1960 or so, I was in a group of six or seven people from DuPont who were invited by Meredith [Corporation in DesMoines, Iowa], who put out *Better Homes and Gardens*. They showed us the entire process of printing, proofing, and everything. Their idea was—they didn't have the research staff at Meredith to invent new products. But they felt if they alerted people in companies that had research capability then they would maybe invent something that would be useful for the printing industry or for some other industry. So I knew a little. I mean, I knew what a proof was.

In my days, when you [graduated from] high school, there was a yearbook. They would give you a bunch of pictures [to select your favorite one] that would fade out eventually, because they didn't want you to send these to your relatives. So, I mean, these were bad proofs, showing your facial character and so on. So I knew a little about it, but it was really John de Campi who had […] learned something at Harvard. He knew how to present things. He and I had a great time going to these markets.

Now all along in this, we had to have brochures, [which I wrote], and other information to show off our materials. I must say [these] didn't always work perfectly. So I thought, well, the way to solve that problem was to make a movie. Make a movie that would show what we could do with our technology.

DOMUSH: So a promotional movie.

DESSAUER: Promotional movie. DuPont had a full-time movie photographer. His name was Franny Ryan. So he was intrigued, except he said, "Making a movie about a light-sensitive material is a bit of a challenge." Surprisingly, [commercial Kodachrome] was very, very slow. I mean […] we literally had to photograph that [movie] at night. I mean, he came out to the Experimental [Station] at midnight when the power consumption of the [place] was low. We photographed this movie from midnight to about four o'clock in the [mornings].

Ultimately, this movie was produced. It was eleven minutes long. We went to New York [to visit] an announcer who had worked for DuPont, on—DuPont [once had a weekly radio show] called *Cavalcade of America*.[34] […] I wrote the text and he recorded it. This movie had about six or seven copies, and whenever [somebody] said, "Well, what does UVI or Dylux do?" We sent them the copy of this movie. So that was actually a good idea. For me, it was interesting. I enjoyed making movies. I thought it was quite clever. [I even showed it to MGM (Metro-Goldwyn-Mayer Studios, Inc.) in Hollywood].

[34] *Cavalcade of America*, CBS, 9 October 1935-29 May 1939, and NBC, 2 January 1940-31 March 1953.

DOMUSH: Now at this point, when you—when you're making the promotional video and you are sending it to people, is most of your time kind of spent promoting Dylux and managing Dylux? You're not—you're not really in the lab anymore at this point, I'm assuming…

DESSAUER: Well actually I stopped actually doing lab work about 1963 or so. I had a group of chemists working under me, and I was the guy that coordinated. One of my tasks was simply to identify people in the DuPont Company who were not necessarily working on our project, but who could help us. I mean, there were certainly people in the Plastics Department, and Fabrics and Finishes, and so on, who could help. There was a lot of correspondence that had to be answered. I mean, people would ask, if they heard about this or they saw this, saw that. Sometimes—I mean, we were looking for internal support.

Now, one thing which was sort of along those lines <**T: 40 min**> that came somewhat earlier: about [1962], DuPont had a product called Corfam, which was [to be] a shoe leather substitute or replacement. One of the problems with Corfam was that it didn't stretch like leather did, so if you bought a pair of Corfam shoes, they would never—if they were tight when you bought them, they were tight five years later. So they were very concerned that people would always buy shoes and they'd be a little tight because they knew they would stretch, but Corfam didn't, so they were trying to come up with clever ideas of how to market Corfam. One of the managers of Fabrics and Finishes Department came, and he said, "Well, there's going to be a World's Fair in New York in 1964, 1965. If we could measure people's feet as they go through the DuPont Pavilion, we could learn something about shoe sizes and people's feet." So they came up with the idea that maybe we could make a silhouette of a person's foot [using our UVI-paper].

[An engineer built] a device where you step onto it, and there was a sheet of light sensitive paper underneath it, and a bunch of ultraviolet lamps on top of it. After you stepped on them, the lights went on. When you got off, there was a wide silhouette of your [foot]. They thought, well, if we find out what that person's shoe size was, then we could say, "Well, he buys size eight and one-half shoes, and that's his foot image." So I got to be very knowledgeable in foot imaging. This was a totally unscientific field. I mean, every shoe manufacturer had his own preferred [system] for sizing. We were told that they had something called the [Bannock gage] where you put your foot in it, and they clamp it, and figure out what your foot size is. Most of the time, when they do that, they look into your shoe and see what size you already wear. Then they give you one that's a size, half a size larger to try [on]. So I had to spend months learning about foot imaging, and how we could fit our paper into that. Unfortunately, [the gentleman behind that program] died, and foot imaging never became a product [area]. But, I mean, these things were peripheral, but they were interesting.

Well anyway, 1969, when Wartel took over management of the marketing group, all of a sudden things began to jell. We were selling materials. One thing that happened then was that, slowly but surely, Photo Products [became] interested in this chemistry. Then they found out that the biimidazoles were extraordinarily good photopolymerization initiators. Photo Products was developing a group [of novel products] for new markets in color proofing, and in

photoresists, where they needed good photoinitiators. All of a sudden, they began to use our chemicals. […] Orchem made [money] transferring chemicals to Photo Products, and so on. Well we kept on finding interesting markets. The military was interested in using our light-sensitive materials to make contact prints of aerial reconnaissance negatives […]. Anyway […] we visited a great many Army bases, Air Force installations, CIA, [and the National Security Agency]. I had a high military clearance, so we could go to these places.

It was interesting. We wound up with quite a [warm] relationship with the U.S. Navy. They wanted to spend money to develop a color duplication film. So things were sailing along quite nicely. We were beginning to be for real. Then the company decided they would move the Dylux Venture to Photo Products department, which was a logical thing…. [The first major change was that Photo Products did not want to accept government money for us to do research. Naturally, the military then supported our competitors.]

DOMUSH: This is moving you out of the Orchem…

DESSAUER: [Yes.]

DOMUSH: Moving out <**T: 45 min**> of Orchem?

DESSAUER: Moving it out of Orchem. Well when we first heard about it, the message was lock, stock, and barrel. [It meant] everybody. Then in reality, it came out that they took less than half of our group. They purposely eliminated some of the people who were market specialists, like [de Campi and me]. So all of a sudden, I was without this chemistry that I [had worked on, by then, for ten years, and knew more about] than probably anybody else.

I traced that—the fact that—they didn't take me due to some personal animosities, I fairly well documented that. The [head of research] in Photo Products was […] [Robert "Bob"] Upson, who felt that research was done by [telling somebody] what he should work on, not find someone like me, who would endlessly come up with new ideas for developing, expanding the technology. So I must say I was a little heartbroken at that point.

I wound up working again with Bill Remington who had worked on [all of the above] when we started this thing. We thought, what could we do that would be different? Well, somebody in Central Research had come up with an idea of using…

DOMUSH: I'm sorry, before we start in on what your transition. I'm thinking we should take a quick break, because I need to use the restroom, if that's okay.

DESSAUER: Pardon?

DOMUSH: I need to use the restroom, if that's…

DESSAUER: Okay.

[END OF AUDIO, FILE 2.1]

DOMUSH: Okay…

DESSAUER: [A chemist named Walter Mahler] had invented, or discovered, that under certain conditions he could produce iridescent materials, like liquid crystals that were hard, not fluid.[35] So [one] could actually coat some of these onto a surface, and by heating them and applying pressure, they would form very beautiful iridescent patterns. […] Bill Remington said, "Well, why don't you look and see if you could develop some products based on that?" We decided to give this a name, and call it Irlux, [as from] iridescent light, and started embarking on that. We made some quantities of material. I had a couple of chemists work on developing them.

Then there was a development conference with General Motors, [that I attended] and tried to interest them in Irlux. They were very interested. Oh, they wanted to hear all about it. Could we come out to Detroit [Michigan] to [paint] a car that looks psychedelic with all the iridescent colors?

Somewhere during that conference, I met up with […] Bob [John R.] Ellefson, who [was] a designer. His past work was designing leather patterns for Corfam. He was sort of looking for something new to do, because Corfam was about to expire. He came over to me and said, "Rolf, this is something that's exciting. Let's meet and have lunch." […] Well, lunch with Bob Ellefson always was a two- or three-martini affair, but he was very creative.

I told him what I had done with Dylux. He said, "Could you put [a Dylux coating] on shoe leather?"

I said, "Sure."

[35] W. M. Mahler and Panar, "Cholesteric solids," *Journal of American Chemical Society* 94 (1972): 7195-7.

He said, "Well, why don't we do that?" So he got me some leather. We coated it, and we created all sorts of patterns. He said, "That's fantastic. Why don't we go to New York, and we'll talk to a couple of people in the shoe leather business?"

So we're off to New York. We talked to a lady named, Nancy [Quinn], but I'm not sure, who had been [a] designer of the Clark Desert Shoe, […] [and one] of the foremost shoe designers. Well she thought you could put all these fancy patterns on shoe leather. This would be interesting because [one] could print things on demand. So all of a sudden, a new concept was created: "photodecoration."

Ellefson, who knew all sorts of people in the company, had a contact with the [DuPont] group in Greensboro, North Carolina […]. DuPont sold lacquers for furniture in those days. So Ellefson and I went [to] Greensboro and [showed them what one] could do with Dylux. [These people were excited], "Oh, this is great." We can now use plywood, and produce a pattern on it that I showed you. Before long, we had half a dozen people working in Greensboro trying to develop lacquers that could be applied to plywood or composition board for patterns on wood. The advantage, of course, was we could produce inlaid patterns. You didn't have to glue anything together. We could do it all photographically in high quality. This thing went on. All of a sudden, we had—in Orchem, we had a small group, five people working with us, to develop these markets. So we were in the shoe leather and furniture business.[36]

I was having a great time, because [this was totally new]. These were people who were used to having nice expense accounts [to] travel with. You could eat well. […] Everybody was very excited about it. Well unfortunately, at some point, DuPont decided the furniture business was no longer of any interest to them. Even though we had a line going into Thomasville [Furniture Industries], which was a major furniture manufacturer, who had already bought equipment to make photosensitive tabletops, this project got killed.

DOMUSH: Now when something like that happens, like Thomasville had made a commitment <**T: 05 min**> because they had—as you said, they bought equipment…

DESSAUER: [Yes].

DOMUSH: …to do this. DuPont makes a decision to kill the project. Who makes that decision? How does that actually…

[36] R. Dessauer and J. R. Ellefson. Photodecorating sheet material with matched colored designs. U.S. Patent 3,847,608, filed 8 August 1972 and issued 12 November 1974; R. Dessauer and C. E. Looney. Decoration of substrates by thermal transfer of photosensitive thermoplastic, dye-imaged film. U.S. Patent 3,909,328, filed 10 August 1973, and issued 30 September 1975.

DESSAUER: Well, upper management makes the decision based on whatever information is fed to them. Frequently this means that you have to pay people for equipment that they had—we had to pay for Thomasville putting in some equipment. It doesn't help your reputation. I mean, this is one of the great problems […] of being a materials supplier, because you need other people to build equipment for your materials. Then if you don't make the materials anymore, they're very angry with you when this happens. This happened to us certainly here.

We also spent a lot of time and energy trying to decorate leather, and, again, that closed. I mean the interesting thing with leather was—I don't know if you know that when an animal hide is used, it is split into sort of—split like this is the upper, this is the lower. Did you know that?

DOMUSH: It sounds—it sounds somewhat familiar, but…

DESSAUER: I think you know about Hush Puppies Shoes and things like that, they use the split. They don't use the top grain. The top grain has the pattern. But the split doesn't. So if you could create a leather pattern on the split by coating […] [a photosensitive layer and form] a photographic image of leather you could have two high value things from one split. So the leather industries that we spoke with were very interested in what we could do. It seemed a rational opportunity.

But our leaders didn't have the patience to develop it. Then, of course, one of the problems was DuPont likes to work with [big] companies—in those days liked to work with companies who were big in their field, like Boeing, or General Electric. They don't like to work with little guys who make leather patterns or something like that. So this tends to be somewhat of the problem.

Anyway, we had enough license then to examine other markets. Then along came something called "Photo Marker," which [was] making patterns for cutting [garments] These were not for home use, but for [industrial] use. For instance, when garments are cut, they take a whole stack of pieces of fabric. They mount a paper pattern on top of it, and the cutter goes through it. Actually, there was a company that was very close to where you work now, [it was] called PMC [Pincus Brothers-Maxwell Clothing], I think. I don't know if you've seen it. It's next to the Bourse. [It was] a big clothing manufacturer there. We market tested a light-sensitive paper with them. But the company that was going to develop it was a small company in New York. We had a very good relationship, and it pleased me, because it gave me lots of excuses to go to New York. But the product was to come on the market just about the time when interest rates were about twenty percent, and they couldn't finance it. So, you know, you run into these problems perpetually.

Well in 1976, I got a phone call from […] one my friends […], "Guess what. They're finally going to put somebody into a marketing position in Photo Products, who supposedly knows what he's doing, and he's going to help you expand markets."

I said, "Oh, that's great." […] His name is Philip Botsolas […]. [He] was a remarkable person. I called him up and said, "I hear you're coming."

He said, "Yeah, I heard about you, Rolf."

I said, "Oh, I tend to be quite enthusiastic about this technology. And I'm glad [if] I ever find somebody who's as enthusiastic."

