CHEMICAL HERITAGE FOUNDATION

DAVID R. BRYANT

Transcript of an Interview
Conducted by

James G. Traynham

in

South Charleston, West Virginia

on

8 April 1998

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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David R. Bryant, interview by James G. Traynham in South Charleston, West Virginia, 8 April 1998 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0169).

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

DAVID R. BRYANT

1936	Born in Greensboro, North Carolina on 8 May

Education

1958	B.S., chemistry and math, Wake Forest University
1961	Ph.D., organic chemistry, Duke University

Professional Experience

	Union Carbide Corporation
1961-1967	Senior Chemist
1967-1975	Research Scientist
1975-1979	Senior Research Scientist
1984-1987	Corporate Fellow
1987-present	Senior Corporate Fellow

Honors

1989	Chemical Pioneer Award, American Institute of Chemists
1990	Honorary D.Sc., Wake Forest University
1992	Industrial Chemistry Award, American Chemical Society
1993	Carothers Award
1998	Perkin Medal, Society of Chemical Industry (American Section)

ABSTRACT

David Bryant begins the interview with a discussion of his childhood. Bryant was one of seven children and grew up in North Carolina. He began working at age ten, and held various jobs until he earned a scholarship to Wake Forest University. Influenced by his high school science teacher, Bryant double-majored in chemistry and math. While at Wake Forest, he became a lab assistant, and conducted some synthetic research. After receiving his B.S. in 1958, Bryant decided to attend graduate school at Duke University. Focusing on organic chemistry, he worked on the conversion of organic compounds into dianions under Charlie Hauser. Bryant earned his Ph.D. in 1961 and immediately took a job with Union Carbide Corporation. He worked on developing a method of producing vinyl acetate without halide, and later worked with benzyl acetate, acrylic acid, and rhodium triphenylphosphite in the Oxo process. In the 1970s, Bryant became involved in the scientific side of intellectual property disputes for Union Carbide. Bryant concludes the interview with comments on the nature of industrial research and development, the difficulties of government regulation, and his approaching retirement in 2000.

INTERVIEWER

James G. Traynham is a Professor of Chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

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INTERVIEWER: James G. Traynham

LOCATION: Union Carbide Corporation
South Charleston, West Virginia

DATE: 8 April 1998

TRAYNHAM: Dr. Bryant, I know from things that I have read, that you were born in Greensboro, North Carolina on May 8, 1936. Can you tell me something about your childhood?

BRYANT: Certainly. I was one of seven children. I'm in the middle. There were four boys, three girls. I grew up during World War II. The family was not well off. We weren't poor but there wasn't much money and there weren't many goods, and being in a large family, you had to learn to share, and make do, and do without. A neat thing about my childhood was that I actually grew up in two environments. During the school year, I lived in the city. I stayed with my parents. In the summer months, and also on some weekends, I went and stayed with my grandmother. That was in the country. So I had two completely different sets of friends, two different environments, and really enjoyed them both.

TRAYNHAM: How far out of Greensboro was your grandmother's home?

BRYANT: Not a long way out, but it was far enough out to be in the country. In traveling to her house, this is at age ten or eleven, I would take a trolley from the end of the trolley line into town, take another one to the end in another direction, and then probably walk a mile or mile-and-a-half, something that would be unimaginable today with the kind of problems that you have with children out. But I had, in retrospect, a lot of freedom. I can't think of many times that my mother ever told me that there was something that I couldn't do. You know, barring things that have grave safety risk. I guess she had so many kids she really couldn't keep up with all of them.

TRAYNHAM: Where were you among the seven?

BRYANT: In the middle.

TRAYNHAM: Oh, fourth.

BRYANT: Yes. There's an older brother, two older sisters, me, and then two younger brothers, and a younger sister. The first employment I had was at age ten, working in a salvage place. Again, at the time it didn't seem unusual, but now if you think of a kid ten years old out working, getting money for it, even though there were no W-2 forms or anything like that, it's amazing. The company bought lots of salvaged goods from places such as Sears, Roebuck [& Co.]. People would return strollers, for example, that had a broken wheel or a broken handle or foot pan, and we'd get collections of these, and one of my jobs was to cannibalize the pieces to make something that would work that could be sold. It was done with, as I remember, essentially no supervision. They'd say, "Here's a few tools. There are the pieces. Just go put together something that works." One of the more challenging things that I had to do was to assemble some of these pedal car jeeps. You've seen those that look like little automobiles. Those are relatively easy because the body was all one piece. But the jeep had a number of pieces and they just turned me loose with a few tools and instructions, and I'd get the things put together. It was really a great experience. You saw all this collection of stuff, got to meet a lot of different types of people. I had a lot of freedom in what I was able to do.

The salvage business was bought out entirely. The entire stock was bought by a company, I think, in Texas, so it disappeared. But the people that had run it, I think when I was fourteen and could legally work, hired me as a curb hop in a barbecue restaurant—one of these people who go out, put the metal trays on the cars, which you don't see any more. One of the skills I developed there was that—this was back when we still had silver money—if somebody dropped the tip on a tray, I could tell how much money had been dropped even though it was all dropped at the same time, just sort of a Fourier transform on the ringing of the money as it settled on the tray.

TRAYNHAM: That was also back in the days when you could pay for it with coins. [laughter]

BRYANT: That's right. I don't remember exactly how long I stayed at that job. I can't say that I was terribly fond of it. It was just a way to make money. There were some interesting people there. I learned a lot about how people cheat. [laughter] They had a manager there. The rules on the eating was that barbecue or Brunswick stew you could have anytime you wanted to, all you wanted to eat, but you had to pay for beverages. Pie you would certainly have to pay for. They had one of these big stacked, open pie cases which you don't see much anymore because of refrigeration requirements. But the manager would come around regularly and check to see how many pieces of pie were gone and then check that against the tickets to confirm that they'd actually been sold so that he could keep a close eye on the employees. I discovered one day, one of the employees in the back room eating an entire pie that he got out of the big refrigerator because they didn't keep count of how many pies were around, only how many pieces had been removed. I guess that one taught me something about human nature.

I went from a curb hop to work in a toy magic novelty store. It was owned by a couple, Eve and Murray Weinberg, that had just moved down from New York City. So here you've got this contrast—I mean, the real New York types and this southern boy—and we hit it off great. Other than family, it would be the longest relationship that I've had with anybody. He died a few years ago, but we still send cards to her, and went down for an eightieth birthday party for her a few years ago. I learned a tremendous amount in this job, and in particular in the association with magic. I think that anybody that gets into science needs to learn a little bit about magic, because if you're looking at somebody doing a trick, you see something that then you need to ask yourself, "What else could have happened to make it appear the way that it seems to have appeared?" Mother Nature doesn't set out to fool you, but she certainly doesn't keep you from fooling yourself, and just to have the mind set that just because you see it, you believe it, is not the best way to go into science. You need to think about all of the other ways, all the other things that could have happened that would have given that same appearance.

There were a lot of odd people that came into the store, because of the fact that we sold novelties. There were these regular people, I'll say "regulars", who would come in every couple of weeks to see "what's new." Some of them actually had some skills and talent, and again, here's this kid there, and they would pass some of their wisdom off to me. One that I can really remember is: you never play the other guy's game. This has come in in, I'll say, intellectual property battles, patent disputes. You try to arrange things so you're playing your game rather than their game.

After the retailing experience—so I've gone from salvage to curb hop to retailing—I then got a job based on my typing skills as a shipping and receiving clerk in a place that specialized in scissors. I think we had, it was over one hundred, maybe one hundred and twenty different types of scissors at this place. They catered to the hosiery and textile industries. Every job that I've ever had after that seems like a vacation. It was one of the most oppressive working environments that I've ever seen. There were three people that worked in this little office that was in the back of the man's house. He only hired boys because he said it might look bad to have a girl coming into his house to do any kind of work, so that's how you end with male typists there. There was a friend that sat within three feet of me but we could only converse if it was directly related to business. So you'd say, "Good morning," and then if there were any business questions, you discussed them; finally, you said "Good evening," and that was the end of the day.

While on that job, I got to go out as a traveling salesman visiting textile mills, hosiery mills, trying to sell scissors, and I decided that wasn't for me. So as we go through this sequence, I'm sampling different types of work and deciding, "No, no, no."

TRAYNHAM: How old were you when you were employed in the scissors business?

BRYANT: I was still in high school. It was the last two years, so I was probably sixteen to eighteen, something like that. The scissors were absolutely first rate, but as I say, the work environment was appalling.

While I was in high school, I actually had no real direction heading towards college, but I had this mentor, Arnold Bolen. I never had a course from him, but he sort of took me under his wing, administered the Wechsler and Stanford-Binet IQ test and so forth, and decided that I ought to be taking some competitive scholarship tests. At that time, there were some that you would go take an exam and, depending on how the score came out, you may or may not get any scholarship. I took one that he told me was just sort of for practice and got a first place scholarship to Elon College, and then he wanted me to take one for Wake Forest College. It was absolutely one of the most difficult exams I ever saw. You know, I'm in a room by myself and there were questions like, "Which of these authors were contemporaries?" and I'd never heard of either one of them. But I went ahead and finished it, turned it in, and it turned out that I won one of the top scholarships to Wake Forest. I didn't know much, but the other people apparently knew even less, and it's all relative.

I did have one chemistry course in high school, and a nice chemistry teacher. I can't remember anything in particular that I learned in the class. I had an eighth grade teacher—I don't remember her first name—Mrs. Smith, certainly one of the most influential, memorable teachers that I had in my entire career. She did some chemistry experiments for us one day, and the thing that impressed me most was how sparkling clean the glassware was. You know, I'd only seen sort of smudged beakers or dirty beakers, and she polished that stuff to where it looked like crystal, and that stuck with me.

I can't say that there was ever a time that I decided I wanted to be a chemist, not a single event. I know I had one, I may have had two, chemistry sets as a kid which I loved to mess with, but then I messed with a lot of things. But by the time I got to college, I got into the chemistry curriculum and it never varied an iota. There was never a moment's hesitation that that was what I was going to do. So that certainly at the time I started to college, the decision was made, but exactly when it came prior to that, I don't know.

Any questions about childhood?

TRAYNHAM: No. I think you've covered it very thoroughly. So you left high school, headed for Wake Forest with a commitment already to chemistry?

BRYANT: Yes. I'll talk a little bit about the high school. There were two school systems in Greensboro. There were city schools and there were county schools. The city schools were the good ones and the country schools, where I went, was where the hicks went. At least that was the impression that all the city students gave you. That's one of the reasons it's a particular pleasure to get the Perkin Medal, to show all of the city school students that somebody from a county school can do something.