He said, "Rolf, if you're not enthusiastic about what you're working on, you're not worth [expletive]"

I thought, "This is going to be interesting." So we got together in Wilmington. <**T: 10 min**> I showed him what I was doing. He was a little skeptical: […] "If it's a good [idea], why doesn't anybody do anything?" Well, it turned out I didn't have the authority to do more than suggest the market. You need support from upper management to get out there.

Anyway, the first [place] we went to was…. We had found a [professor, Martin Rubin, at Georgetown School of Medicine], who wanted to [put] bar codes on samples they were taking out of patients. […] He had seen a machine that was called the DuPont Automatic Clinical Analyzer. Have you ever heard of that? Well ACA, it was called, was invented somewhere in the early 1960s. It was a wonderful machine, […] where fluids from patients—urine, or blood, or whatever—could be analyzed automatically. It involved some color-changing reactions.[37] The fellow who was really the brains behind that, was […] Don [Donald R.] Johnson, who believe it or not, also went to [the University of] Wisconsin, and was my upstairs neighbor.

DOMUSH: Oh.

DESSAUER: […] Well, this machine was quite successful. It had one very nice feature. It printed out the results on Dylux paper. The way it worked was this: The nurse would take a small transparent pouch and insert, let's say, a sample of urine, and there would be pods on the pouch, and the machine would squeeze these pods. They would give a color reaction for this, that, or the other thing. Attached to the pouch was a translucent sheet of Mylar film. The nurse would write the name of the patient on that transparent sheet with a marker or a pen. Then when the analysis was complete, the pouch and the sample would be in contact with a UV light over a piece of Dylux. So this handwritten information would be [transferred] to the sheet on which the results were printed. So you basically had a foolproof method of transferring information.

[37] M. Rubin. Patient Treating Method. U.S. Patent 4,476,381, filed 24 February 1982, and issued 9 October 1984.

This, at its height, consumed about three million dollars' worth of Dylux every year, so that was a market for us. It was internal. Every time somebody did an analysis, they used Dylux paper. So [Rubin] had the idea that we could use this technology to make bar code contact prints. Earlier I had been asked to go to a meeting in New York, where the supermarket industry was discussing the future of bar codes. This was at the Plaza Hotel. It was very enjoyable. I suddenly realized this was a great opportunity for printing bar codes using Dylux.

Basically, in those days […] bar codes were supposed to be on the bottom of each item. There were some items that were very hard to print on, like bottles. So companies like PepsiCo [Inc.] and Tropicana [Products], and so on, came to us and asked us if we could squirt Dylux [solution] onto the bottom of orange juice containers, or glass bottles [and image by projection rather than contact]. Glass bottles are [not flat] at the bottom. [We could then] optically print bar codes. We only had a blue and white image, which was ideal because [the scanners] used helium-neon lasers, […] [that were matched to our blue (cyan) images].

But it seemed there was a better opportunity even in bar codes for items that would be marked in the store, which was meat products, or dairy products, that [have] different weights. So the people <**T: 15 min**> in our Film Department at that time […] who were involved in selling packing films to the meat and dairy industries [saw a market]. [They asked], "Oh, can you print bar code labels on Dylux?" I said sure. So we had a new opportunity.

[They] decided, [to] go to the biggest manufacturer of weighing scales, […] Hobart [Corporation] in Dayton, Ohio. We showed them what you could do with Dylux, and our analysis was that […] at that time there were thirty thousand supermarkets in the country. They [each] weighed about two million items a year, so you had a huge number [sixty billion] labels that had to be printed. They could all be printed on Dylux. We thought, well, the bar code would be about an inch and a half square. [At about ten cents per square foot, this would turn] out to be about a sixty-million-dollar business. That was pretty good.

Well, when we tried to sell that to our bosses, one guy said, "My wife would never buy anything in a supermarket that didn't have a price on it."

Somebody said, "Have you ever been in a supermarket?"

"Well, my wife does all the shopping." I mean, the idea of bar codes for everything was totally foreign to people in 1972, 1973. So we thought there was an opportunity here. As we went to Hobart, we told them what we could print optically, without any waste. And the [label] would be stable. The chairman of the board put his arm around my shoulder and said, "Rolf, I'm glad you came here." So we worked with Hobart for a number of years. Then DuPont decided they didn't want to get in that business. We had gotten some patents on some ideas and label printing and so on.[38] But nothing came of it. I mean this was sort of a disappointment in

[38] R. Dessauer. Marking transfer sheets and process. U.S. Patent 4,207,102, filed on 8 March 1978, and issued 10 June 1980; R. Dessauer. Universal Product Code Marking Composition Containing a Photosensitive Dye Former, a Pigment and a Binder and the Use Thereof. U.S. Patent 4,029,506, filed on 22 September 1967, and issued on 14

my career that we had all of these opportunities to pursue, and for some reason they got killed, and…

HUNTER-LASCOSKIE: And you had very little—I mean, you had very little explanation on your end as to…

DESSAUER: Well, the company said, "Well, this is not a business we want to get into." Or, "We don't have a marketing organization to support this." It can be—throughout my life, I found it's easy to invent things. You have no problem getting support for [inventing], but then when you try to commercialize things, then you're talking about real dollars. You need different skills, and some of those aren't available. They'd have to hire new people. They'd have to find new locations and buy new equipment […]. [A] research chemist in those days might have cost [two hundred thousand dollars] a year. When you're trying to develop a product, now you're talking about millions of dollars. Of course, in a company that is relatively risk averse, as DuPont is, or was, those are hard things to fight. I think the lack of experience in different businesses always hurt us. We were fortunate in the proofing business that we had John de Campi, and me, to a certain extent, and a high level of enthusiasm and being able to go out to people. I mean, one of the ways that DuPont marketed things often is they'd go to companies with secrecy agreements and say, "We'd like to show you something. What do you think about it?"

We did this—I had one remarkable experience. It was in [April] 1969. It was about two or three months after Wartel came into the program. I was [in] St. Louis [Missouri] to visit some Air Force lab. They […] cancelled the visit after we got to St. Louis. So the tech rep, whom I was […] to go with, said, "Well, why don't we go and visit a couple of printing companies and see whether they have any interest in what you have?"

So we went to one company [Von Hoffman Press Inc.] and said, "We would like to show you this." We had these little printers I showed you yesterday, so you could demonstrate [image formation on Dylux 503 paper].

The manager of that plant said, "Boys, I'd like to buy your lunch." Well, that seldom happens that somebody else buys you lunch. He said, "You want to know what my opinion is of DuPont?"

I said, "Well, yes." He said <**T: 20 min**>, "If they don't know what to do with this and have to come to ask us what to do with it, they're even dumber than I think they are." [Then he asked for samples.] So I thought this was a great plus.

June 1977; R. Dessauer. Marking Transfer Sheets. U.S. Patent 4,232,108, filed on 1 May 1979, and issued 4 November 1980.

Then we went to another company—same thing. […] We had to go to the third floor of an office building, where the manager sat. I had to lug this little printer up three flights. [It was] heavy. The tech rep who accompanied me said, "We'd like to show you a new product from DuPont."

The manager said, "Listen, guys. I'm up to my eyeballs in work. Come back some other time."

I said, "Can you give me one minute?"

He said, "Okay." So I made a proof for him in one minute. He said, "Sonny, I've got all afternoon." He said, "Listen, the Printers Club of St. Louis is meeting tonight. Can I borrow your equipment? I promise not to keep it. I'll give it back to you tomorrow, but I want to show everybody what you can do." He said, "Look, we printers—we're not competitors. Everybody is in some other aspect of printing. So this will make for a lively evening." So I picked it up the following morning. He said, "For God's sake. Just make this stuff for people in St. Louis."

I called up Bill Wartel. I said, "Listen. We can sell all the paper we can make in St. Louis. Why don't we start making it?" Bill [believed] me, and he started the wheels rolling, [but] we didn't have any coating equipment. We had to go to [a custom coater] in Massachusetts to do the coating. But that sort of started it.

So this business of finding the right person in another company to work with […] is very important. Well anyway, so…

DOMUSH: Do you think that…. You said that in some ways invention is easy, but the commercialization is hard. I guess in some ways I would imagine that a company like DuPont would have an advantage over a small company or an individual in commercialization because they do have marketing experience. They do have resources, but do you think, or in your experience is there just a certain amount of inflexibility? Like you said, they don't want to work—or they didn't want to work with small companies. They only wanted to work with kind of the big companies. They were very conservative in their choices, so was there just an inability at the upper management level to see some of those risks as worth it, maybe?

DESSAUER: […] In this [recent] book about Bell Labs [it was reported that their engineers and scientists] could invent things, because they had a rich backer, AT&T [Inc.].[39] They didn't have to worry about funding from day to day. For a while at DuPont, this was also the case. […] The company was generous. […] As long as the DuPont family really owned the company, which was until around the 1970s or so, I think there was a great emphasis on long-term results.

[39] Jon Gertner, *The Idea Factory: Bell Labs and the Great Age of Innovation*, (New York: Penguin Press, 2012).

In their more recent past, the results—the short-term results, [became] ever so much more important than they used to be. I mean, all of the people that I know who work for DuPont now [are] working on three- or six-month cycles for product development. Well, if you can't do that in three months, then maybe we should do something else. Of course, who is to know at what point it should stop?

So I think, every morning when the DuPont Company wakes up they have spent millions of dollars to keep all the office buildings, all the experimental stations, and all of the factories and so on. So they have to produce revenue, and produce a fairly high level of revenue, to support all of this. In my opinion, good things—bringing good things to the market takes time. I mean, it took us from the time we could have, at the first Dylux [paper], could have been in 1963, and we didn't get on the market until 1969.

I had another case in point. In 1975, I invented a new photoinitiator <**T: 25 min**>, which was patentable.[40] The patents didn't issue until 1981 and 1982, but it wasn't until around [1984] that this material was used. It took us—from the time we decided we would use this new initiator until the time it had replaced all of the old initiators was four years, in part it was because we had to do all of the environmental testing. We had to do market studies of how cheap or how expensive it was. We had to make endless comparisons. I wondered. I asked some of the upper management why it took so long. They couldn't figure it out. But it's just—a large company tends to be slow. Now, I think a lot of this has changed now. There's a lot less middle management to slow things down than there used to be, so maybe things have—somebody has learned.

But anyway, we found out then that, as time went on, what was very important was to get equipment out that employed—that was designed for our products. As I showed you yesterday, two different light sources, the visible and the UV, are responsible for the imaging or the deactivation. In the graphic arts field up to that time, until we came along, […] [they] used one intense light source, which [emitted] both UV and visible light. So when you expose the material with both, it images a little—it fixes a little but it doesn't really do what you want, so we had to persuade people to buy equipment that was rich in black light, rich in ultraviolet light. We found some companies who would make it. […] [Harold Wilbur], who built that little printer for us, started a family business to make equipment for DuPont and customers.

But somewhere along the line, somebody said, "You know, if we had a filter, we could use a xenon lamp, which is […] rich in UV, and a glass filter to separate the wavelengths, so that only UV or only visible would come around." Actually, we found out early in the game that there was a company in Kokomo, Indiana, called the Kokomo Glass Company, who made a cobalt [glass as a filter] that suited our purpose, so we used that filter. We gave hundreds of

[40] R. Dessauer. Dimers derived from unsymmetrical 2,4,5,-triphenylimidazole compounds as photoinitiators. U.S. Patent 4,252,887, filed on 14 August 1979, and issued 24 February 1981; R. Dessauer, E. I. DuPont de Nemours and Co. Dimers derived from unsymmetrical 2,4,5-triphenylimidazole compounds as photoinitiators. U.S. Patent 4,311,783, filed on 17 July 1980, and issued 19 January 1982.

pounds of that filter away, and people interposed that [between] the light source and the paper to generate relatively pure UV.

But then there was a need to develop a flexible filter so that people who had to roll-through devices—you know, they put in things and they come out. Then we were given the task of whether we could develop a color filter that would absorb visible and pass UV. I had collaborated for much of my technical life with a very talented woman chemist named Kay [Catharine E. W.] Looney, who was a spectroscopist. I gave her several dyes that might work. She measured the spectra and optimized the system, and we came up with a flexible filter, where people could simply interpose that between broadband light sources and the light sensitive material.

So, I mean, it was a matter of identifying opportunities that would enhance the product, and that worked. Kay Looney […] had worked at Central Research when I first met her, had left the company to have children. After all that, she was still interested in science. I got her hired as a consultant. She came and contributed hugely to our success <**T: 30 min**>. We remained friends for a good number of years. Unfortunately, she died [several] years ago.

As time went on then, Philip Botsolas, [whom] I spoke of, was this enthusiastic character, slowly became convinced that maybe what I was saying was right. We weren't getting the right kind of support. I want to say, I have a good slogan for Dylux. "We don't need any development. We're surely not getting any."

So the next thing that happened was, there was a gentleman named Glen [A.] Thommes, who was a rising star in photo products department. He was [allegedly] a marketing expert. So he and I and Botsolas had a meeting. Glen said, "[…] Look, you invent all of these things and [nothing has] ever come of them, except for proof paper." […]

[I felt it was that we lacked support to commercialize what we invented and] said, "Glen, you don't know God damn well what you're talking about." Two weeks later, Glen was my new boss. But I must say, he was very supportive, [when] he was my boss, so I think he probably realized it.

Anyway, Botsolas then decided maybe Rolf Dessauer should be working for Photo Products, but he couldn't get me in, because Bob Upson didn't like me. So Botsolas said, "Well, why don't we write a couple of projects that […] Photo Products would pay Orchem to do the research." So I said, "Fine." We decided that if we could make a four-color proof that would be great. [Also, if] we had really black [image forming] paper, that [would be] great, because our paper was sort of off-black.

In color proofing there are several products that require more or less time to make a proof, but you basically deal with four negatives, one for making a [cyan-forming] printing plate, a [magenta-forming] printing plate, a [yellow-forming] printing plate, a [black-forming] plate. So the question was, could you develop a Dylux film that would produce these colors,

and then you could overlay them to see a color proof? I said, "Oh, sure, I can do that." I did it. […]

[The general manager of Photo Products was a nice gentleman named Philip Wingate. I had known Wingate; he had been in Orchem a long time earlier.] I would occasionally meet Wingate when I picked up my mail at the post office in Greenville. So Wingate would say, "Rolf, what's new?" He kept on saying, "What's new with Dylux?"

One day I said, "I'm going to wait and see if I can intercept Wingate at the post office and show him what we had done." He was very [interested in what I showed him], and then we got the money to do the research on [colored Dylux] from Wingate. So it involved personal relationships.

Well in 1978, Upson retired. Then they took me into Photo Products and they had me work there. Except for the initial reception, which wasn't all that glorious, it was pretty good. […] In Orchem, if you had a good project, you'd get as many technicians as you [needed] at that time. […] When I was in that final stage in Orchem, before I transferred, I had three lab technicians, Kay Looney, and another chemist working for me. So it was—when I went to Photo Products, they said, "Well, everybody gets half a [technician]." Now that's idiotic. Here's a person who's an expert […] [and] has worked in this field twenty or thirty years, and he's [treated] just like a new hire who gets a half a technician. But anyway, we struggled on.

Then, gradually, Photo Products changed. We got to the point where we [tried to] develop new proofing systems using electrophotography […]. Then at one point, it was a director—well, he was an assistant director […] [Thomas D. Smith], who <**T: 35 min**> felt that I should have gotten more of a reward than I did for my contributions up to that point. He initiated that, and I did get a fairly sizeable chunk of money, somewhat late in my life, in 1986, and another promotion.

[He had said, "Why don't you write me a note about what you did and what you expect?" I took this document to his office—there was no email then—and he looked at it and said, "Wow. How much would you think was fair for a bonus?"

I said, "How about one tenth of one percent of net profits?"

When I told him that the profits from all the proofing products and photoresists that contained HABIs was way over a billion dollars, he said, "You will never receive one million dollars." He was right. But he and Vaughn Chambers went to bat for me, and I did receive ninety-seven thousand dollars on top of the earlier three thousand dollars. Also a nice lunch with the new Vice President, Mark A. Suwyn, and several of his flunkies.]

Then [in 1987] DuPont and Xerox [Corporation] formed a joint venture, called DX Imaging [Inc.], which was to be located in Lionville, Pennsylvania. We developed some

exciting new technology, [photopolymer electrography] and I think we did very well.[41] [Here the goal was to produce a consumable coated film product, which on exposure through lithographic negatives changed its charge-retaining properties, and could be toned with liquid electrostatic toners that would rapidly yield several multicolor proofs.] But at some point in time, DuPont lost patience with it, so we didn't have the money to support it. We had a falling out with Xerox on top of it. So DXI closed up, and I suddenly was retired.

It wasn't too bad though, because the day after, I was consulting for my friends in Towanda [Pennsylvania] on Dylux. I found that way I could consult for a lot of people. I wound up consulting with Xerox, for Polaroid [Corporation], for International Paper Company, [Hewlett Packard], and so on, so forth. It was pretty good. But the most exciting consultation was the Hewlett Packard. One day, I…

DOMUSH: So wait, let me just understand. This is about 1987?

DESSAUER: 1991.

DOMUSH: 1991, okay. Sorry.

DESSAUER: [Yes]. I should say that. I could put this in some chronological order. In 1987, I had worked for DuPont for thirty-five years. If I joined DX Imaging, I could get my pension and a full salary, so that sounded very inviting.

DOMUSH: Yes. It sounds like a pretty good deal.

DESSAUER: So I decided I'd retire from DuPont, and I had assumed that DXI would last ten years, but it didn't, unfortunately. It was sort of interesting. I was talking about this yesterday, connections. Now in 1969, when Bill Wartel sent me to Europe to look for markets for Dylux there, when I was in Munich [Germany], the DuPont Company hired a young physicist [Reinhold Epping] to take me around—to a couple of companies […] and a couple of printers. I liked the guy. My wife, Nicki, came along with me.

I said, "Well, we ought to invite the Eppings for dinner. So we took them out to a very nice dinner.

[41] J. Riesenfeld, W. Bindloss, G. Blanchet, R. Dessauer, A. S. Dubin. Xeroprinting with photopolymer master. U.S. Patent 4,732,831, filed 1 June 1987, and issued 22 March 1988.

Then I met Mrs. [Angela] Epping. She said, "Oh, you have to come to our house for dinner." We went to her house for dinner, so we became lifelong friends. In 1990, [Angela] came to the United States to learn English better. She and her husband [by then] owned a small company that made very sophisticated electronic equipment for [charge to mass] toner [measurements]—for toner quality. She felt she was doing all of the correspondence, and her English wasn't very good.

[…] She stayed in New Jersey for a couple of weeks. [We went out to dinner a few times and once told me she did not like the family with whom she stayed and] called me up and I said, "You can stay here. I've got a big house." I was a widower at the time. Three months later, we decided to get married. So if it hadn't been for all of this technology, I wouldn't have married Angela. […]

There are lots of little coincidences like this. Well in 1991, we got married. In 1991, I started what I thought was a really nice consulting activity. […] At that point, my relations with DuPont were really great. They were very generous in consulting fees and supplied me with chemicals, so I had to use my laundry room upstairs to mix things. Well, I mean, I was so familiar with these things, I knew what was toxic and what was not. I'm still alive. I mean…

DOMUSH: It worked out okay.

DESSAUER: I've had—I've had one cancer operation, but not too bad, so I feel I was fortunate. But I said the most interesting consulting activity was with Hewlett Packard. This was in 2002. I [received] a phone call from Hewlett Packard. They said, "Are you [interested] in consulting with us?"

I said, "Oh, sure. Well, what about?"

"Well, we really can't tell you. You have to sign some agreement first." <**T: 40 min**> I thought, okay. So they e-mailed me some agreements and I signed them. Then we had a long conference call. What they wanted was [this]: "We have a technology which does what we want to do, but we'd like to know how quickly somebody who is a possible competitor could develop technology that would surpass what we have. Would you be interested in working on that?"

"Of course."

They read the requirement. They said, "Well, all right […]." This was on, I think maybe a Wednesday. "Can you come out next week, make a proposal on what you want to do? We want three projects—three different ideas. They should be patentable. We'll give you six months to do this."

I said, "All right, why not?" Then they sent [an engineer] to coach me on how to address their project.

DOMUSH: Wow.

DESSAUER: Then Angela and I flew out to Portland [Oregon], and went to Corvallis [Oregon], which [is] a lovely city. I had a very nice relationship. I worked for them for six months. I got two patent applications out of it, one patent was issued.[42] They were happy with me and gave me a couple of printers. It was really enjoyable. […] They treated me like I was an expert on something. That was very good.

I had another situation that's—I mean it's sort of necessary to do some publishing now and then, so people know you. I had—if you look over there, there are a couple of things I have written, in addition to the book, the *Imaging Processes*. I wrote a chapter in there with Kay Looney.[43] The green copy was […] *Advances in Photochemistry* [Volume 1], on photochromism.[44] The second one over, that is on *Advances in Photochemistry* Volume 28], where I actually had the whole Dylux story as a personal memoir, which was sort of unusual, a technical book, to put something personal in there, but I did.[45] Then I wrote the book for Elsevier [B.V.].[46] [Bowling Green State University's Center for Photochemical Sciences conducted an interview with me in 2003.[47]] Now I'm trying to write an electronic book on color, which—I'm not sure where [it is] going. I enjoy writing it though; […] I go down to [the] Apple [Store] a couple of times a week, at the Christiana Mall. They help me with it. What I'm trying to do is reduce words to graphics, animated graphics, wherever possible. People, with the exception of the people at the Chemical Heritage Foundation, who have seen it, think it's very good. […] I wanted to give it to [CHF]. They haven't had any enthusiasm. I don't know why.

DOMUSH: Well, we don't really have…we're not really in the education business very much.

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[42] R. O. Willard and R. Dessauer. Leuco dye-containing coating compositions. U.S. Patent 7,462,443, filed 5 September 2003, and issued 9 December 2008.
[43] R. Dessauer and C. E. Looney, "Low Amplification Imaging Systems," in *Imaging Processes and Materials*, eds. John Sturge, Vivian Walworth, and Allan Shepp (New York: John Wiley & Sons, 1989).
[44] R. Dessauer and J.P. Paris, "Photochromism," in *Advances in Photochemistry*, Volume 1, eds. W. A. Noyes, G. S. Hammond, and J. N. Pitts (Hoboken, New Jersey: Wiley, 2007).
[45] R. Dessauer, "The Invention of Dylux® Instant-Access Imaging Materials and the Development of HABI Chemistry—A Personal History," in *Advances in Photochemistry*, Volume 28, eds. D. C. Neckers, T. Wolff, and W. S. Jenks (Hoboken, New Jersey: Wiley, 2005).
[46] R. Dessauer, *Photochemistry, History and Commercial Applications of Hexaarylbiimidazoles: All About HABIs* (Amsterdam: Elsevier, 2006).
[47] "Perspective on the origins of industrial organic photochemistry," *The Spectrum* 16 (2003): 4-10, 17.

DESSAUER: Well, […] for instance, if they could mount it over that exhibit in your museum on color, it would tell people a lot more than the exhibit would. I mean, it's—I'm still going to propose doing that. For instance, one thing that's actually very disappointing in this whole exhibit there, they don't have one single chemical structure. You know, this is chemistry, and it ought to have a chemical [symbol] of some kind. There's so much more than you can show. So I'm still going to see if I can pass it on to someone.

But I think it's important for [me to publish], as much as it's difficult to get patents and to get your name out so that at some point, if you want to consult, people know about you. I was fortunate they knew about me. I think I had a nice career, I felt […] chemistry is still interesting. I wouldn't mind going back to work. I find retirement is too strenuous. You don't have time for anything. […] I work [on] my electronic book. […] I do some consulting still for a company <**T: 45 min**> that is working on wave guides. I try to read and stay abreast of technology.

There's one very interesting sidelight to all of this. I don't know. Did we talk about watermarks at all, yesterday or not?

DOMUSH: We didn't.

DESSAUER: No? Because that's sort of interesting. Postage stamps used to [contain] watermarks. Paper used to have watermarks […] as security. […] The way you see whether you have a watermark was […] dip it in a little bit of benzene and hold it up against the light, and [there is some] transparency that is created that makes it possible to see this watermark.

One of my friends, who was a stamp collector, came to me once and said, "Do you think I could use Dylux [paper] to record watermarks?" In other words, put the stamp over a piece of Dylux paper, shine light through it, and get a record of it.

Well, "I think it takes a long time, but if you've got—"

He said, "I've got time." So he actually found out that watermarks could be recorded on Dylux from stamps. Well, he sold his stamp collection, but he kept very involved in going to places like the Library of Congress, the Winterthur Museum here, and so on, to record […] watermarks on old paper documents, and so on. […] And he published.[48] He published wherever he could. We went also to places together like the Folger Shakespeare Library, and we watermarked some of Shakespeare's folios, things like that.

[48] T. L. Gravell, "A New Method of Reproducing Watermarks for Study," *Restaurator* 2 (1975): 95-104; T. Gravell and G. Miller, *A Catalogue of Foreign Watermarks Found on Paper Used in America 1700-1835* (New York and London: Garland Publishing, 1979); T. Gravell and G. Miller, *A Catalogue of American Watermarks 1600-1835* (New York and London: Garland Publishing, 1983).

Well, Tom [Thomas L. Gravell] passed away a few years ago. In the meantime, I decided, maybe with my expertise on Dylux and having a stamp collection, I would become an expert on watermarks on stamps. I have probably the only collection of watermarks of the stamps of Bermuda—that anybody in the world has. […] It's sort of interesting, because stamps used to be produced on sheet-fed presses. You know, that means you put a sheet [of paper] into the press, and so on, instead of rolling through. Sometimes a sheet would [be] put in upside down, so sometimes the stamp is a great deal more valuable because of the watermark being inverted or otherwise displaced. So I have this nice collection of [these] watermarked stamps. I think someday [I] hope to sell that. Anyway, I'm still kept fairly busy. I don't get around to doing a lot of things that Angela wants me to do, but we, I think, are reasonably content with what we've achieved.

DOMUSH: You guys do a fair bit of traveling as well.

DESSAUER: Pardon?

DOMUSH: You do a fair bit of traveling?