The classes were small. There would normally be two of each grade—two eighth grade classes, two seventh grade classes—so that you criss-crossed with the entire population for your class and you got to know everybody pretty well. I think the graduating class was fifty people.

I had what I would consider to be a really weak background in terms of preparation for college, and there were a couple of reasons for this. One was that I was leaving school about noontime to go to work at these various jobs, so I wasn't putting in nearly as many hours as the other students were. The second was that I could do well enough that you'd get exempted from final exams. That's fine in high school, but when you get to college, they don't exempt you, and I had never really learned how to study for a final. It took a lot of effort to figure out what I needed to do as I went through the semester, to gather the information so that when the final came, I was prepared for it. I didn't take as many courses as I could have. In retrospect, Latin was offered and we had a good teacher. I should have taken that, but I didn't. Even second year algebra was offered and I didn't take that. So when I got to math in college with only one year of algebra, it took some doing to catch up with the people that had had more advanced courses. That, I think, actually worked to my benefit. You can say if you can survive the first two years, then you've developed the kind of study habits just in the process of catching up with other people and staying even with them, that as soon as you have caught up, it makes it real easy the last two years. The first two years were just rock hard. I had, I think, in a single semester analytical chemistry, physics, German, English literature, and calculus. I mean, there wasn't a single soft course. There was nothing where there was any breathing room, and 2:00 a.m., 2:30 a.m. was the typical going-to-bed time. In the day time I could go to sleep anywhere.

TRAYNHAM: And did! [laughter]

BRYANT: Yes. But I survived it. The scholarship that I had required that you maintain an average—I can't remember what it was now, but there was a spec that you had to meet in order to keep the scholarship. Without a scholarship I couldn't go to school, so that also kept pressure on me to perform. So it was just back against the wall, and this is what you've got to do, and I did it.

The scholarship provided tuition and fees at Wake Forest. At the time I think it was seven hundred and eighty dollars a year for the combination of tuition and fees. Now you could hardly get your textbooks for that amount of money. While I was in college, I was minoring in math, and I decided I'd go ahead and major in math also. I had to go talk with the chairman of the department. I think I may have been a senior at this time. To get a major, it required you to take solid geometry, and at that point I could derive all of the equations that were used in solid geometry, so he permitted me to skip geometry and substitute some senior level course, so that I did get a major in math. But as I say, I never had a knack for math.

TRAYNHAM: You mean you had a double major of chemistry and math?

BRYANT: Yes, chemistry and math. Chemistry I had a knack for. Math I didn't. What do I mean by "knack"? If you don't have a knack, you can learn something and you can do it. If you do have a knack, you can be presented with a new situation and figure it out without being specifically told what it's all about.

Because a lot of my electives were then "ground up" in this math major, the only real elective that I had the entire time I was in college was spent taking French. At the time I got my Ph.D., you were required to be able to read two foreign languages and the common ones were German and French because so much of the chemical literature was in German and French. I took German the first two years and then figured I'd better pick up French so I did it. I found it to be easier than German.

The last two years were relatively easy, and that's a very comparative term. I mean, there were long hours, a lot of studying, but I had figured out by this time how you gather the information as you go through a course so that when you get to finals you've got it all organized and you get some test savvy-ness. You know what things are likely to be asked. So I think the last two years it was straight As with the exception of one course that I slacked off on. Wake Forest was a Baptist school at the time and one of the requirements was two semesters of religion. The one that people typically took was "Old Testament History". It was taught by a superb professor but the course was rock hard. I learned that stuff so well I could have gone in a year later and taken an exam, and I can still, after forty years, tell you some of the mnemonics I learned to keep up with all the kings when the empires divided. He was a fascinating lecturer. He'd done archeological work. That course I got an A on. The next was "Life and Teachings of Jesus". That was the last semester in school. I had already gotten information that I got a National Science Foundation Fellowship and I could have broken my back and maybe made an A, or just cruised and gotten a B, and I decided it wasn't worth breaking my back to maybe get something that wasn't going to matter. So if I ever slacked off, that's where it was. I slacked off and took a B.

TRAYNHAM: That was your only B?

BRYANT: No, I had gotten some Bs in the first two years while I was learning, but it was the only B in the last two years.

Wake Forest moved its campus about eighty or ninety miles during the period that I was in school, so I had two years on the old campus, which was in Wake Forest, North Carolina, and then two years in Winston-Salem in a brand new facility.

TRAYNHAM: How was the move? Were you involved in moving anything?

BRYANT: No. All I had to do was just show up. There was a neat thing about Wake Forest at the time that helped me a great deal, and that was, it was Wake Forest College, not Wake Forest University. There were no graduate students. So the second year, during my sophomore year, I was offered a job as a lab assistant for the freshman courses, setting up the lab, helping people run the experiments, and I think that was certainly beneficial. Then when they moved to Winston-Salem, I had a job tutoring in general chemistry problems. I would show up at a room at a specified time and anybody could come in with any problem that they happened to be facing. The pay was seventy-five cents an hour, but the professor who was monitoring very graciously said that for each hour that I worked, I could put down an hour for prep time, so that in a sense, I got a dollar and a half for each hour I spent there, which even then wasn't very much money. But you sure did learn how to solve chemistry problems. I mean, you'd learn them backwards, forwards, upside-down, to be able to explain them to people. So that I think that was probably the most beneficial aspect there.

I had other assignments. One was setting up the P-Chem [physical chemistry] lab. It was one of the more enjoyable classes, the lab. Probably of all the lab courses, I'd put it up near the top. The person I was taking the class from, Jack Nowell, was also a co-author of the lab book, so that the equipment that you had matched what was in the book, and you didn't have to spend hours trying to figure out why you had something left-handed when the instructions said it should be right-handed.

Another professor, Harry Miller, had me doing some research work for him that was some synthetic work on a fairly big scale for a university. A big flask. All of these things I did as learning experiences. Harry Miller taught a course in qualitative organic analysis. I can't imagine that it's taught anywhere the way that we had to learn it. It was, again, a rock hard course, it separated the men from the boys. His philosophy was, you've either identified the compound or you haven't. You got an A or an F. This made you ultra cautious. After you had figured it out, then you'd run several other tests just to be safe. That was the course. Up to that time, if you were asked to give me a procedure for converting A into B, you might put down anything. There was no concern about yield, conditions. When you had that class and you had to make derivatives, you had to have two solid derivatives of everything. Then you really thought about the reagents that you had to use, how aggressive they were, how long it was going to take, what kind of yield you were going to get, how easy it was to isolate.

[END OF TAPE, SIDE 1]

BRYANT: It's a shame that this kind of course isn't taught today. We were not permitted to use any spectroscopic techniques. There was no infrared, no NMR [nuclear magnetic resonance], no GC [gas chromatography]. It was strictly the solid derivative technique, boiling point, refractive index. What I see now is that we get Ph.D.'s who are extremely skilled at NMR interpretation, but when they get to the point that NMR lets them down, they don't know where to turn. They can deal with information that's, I'll say, black and white. A crystal structure is the extreme of black and white, and then you can get some rather definitive information out of NMR. By the time you get to IR [infrared], you're having to think a little bit, and if you get into UV [ultraviolet], you're going to have to think a whole lot because you just don't get that much information out of it. Techniques beyond that are just a mystery to them.

They won't come up and say, "Hey, I don't know what to do." But it's obvious when you look at the program that they're just stalled and they don't get the benefit or haven't had the benefit of the logic exercises that were involved when you had to figure out how to identify things by making solid derivatives.

TRAYNHAM: One doesn't even see solid derivatives mentioned in most of the experimental materials that appear in the journals these days.

BRYANT: I know. It's understandable. With the spectroscopic techniques, you can get an answer so much faster, but the real deficiency is that they haven't gotten the training, in extreme, that I had. I'm going to take a detour and talk about a particular problem that I was assigned. I didn't always work on homogeneous catalysis. For a while, I was a chemist in support of the acrylic acid unit and we had a problem with acrylic acid in that it would develop color on storage. It should be water-white or, you know, just have the appearance of water, and over a period of a month or two, it would develop a very pale yellow color. I was given the assignment of figuring out what was causing it, and then, of course, what to do about it. I was told that if you added ammonia to the acid and heated it overnight, it would turn yellow overnight, so that the only technique that I had for identification was this test which was—you can view it as digital technology. I either got a yes or a no, or sometimes a maybe answer.

So I started through effectively all of the organic compounds that are known, by class, trying to identify classes of materials that would give you a yellow color if you put them in acrylic acid and you heated them. We had acrylic acid that didn't color, so we knew that it was not an intrinsic property of acrylic acid. I was never able to get any spectroscopic information. I never saw anything on a gas chromatogram, and through, I'll say, logic and a series of tests, I narrowed it to five compounds that existed in two different families. They were tautomers. I ended up writing a report that said I really can't tell. It could be this group of compounds, or it could be this other family of compounds. I wrote the report, and then, I don't know, a little while later I realized there was a stream in the plant where these things should be concentrated, so I got some of that and made the 2,4 dinitrophenyl hydrazones which, in fact, were osazones, and did TLC work, and found out that both families of materials were present. So I'd gotten the double bull's eye simply through logic in a test that was YES-NO. The report is about color in acrylic acid, but the methodology that I developed was sufficiently general that anytime anybody has a color problem or an odor problem, you can go back and follow the procedures that were given and use these techniques to work on those specific problems. It's the most referred-to and referenced report that I've ever written here, that people working in ethylene oxide or ethylene glycol chemistry will come around, and even recently I've given copies of the report to people. What it amounted to was simply developing an analytical procedure for going through and diagnosing what type of material was present.

TRAYNHAM: Do you think that report was more or less a direct outgrowth of your qual [qualitative] organic course?

BRYANT: I can't imagine anybody that didn't have the benefit of qual organic as it was taught then being able to come through and do this sort of thing. There's one technique in there that I've actually got a chemist using right now that, you can say, I learned from an experience I had in surveying a lot. I went out, there was a lot that needed to be surveyed, and I was the guy that held the post and there were two other guys that were looking through the scope. We started by driving a nail in the middle of the road, and then we went out and measured various angles and wrote it down, and when they got to the end, they said, "Let's add up the angles and see if they make three hundred and sixty." You say, "Well, of course! What a neat blunder-checker!"