DESSAUER: Well, I would say, because my wife's family is in Germany, […] we used to go to Europe once or twice a year. But I think in recent years, [my] enthusiasm for traveling to Europe [has] somewhat diminished. I find everything connected with flying has become a pain, so our usual travel is to drive down to Florida and get on a cruise ship, and spend a couple of weeks on a cruise, and then go travel around Florida for a time. […] The only enjoyable flight I've had in recent years was quite an adventure. I took ill on a cruise ship, and they flew me back to the United States in a private jet.

DOMUSH: Oh, wow.

DESSAUER: It was not the ideal way to travel, but, I mean, it was a nice flight. […] We used to go to New York a lot. I think we go less, but we go to Philadelphia [Pennsylvania] a great deal, […] almost once a week. We like to eat out. I find the days are never long enough. Unfortunately, there's too much <**T: 50 min**> to do. I don't read scientific journals a lot anymore, hardly. I look at *C&E* [*Chemical & Engineering*] *News* quite thoroughly. I do follow the literature on biimidazole chemistry. I check the patents every once in a while, and have a big tabulation of all the patents. I have also a fairly long collection of all the [Chemical] Abstracts literature […]. I don't know that's of any importance, except to give you the feeling this is something that I should know more about. I intend, time permitting, to write a section about biimidazole chemistry for Wikipedia. They don't have it yet.

[…] One thing that was very enjoyable, which I mentioned yesterday, [was meeting] Professor [Douglas C.] Neckers at Bowling Green [State University]; he had invited me for a week to give five lectures on whatever I wanted to talk about. I gave a lecture on dyes, a lecture on Dylux, so on and so forth. I've been out to Bowling Green a number of times, and [I've] given talks. The last technical talk I gave was on the occasion of his seventieth birthday, they had a symposium. I talked about some chemistry that was [under-reported, using technology that I] don't understand, and nobody else does. We had a system then, you could mix two chemicals, of which one of them was a biimidazole, of course, […] and [another ingredient], a plasticizer. Depending on the ratio that you have, when you expose it to light, it will change from tacky to not tacky, or […] [with another ratio] the other way around. […] Why that is, and I haven't found anybody who has. I hope that if I talk about it long enough, somebody would take it up. But nobody has, so here we go.

HUNTER-LASCOSKIE: I'm curious. You know, you have this education focus, from the things you've been involved in after you retired. But were you involved at all in any kind of mentoring too? I mean you had an opportunity through a really long career to interact with a lot of different people…

DESSAUER: Well then, [there are] some people who claim or accuse me of having taught them everything they know about this, that, or the other thing. Whether that's facetious or not, I don't know. […] The people in Towanda, […] who didn't have all of the background that I did, and who had been in charge of manufacturing Dylux, would call me when they had problems, or they would ask me to give talks. So through my career, and post-DuPont career, I gave a lot of talks at DuPont about certain aspects of this technology.

The unfortunate thing is, [in certain] areas like dyes, and pigments, the technology has really moved to China, and to India, so there are fewer and fewer people who really know the difference between this, that, and the other thing. It's—you can't be an expert [in] everything, and so I probably know more about dyes and photochemistry than a great many people. I don't know everything, of course. But, I mean, I could make useful contributions if somebody wants to know.

Of course, now that the industry has moved offshore, the universities don't teach color chemistry anymore either. [When] I was in Wisconsin, there was only one course [taught] by [a] professor that dealt with dyes. Dyes were basically the beginning of organic chemistry. I mean everything that came—even the pharmaceutical industry [came out] of dyes <**T: 55 min**>. I don't know if you know that. I mean going back to my father's professor, [Paul] Ehrlich, although I think he probably didn't know my father. He started off staining bacteria. Then he found that if you put some arsenic into some of [these] molecules they would stain or maybe kill them. That worked pretty well. That was the beginning of pharmaceutical chemistry.

[One] of the problems with chemistry nowadays is that [if] you have a new material in hand, it's used [only after] it has passed all the tests, so you're [not] going to like to make new

materials, except in the pharmaceutical industry, and you wind up with more people who measure things, that do different kinds of chemistry from when I grew up. I always thought you could solve most problems by synthesizing the molecule that would do what you want it to. This is no longer the case.

When we started on this whole business here, we thought, well, we would make a molecule that works with [deep] UV. We found that, [ortho-chloro-HABI had an absorption] tail that was a little into the visible spectrum, so I thought, "We're good enough chemists, we could add a molecule with a tail that's disappeared." Well, we never did, but, I mean, that was the attitude we had in those days. But nowadays you wouldn't approve that. You'd mix some other things in it, all chemistry would change.

To me, the real question or contribution that I made, I think, was that I kept [this chemistry] alive. […] I kept on finding people who were supportive. I think I was always honest about the limitations, although possibly somebody occasionally misunderstood me. I felt, given enough resources, we could have done much more, much better. I mean there were other opportunities.

Now you have to recognize that inkjet didn't exist before 1976. Inkjet did not—and inkjet will do many of the same things that we could do here, and a lot of them perhaps even better. What it couldn't do was to shine light to form color through a negative. The negative—in proofing—the negative was important because that's how you [made] a printing plate. So if you had a negative doing anything with inkjet, it wouldn't work.

Now people proof using [displays] […], to show a result. These are all new developments. Probably if we had come up with Dylux now, we wouldn't [even] have the proofing [business], because other technologies have surpassed us. I mean there was no electrophotography to speak of when we started, or no computers.

We were fortunate that what we had invented fit in at the right time. What always troubled me was that in the way that research at the DuPont Company is structured is that after a while when a project closes, it's all forgotten and people move on to other things. They can't remember. If you ask them five years later, do you remember what you did here? They say well, maybe it's in my report. But they don't necessarily have the continuity and interaction with what else has happened since then.

If you're in a situation where, right now, somebody comes up and says, "Gee, could you do that?" They would have to go to me, or probably no one, because there wouldn't be anyone to know what else we did then. So this question of whether you can maintain a stewardship in an area of technology past its original lifetime. Maybe it's a luxury, but maybe it's not. I think it's something to consider.

I'm blessed with a good memory, and I always seem to remember things in the past so well. Other people <**T: 60 min**> have a very great talent. They can exclude things that happened at a certain time, and they don't worry about it anymore.

DOMUSH: Do you think that the need to commercialize products at DuPont forces people to just keep moving forward with something, and hinders their ability to really concentrate on something for fifteen or twenty years? If you were going to work on something for fifteen or twenty years, you're going to remember a lot more of the details, whereas, if you only get to work on something for one, two, or three years, maybe...

DESSAUER: [Yes]. I think—the question really is, how does the company innovate? Let's just say you and I are the head of this large company that has lots of money. We say, "Well, we want to make something that is really superduper that the world needs." So first of all, where do we get the idea what the world needs? Something that's damn hard to come up with [is] a fifty million dollar idea. It's hard to come up with a ten million dollar idea, but fifty million dollars is even….

But in my days, the company said if it wasn't a hundred million dollar idea, it was really not worth pursuing. This [was] part of the problem, inventing another Nylon or Orlon. It's a very formidable task. Then you go up against the establishment, which feels threatened because we've been selling this Nylon. So why do you want to come with another fiber? So you've got all of these internal issues, which…I don't know you solve them.

I mean, I remember on one occasion—I mean, by that time, I had perhaps gotten a little bit of a reputation of being innovative. Somebody came, and visited me and said, "I'm supposed to look at new opportunities. How do you do it?"

I said, "I think you've got to be the right person for it. Let me ask you one thing: Do you own a VCR?" This was maybe 1980.

"No, I don't own a VCR."

"Do you own a computer?"

"No, I don't own a computer." I kept on asking something else. "You know you're a nice guy, but somehow you're not immersed in new technology at home, so how can you be very immersed in technology for the company?"

I mean, I knew one example. Once upon a time there was […] Bill [William D.] Phillips, who was in Central Research. He was a physical chemist. He probably had the first NMR [nuclear magnetic resonance] machine that anybody in a commercial laboratory had in this country. He did some very, very impressive work. People were all overwhelmed by how good he was. He was a nice man, really bright. I found I could talk to him about things that—it was sort of like a scientific bull session. I'd say, "Could you do that?" He would say something that would inspire me to something else. We became pretty good friends. Then all of a sudden the company decided to send him to MIT [Massachusetts Institute of Technology] for a year to

work on, essentially, the intersection of biology and chemistry. He spent one year, then got to a job at planning opportunities. I think that was a good thing, whether all of the things that have happened since then can be attributed to Bill Phillips, I don't know. Of course, he retired—left DuPont and became a professor at Washington University in St. Louis, and died. So I don't know. You can't ask him.

I found a couple of people whom I knew, who were creative in that sense. I met [Benzion "Benny" Landa] who had developed an idea of using liquid electrophotography—liquid toner for electrophotography, and he attributed all sorts of […] things to that, that you could not do with [dry] powder toners. He […] talked DuPont, Xerox and Harris Printing [Company Inc.], whatever they're called, into a joint venture <**T: 65 min**> which ultimately became DX Imaging, where we were trying to use liquid toners for making proofs, and we made some beautiful proofs. It was a great idea.

He had his own company in Israel, named Indigo [Digital Press]. He sold the company to Hewlett Packard for a [billion dollars]. Now, he's off to something else called "nanography", where he's trying, I think, to marry nanochemistry with all sorts of printing opportunities. I haven't seen him for a number of years now. He […] is probably in his seventies now, who has, through his personal inventiveness and cleverness, developed new technology, but always in an area in which he had some knowledge. I mean he didn't go off and make better [automobiles] or something like that.

[In 1985, when we started the electronic imaging program, using Landa's liquid toners, our management found new ways to motivate us: if we had something that we could show at the printing equipment exhibition Drupa in Düsseldorf, Germany, in 1986, we would all be invited to visit Drupa at company expense. Feverish activity ensued, and proofs made with our new technology were shown, and with exception of Graciela B. Blanchet, who was pregnant, we all went to Germany. Landa said he would like to know more about the details of what we were doing, and asked if I could give a talk to his group. Smith said okay, and Landa flew six or seven of his staff to Düsseldorf, where I rented a room at the airport for a nice get-together. Afterwards he said, "You have to come to Israel and work with us". When I returned to the United States and asked Smith if he approved of that, and he said I should go. A few days later I received a phone call from Landa, and he said that he would come to Philadelphia to meet me at the airport, and we had dinner, and then he came to my house to set up my visit to Israel. He wanted to hire me as a consultant, but that was a no-no—after all, I was employed by DuPont. I suggested that he hire my former associate, Larry Cescon, who had retired to Italy. As a result, Cescon and I spent a week in Israel, and in lieu of compensation I received three days of sightseeing in Israel. It was an interesting visit.]

So I think the question really is, how do you develop enough expertise in somebody that he can see an opportunity for technology that he is familiar with? I don't know. If somebody said to me right now, what would you like to work on next? Well, I mean, there are lots of things that haven't been colored terribly well, like cement.

DOMUSH: When—I've spoken with some other people through these oral histories about innovation in the chemical industry, and one person remarked that over the last thirty years or so there's been an interesting move in the chemical industry; instead of just coming up with new ideas and then trying to force them on a market—trying to look at a particular market and see what they're missing. You know, as you just said, cement hasn't been colored very well. There are probably a lot of ways in which people would like to use colored cement better. That would be an opportunity for a market. But even with the people we've talked to in oral history and people having these good ideas, there is still this problem that we mentioned earlier, that overall these companies are conservative. They are risk averse no matter how much they want to innovate…

DESSAUER: Yeah. Have you read Steve [Steven P.] Jobs' book?[49]

DOMUSH: I haven't. I've heard all about it. My husband gave me a summary pretty much every day that he was reading it, because he liked it so much.

DESSAUER: Well, obviously Steve Jobs wouldn't have lasted two days at DuPont, and that's a given. How come Apple [Inc.] is as successful as they have been? Obviously, you do need people who are geniuses to do these mega advances. I mean, I don't know how you nurture someone like that in a company like DuPont, but] I've found […] heroes in the DuPont Company.

Bill Wartel managed to [invigorate] a marketing group that was pretty well demoralized because things weren't happening. He listened to people, and he was dynamic. He gave a talk to our group on a day in which I wasn't there. One of my friends said, "You're going to have a hard time. He doesn't like you."

So I went into his office the first opportunity I had. I said, "I understand there were some comments about me." He was a little evasive. I said, "Well, it doesn't matter. I think you ought to hear my story before you pass judgment."

He said, "What are you doing for dinner tonight?"

I said, "I'll call my wife and tell her I'm having dinner with you." We were at the DuPont Country Club until midnight. He put his arm around my shoulder.

He said, "I hear you, Buddy." The following day all the work on the Venture simulators ceased. Six people, who had done nothing but crank out numbers to put into the computer, were suddenly employed in new activities. All of a sudden things started moving. So he was great.

[49] Walter Isaacson, *Steve Jobs* (New York: Simon & Schuster, 2011).

[…] George Coraor [died in 2011]. <**T: 70 min**> He was an individual who could listen to people. He was technically very savvy. He was willing to do his homework. He was willing to learn. He encouraged people to do what they felt they had to do in order to do a good job. It was routine in our group when somebody new came in that you gave him three or four months to work, to read the literature, and to talk to people, and not to have to invent something the first or second day you were there. That's not heard of anymore in a company that assesses merit of everything on a weekly basis.