The way that I did use that in the acrylic acid work is that I would perform a separation of some description. You distill at elevated temperatures, or you distill at low temperature, and then after you've distilled, you put everything back together and see if you've got the same answer that you had before you did anything. I mean, one explanation would have been, oh, the color formers exist as some material which is being cracked on distillation. Well, if it's being cracked and I distill it, I ought to get more of it and I should have the same amount if I distill it at low temperature. You know, twenty-five degrees. It's analogous to the surveying technique. You separate something, put it back together. Are you at the same place? If you are, then you can take the fractions and see if you have any material that's concentrated in one of the fractions and then you go on to another step.

I had another project here that was using the same technique. We were attempting to make ethylene glycol from synthesis gas, and every additive imagined was being checked. We had a guy here that was what I call the "green thumb" type. I'm not even sure he was a Ph.D. His logic, scientific understanding was limited, but he came up with one hell of a lot of discoveries, and his secret was, he wasn't constrained by what he knew and he ran a lot of experiments. He was methodical. In the course of this, he found that he had two batches of 2 hydroxypyridine, which we'd been using as an additive, and one batch gave a lot better results than the other batch. Various people had tried to figure out what the difference was and nobody had been successful, and I was given the problem.

Well, one of the first things that I did was to put some of the material in a sublimation jar and sublimed it, and got two fractions. I went through the procedure to put it back together. Yes, we get the same result. Then there's this fraction that didn't sublime, that when added to normal hydroxypyridine gave it the beneficial effect. It was a white powder, but it was an absolute bear to figure out what it was, and the reason is, it was sodium chloride. Both sodium and chloride are incredibly common, so that qualitative analyses don't help you at all. If you try to run an infrared on it, you get nothing whatever because it's no different than the sodium chloride cells that you've got in there.

Once we finally did figure out that the magic component was sodium chloride, then we went through a sequence of improvements and finally ended up adding cesium carboxylate to the process. The bigger cations were better and the anion was just a way to get the cation into the system, and then the carboxylate worked better. But it goes back to: separate, put it back together, test it, are you okay? Then continue on with the next step.

TRAYNHAM: You finished up at Wake Forest, and what prompted you to select Duke for graduate school?

BRYANT: I had a lot of campaigning by one of the new professors at Wake Forest, Phil Hamrick, that I took organic from and had a lot of respect for. He had gone to Duke. He spoke very highly of it. He drove me down to meet people, and I just liked it. We had a mini-seminar session, I guess, with his former thesis professor and some students, and I liked the way the interactions occurred. I worked for Charlie Hauser, who was well-known, possibly the best-known professor at Duke. I'd say as much as anything it was the campaigning on the part of Phil Hamrick. I was also considering the University of North Carolina. When you apply for a National Science Foundation Fellowship, you have to designate two schools, and you've got a bizillion, at least at that time, a bizillion forms you've got to fill out. What school would you go to? It's sort of like, well, you just write down a couple. Well, then you find out, "Hey, I'm sort of locked into these things." [laughter] So I will admit that I did not go through a detailed analysis of all of the features of the school. What I saw I liked, and so that's where I went.

The fellowships were renewable yearly for a maximum of three years. They paid tuition fees and, I'd say, a modest amount to live on. I think about three hundred and sixty dollars a month, which could get you by, but you did not see ground steak very often. I can't remember ever having pizza. The fact that this had a limit of three years meant that it would be desirable to complete the work in three years, which is what I did. It was extremely tough. If I look back on mistakes that I've made in my life, things that I wish I could do over again, that would be one of them. I retrospect, it seems like unrelenting work. At Wake Forest, you know, I was doing great, working hard, but there was some time for relaxation and play. At Duke, the balance was all wrong. I hear of people that don't get to talk to their thesis professor. "Oh, I haven't seen him in two months" or whatever. As you got to be one of the senior people there, you might get to talk to him twice during the day, and maybe he'd call you at home at dinner time to talk another hour about what you just talked about. So I found it more fruitful to simply go back to the lab so I wasn't there when he called. You get on this day and night schedule. My wife was working, so that I would be into the lab right after I dropped her off for work, and then I'd pick her up, go home, eat, and then back to the lab 'til, I don't know, ten or eleven o'clock at night. It was a real drag.

TRAYNHAM: I'm sure that, though the tenure of the fellowship was a driving motivation for such hours, that your professor was pleased that you were there all those hours as well. He probably didn't discourage you.

BRYANT: No, I never got any discouragement on the hours.

TRAYNHAM: During your time in graduate school, did you ever waver about your choice of organic chemistry as your major field?

BRYANT: No. Never. I did something crazy in graduate school. There were some people from Wake Forest that had gone to Duke. "Well, what are you minoring in?" Well, at the time that I started, there were really two choices. You could minor in biochemistry or there were people minoring in physics, and I elected to minor in physics. I had some tough courses. In retrospect, I think it was the right thing to do, but when the time came to take your oral exams, there was a professor from the physics department on your committee that could ask you anything in the world that he wanted to. A lot of stress there. But he was a great guy, and he asked some good questions, but I think they were legitimate questions.

TRAYNHAM: What was the focus of your dissertation research?

BRYANT: That was on multiple anions converting organic compounds into dianions, and I think I may have even had a trianion, using potassium amide and liquid ammonia as the base to pull these off. The chemistry was neat. You'd get a lot of color changes. In retrospect, some of the things that we did were crazy. We were working with liquid ammonia and to get it out of a cylinder, you'd tilt the cylinder of liquid ammonia on its side, open the valve wide up, have a hose sticking in a flask, and with it pouring out at full speed from the cylinder, it would eventually cool enough that you'd start getting liquid ammonia, and we'd get 500-800 cc of liquid ammonia in our Erlenmeyer flask, and then walk down the hall holding the flask behind you. Woe unto anybody that happened to be following you! [laughter] I was using potassium metal which you'd cut up into pieces and throw into the liquid ammonia, and we were cutting it out in the open, in the humid Durham air in the summertime. You could see little sparks coming off of the metal. You wanted it to facilitate conversion of the metal into its amide. The folklore was that it was nice to have oxide present, so you wanted it to oxidize some, but you didn't want it to catch fire. So that was sort of the balance we walked. We used huge amounts of ether. I mean, the lab reeked of ether. Storing these things over sodium wire which you had to dispose of, which didn't always go real well. It was so different and foreign to the way work is going on here now, it's like it was another age.

TRAYNHAM: I was about to say that such reminiscences would probably make the Union Carbide Safety Officer cringe.

BRYANT: [laughter] It wasn't done here. It was done there. I never talked about a summer job that I had as a lab technician. The last two years in college I worked for Morton-Withers Chemical Company in Greensboro. They were trying to make a motor oil additive. They were trying to alkylate benzene and then with oleum, sulfonate it, and then with some barium salts make—I think they call them "super bases." You would get more hydroxide, or barium hydroxide, in the mix than the stoichiometry would indicate. These were put into motor oils to neutralize the acidity. I had jobs, you know, distilling the alkylate, doing various analyses. All of the work was done with either Bunsen burners or Meeker burners. Fire was an everyday occurrence. You'd hear the whoosh of a CO2 extinguisher. A lot of times people wouldn't even bother to look. As you were titrating in isopropanol and you'd have to heat the solution to drive off the CO2, the flask would catch fire and you'd put it out.

I'll tell you, I got a lot of exposure there. I mean, it was crazy stuff. Washing centrifuge plates in a bath of benzene. I can remember the first day I did it, I just couldn't stand it because the benzene was burning the backs of my hands so much, so I started using neoprene gloves. The safety conditions were just medieval compared to what we're doing today. I thoroughly enjoyed the work. Some weekends, while I was in college, I'd go in and do analyses for them. There were some that were long and tedious that I'd learn how to do as they were taking the motor oil additives apart. But overall, those were good times, and I did maintain contact with some of the people while I was there.

While I was at Duke, I didn't do the lab tech routine for two reasons. One, I had a fellowship so I didn't have to do it to earn the money. Two, I'd already done it. For some of the other things that they might pick up as a lab tech, instrumental techniques, I had already gotten experience with them in the two years of summer work that I had. I really think that the two summers facilitated my getting through graduate school as rapidly as I did.

TRAYNHAM: So you completed your dissertation research and had your final exam at Duke, and then were ready for employment. Did you have several interviews, or did you settle on your first one?

BRYANT: Actually, I had several offers of trips while I was still in my second year. I guess you know that whenever the industry comes around, the question is, "Who do you have coming along that looks pretty good?" These people would come in the lab and talk to me and say, "Hey, would you like to come to Midland?" or "Hey, would you like to come to Philadelphia?" or whatever. I guess I just didn't want to be distracted because I didn't take any of those trips, and it was only when the end was in sight that I went out and started interviewing.

There were some things that impressed me at Carbide. One, when I was in grad school, there's always this fight for equipment. Can I get another flask? Just, you know, little things. I can remember, I was in an interview at Carbide and while we were talking, the delivery guy came and left an analytical balance in the office that belonged to one of the people I was talking to, and what struck me was the casualness with which he received this equipment. It was like there was no excitement. It was no big deal. Whatever. What that told me was: you're going to be able to get any kind of equipment you want. It's not a big occasion to get these things. In my entire career here, I cannot remember anything of any significance that I wasn't able to get. I hit that one right.

I liked the attitude that the company displayed towards technical people. I'll give you a comparison. When I came, distillations were common. You'd go into laboratories, and there'd be this entire bank of distillation columns, and you might find twenty distillation heads in there. It was pre-GC, just-barely-GC when I arrived. Carbide's attitude at that time was: to start with, you'll get a technician and as soon as you can utilize a second one, you can have the second technician to do the manipulations, the drudge work. Now, this is in contrast to DuPont. I went to Experimental Station, and what they said was that in the total cost of the project, the difference between the salary of a Ph.D. and the technician is small, therefore we want Ph.D.'s to be doing everything and we'll give you a half-time technician to wash glassware. I just liked the former approach better than the latter. You know, it's just a particular measure.

I've read hundreds of resumes and you know, people put down "no geographical preferences." I don't believe that one iota. They probably think that it'll hurt their job chances if they put it down, but they're something that are true. I had offers to go to Dow [Chemical Company] at Midland or Texas. Well, neither one of them appealed to me, so I didn't bother to go there.

While we're on the subject, Carbide has a formalized Ph.D. interviewing process. I've never been part of the formal organization. I've gone out to campuses to interview, but I don't consider that. That's part-time work. Of course, I've talked to hundreds of people that have come through. But at one time, the director gave me the job of handling the hiring of Ph.D.'s. He needed to hire a bunch of people and our acceptance rate was real poor. The year before I started, it was 16 percent; roughly one in six people accepted the job. There was a combination of factors. At that time, West Virginia was improving, but it certainly wasn't as desirable as it is today. In the meanwhile, other places have gone downhill while West Virginia's improving, so that at the moment, location, I would say, is a positive attribute rather than the negative attribute it was when I first came here.

We had an interviewee here that I talked to, and later I heard that he had complained about the treatment that he had received. Well, it turned out he was real unusual. I mean, people can come through and maybe a week later I'd forget them, but I didn't forget this guy. I told the Carbider who had asked about it, "I can tell you exactly where we sat in the cafeteria, and we gave him the prime spot, looking out on the grounds. I can tell you the kind of people that we selected to go." You know, if somebody says they have an interest in jazz or the trumpet, we'd find somebody here for the lunch that could talk to them about that. Then we went to a restaurant. It was called "In Top at the Inn". It's at Charleston House. We were seated at the window and gave him a seat that overlooked the river. There were three other people there, and we go through this routine of trying to get the person engaged in conversation. Throughout this conversational gambit, another, another, another, and he just wouldn't get involved in conversation.

[END OF TAPE, SIDE 2]

BRYANT: So we've got this guy that's interviewing at over twenty different sites, and I just could not imagine how you would keep up with the information, how you'd catalogue it. I said, "When you finish all this, you ought to write a book." Well, then that just opened the floodgate and this information started to come out. So I said, "When you travel, what things do you like and what do you dislike about the visits?" I said, "Do you like people to pick you up at the airport?" "Well, it depends. I think if it was a small place, no. A large place, yes." "Would you like to have the literature? Would you like to have that before you come? In your room? When?" He gave an answer there. One of the stranger things he said was, he would like to have a schedule for the day. They are totally hosted, which means that every place they're going to go, somebody will take them there so they can find the location. He had pictures and some biographical information on people, but he said, "I would like to know, is this a fifteen, a thirty, a forty-five-minute interview?" When you sorted through everything that he said, anything that lowered anxiety was good. Things that increased anxiety were bad. So when the director put me in charge of recruiting, we went through everything in an attempt to lower anxiety.

We figured that in terms of salary, benefits, whatever, all the big companies are equivalent. You know, yes, they're going to offer you one hundred dollars more a month, but maybe you don't get raises as soon. So that you'd say we're even there. So what we worked on were the fine details, figuring, then, if everything else is equal, it's these little things that'll make the difference.

Now, Carbide is going to pay for all their room, plus other expenses, but we'd have somebody go down and register them. You know, fill out their name, so that when they came up, the key would be handed to them. Because nobody else had done that. When they got up to their room, there was a business card with a note on it from whoever the head person was that they'd talk to. "Looking forward to talking to you." There'd be a gift pack in there. We'd get these Hickory Farms, you know, meat, cheese and crackers, and a little bottle of Sangria, and it was simply a way to let people know, "We knew you were coming. We're happy to have you here, and we recognize you."

TRAYNHAM: Sounds like a great investment.

BRYANT: Oh, and I mentioned driving here that one of my standards was that I would go to dinner with everybody that came, and I went to dinner over fifty times in two-and-a-half months and, on weight control, said, "How am I going to handle that?" So what I decided was that whatever was served I would cut in half and only eat half of it. You talk about some soul-searching. You have this gorgeous filet, you cut it in half, or a lobster tail, and the rest is going into the trash. But I managed to keep the weight under control then.

We increased our acceptance percentage in one year from 16 to 50 percent. We still use many of the techniques that were incorporated during that program. We got some great people. Several of the people that were hired then are now the ADs here. I have, I guess, a significant role in establishing culture, if you want to view it that way. Attitude. I had a good friend that worked at three different Carbide technical centers: here, Bound Brook, and Tarrytown. He told me that every site had a different culture. It was the first time I'd ever thought about it. I figured Carbide is Carbide. He said, "No, it isn't." We strive for cooperation, among other things. We don't want individualists working off in a corner. Essentially, any of the projects that we undertake require a multitude of skills and disciplines and it's essential that people be able to interact freely. It's okay to make mistakes. We prefer they don't make the same ones twice. But that does not apply to safety. There are some things that are unforgivable. One is being in the laboratory without safety glasses on. I'd say I've got zero tolerance. Another one I tell people is: never, ever make a mistake in the title of a report.

TRAYNHAM: In the title?

BRYANT: In the title. Don't have a typographical error, because if you've got a misspelling in the title, it will subtract significantly from the rest of the report. People tend not to look at titles. They'll start reading down in the text. I've got loads of wisdom, if you want to call it that, that I pass on. I enjoy doing it, and it's a way that you figure you can continue after you retire, because you've got people that are trained in the same thought processes, work ethics, approaches to doing R&D, and it's satisfying. I've done just a big variety of projects over the years.

TRAYNHAM: When you were at Duke and graduate school, you, as you said, were concentrating on carbanion chemistry. Did any of that carry over into your employment at Carbide, or did you move completely to something else?

BRYANT: Yes, it was a complete move. This gets into really what a Ph.D. chemist is about. Ph.D. engineers or B.S./M.S. engineers we can have productive in a week or two. You can sit them down at a computer or give them a unit, and they can start doing useful things. For a chemist, a Ph.D. chemist, it probably takes two years for them to get up to speed. On the other hand, the engineers are, I think, more prone to obsolescence because they come in with a toolbox that they've acquired in school and unless they maintain their continuing education classes, it's easier for them to fall behind. I think the nature of the chemist's work is such that they have to have continual renewal. If they're going through different projects, it's the usual routine of search the literature, see if you can repeat a reaction, see if you can understand it, expand on it, get something useful out of it.

TRAYNHAM: What led to the choice of the early work that you did at Carbide?

BRYANT: The first project I had was actually an assigned project, and that's common for the first few years, first three to five years. The expectation is that whatever a person is working on will be something that somebody else has identified or figured out. But in the five year time frame, we expect people to start generating their own ideas and ideas for the newer people that are coming in.

The first project I worked on, I came in right at the dawn of homogeneous catalysis, using precious metals. The Wacker process had been running, I think, two years, and there was a Russian article, by a Professor Moiseev, where he was studying the mechanism of the Wacker process and instead of running it in water, ran it in acetic acid with sodium acetate and found that he made vinyl acetate. Well, this was an open publication and what it did was to start about twenty companies working on procedures to develop a homogeneous vinyl acetate process.

Carbide's twist on it was that we wanted a non-halide route. In industrial chemistry, halide is bad. I mean, you just, you do everything you can to avoid it. The Wacker process operated either in titanium-clad reactors or brick-lined, rubber-lined reactors, and aqueous HCl. Just ferocious conditions. What we wanted was something that was just acetic acid and sodium potassium acetate, along with copper.

What I concluded after several years of working on this was that you couldn't get there. You simply didn't have the redox capability in the copper (II) acetate/copper (I) acetate that you had in the copper chlorides, so that after a relatively small amount of conversion, the palladium would fall out of solution.

What happened on the halide-contained vinyl acetate units is that, I think it was ICI [Imperial Chemical Industries] that commercialized one. There was a quote in one of the chemical journals, C&E News, Chemical Week, that said in a disclosure rare for a chemical company, ICI has admitted or announced that the process wouldn't work. You'd get a titanium reactor but you'd get halide-containing by-products—chloro-whatever—that would eat up the downstream stills and pumps and so forth. So today, as far as I know, nobody runs a homogeneous vinyl acetate process. It was converted into a heterogeneous. One of the unusual conversions. Then it went to palladium and gold on the support with either sodium or potassium acetate, a lot of the same components that were present in the homogeneous.

Now, I was pretty happy that we had concluded that it wouldn't work, and the company shouldn't continue development. What I found out a few years later from one of the higher-ups was that "we weren't successful with that." It was an interesting perspective. I always thought we were successful, that we'd gone in to check it and it just won't work. We didn't commit the company to spending millions. But his view was, if you didn't end up with process, then you weren't successful and in the big record book, it's marked down as a failure for you. An interesting approach.

Following the vinyl acetate, we looked at other things. We looked at allyl acetate, benzyl acetate. Benzyl acetate, we got some interesting work, and at that time, I was in something called "exploratory," I can't remember the exact words. But you could pretty much work on anything you wanted to. When you were finished, a very likely response was that nobody cared. It would be, "Oh, that's real nice work. (Yawn.) Why don't you publish it?" I made a conscious decision that, wherever there was a fork in the career road, I wanted to take the fork that I thought would take me in the direction of getting something, even if it's just a pump someplace, in a plant that was there because of some work that I'd done in the laboratory. Carbide had a researcher, Roy Pruitt, who was looking at the rhodium triphenylphosphite catalyst for hydroformylation, and they had a major setback in the pilot plant. After some eighty hours, the activity just crashed, and it was just out. There was a chemist down there, and that chemist decided that there wasn't any future there, and he asked to be transferred somewhere else. They asked me if I would serve on a team with, I think it was four other people, to try to develop the technology. We shifted from triphenylphosphite to triphenylphosphene. This group met, I think, once a week. Pruitt was sort of the chemist/general manager. I was given an assignment that you can express in a single sentence, and I think it took me a decade to achieve. It was to figure out how to introduce rhodium—because at that time, the rhodium sources were rhodium nitrate, chloride, sulfate—analyze for it in any stream, and then recover it when its useful life is done. Well now, how do you analyze for all these things? Then how do you recover it when its useful life is done when you don't know why it died in the first place? That's why it took so long. We had to wait until we actually had a plant running and find out which, of all this array of things could be the limiting factor, was actually the real factor that limited catalyst life.

But we had other people on the committee, there was what I'll call a "hands-on" engineer, somebody that ran the pilot unit, and then there was a statistician/mathematician that. Carbide has a lot of respect for statistical experimentation, factorial designs or self-directed optimizations, or whatever approach. There was this group of five people and we met regularly. It was fun work. Problems arose. I guess you could say I was the "clean-up" guy. I wasn't doing the first-line stuff. I was doing the mop-up stuff when things went wrong. We were running in probably a one-liter pilot plant, and found that we were getting excessive conversion of the aldehydes to dimers, trimers, and some tetramers. The question was: why?

So I started in the laboratory with Fisher-Porter test tubes sealed with rubber septa. I'd get results all over the place. One tube shows low heavies formation, another one is just phenomenally high. I concluded that it was something that was in the rubber septa that we were using, and was on the verge of checking into various types of stabilizers or initiators they use to make these things, and somebody pointed out, "Hey, there's a lot of zinc oxide, magnesium oxide filler in rubber." So you checked a little magnesium oxide, zinc oxide, and you find out they're fantastic heavies promoters. So there's what called an uncatalyzed Tishenko reaction that gives you a monoester of a diol, but if you happen to have metals around, then it is catalyzed and it goes rapidly. We then tracked back and found out the trouble in the pilot plant was coming from iron pentacarbonyl from the CO that had been stored in this cylinder for a while.