Philip Botsolas […] had been the district manager, a fairly high position in New Jersey, but he was a very combative person. He had a tremendous work ethic. He would be up in the dawn and late at night for whatever he believed in. […] I remember one time we went on a coating trial in Towanda. He decided he wanted to see what we were doing. Well, the coater broke down. He got us in the car. He said, "We go home."

I said, "Do you want to eat?"

He said, "We don't deserve it."

I said, "Look. We didn't do anything." We went home. I mean, I was enthusiastic about working with him, and we remained friends until he died [in 2009].

Al Maclachlan was one of the few people I knew who could actually work [in many] different areas and was smart enough and studious enough, but he would learn enough about it to do whatever had to be done. He was a very good chemist, and…

DOMUSH: That pathway that you showed us the slide earlier, of the Maclachlan…[50]

DESSAUER: [Yes].

DOMUSH: That's the Maclachlan that you're talking about?

DESSAUER: [Yes]. I mean I found the interview on my HABI book with him to be [great], here's a guy, very enthusiastic, realistic, [yet knew] his limitations.[51] He's done very well. Cescon was a very excellent chemist, who was willing to listen to people, just a pleasant person. Bob Cohen was there, was very good. We had good people. […]

[50] R. Dessauer, "AAH HABI Lecture," in *Photochemistry, History and Commercial Applications of Hexaarylbiimidazoles*. CD-ROM.
[51] R. Dessauer, "Maclachlan608."

DuPont had commissioned two historians to write a history of the scientific research at the DuPont Company. Are you familiar with that?

DOMUSH: I don't think so. I'm not…

DESSAUER: [Kenly] Smith and Hounsbury [David A. Hounshell].[52]

DOMUSH: I'm not familiar…

DESSAUER: Well, [in] my humble opinion, not very good. I can only talk about the areas which I'm familiar with. I think […] they interviewed managers who managed to give themselves a lot of credit for whatever happened. One man who looks liked he was [in charge] of everything was […] Abe [Abraham B.] Cohen. Abe was [very good], and I shouldn't diminish it. He had lots of ideas. He was very innovative, but he was a person who was difficult to work with. He did not give other people credit for what he claimed credit for. […] We were sort of contemporaries for much of the time. Abe just couldn't listen. […] If you would talk, he would just interrupt [with] his ideas. […] I found he believed that almost everything could be done with photopolymerization, from curing syphilis to flying to the moon. Fortunately he found out about biimidazoles, which made [much] possible, but he never gave anybody credit for the biimidazole work. If we had succeeded in establishing a good relationship with him, we would have invented better biimidazoles that would have made for [much] better products, and [we] would have done things we couldn't have done otherwise <**T: 75 min**>.

I had a number of bosses who I thought were very bright. I found, generally, they did a good job, but I felt in most cases they didn't risk getting fired, you know, for solidly supporting either their coworkers or themselves. In other words, for instance, when DXI closed up, there was a question of what to do. I was very disappointed because suddenly I was going to be unemployed. I didn't realize what was ahead of me. […] [So I videotaped] about ninety people. I took my video camera. I said, "I want to ask you a question. Do you think we could have succeeded?" That's the subject of maybe a book someday, if I live long enough. It was sort of interesting. With the exception of one person, they all—we all thought we could maybe get to the market with what we were developing if we had another year.

What we were working on was a big machine, which was originally supposed to cost a hundred thousand dollars. Instead it [would] cost […] two hundred thousand dollars. [It] would take […] lithographic negatives and [make] proofs using electrophotography on plain paper.

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[52] K. Smith and D. A. Hounshell, *Science and Corporate Strategy: DuPont R and D, 1902-1980* (Cambridge and New York: Cambridge University Press, 1988).

[…] It was a big machine, and I have pictures of it somewhere. […] At the time we closed down there were two hundred people working on it.

I was working on the films that were required as intermediates from the photographic negative to the paper. The rest of the group I was in was working on liquid toners. They did a good job. […] I mused about it. I thought, "Well, we had eight machines that we had built." They were pretty good solid machines. One of them had been flown to Europe for a tradeshow [Drupa in 1990]. We actually found people who wanted to buy it. I don't think it was a big deal of making the film, because I knew how to do that. I figured the liquid toner we could get made because that was a technology that at least was documented. So I thought, "Well, why don't I buy three machines and start myself a business?" […] I probably would have had to mortgage this house heavily. But I thought it would be an interesting idea, maybe.

The DuPont philosophy was always to work with an uppermost technology to see that we would have the highest quality. I said, "Well, maybe we could find a middle ground for people who weren't intensely interested in having something that was perfect, but something was good enough for their market. We knew there would be people like that. So I said, "Why don't I buy the machines? Three machines." And I figured if I get three machines, I could hire a couple of my fellow employees, and pay them about as much as they were getting before, and cut them in on the profits. I might have a business. It sounded good.

I did all sorts of [studies]. I worked for months on spreadsheets proving how we could get government support, and doing it in Chester [Pennsylvania], which is a depressed area. I had it all figured out. My main concern was there wasn't a heck of a lot for me to do, because we had enough material technology developed that that wasn't the limitation. But I thought this would be fun.

Our business manager at the time was […] [Michael] Kullman, who [is] Ellen [J. Kullman]'s husband, so I said, "Mike, why don't we talk about this?"

He said, "Well, I'm very busy."

"Why don't you come over for breakfast, and let's talk about it?" So he did dutifully come here for breakfast, but he wouldn't sell me any machines. The machines were all stomped to death in an automobile graveyard. I said, "Look. What a glorious opportunity if this thing succeeds. DuPont can buy it back from me. If it fails, no loss." I think they were more interested in the tax write-off, I suspect, than in making me rich.

So here, you know, people—I mean there are a lot of people who, I think, want to make things go <**T: 80 min**> and try very hard. A lot of them have—I'm sure there are dozens of people at the DuPont Company who have similar dedication to what they have done and believe that what they did should have been commercialized, or should have been made into successful products. They have a hard time. Maybe it's good they have a hard time, because a lot of those good ideas aren't so good.

My disappointment is that nobody's ever asked, you know, at upper management level, ever, "How the hell did you guys do this [Dylux thing]?" Because I think we pulled it off. It was quite a remarkable achievement. We took some chemistry that we had intended for a particular opportunity, expanded it; developed a whole flock of products that used the technology; made a great deal of money, and what else can they ask for?

As I said, I had a good time. Would I tell anybody else to do the same thing? Probably, it'd be fun, but I think the parameters for everything have changed. I mean, life is so different. […] I still have some friends who are my age, or a little younger. We often talk. Well, should we have become chemists or not? I still think I would like to be a chemist, but a lot of people don't.

DOMUSH: No?

DESSAUER: No, and I…

DOMUSH: Your friends say they would choose something different?

DESSAUER: […] I was associated with [several chemists] at the Wilmington Ski Club years ago. We have lunch once a month, or so. I mean, this is sort of, I guess, fashionable for retirees. […] Inevitably discussions are filled with issues of health, but nobody [is] very interested in talking about science. I'm still interested in science; I still like to watch *Nova* whenever possible.[53] I'm still happy when I consult with someone. I'm [happy] when I read books or so. So I find some people are trying to retain this youthful enthusiasm and others move on to other things. So I think I'd be happy to go back to work. It's probably still interesting.

DOMUSH: Do you have any other questions?

HUNTER-LASCOSKIE: I don't. Well, I think our questions are kind of over with, so is there anything else you would like to add, or anything we didn't discuss?

DESSAUER: Let me think. Well, I feel a couple of things are important in my career at least. One was, I had [good] health all the time. That's important. You have to be able to concentrate on what you're doing, and not be worried about pain or agony. I think it's helpful if you have

[53] *Nova*. Broadcast on Public Broadcasting Service, produced by WGBH Boston.

enough resources that you can risk getting fired for things, if you get into too much difficulty. I think it's important to cultivate friendships with people who are similar to you. I'm still good friends with Rudy Pariser, who has been involved with Chemical Heritage Foundation and got me involved with it. I think it's probably nice to live in an area where you have access to [organizations] like Chemical Heritage Foundation, or University of Delaware, [or the Hagley Museum and Library], or whatever, that you can interact with people who have similar interests.

I think my educational background probably was good enough for the job that I was to do. I found very few people, with the exception <**T: 85 min**> of this director of research of Photo Products, Upson, whom I had difficulty with. I don't know what really bothered him about me, but he sure was bothered. He was not a popular person, and I could understand why. There are lots of stories about how unpleasant he was to other people, so I wasn't unique in this.

[…] I mentioned that Phil Wingate, the [VP] of Photo Products, [who got us money to do] research. But I needed a lab where [I] could do photochemistry in, and such labs were available at the Experimental Station, but [Upson] arranged for me not to get one. So […] after we got the project approved, we had to build a photochemistry lab over at Jackson Lab, which cost a great deal of money and time, and delayed any work. Really, in many ways, I thought [he] was evil. I'm sort of glad I outlived him, so I can tell the world what an evil S.O.B. he was. I never found anybody who liked him. But there were lots of people to like, so it wasn't too bad.

[Another] thing that is interesting: A few years ago, the question was, what do I do with all the notes that I had collected for my book? Somebody suggested that I go to Hagley. You're familiar with Hagley? Of course, both of you. Well, you know, Hagley was started as a repository for the records of the DuPont Company. When the building was finally open and they wanted to get the records, the DuPont lawyers decided they would have to go over every document that [was to be turned over] before they could do that, so they weren't very interested. DuPont decided not to do that.

I [went] to Hagley, [about] twenty-five, thirty years ago, with my friend, Tom Gravell in connection with watermarks. The then director of Hagley and I had a nice chat. I said, "I'm collecting a lot of stuff. Can I ultimately deposit that at Hagley?"

He said, "Oh, yes. We can take private donations."

About three years ago, I went over to Hagley and I talked to Lynn Catanese. She said, "Oh, [yes]."

I said, "Would you like to come over here and see what I have?"

She said, "You know, we don't have any record of any chemists. You know, just papers and statements, and so on. So we'd be very glad to take them." So I turned over twelve boxes of loose-leaf notebooks. I have now gotten through two of [those boxes]. I have been making an index of the various contents of these. There's another twelve boxes sitting in the garage. So

at the rate I'm going, I'll to have to be about a hundred-forty years old before I complete my task at [Hagley].

But, you know, for instance, I saved these samples that I showed to you yesterday. Maybe someday, somebody would be interested, or maybe not. Or maybe someday, somebody might want to exhibit something like that. I think it might make an interesting exhibit at Hagley or […] the Chemical Heritage, if they wanted. These are visual things that you can see.

So you know, on one hand, you take a lot of grief at home. Why are you saving all that junk? On the other hand—and [there is] some [cultural] value, I think for us to preserve these things. You know if Mrs. Gutenberg had said to her husband, "Forget this printing press!" then…

DOMUSH: Where would we be?

DESSAUER: Right. You know, I [want] to start my book, and I think I actually did [like this]: These different people approach a company like DuPont with inventions. So the first guy was Thomas [Alva] Edison. So they said, "Tom, what have you got?"

"I've got a light bulb."

"What? A light bulb? It means it's not very heavy bulb?"

He said, "No, no. It's something <**T: 90 min**> that gives off light. You know, so you can see at night."

"Why do you want to see at night? You've got kerosene lamps. If the Lord had wanted us to see at night, he could have turned the solar system on. [You] would belittle all of him."

Then, "Well, how does this damn thing work?"

"Well you have wires, and you bring electricity to the lamp."

"What do you do with all of those wires? [They are a tripping hazard.]"

"I don't know. You put them on the floor, and put something over them, or maybe you can put them in the wall."

"In the wall? You know that's hazardous. Suppose a fire breaks out, or short circuits and the house burns down? We'd be sued. By the way, how do you know that this light bulb isn't going to affect people's eyesight? There's a big danger in all of these people suing this company, because their eyes have been ruined."

So the next guy comes in was Henry Ford. "Henry, what have you got?"

"I've got a car, an automobile."

"Where is it?

"It's outside."

"Well, can't you bring it in?"

"Well, it's outside."

"So what are you going to do with it?"

"Well, it's got an internal combustion engine, and it runs. You have to have some gasoline in it."

"Doesn't that gasoline burn?"

"Yeah, it's explosive."

"You want people to ride on twenty gallons of explosive mixture? That's very hazardous. Where are you going to go? Aren't you happy where you are?"

He said, "Well, I don't know."

"You know, we don't have any roads for these automobiles. Why don't you come back when the roads are built, and let us know if there's any business for the automobile."

You can take any invention, if you're clever enough to do this. I think Bob Newhart used to do that. I don't know if you remember Bob Newhart. I mean, he had a couple of episodes, I thought, boy, I could have given him material. For every enthusiast, there are probably five people who are naysayers, corporate naysayers. They can sink something irretrievably. This makes it difficult for the innovators.

I think that, probably, culturally, the period when we did much of the good work in 1960 [to] 1965, really I think was a time—it was post-Sputnik. [People were] interested in science. I think the Company at that time still had a reputation that people really [wanted] to come to work for DuPont. We got very good people. I mentioned it yesterday that we had people coming in on weekends and nights to work on this thing because they were so interested. I don't see that kind of enthusiasm anymore. But maybe it's there and I just don't know.