One of the things that happened at Duke that really helped me, was that I had, the first semester, a course called "Library." It was taught by this lady that just gave us, again, rock hard assignments, but when you finished her course, you had the confidence that if it was in the library, you could find it. I mean, some of the things that she had us look up. Again, you'd say, how after all these years do I remember? One was: "What is the estimated age of the earth as determined from isotopic lead found in Joplin, Missouri?" I don't know whether you've ever used Landalt-Bornstein but that's where we had to go to track this down. So the facility with the library was a big help.

I got in on the iron carbonyl and found that problems existed back in the days of railway coaches that had gas lamps. Town gas is CO, hydrogen, methane, and who-knows-what. But it was prone to get iron and nickel carbonyl in it, and if it was present, you'd get metal spurs on the burners of lamps, the metal spurs would catalyze decomposition of the carbon-containing components, and you'd get sooting of the lamp. Then more recently in France, they were generating town gas all year, and then in the off-seasons, storing it in subterranean caverns, and then they'd pump it out in the winter time. Well, the CO in contact with the minerals in the ground would form nickel and iron carbonyl and you'd get this same metal spur problem, and they describe technology using activated carbon in various ways to remove it. So we adapted this technology for use in keeping the metal carbonyl concentration low in our feed streams. The cleaner the catalyst solution, the better off you are. Ultra clean is even better, if you can figure out a way to do it.

We had an assignment, program, at Carbide — why, I don't know — but they built a unit at Institute, down the river, for making chlorocarbons from methane, HCl, and a heterogeneous catalyst that was copper chloride and dydimium chloride, which I think is a mixture of rare earths, and there may have been some other component in it—at very high temperatures, 300º C or so. They had already built this unit. They were going to convert the chlorocarbons into fluorocarbons. That was while we were still in the fluorocarbon business. We were given the assignment. In some absurdly short period of time, we had to increase the efficiency of the catalyst substantially. We used some real short-cut techniques for experimentation, almost changing things on the fly. One of the things we had was an index of merit. You had to be able to judge, "I've got a reaction." You know, you've got things like "How fast is it going? How selective is it?" And these other factors. We had this index of merit worked out, and each time we got a result, we'd throw it in and see how we were doing. Then it dawned on us what this index of merit was doing. It was just cranking the temperature up — just up, up, up. The lesson out of that is that you need to be real careful where your index of merit is because you'll get whatever the direction is that it's taking. That doesn't just apply to chemical reactions. It applies anywhere in life. Whatever you reward, people are going to do. You need to think about it.

I again had sort of a mop-up position on this project. I was given the catalyst that had become inactive and my job was to figure out why these things had deactivated. I was using a stereomicroscope. The particles were 100 microns, about like a period at the end of a sentence. Under a stereomicroscope, you could look at them and you could see that the salts weren't uniformly wetting the surface, they had pooled up. So we had the idea, well, if we could find out where they seem to be wetting, then we'd get that composition, and this is the only composition we'd put on them. So we tried that. It didn't work. We tried something else and it didn't work. I was traveling to our metals company in Niagara Falls for the analyses. I was there one day with a guy looking through the electron microscope and I said, "What is this little crystal that's sticking off from this particle?" He goes over and he looks at it and it's sodium chloride. "Well, what's sodium chloride doing there? We didn't put any sodium in this thing." It turned out that the secret on this one was that sodium chloride was a catalyst for the decomposition of the chlorocarbons. What we would measure was CO2, and when we'd see CO2, we'd assume that the burning was worse and that we hadn't been successful in the making the chlorocarbons, when in fact, we had been successful but we were just decomposing then. So we located some alumina that had been made with KOH rather than sodium hydroxide, so that you couldn't get the ion-exchange effect, which was how the sodium was ending up. We found out, yes, we got much better results and we were at or essentially at our target, and they canceled it anyway! You say, "Well, that's life!"

TRAYNHAM: At least you'd had the experience of solving the problem.

BRYANT: Yes. I mentioned the acrylic acid color work. At the conclusion of that work, I probably knew more about acrylic acid impurities than anybody else in the world, and the chemistry of acrylic acid is very much the chemistry of the impurities that are present, not just color, but its behavior. Because in the process of its manufacture at part per million levels, there are natural inhibitors made and there are natural accelerators. The way the acid behaves depends on the balance of these things. You'd say, "Well, inhibitors would be great. People don't want it to go off too soon anyway." But when they get ready to use it, they want it to polymerize. That's the whole reason they bought it in the first place. So what you're doing is, you're trying to find an inhibitor package that provides all the protection you need. Yet when somebody wants to, they can override it by adding extra peroxide and you're underway.

I think as a result of the acrylic acid work, I ended up as the principal technical witness, expert, whatever, on a multi-million dollar lawsuit involving the parcel tanker Alchemist. A parcel tanker is a ship that's divided up into a series of tanks, unlike oil tankers that, as far as I know, just carry one thing. This could carry a whole variety of things, and the tanks had different compositions, could be kept at different temperatures, and so forth. Well, in our first bulk shipment was a half a million pounds of acrylic acid that we were shipping overseas. Three or four days out to sea, the acrylic acid starts to polymerize. The ship sends out an SOS. The temperatures probably go 300-400º C—blisters the paint on the hull of the ship, ruptures the bulkhead, puts the ship out of commission for six months. They sue Carbide for loading defective acrylic acid. I get the job of defending Carbide, even though polymer chemistry had been very low on my list of activities.

Fortunately, while I was at Duke, I had a superb two semesters of polymer chemistry from a great teacher. That was all the polymer chemistry I knew. When we finally got into court, it was an arbitration rather than a trial, the opposition had a professor from Brooklyn

Polytech [Polytechnic Institute]—he had written three books and published fifty papers and whatever. So it was him on one side and me on the other with six hours of polymer chemistry.

What I did in this was to go through and eliminate possibilities. Is it possible this could happen? We'd do experiments and say, "It's simply not reasonable. No support for it." Then we'd go through another sequence. Same thing.

[END OF TAPE, SIDE 3]

TRAYNHAM: You were about to talk about your hearing on the acrylic acid polymerization.

BRYANT: Yes. I eliminated everything except the possibility that the crew—well, one, it's a fact the crew was operating a steam winch on the deck and the mechanical engineers that I talked with said that if the steam trap leaked, then steam could make its way into the coils underneath the tank that's holding the acrylic acid. Once the acrylic acid starts to polymerize, it's very much like a pile of wood, you know. The heat from one piece burning will dry out the adjacent piece, and then it'll just go faster and faster. The outcome of the arbitration was that Carbide was ruled to be not at fault, and we were real happy with the outcome.

There were three arbitrators. There was a ship captain, a lawyer, and a business executive. I'm there trying to explain about monomers, polymers, inhibitors, polymerization, heat of reaction, chain transfer, and so forth, and across the table from me is this professor of polymer chemistry and there was a day they asked me a question and I had to decide who I was going to answer the question to. Was I going to answer to the professor to let him know that, yes, I knew some polymer chemistry, or was I going to scale the conversation way down and just talk to the ship captain? It was a soul-wrenching moment, but at that time it was obvious to me that if I didn't talk to the judges, I might as well not talk at all. So I scaled the conversation way down and, as I say, we were successful.

One of the great things in being on the project was that I had an absolutely blank check to do anything I wanted to do. I had no supervision whatever. The people at Carbide—well, it was certainly a hot potato; nobody wanted to touch it. They thought we'd probably done something wrong. But the more I dug into it, the more I was convinced that everything we'd done was okay and presented facts. This isn't getting up and talking about "maybe this" and "maybe that." "We ran this test. This is the result. This is the conclusion." And on to the next hypothesis.

One of the activities that I've been involved in is what you'd call "incident investigation," when something unfortunate happens, and then you go in and try to unravel it. As a result of the success on the Alchemist ship, acrylic acid shipment, there was what's euphemistically described as an "incident" involving a acrolein tank at the Taft facility where, during a shut-down period, something happened in the tank that caused the tank to over-pressure. This tank, which was the size of a railway tank car, leapt out of the ground, opened up like a sheet of paper and probably went 100 feet into the air. There was a big investigation as to what went on and again, it's a matter of listening to what people have to say, putting the pieces together, applying logic, being able to deal with what I call "gray" information, contradictory information. I view that as a great attribute in a researcher. It doesn't have to be black or white. It can be 70 percent gray. It reflects your certainty. If you've got two pieces that seem to be contradictory, well, you drop back on your magic training and say, "Okay, I'll accommodate that. I'll just carry it alone. Sometime I have to resolve it, but I don't have to resolve it at this moment. Maybe there'll be another piece of the puzzle that comes along that makes all of these things get together."

We've had a more recent incident. It was in South Charleston. It involved a waste residue tank. It was an intermediate facility before it was sent to the boiler for burning. In that case, I think we really nailed that one down solid, and it took a sequence of six things in the way of a chain for the event to occur. I say a "chain" because if any of one of those six had not happened, the event wouldn't have occurred, and it gets beyond the ability of anybody to think and anticipate. One of the key bits of evidence was a valve that had material still on either side of the valve. On one side was the last material to go in the tank, and on the other side was the composition just before the tank over-pressured. We could look at it and say, "The only place this material could have come from is x-unit," and the operator says, "There's no way for it to get there." We said, "Well, here it is. There has to be a way." We went out and looked, and I don't think anybody ever figured out how it could have possibly gotten there, but you just look at the facts in front of you.

Another area that I've been active in for many years—I don't know how long, twenty years—is what you could broadly describe as "intellectual properties." I pretty much handle all of the intellectual property activity—I'll say from the scientific side. Of course, there are attorneys that are doing the other side, but in terms of office actions, looking at what the examiner says, figuring out what additional experiments of data might need to be collected to convince the examiner. In Europe companies can oppose our patents. Going in and developing the arguments to counter whatever arguments they've made. If we're opposing somebody else, developing the arguments. It's an exceedingly challenging activity because, depending on what you happen to be doing, the logic process is different. There may be three to five different logic processes you would follow. If this is the situation, this is the way you would interpret a document. If this is the situation, you interpret it differently. This situation, you interpret it still differently.

Probably there are a few people that have the knack for it. I used "knack" earlier with respect to math and organic [chemistry]. There's a very definite knack associated with patents, being able to understand what they actually say. I had a friend here that told me actually a couple of times that I was one of the few people he knew that actually read stuff. In patents, a bit of freedom you have, if you're the inventor, is you can define what the words mean, and the way you do that is in the specification. I'll give you an example that the government used: "RCRA [Resource Conservation and Recovery Act] hazardous waste." There are regulations about "RCRA" solid waste, and then the regulations go on to say, "By 'solid waste' we mean any solid, liquid, or gas." [laughter] So you say, "Well, that isn't exactly what I learned in school!" But it's "what do the words say?"

Now, this sometimes does not serve you well at home. I think it drives my wife and daughter crazy because they'll tell me something, and I'll say, "Well, by that do you mean—?" and then I will give three possibly interpretations of what they've said, and their response will be, "Oh, you know what we mean. You're just trying to bug us or something." When, in fact, I don't know what they mean! Because I could see reasons why it could be any one of these things.

TRAYNHAM: Did you move into this area of intellectual properties because you did have a knack, a particular interest, or was your move into that area promoted by supervisors in the company?

BRYANT: By loss of a key individual. Roy Pruitt, who had been the father of Oxo, retired. He later moved to Baton Rouge, by the way. Did you ever meet him?

TRAYNHAM: The name sounds familiar, but I'm not sure I've met him.

BRYANT: A very skilled researcher. He left and I had the most knowledge of what had happened in Oxo since I'd been on it effectively from, if not day one, month one. I enjoyed it, and I think I do have a knack for it. We've been exceedingly successful. Last year, it was phenomenal. We did something I think is unparalleled in the history of the corporation. We got seventeen patents allowed in a single day. I'm not sure if many people ever have been able to do that.

Let me talk a bit about just the laboratory. Laboratory procedures: Do you know how to distill? Can you crystallize? Can you filter? How many different reactions can you run? Do you know how to prepare a crystal for crystallography? What about making solid derivatives? Dinitrophenyl hydrazones. I view each of these things as being equivalent to a word in your vocabulary, and if you only know a few words, and somebody asks you something, you can't give a reply. I'd say it's like my ability to speak German. You've got to keep it down on a real low level. The more laboratory techniques you know, when presented with a question, the more eloquently you can answer the question. So I'm a great espouser of learning every technique you can imagine.

I can give you an example on analytical chemistry. I've used all kinds of analytical techniques and one of the great things about working in a technical center like this is there are loads of super-experts around. So my philosophy is: I learn it, although learning may be as little as taking a three-day ACS [American Chemical Society] short course; get the vocabulary so that you know what the technique's capable of and what its artifacts are, so you can converse with a person that really understands it. Then once you feel comfortable with them, you can let your skills slip there. You go on and learn something else. But you remember the guy that knows and, in particular, you remember what about the technique can fool you. I don't think I've ever seen anything taught this way, but in all these techniques there are things—well, the sodium chloride example I gave earlier. You need to know that you can't detect sodium chloride with infrared. I mean, I think that anybody would figure that one out. But you're going to find that type of thing throughout the analytical techniques.

I have a book—it's an old one now—I think it's called Process Control Handbook (1), and in a pocket in the back of this book there's this huge spreadsheet that lists over one hundred analytical techniques, what they are good for, how sensitive they are, and what their deficiencies are. At the time that I had the problem on figuring out how to analyze rhodium in any stream—my initial assignment in Oxo—I went through this sheet and looked at, one by one, all one hundred or so methods. It isn't just analytical techniques and it's not just organic reactions. I was on a committee once when Carbide was a much bigger corporation. One person was selected from each of our sites. I was the one from this site. It was a super assignment. We would go on what I would call a "cook's tour" of some other site. They would take us to this location and somebody would explain what happens there. We were very different then. We had a Films and Packaging Division that made the wrappers for salami and bologna and the skins that hot dogs were prepared in before they're pulled off. They'd give us a tour and show us the equipment, explain how things were done. Got a tour through a foods research laboratory where people figure out how to make Slim Jims or whatever. Well, one time we go to the Linde Corporation—you know, Linde's big in gases—and they had this huge laboratory with a control room just like an airport, where you're up in the control tower looking out. It was a process called "scarfing steel." They'd get these steel ingots and there would be this oxide or crud on it that needed to be removed. The way they did this—just in the pilot unit, these things are probably four feet long and maybe 18 inches wide and thick—they would turn on a tank of oxyacetylene torches and heat the ingot red-hot and then turn off the acetylene so that they're just feeding oxygen and just burn this stuff off in the most incredible display of sparks that you can imagine. At that time, I think they said that Linde had something like 90 percent of the business in the world on scarfing steel, which I'd never heard of and never run into again.

Well, we got involved in a problem. The syngas generator cooler cubes were not lasting as long as we desired. Well, how do you make syngas? You feed methane and oxygen. I remember this demonstration of what happens if oxygen gets on the steel. I mean, you can burn it away in seconds. The hypothesis to be tested was: we're getting periodic intervals where the oxygen concentration is high. We had a tough time selling that to anybody, but I finally got somebody that would get some plant data for me, and so finally they send me this huge amount of operating conditions and then it's, "Well, now I've got it. What am I going to do with it?" I think in a matter of twenty minutes of messing in an Excel program and graphing stuff, I had the clue as to what was going wrong. The first hypothesis wasn't right, but it got me close enough that you could see something that was most unusual happening, and it was happening in a periodic way, and when we eliminated it, we eliminated the problem and ended up doing a lot of reading on various types of metallurgical problems.

I just think that if you've got a set of investigative skills, you can pump almost any kind of information into them and get a long way toward problem-solving.

Although Oxo is considered one of the three major business thrusts for Carbide, it has a relatively small staff compared with the other two. Number one is Unipol, polyolefins technology; Number two would be ethylene oxide, ethylene glycol, and derivatives. The number three business where Carbide feels it has a competitive advantage is Oxo. An advantage of working in an area that's relatively small is that you get to do a lot of different things. One of the things that I've done over the years that's challenging, interesting, stimulating, is the presentation of the Oxo technology to prospective licensees and also the training of licensees once the agreements are signed. This assignment has allowed me to travel to many of the world's capitals. One of the challenges is to figure out how to take the technology and explain it to whoever the audience might be. Occasionally it's chemists. More often it's engineers. But then you'll get off into bankers and financial advisors. I have sort of a saying that anybody that really understands their subject can explain it to anybody. It's a matter of figuring out, taking the material and gearing it for the audience. We've given some untold number of presentations and when the next one comes along, we say, "Oh, it's just like the other one." But it turns out, every one is different because the questions you have to ask are: who's the audience? what is their background? what points are you trying to make? and how much time do you have? I had one technology, the Oxo technology we developed for making 2-propylheptanol, that on a trip to Japan I presented in a day, in a half a day, in an hour, and in ten minutes.

TRAYNHAM: The same talk.

BRYANT: The same talk. There's an exercise in terms of presentations. As I said, I do a lot of mentoring, training people, and I say it's something that's called an elevator speech. There's an absolutely classic example in a movie—I think it's Working Girl. Melanie Griffith and I forgot who the "bad guy-gal" was. Melanie Griffith has been fired. She's leaving the office building with her worldly possessions in a box, and one of the big cheeses bumps into her, knocks the papers to the ground and starts a conversation and asks her how she came up with the idea for merging some radio with a TV station. By the time the elevator has reached the top floor, she has convinced him that it was she that came up with the idea, not her supervisor.

What I tell people is, "In your career, you will get access to high level people that can make decisions, but the time is going to be limited, and you're going to have to be able to present your case in three minutes or less." That's what I call an "elevator" speech. I had one one time. I was interested in seeing if I could get approval for a new library building for the technical center. I don't think a new building had been put up in a decade or more. I was at a conference. I saw our president during a break. Probably in less than two minutes I explained the idea to him, and he said, "Go for it." It involved hiring architects and selecting plots and designing what it would be like. Then we had some drawings—that was as far as it got. What happened was that we were in a trough. Money wasn't available. It got put on the shelf. Then when money did become available, the circumstances that made the location attractive had changed. It was going to be put where the cafeteria, medical, and human resources all had their facilities, and we figured that would be a draw and maybe people would stop in the library on the way back. Then they moved everything except the cafeteria, so that it was less desirable. But the real thought is, be able to express your concern, need, case in just a couple minutes, and when you do that, one of the requirements is that you understand it pretty well.

In terms of philosophy—you can say, general concepts that relate to the work leading to the Perkin Medal—the philosophy that we have adopted is: get something that works, and then figure out everything you can do to it to mess it up. What can possibly go wrong? How can you poison it? How can you inhibit it? What are all the things you can do? Then either figure out ways to keep that from happening, or, in many cases, assume that it's going to happen anyway and figure out a way to correct it if it does. For the, I'll say our mainline Oxo technology, it's exactly what we did. We went through tests to determine essentially every poison or inhibitor you could imagine, and then put in schemes to keep it from happening, and in certain cases, if it did happen, figure out how to correct it. I think that that has contributed to the success of the program.

In terms of licensed plants, I think the number since 1977 is that over 90 percent of the plants that were built are using our technology, and I can only remember one or two cases where in head-to-head competitions, the other technology was selected, and in at least one of those it was done based on financing arrangements rather than anything to do with the technology. We want it to be the best in the world.

Oxo has gone through two phases, maybe even three phases. There was an initial phase starting in 1967, when I joined the program, that lasted until 1970-ish. It could have been 1971. At that time, Carbide was still in the ethylhexanol business. A judgment was made that we were not going to be in the plasticizer business anymore, so the technology was mothballed. We were told, "Shut everything down. Go off and do something else," which is the interval in which I was working on acrylic acid.

The technology was brought out of mothballs for our Ponce, Puerto Rico plant. There was a need to make the Oxo process competitive. What we were running was "me-too" technology, cobalt technology, and so the decision was made to develop the rhodium triphenylphosphine technology to the point that it was competitive and good, which is what we did.

TRAYNHAM: At the time that you entered that program, did you know that a rhodium triphenylphosphine catalyst would be effective, or was that part of what you had to develop?

BRYANT: No, that was part of what we had to develop. The initial work was actually done with phosphites. If you go back and look at our basic patent, I think you'll find that most of the high isomer ratio examples are triphenylphosphite, or some other phosphite. But in pilot work, we made the judgment that they didn't have sufficient stability, but we didn't have enough understanding of the chemistry to do anything about it at that time. TPP [triphenylphosphite] was available. I think it was ten times more expensive. My recollection is triphenylphosphite was fifty cents a pound and TPP was five dollars a pound, but the cost was still acceptable, so we simply shifted to triphenylphosphine, and that's the technology for which we got the Kirkpatrick Award in 1977. That technology—we speak of it as if it's one thing, but actually it's a variety of things. The propylene hydroformylation is what the world knows about, but there was a variety of other processes that we commercialized that really have not received much publicity.