But I think on a personal level, I found I had a good time. It was, financially, reasonably rewarding. We lived fairly well. I think that the question is, what else could we have done, and why didn't we? That's unanswerable.

But the question is, what do I do with all this stuff I've got? Will Hagley continue to be a repository? But I don't even really know exactly how to handle Hagley in the best way. I don't have any—do you know Erik Rau?

DOMUSH: No.

DESSAUER: Well, he's the new director of the library. I've talked to him. I wasn't quite sure. Because I figured if I give them my papers, they belong to them, and whatever I want to do with them needs their approval. I still want to do something. A couple of weeks [ago] they had a very small symposium. There was a man from the [Georgia Institute of Technology] there named [Steven W.] Usselman. Does that name ring any bells? He was discussing, basically, the issue of time relative to events and how long did it take to bring things to fruition. I thought I had some insight. I could have some <**T: 95 min**> supportive data. So I thought maybe I could get someone to study this, […] I don't know if it's worthwhile.

I just think [the Dylux development] was an eminently successful thing that somebody ought to get something out of it that, they could apply to another area or another activity. Whether that's just personal self-aggrandizement or whether it's real, I don't know. I mean, what I would take out of it is simply that we were fortunate we could interact with people who could contribute and they were not necessarily part of the team, but they were interested in science, and what we were doing.

So I guess I have told you in some detail some of the things that I felt or know. If you wish, particularly for the file document that you have, I will give you an image that you can include in it; you'd like a piece of Dylux paper…

HUNTER-LASCOSKIE: That would be great.

DOMUSH: That would be wonderful.

DESSAUER: And I showed you this morning some of the slides that I have on it. If you wish, I could give you a DVD with some of this information on it. Then we can refer to it if you think that's…

HUNTER-LASCOSKIE: Sure, absolutely…

DOMUSH: That would be great…

DESSAUER: That could be worthwhile.

DOMUSH: Yeah. No, I think that would be great.

DESSAUER: And I have scanned a lot of material such as some of the trade literature and so on, if you want that. You know, I mean I can give it to you. It's no pain, because it's already done.

DOMUSH: Great.

DESSAUER: Is there anything else that you could think of that you would…

DOMUSH: Well, I know that we're going to take some pictures, but I think we're all set with the recording. So I think…

HUNTER-LASCOSKIE: Yeah, great.

DOMUSH: I think this has been wonderful. Thank you so much.

HUNTER-LASCOSKIE: Thank you.

DESSAUER: Well, yeah. Well, I enjoyed it. I think the—well, let's go through some…

[END OF AUDIO, FILE 2.2]

DESSAUER: […] Cromalin, which was [unusually] successful, was primarily positive working, which meant that if the area that was exposed became hard, and the unexposed areas accepted a colored powder or toner, but [with] that technology we were able to make positive working proofs.

We'd never made a very good negative proof with this technology. So two of my colleagues [at DuPont's Neu-Isenburg laboratory, Werner Abele and Mario Grossa], developed

the following technology. They coated a white surface with a mixture of biimidazole and some other [chemicals]. It turned out that the exposed areas became tacky. The unexposed areas remained non-tacky. That meant—now the areas that you shine light through could be toned to be colored. So this was a negative image.[54]

Now the idea was then you'd have a negative proofing system, which would complement the positive proofing system that was known as Cromalin. They produced some very nice coatings, but there was one limitation. Once you had toned an area, it was no longer photosensitive. So if you wanted, say, to have a […] green image, you would want to tone blue and yellow, because you had to use the printing colors cyan, yellow, and magenta. Of course, you couldn't tone over an area that had been covered with toner. So this project never became a big hit.

Mario Grossa gave a talk on that in [1979 at] Washington, D.C., at one of the SPSE, Society of Photographic Scientists and Engineers, meetings. Somebody from RCA came to him and said, "We'd like to work with you on this."

He said, "What for?"

He said, "You have the ideal medium for making color television tubes." Well, we didn't know how color television tubes were made. As a matter of fact, hardly anybody knew how color television tubes were made in those days.

It turned out that we could coat the glass of a television tube with a mixture of these chemicals, expose them through a mask, and tone the red phosphor, the green phosphor, and the blue phosphor. We didn't want them to overlap because that would destroy the purity of the color. So RCA got very excited about this. We […] agreed we'd work with them. Since RCA was in Princeton, New Jersey, at that time at [the] Sarnoff Lab [David Sarnoff Research Center], I got that project to work on, and with some help from Mario.

We were doing pretty well. The people at RCA liked this idea, because it involved coating, exposing, toning, exposing, toning, exposing, toning. This would be seven steps. What they had hitherto been doing was they would coat the glass with a mixture of toner embedded in a photopolymerizable matrix. So you toned this, and then you had to wash off the unexposed area, dry it, recoat it with another color phosphor, wash and dry it, and re-expose it again.

So I think that, actually, they figured out there were twenty-three steps in the way they were making television tubes, compared to what we had to offer, which would be a lot fewer and faster. So they were very excited, and they invited us to visit labs at their manufacturing site. They had one up in the Scranton [Pennsylvania] neighborhood and one in Lancaster [Pennsylvania]<**T: 05 min**>.

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[54] W. Abele and M. Grossa. Negative tonable systems containing dihydropyridines and photooxidants. U.S. Patent 4,243,741, filed 21 December 1978 and issued 6 January 1981.

I got to work with a nice lady in RCA named Phyllis Brannin. We're still [friends] after all this time. […] Well, this was about [1982, 1983]. Then RCA [was] taken over by General Electric and they changed the orientation of their program. So we decided we'd ship it back to Neu-Isenburg [Germany] and have our lab there work with [Koninklijke] Phillips [Electronics N. V.]. Then Phillips, in Eindhoven, Holland, developed some specialty application needs for tubes. They were actually supporting the research at DuPont to develop that. It got up to a point then, it turned out in the end, they had all this automated equipment for making tubes. I mean, when you saw all of these tubes being made automatically, and you have to reinvent all the equipment, they decided it was too expensive. So that project failed.

But we could make cathode ray tubes by a totally simple process. Of course cathode ray tubes now are pretty well history, because we're using LCD displays or plasma. The one thing I mentioned to Ben [Benjamin Gross] was that he might be interested in that interlude. We were in the tube business. I've got to invite him to come down here sometime.

DOMUSH: It's an interesting overlap.

DESSAUER: [Yes]. So that was called add-on toning. That was the technology that I said I never could understand exactly how it worked because, depending on what ratio you used, it became tacky or non-tacky. I haven't found anybody who knows how it works. So much for science.

HUNTER-LASCOSKIE: Right. Well thank you for that addition.

[END OF AUDIO, FILE 2.3]

[END OF INTERVIEW]

APPENDIX I: Video Interviews

INTERVIEWEE: **Rolf Dessauer**

INTERVIEWER: **Hilary L. Domush**
Sarah L. Hunter-Lascoskie

LOCATION: **Wilmington, Delaware**

DATE: **1 October 2012**

DOMUSH: Okay. I'm going to just get the…best…okay. So we're going to start recording, and…

DESSAUER: And this is a painting of Paul Ehrlich, a German physician, chemist, scientist, who won a Nobel Prize [Physiology or Medicine, 1908] sometime in the early part of the 20th century. My father was a physician, and he interned in the institute that was headed by Ehrlich in Frankfurt, Germany. And at one point, a painting of Ehrlich that looked very much like this appeared on the cover of a German medical magazine, and my dad, on a vacation, bumped into the painter who made that painting. And my father said, "Would you please make a copy? Paint another one for me?" And he did. This painting hung in our living room in Nürnberg, Germany, where I grew up. And I always thought it was my grandfather. I had no idea when I was a child who Paul Ehrlich was.

Well, there was a rather unpleasant event in November 1938 called *Kristallnacht*, where the Nazis sent troops, or storm troopers, into Jewish homes and tried to demolish as much of the furnishings as possible, and this painting was a victim of that also, and it was torn to shreds. And we were about—my father was about to throw it out when, the day after *Kristallnacht*, a gentleman appeared in our apartment and said he was a restorer of paintings, and he had heard of my father's loss, and he would like to restore it. So we gave it to him, and two days later, he came back with the painting under his arm, and it looks pretty much restored. My father said, "Well, I'd like to pay you for that." And he said, "No, no. I want to show you that not all the Germans were in sympathy with what happened on *Kristallnacht*." So the painting hung in our living room again for a couple of days before we left the United—left for the United States. So that's the history of this painting. I don't know the name of the artist, and I think that at some point in time this painting will be donated to the Chemical Heritage Foundation. So that is the story of the painting of Paul Ehrlich.

HUNTER-LASCOSKIE: Great.

DOMUSH: Great.

DESSAUER: Is that what you wanted?

[END OF AUDIO, FILE 3.1]

DESSAUER: It's necessary to have on paper some kind of a copy that allows the user to see whether the negative is free of pinholes, or free of typographical errors, or that the paint—pictures are upside down—are not upside down. So anyway, proofing is a way of doing that. Our technology consisted of holding a fairly complex mixture of organic chemicals onto paper, and then this paper was somewhat yellow in appearance, and I will now illustrate to you how it's employed.

Here's a sample of a photographic negative, and here's a little printer that contains both visible and ultraviolet light. Right now it's set for UV, [flips switch] and now it's set for visible light. Dylux is used in this manner. The paper is put behind the negative, and the UV light is turned on, and now as this exposure takes place for a period of about 30 or 40 seconds in this device (but in actual practice, the commercial machines do it in about 10 to 15 seconds) and an image is formed without any processing, no toning, no heating, no washing, anything of that sort. So if you look at what we've got now, you can see this image. Now you notice the image is blue where the light went through the negative. It's yellow where no UV impinged on the paper. Now to stabilize this image, we need to expose it to visible light. Now we can leave it sitting in room light, or we can intentionally expose it. Now I'll cover a little bit of the—so you can see the difference. So now we expose it to visible light, and a series of chemical events take place that desensitize that area that has not been previously exposed.

Now we have [the] option of making this go at different speeds. It's convenient that the sensitivity to visible light is not as great as it is to UV light, so that we can actually use this in room light. You notice that most photographic materials are used in dark rooms. Dylux could be used under ambient conditions, so that's a plus.

Now why is all this so useful? Well, it's a very fast way of generating a proof, and it is used in commerce to do just that, and DuPont commercialized this in 1969 under the name Dylux proof paper. […] And it was a product that DuPont sold until about [2010]. [It was a great financial success: DuPont had the chemicals manufactured outside the company; another company, who also packaged it, coated the paper. All DuPont had to do was control the quality and ship it to customers. Then] suddenly—the printing industry switched to different technologies [as digital imaging supplanted analog imaging].

Okay. If you can look at this here, you can see we have essentially a white background here. It's still yellow here. Now it's obvious that we can do something else. This is the same paper. This time we're going to expose it to visible light through the negative. I'm exposing to visible light. It's sort **<T: 5 min>** of interesting. These little printers allowed DuPont salespeople to demonstrate a new technology in very little time and at great convenience. […]

Now if you look at this—can you pick it up with the camera? There's a difference between some areas are yellow and others are not. Now we can [make] what's called the flooding exposure, and we'll…. So I think this pretty well confirms that we have a system of technology that allows us to make both the positive and the negative on the same material solely by controlling the sequence of events, first in one case exposing to UV, and then visible light, or visible light first, and then UV.

And to illustrate that this actually can be coated on both sides, and image one side at a time, we have a book that I wrote where I persuaded the publisher to put in a proof. So that is pretty well the story of Dylux [503] proof paper. Now—well, I should say a couple more things. We also sold this as a near black paper, the black forming paper, but actually, people liked the blue, because it's different from the ultimate copy, so it wouldn't get mixed up. And I've been documenting all this, and I have some images that we made over forty years ago, and they're still in pretty good shape.

The—there were many—many people worked on this technology. I think I counted about two hundred people, contributors at various and sundry times to this technology. It's fairly complicated chemistry, but seems to work consistently. And I made this in my laundry room for this occasion. Okay? That's—is that more or less what you want?

DOMUSH: That's great.

[END OF AUDIO, FILE 3.2]

DOMUSH: So we were hoping that you could chat for just a couple of minutes. In your oral history at length, you talked about kind of the things that help to stimulate innovation and things that can hinder innovation. And I was wondering, or we were all wondering, if you had advice for a young chemist starting their career, particularly in industry, where there can be so many hurdles, what advice would you give someone who wants to be really innovative, who has a lot of good ideas? How would you tell them to go about trying to make them happen?

DESSAUER: Well, okay. Well, I've been reading the book called [*The Idea Factory*] about Bell Labs, and some of the things in there are really applicable to all the inventions that we did at DuPont, and the innovation. I was considered to be innovative. I was also considered to be a pain in the neck because I had ideas that didn't exactly conform to the status quo. And one of

the problems is that innovators have fights on their hands because they want to do something new and they want to do it differently, and the people who—whom you have to persuade, have to back risky things that may or may not pan out, and they're going to get caught and say, "Oh, you backed the wrong horse."

So innovation has to do a lot with persistence. I was pretty persistent. As you can see, I worked for forty-odd years on the same thing, and ultimately I was rewarded for it, but on the way there, there were a lot of pitfalls.