[END OF TAPE, SIDE 4]

BRYANT: We were running a rhodium Oxo on propylene, a rhodium Oxo on ethylene to make propanol, proprionic acid, and there was a desire to convert our last cobalt Oxo plant, which was butene, to C5 products. The interesting thing there is that in contrast to propylene, where the desired product is one with a high normal to branched aldehyde ratio, in the case of the C5 products, what was desired was exactly what cobalt was making around two to one. Markets had grown up around this product mix and they simply didn't want to change. So that we had to take technology that, you can say, intrinsically wanted to give a high isomer ratio, and now get it to give a low isomer ratio, ideally one that was exactly the same as cobalt. We managed to do this. It was the first time I got to go out and travel with salesmen and see their side of the world.

One of the things that we found out that was fascinating is that, as an organic chemist, I always viewed mixed products as less than desirable, schlock products. You know, I want pure linear acid, I want pure branched acid. We were selling this product as a mixture of acids, and we talked to the people that were using them and said, "Hey, would you like to have more branch? Would you like to have more linear?" The answer was, "Absolutely not. We want exactly what you've been selling." The reason was that these were used in synthetic jet lubricants. The qualification program for a synthetic lubricant is so expensive and intensive that once something is approved, you don't want to change it regardless. It's hard to imaging anything that you could do financially, in terms of lowering the price that could justify going in and changing the mix.

One of the memorable statements was that if we quit making the branched acids, branched C5 acids, it would ground every jet in the free world. [laughter] So we said, "Don't worry. We're going to give you the same product that came out of the cobalt process." We did, and that has gone well. As I say, there have been others, but the world knows relatively little of them. There have been a lot of programs that got through the R&D stage that then stalled. As a result of Carbide having the world-wide recognition as being a leader in Oxo chemistry, we have a lot of companies bring their problems to us. "Can you do this?" Sometimes we do it partly on speculation. Sometimes it's done on a fee basis, you know, still with the thought that it would be licensing income available.

One of them was to take butenes, make entirely branched isomer andions, so this is totally the opposite of what you would want in a conventional TPP rhodium process, and then this branched aldehyde was going to be, I would say, dehydrated to give isoprene. The explanation was that people are moving—in the Far East—off the rubber plantations into the cities. There are not going to be enough people available to tend the trees and they need a synthetic source. So we worked out the technology and then the company announces that once they told their management they had a way of making it synthetically, they found more in their refinery streams. So we've had a number of projects that have gone that way. You can say technically we achieved it, but it stumbled somewhere down the line.

TRAYNHAM: Do you feel a sense of disappointment at that stage?

BRYANT: Yes, but we learn from all of these things in terms of research philosophies. We've got one that goes like this: from the time somebody brings a request in until they want to start building a plant is about two years. But it takes us about five years from scratch to get to the place. So what we do is, you could call it "brown-and-serve" technology or "on the shelf" technology. We'll make discoveries, develop them to a certain point and simply put the information on a shelf. It's a solution waiting for the problem, a cure waiting for the disease. Somebody brings the problem to us, and then we don't have to start at scratch. We can start further down the line.

The shortest one we ever did is a company brought us, I'll say, a challenge. "Can you do this?" In six weeks we delivered a definition of technology that included a pilot plant run. We were able to do this simply because we could eliminate most possibilities as "this can't work; this can't work; that couldn't work; it'll have to go with this thing." We tried it and it did work with that. Then once we'd done it, they didn't express much interest.

For homogeneous catalysis, I've got what's now become the institutionalized rule of the four Ss. There are four major things to consider: speed or reaction rate; selectivity; stability; and separation. For a homogeneous catalyst, these are the four initial biggies, but hidden and not commonly discussed are over twenty other things that you have to keep in mind in order to have a commercially successful process. For example, if it's a catalyst that's using an organophosporus ligand, you have to be able to manufacture it at a reasonable price. It has to have an acceptable level of toxicity. Some that we're using now have about the same toxicity as table salt.

You get into things like storage stability. Can you put it in a container and keep it for a year or two years or three years until it's needed? What about the catalyst solution itself when it's out of the reactor? What does it do? A realization that, I will admit, has only dawned on me relatively recently, but one that's really important, is that people will talk about, "Oh, this is the reactor. This is the separator, the vaporizer, the recycle, the hold pot." Everything is a reactor. An engineering friend told me that there's only two things in chemistry that matter: temperature and concentration. If somebody brings some other concept in, they'd better think about it because it can be reduced to those things. You've got temperature and concentration in every piece of process equipment. To be successful, you have to think about the chemistry that's occurring in every one of these pieces of process equipment.

The vaporizer and the catch pot after the vaporizer are places where a lot of surprising things can occur because you concentrate the catalyst, the metal concentration goes up. If metal clustering is a possibility, you've got prime conditions for it. You don't have the CO partial pressure you have in your reactor so this is going to facilitate clustering or aggregation.

We had a third phase of Oxo chemistry, and I'm not counting in this the changes from gas recycle to liquid recycle and various other improvements. Those we were running along with rhodium TPP. The original gas recycle has an interesting origin. In gas recycle, you keep the catalyst in the reactor or tank. You don't circulate it, and you blow gases through to vaporize the product and the aldehyde condensation product, which you have to keep controlled, otherwise the reactor will overflow. Part of the reason for doing this was an emotional issue: the concern over circulating catalysts through pipes and pumps, and the fear of leakage of the valuable catalyst. After we gained operating experience, we came out with what's termed a liquid recycle variation on the process. There the hydroformylation is conducted in the reactor, and the product and by-products are separated in a vaporizer. What this does is that, you could say in mathematical terms, it gives you a second equation. You can have conditions for reaction. You can have conditions for separation, and they no longer have to be the same. It gives you a degree of freedom so that you have better removal of heavy by-products should heavies get out of control, which they can if you get iron or some other metal contaminant in there.

The third phase, I would say, really starts with our going back and looking at phosphite chemistry. We started with phosphite chemistry and we abandoned it or shelved it because the decomposition chemistry of phosphites in the presence of aldehydes was simply too complex. Between the interval from the first phase to the third phase, phosphorus NMR came along. I think that in the absence of phosphorus NMR, we could have never unraveled the problems that are associated with this system. They are so incredibly complex. We have a couple of named reactions in the sense that they were particular reactions that we ran that, oh, you could say they were failures, but they were incredibly informative. We have various types of experimental apparatus. We've got small Parr autoclaves. They're not actually Parr autoclaves, but they're similar enough. They're batch reactions, where gases are fed, no product is removed and chemists look for rate and selectivity information. But you don't get any information on the catalyst's life. Then we go to a stage that we call "continuous reactors." They're really high pressure Fisher-Porter test tubes in which gases are sparged so that they operate as a gas recycle reactor, or gas stripping reactor. We really don't recycle the gases. These can give you information. They'll run from a few days to a couple of weeks, and they'll give you catalyst stability information in addition to the rate and selectivity. Then we have pilot plants that can simulate every feature of a commercial plant. We typically will scale up about one hundred thousand-fold in going from the pilot plant to the commercial facility.

We were running in these gas-stripping reactors and running through lots of different ligands, and we were coming into Labor Day, and one of the reactions wasn't going very well, but people were interested in getting home for Labor Day weekend, so we let it run. When we came back after Labor Day, we found that the reaction had gone from having almost no activity to being super-active. It filled the entire reactor, filled the tube that led to the automated gas chromatograph, filled the gas chromatographic column, destroyed it, but we were happy about it because something really unexpected had happened. From that, we learned about the severe inhibitory effect of phosphites, and to counter that, well, the way to counter it is you put bulk on the ligand. But you put bulk on and you still have stability problems, and we made a discovery that by incorporating certain structural features, say, a bond between two aromatic groups so they became biphenyl, and putting in stearic bulk, the patent says, I think, of isopropyl or greater, isopropyl, t-butyl, amyl, t-amyl, that we could get phosphite ligands that were substantially more stable than what we had been seeing in the triphenylphosphite. Big advance.

Another one of these discoveries that we saw is, we were feeding 2-butene into a process in the gas-stripping mode and coming out of the process was 95 percent 2-butene, 5 percent 1-butene. You say, "Well, what's the significance of that?" Well, we knew that the catalysts were vastly more reactive for alpha-olefins than internal olefins, and we also knew that that happened to be the thermodynamic mix for butenes, that at equilibrium you're not going to have more than 5 percent, so what it said was we were feeding an internal olefin and isomerizing it so fast that even though the reactor hydroformylation was occurring at a good rate, we were ending up still with a thermodynamic mix. It was a big deal discovery.

We had another bit of good fortune when a technician hurried up a synthesis and made a product, a ligand, that gave a little better than expected result. Ernie Billig was doing the work and whereas we'd been seeing isomer ratios in the one or two at most, this could have been up to three or four or five, and after a substantial investigation, we figured out that what had happened is that we had made a class of compounds that we call bis-phosphites that simply have phenomenal performance in hydroformylation. It's as close to magic as anything I've ever seen. We had put the examples in the patents and there's an example in there of using 2-butene, and I think that the isomer ratio that we got was twenty-five to one. If you're familiar with Oxo chemistry, for decades people couldn't get above four to one. There were just hundreds or thousands of articles. If you were familiar with rhodium TPP, you'd know that internal olefins are effectively unreactive. It only works for alpha olefins. So here we were feeding an internal olefin and getting a phenomenal isomer ratio, and I'll admit, if somebody else had done it and I had read it, I'd say they got the decimal point in the wrong place. It should be two and a half, not twenty-five. But in fact, it was twenty-five. It laid there for years, and nobody noticed it. Then I gave a presentation in Cambridge, in England, a symposium on the platinum metals, and that's when it was revealed to the world for the first time what we'd been doing and the significance of it, and then after that work really accelerated around the world, by our competitors and by people in academics, looking at this class of ligands.

The performance of these things is so good that it's mind-boggling. I would say most people don't believe—and "people" would be both academics and industrial types—that you can develop a commercially viable process using them because they're too unstable. I'll just say we have new technology running in Taft, making n-butanol, and the process is a chemist's dream. It started December 1995, and there has not been a single problem that's come back to the chemists to be solved. During the stages that we were researching it, we managed to address every issue that might have arisen so far and built them into the process. It was just a phenomenal performance. So that you can say, "What work really leads to the Perkin Medal?" It was starting with "me-too" technology, coming out with the world's best Oxo technology in the mid-1970s, and then in an event that may be unprecedented, coming out with a generation that will succeed it. What typically happens is that a company gets good technology and then management says, "Hey, we've got the best. You don't need all that R&D money." It cuts back on the funding, and then some other company goes in, does the R&D, and they get the lead for whatever the subsequent generation is.