[…]

You have to remember, sixty years ago [there was not] quite the plastics business that there is now, and there were a lot of things that were different than they are now, but I was fortunate. I managed to get involved with people who wanted new things, and they weren't necessarily chemists. As a matter of fact, they didn't know any chemistry, but they said, "Oh, maybe there's a chemist at DuPont who can give us some samples that we could demonstrate something."

So I was fortunate, I had—I got out of the laboratory and got to talk to people who were interested in new technology, but frequently that technology wasn't exactly big. I mean, at DuPont we used to say, "Well, if it's not a ten million dollar business, it's not worth pursuing," but, unfortunately, it's very hard to think of many [ten] million dollar businesses, so I was sort of put in a situation where I had ample technicians and ample opportunities to go out and talk to salespeople or their customers and try to do new things.

At that time, DuPont had something called development conferences where we would invite another company to come, and they would tell us what they would want, but usually, this was attended by bosses, and then they'd tell somebody, "Oh, that's what they want," but nobody did anything about it. And my situation, when I went to actually make contacts with the people from the other companies, and really developed novel ways of dying anodized aluminum, for instance, and so this got me into a lot of background in the dye business. And I think I was sort of happy to get away from the laboratory. I mean, I really didn't enjoy synthesizing dyes all that much. I enjoyed finding things to do with those dyes that hadn't been done before.

And so by the time I'd been a dye chemist and in the dyes field for about ten years, I was beginning to have ideas of things that **<T: 5 min>** weren't available, and Dylux was a wonderful example. Somebody said, "Oh, we ought to do something about photochromic materials for Butacite," so I didn't just leave it there. I went and I got some Butacite, and I got some materials, and we tried to do it. And then when that didn't work out, we understood why the chemistry was the way it was, and we found other things to do with this chemistry. As a matter of fact, we got into hot water because we had told our bosses we had great opportunities in the photochromic field, and then when we realized there was something else we could do with the photochromic materials that wasn't photochromism, but forming permanent color, they said, "Well, you told us you were going to do that." And I said, "Yeah, but this is a…. Look at it this way. A photochromic material, we'd be testing for twenty years, and none of us would be

around when we're commercial, whereas if you had—and then you could reuse it over and over again, whereas if we made printed images, they'd be thrown away after a few days, and we'd have to sell more and more material." So in turn, we managed to persuade people of that.

So it involved persistence, and another thing which I think is important for innovation is you talk to people. In the *Idea Factory* book, the author says Bell Labs was in one big building, and the [director] who started it wanted everybody to use the same elevators, so people would bump into each other on their way to work or home, and talk about what they were doing.

[…]

[I found friends who were interested in what I was doing.]

In the sixties, there were DuPont people all over, and we represented different disciplines, and we were all—and I think we were all interested in what we were doing. So that's another aspect of innovation.

Another thing […] *The Idea Factory* says is […] that you have time to do this […]. You have to learn the technology that you are trying to do something with, and you need funding that goes over a longer period of time. [Malcolm Gladwell, in the best-seller *Outliers,* writes about the ten-thousand-hour rule, claiming that the key to success in any field is, to a large extent, a matter of practicing a specific task for a total of around ten thousand hours.[55]]

And so when you're in a situation where somebody says, "Well, this project has three months," that's not enough time. The department that I worked for was fortunate because we had two businesses that made money, no matter what we did. One was tetraethyl lead, and the other one was Freon. So our department was fairly wealthy, and they could afford to undertake long-term projects. So that's another thing. It takes time to develop ideas. You have to know what has been done before you can surpass. And so the question is, did we really give people enough time to understand the businesses that they're supposed to be innovating? So those are some of my ideas. But it's persistence, and I think it'd be nice if we had a reward system that encourages people for being persistent and for being pains in the neck to have their way, but it doesn't work out that way. I mean, I did all right, but I was sort of fortunate.

DOMUSH: Great.

HUNTER-LASCOSKIE: Great.

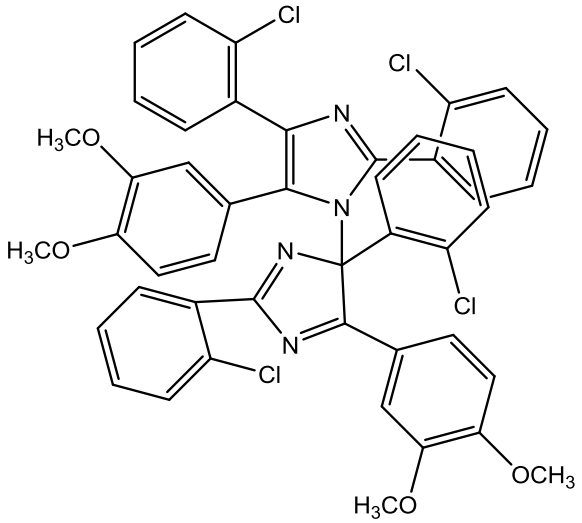
[55] In his book on the nature of success and expertise, Gladwell refers to scholarly interest in the amount of time required for experts to achieve their peak performance. The "10,000-hour rule" is terminology adapted from H. A. Simon and W. G. Chase's article, "Skill in Chess," published in *American Scientist* 61 (1973): 394-403, in which the authors observe that chess masters have spent between ten thousand and fifty thousand hours practicing the game. See Gladwell, *Outliers: The Story of Success* (New York: Little, Brown, and Company, 2008).

DOMUSH: Well, I think that was perfect. That was exactly what we were hoping for.

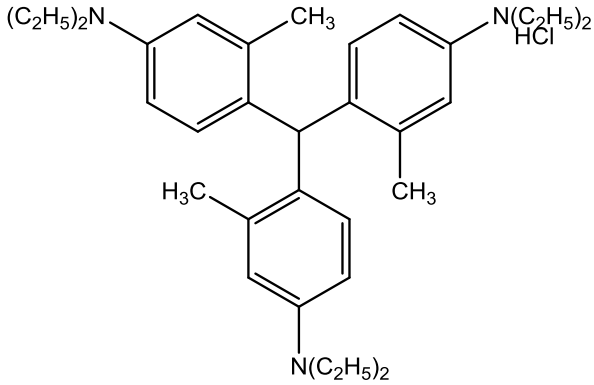
[END OF AUDIO, FILE 3.3]

[END OF INTERVIEW]

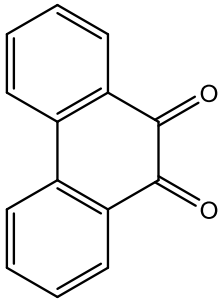
Appendix II: Dylux Chemistry and Image Scans



TCTM-HABI



Leucodye



9,10-phenanthrenequinone

Figure 1. Main ingredients in Dylux 503 Proof Paper coating. Clockwise from the top left, these compounds include a tetrachlorotetramethoxyhexaphenylbiimidazole (TCTM-HABI) photoinitiator, a triphenylmethane leucodye salt, and the hydrogen donor 9,10-phenanthrenequinone (9,10-PQ). The coating also contains a cellulosic binder, plasticizers, an antiblocking agent, and a silica compound that texturized the surface to allow for writing on the paper. Image courtesy of Rolf Dessauer.

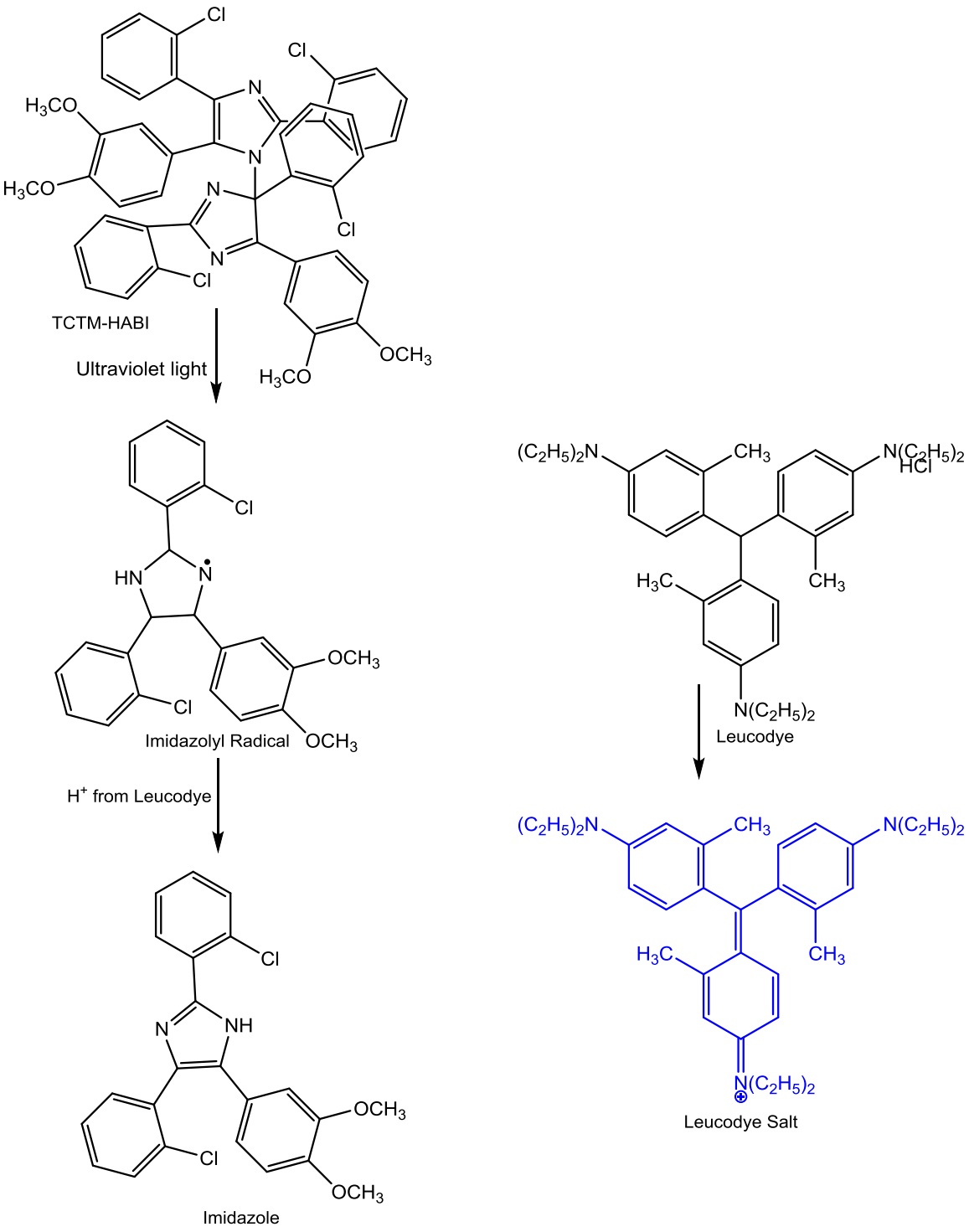


Figure 2. Dylux 503 color formation with exposure to ultraviolet light. The HABI molecule absorbs ultraviolet light, which causes the formation of the imidazolyl radical. The radical then abstracts a hydrogen from the leuco salt in order to make an inactive imidazole, yielding, in turn, a brightly colored leucodye. Image courtesy of Rolf Dessauer.

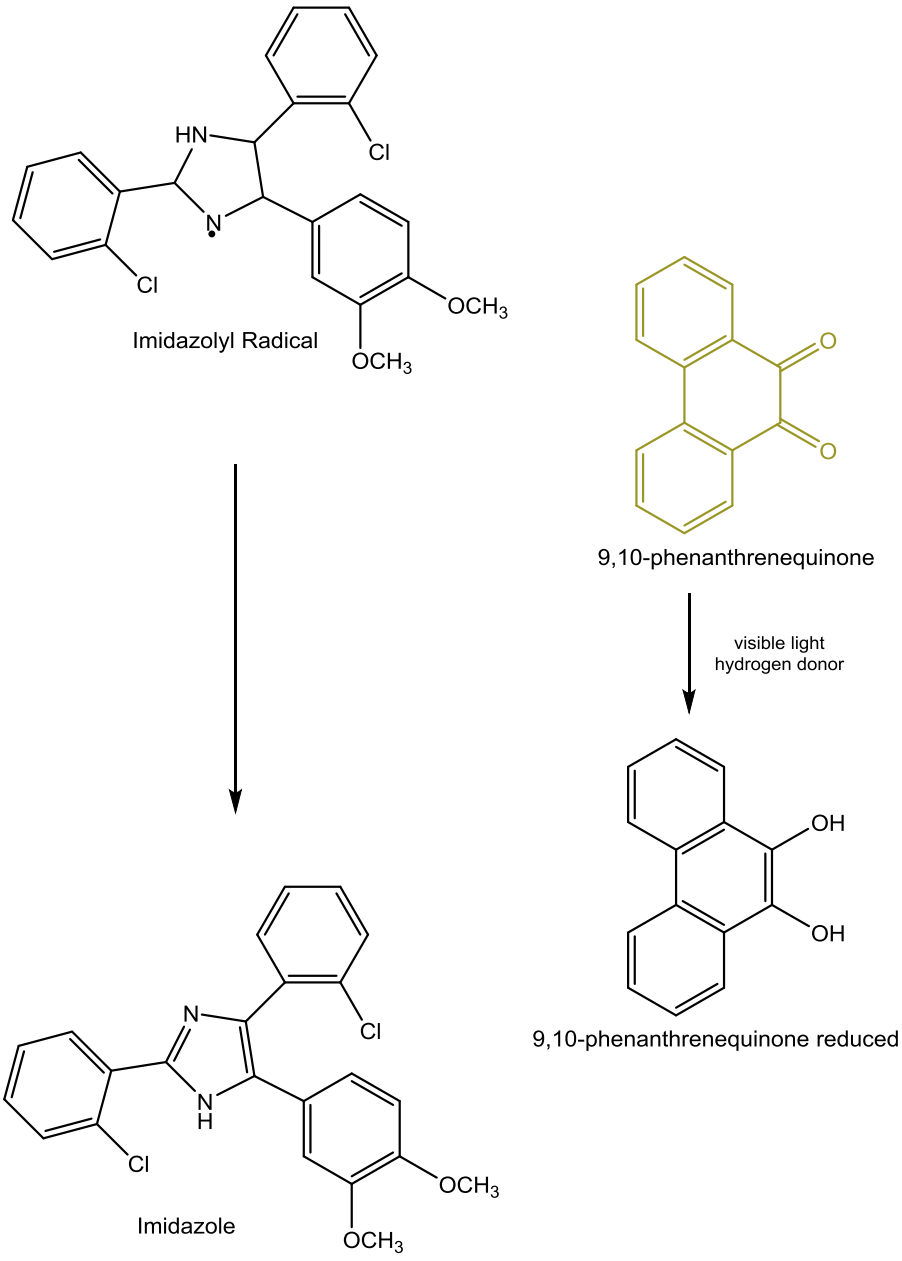


Figure 3. Dylux 503 photofixation. Visible light converts the 9,10-phenanthrenequinone to a reduction product, which plays a role in the formation of the inactive imidazole from the TCTM-HABI radical. This reaction between the imidazolyl radical and the reduced 9,10-PQ is faster than the radical's reaction with the leucodye, so this exposure to visible light stops the color formation process. Image courtesy of Rolf Dessauer.

Figure 4. Continuous tone image produced on Dylux 503 proof paper. Image courtesy of Rolf Dessauer.

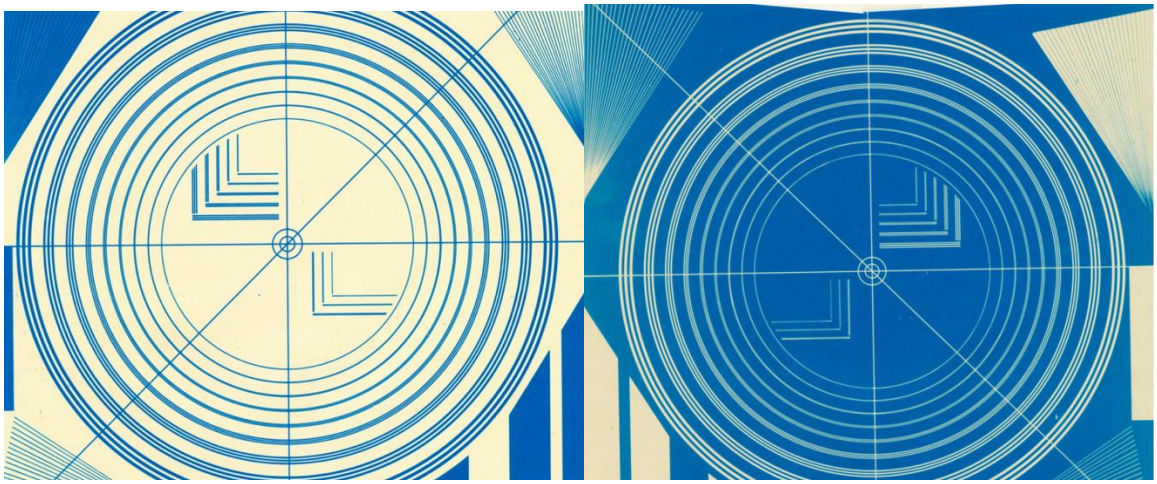


Figure 5. Positive and negative images produced on Dylux 503 proof paper; the different image modes are the result of particular exposure sequences. Image courtesy of Rolf Dessauer.

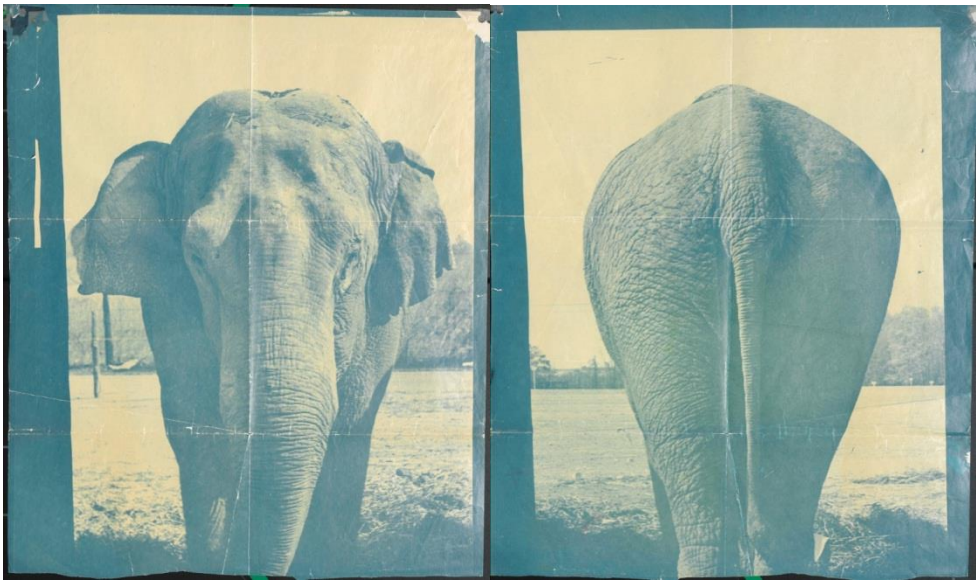


Figure 6. Dylux 503 was available as a two-sided coated product. Image courtesy of Rolf Dessauer.

Appendix III: Dessauer's Description of the DuPont Environment

The DuPont Company in 1952

When I joined DuPont at Jackson Laboratory, it was very differently organized from the way it is now: There were Operating Departments, which had a fair bit of independence; in some cases they competed with each other. These "Operating Departments" then were: Electrochemicals, Fabrics and Finishes, Film Dept., Industrial and Biochemicals, International, Organic Chemicals (dyes, Freon), Tetraethyl Lead, Petroleum Additives, Elastomers), Pigments, Photo Products, Polychemicals, and Textile Fibers Departments. Elastomers came in (in 1957). In time some of these departments split, or were combined, so there was considerable mobility. There were auxiliary or staff departments: Advertising, Chemical (sometimes called Central Research Department), Development, Economist's Office, Employee Relations, Engineering, General Services, Legal, Public Relations, Purchasing, Secretary's, Traffic, and Treasurer's Department.

The Operating Departments were headed by General Managers (later vice presidents), Assistant General Managers, and Directors of Research, Sales and Marketing.

There was a certain amount of secrecy between departments, and it was not always easy to make contacts at the chemist level, though the directors of course talked to each other. Patent applications were generally circulated through the entire company before they were filed.

The research was initially carried out at plant sites, but in time, the Experimental Station was to see personnel from all the Operating Departments. The Organic Chemicals Department had about twenty chemists at the Experimental Station, but employed many hundreds of chemists and engineers at Jackson Laboratory in the Chambers Works.

Jackson Laboratory

DuPont's Jackson Laboratory in Deepwater, New Jersey, was set up in 1917. When I arrived in 1952, it occupied five wings, and employed over five hundred chemists, engineers, and supporting personnel. The latter were organized into divisions, with division heads and supervisors; the divisions were grouped into separate business areas; e.g., dyes, petroleum products, fluoroproducts, etc. Assistant directors headed these. The laboratory was headed by a Laboratory Director (John Tinker, when I arrived), and everyone reported to a Research Director (Harold Elley, when I arrived, later Maurice Ernsberger) and an Assistant Research Director (when I arrived, Ernsberger, later Gerald M. Whitman), both in Wilmington. The latter in turn reported to a General Manager (John Daley, when I arrived) and an Assistant General Manager, Samuel Lenher, when I arrived). The laboratory was at Chambers Works, in

Deepwater, NJ, at one time one of the largest chemical plants in the United States, with over eight thousand employees. Also on the site were several sales service laboratories, e.g., Technical Laboratory, where dyes were evaluated. There were laboratories that concerned themselves with the manufacture, rather than research of various products. The Department, which had originally been named Organic Chemicals Department, became Chemicals, Dyes, and Pigments Department. After DuPont left the dyes business in 1980 it was renamed as Chemicals and Pigments Department. Jackson Laboratory and the Chambers Works were severely downsized, and Jackson Laboratory was torn down sometime after 2011.

Appendix IV: Dessauer's Travels

Japan

I should have talked a bit about my visit to Japan in 1977. The eminent Japanese photochemist, Professor Eiichi Inoue of the Tokyo Institute of Technology was quite impressed with Dylux and issued me an invitation to give a paper in Tokyo [Japan] in 1977. I was still in the Organic Chemicals Department, which did not want to pay for this, and photo products department, would of course not pay for it, so I paid my own expenses! I was well received by Japanese scientists and businessmen that I had met in the States. I had dinner invitations from Canon [Inc.], [the] Ricoh [Company, Ltd.], Fuji [Photo Film Co., Ltd], and others. The meeting with Canon was in a nice restaurant, and was attended by many levels: the president, the head scientists, the planning managers, and a few chemists. In my forty-odd years with DuPont, people at that level never invited me for dinner! Fuji threw a party for visiting scientists, and one of their people asked me if I would give my talk in their laboratory, but "slowly". I checked with the DuPont office in Tokyo, and they had no objection for me to visit Fuji. In the morning that day, I gave a talk to Prof. Inoue's group, and then was informed that a chauffeur was waiting for me at the entrance of Inoue's building. A limousine with a white-gloved chauffeur appeared and drove me to wherever the Fuji lab was. The Director of the Fuji Lab met me at the door, and he said that they had lunch waiting for me in his office. After lunch he took me around and showed me some of the work that they were doing, and then I gave my talk and answered questions. Afterwards, he asked me to come to his office again for tea. He then asked me to sign a document—for tax purposes, he said. I asked why; he said it was my honorarium (six hundred dollars), which I said I could not accept. We had quite a discussion about that, and I said in no way could I accept money from a competitor. He was very emphatic about giving me something; I finally said I would take a little camera as a token (they had just developed relatively compact "110" cameras). Next day, a small camera was delivered to my hotel room (I think it cost around twenty-five dollars at stores back in the U.S.! That was in 1977. In 1993, Fuji offered a negative proofing product somewhat similar to Dylux 503, but it required a heat fix. It also infringed on one of my patents, the one that involved phenidone chemistry. DuPont went to court on that one, but the case was resolved by cross-licensing. My participation: I was paid sixty-five dollars an hour to explain the background of all this to lawyers who were paid over two hundred dollars an hour!

China

Another interesting experience came about seven years later. During the Tokyo meeting I chatted with a mainland Chinese scientist, a Dr. Yang, who asked me for some samples of Dylux proof paper (which I sent him). He thanked me and asked if I ever plan to be in Beijing [China]. If so, come to his lab and give a talk. In 1984 I did go on a tour of China, during which I spent a few days in the Chinese capital. I wrote to him a month before, and by return mail there was an invitation to give a lecture at the Chinese Academy of Sciences. He picked

me up in an ancient limousine, with uniformed driver, and I gave a talk via an interpreter. After five minutes there was some noise from the audience, which was to tell my host that I was speaking clearly enough that they did not want an intervening interpreter. After the talk, we had lunch in a lab, and then I met a number of Chinese chemists who showed me what they were doing. The head of the lab was a Dr. Wu who said he would like to visit me in the U.S., but that never came about. On the way back to the hotel I asked Dr. Yang if he had time for a cocktail and he accepted enthusiastically, but asked me if he could bring along the chauffeur, who after all was a member of the Institute of Photographic Sciences. However, the chauffeur could not have alcohol! At that time, it was difficult for Chinese to visit hotels that were primarily oriented toward western visitors. We chatted amicably, and having established that Dr. Yang liked alcohol, I found a bottle of Scotch in my room, which he accepted gratefully. He then invited me to his house for dinner the next evening, but my tour made that impossible. Before I left Beijing, a container of Chinese tea was delivered to my hotel.

Appendix V: Acknowledgements

I would like to acknowledge people who made a difference. They made Dylux one of the most successful DuPont products:

Edward Abramson, for making meaningful measurements
Philip Botsolas, for his efforts to expand markets
Lawrence A. Cescon, for doing the chemistry that made it all possible
Robert L. Cohen, for synthesizing and studying novel compounds
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John W. de Campi, for effectively demonstrating utility of a novel proofing system
J. Garrett Forsythe, for building equipment to demonstrate our products
Lawrence B. Friar, for always encouraging new applications
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Peter S. Strilko, for relating light sources to materials
William S. Wartel, for directing marketing efforts
Harold Wilbur, for building small exposure units, which permitted easy demonstrations
Charles Yembrick, for optimizing photofix chemistry

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