I think that the reason that we were successful here—there's a number of reasons. One is the licensing program that we were able to license our first-line technology because we don't compete with our licensees, or our licensees don't compete with us. We are interested in butanol. They are interested in ethylhexanol, so that we're able to provide them the best technology that we know of. That brings in some licensing income, and that gives us the basis for telling our management, "We need to continue working in this area, to continue to make innovations, both for ourselves and for the technology that we license. We've had long-term support from management for this technology, over twenty years of continuous support. Now it's recognized as one of the three key technologies for Carbide.

Do you have any questions, want any more detail on the specific work?

TRAYNHAM: No. I think it sounds as though you were very close to being engaged in fundamental research, more so than one might expect in a product development situation.

BRYANT: Oh, yes. This is, I think, a misconception. People think that because you're doing industrial work, you're not doing fundamental work. What I find is that because the stakes are so high, you've got justification to learn way more about what's going on than you would have in just about any other circumstance. The sad part is that the world never hears about it. You can say, "What is a major disappointment if you're an industrial chemist?" If you do something that's great, you can't tell anybody about it, for the most part. You've got to be able to live with that.

TRAYNHAM: Apparently, your company managers have not had difficulty seeing the importance of such fundamental research in the operation of the company.

BRYANT: No. That has changed over the years, and I can say that I really like it the way it is right now. I think I mentioned real early, back when I was talking about the benzyl acetate, that we'd do something and people would say, "That's nice, y'all. Why don't you publish it?"

When I came to work, you could literally work on anything you wanted to, and nobody cared. That wasn't satisfying for me. There was a period wherein we had what was called an "Exploratory Basic" or EB committee that had, I don't know, a few million dollars that it could dole out on projects. Those projects were divided among three technical centers, and what would typically happen is that something would get funded at one lab pair or maybe half a lab pair, and we'd try this idea, and the thought was, that if you try a bunch of ideas, you increase your chance of success. Well, there weren't many successes that came out of it. The current philosophy, and one that's, I'll say, strongly supported by Dr. [William H.] Joyce, CEO, is: figure out what you really want to do and then pour huge amounts of money into it, comparatively speaking. It's much harder to get something funded or approved, but once you do get it approved, you can expect considerable support for it, and the commercial aspects are thought about much sooner than they would have been previously. We have programs for—they're called "gates". They're checkpoints. In order to go onto the next stage, you have to have met these criteria or passed these tests. I think it's a great thing. I really don't want to work on things that aren't going to go somewhere, or at least have a chance of going somewhere.

In terms of memorable lectures that I've gotten since I've been here, one came early on in my career, and this was an AD who'd gone to the school I'd gone to, and I don't know why I happened to pick it, but the advice was: if you want to do something, don't ask anybody, because the chances are they'll tell you no because they don't want to set a precedent. If what you've done was inappropriate, the chances are you won't get into too much trouble. I've heard it expressed differently: it's easier to get forgiveness than it is permission. That was a good piece.

There was one, while I was working in acrylic acid. For no apparent reason, my supervisor calls me into the office and he says, "David, you're too careful in your work." He says, "There are going to be times in your career when business demands an answer and they've got to have the answer by 4:30, and you don't have sufficient information, but you've got to make a recommendation anyway, and you've got to live with the fact that you're going to be wrong sometimes." If I were going to look at a single distinction, what's the major distinction between academic and industrial chemistry, it's that academic chemistry I would say must work at the 99.9 percent certainty level all the time, whereas industrial chemistry has a floating level of confidence depending on time constraints and importance. It's okay to be wrong sometimes. It isn't okay to be wrong in publishing a journal. You've got this 20-80 rule; you get 80 percent of the information with 20 percent of the effort. In academics you have to check it another twenty different ways to be sure.

We've got a nice—somewhere between "nice" and "great"—arrangement for continuing education here. What I tell new people is that once they get past this flood of required courses at the start, they can expect to be taking one or two courses for the rest of their careers. Carbide endorses this, supports it. We've got loads of experts that can teach things on-site, or you can go off-site. Again, I've never been denied any request to get any kind of training I want. In terms of important courses, probably the most important course that I had was what's called the "Burger Writing" course, and this is not one that teaches you about the commas and verb tenses.

It's teaching you about putting unnecessary words in your writing. They blame it on the English teachers where you'll be told, "Give me a five hundred-word thing on sharpening pencils." Well, if it only takes fifteen words, you've got to repeat it and say it five different ways. I realized that before Burger, my ability to discern good writing exceeded my ability to write, and I didn't know what was wrong. Reports didn't get out as promptly as they should, and I took this thing and it was an absolute revelation, that if it only takes you two sentences to say it, just say it in two sentences.

[END OF TAPE, SIDE 5]

BRYANT: Of course, you don't want to mess with their style, but when they've got one of what Burger calls "mistakes of wordiness," you know exactly why you're making a change. We had a fellow here, Charlie Hendricks, just recently retired, that was a real disciple of statistically designed experiments. He preached it, hammered it home. I would probably rate Burger as the number one most important course I ever had outside of college or grade school, and experimental design is number two. At one time we were doing a Keptner Traego course. This works whenever something used to work and doesn't anymore, or Unit A is running better than Unit B. It's an organized way to go in and gather information and ask questions and then test to see whether or not your hypothesis will fit the behavior that's seen. It's no good at all, for example, if you want to make something and you've never been able to do it. It doesn't give you any clue as to how to do that. But if you've, let's say, got something that once was better, or two things that are different, it's a really neat way to organize the data and ask questions.

There are a large number of strategies that can be followed in experimentation. Again, you can say that this is general comments on leading to the Perkin Medal, how you end up to be a successful chemist. Well, I think the more strategies you have available, the more flexibility you have when presented with a problem. The scientific method is the bedrock of many of these, but above that is another layer. There was one I was introduced to early in my career, that was called "strong inference", that I think has some nice emotional aspects to it. In the standard scientific method, you're trying to disprove your hypothesis, and if you've been working on it six months or a year and you suddenly realize it's going to go down the tubes, there could be some reluctance to see this thing go. In "strong inference" you have two hypothesis and you design an experiment that will distinguish between them and you end up with the stronger of the two and so you always end up ahead. I think that it makes it a lot easier for people to be objective in their experimentation if they don't have to lose their only child.

There's one that is credited to a lab technician. You get into circumstances where you can't seem to make something any better, can't get the yield up. If you can't figure out how to make it better, then figure out what things make it worse, and then quit doing them. It sounds silly, but it actually works. We've used it a number of times in accelerated tests. We'll figure out how to make the catalyst activity decline even faster. Is it the presence of some component? Is it the concentration of the metal or the ligand? Then we stop doing it, and you get longer catalyst life. I don't know exactly how many of these things there are. There's one that's termed “onion peeling.” In onion peeling, what you’re doing is going through layers of a problem. There’s some effect that is the dominant effect, and once you can understand it, remove it, or get it under control, then it lets you see an underlying effect. You can do the same thing there, and then again and again, and I’ve had situations where I’ve gone through at least four layers of digging down through a problem, until you finally get to the place where the data that’s left doesn’t have any information in it. You’re just looking at noise.

There’s a question on scientific innovation. I don’t have a lot to say there. My definition of innovation is the implementation of an idea or of a technology, and that is really what we’re after. We do the work. The individual experiments are exciting. But what people really like is to be able to look at a pump, which was one of my original objectives in a plant. Now I can go down and look at a plant that’s as long as a football field and know that I was there when the first few milligrams of this material were made in a tiny autoclave in a laboratory.

TRAYNHAM: You’ve made reference several times to the essence of teamwork. You referred to yourself as one member of a four-person team assigned to solve a problem. How do you find scientific teamwork in the industrial environment? Does it permeate the company or is it an occasional happenstance?

BRYANT: I think that it permeates Carbide, and recently, in what we call the “Taft butanol” process, it was taken even a step further, which I’ll tell you about. When we start a program now, and I mentioned that in order to actually start one, it’s got to make commercial sense and that when it does make commercial sense and has approval, we get the funding that we need. We have engineers that are R&D engineers; we have engineers that are engineering department engineers. Under the old scheme, it used to be that R&D would do its thing, and then we’d tell engineering, “Oh, we’re through. Here it is.” Now, the people in the engineering department are involved effectively from day one of the project, and the way they’re used is to help in deciding which fork in the road to take. Are we better off using a small reactor at 1,000 pounds, or a reactor that’s three times as big at 500 pounds? It has big influences on the chemistry. They come back and they tell us what the answer is, and that’s the road we take. “What is the really expensive part of this process, as we see it today? This is how we imagine it going.” “Oh, this refrigeration compressor is killing you. It costs two million dollars and it’s using more energy than you can imagine. Can you figure out some other way of doing this step?” So then we go back and look at the process and see if there’s something we can do with the chemistry that will keep us from having to use the refrigeration system or the compressor.

This was taken a step further in the Taft butanol design in that once R&D finishes its job, we put together what’s termed a definition of technology. It describes, in a sense, everything you need to know about the chemistry, and that’s given to the engineering department and then they convert the chemist’s ideas into something that would run a plant. Then normally the plant would be designed and built, and then something goes wrong in the plant, and it’s up to the poor maintenance engineers to figure out how to get the converter out of the middle of this mass of pipework. So what they did for Taft was to get the maintenance engineers involved in the early design stages so they could tell people, "Yes, we need this distillation column, but it would be a lot better if you hung it on the outside of the framework rather than putting it inside because we can bring a crane in on this road, lift it up, and do this." So that the unit that was built was very serviceable. The big equipment's accessible.

Another philosophy that's come down from the highest levels is that there are ten different ways to do it, and we don't want ten different ways of doing it in ten different plants. We want the plants to look more nearly the same. So that before our most recent plant was built, Carbide announced the construction of another one just like it, and I mean, they really mean "just like it" because they want to avoid the re-engineering expenses that are involved. This was a tremendous compliment to the people who had developed the technology, that, "No, you don't need to change anything. We like it just the way it is." I don't know how today a company could succeed without having a great deal of involvement of a multitude of disciplines.

TRAYNHAM: What do you see as important for the future of chemical R&D?

BRYANT: In terms of technology, our goal is still to be number one in the world, but the things that trouble me the most are what I'll call "regulations" or "over-regulations." We've got a bunch of Canadian geese on the grounds. This site is 560 acres, and at any given time you might see a flock of one hundred geese out grazing on the grass. I think the problem is common over the U.S., anywhere where there's grass and lakes such as golf courses and parks. The geese can become aggressive, particularly at nesting time. But they are protected by the government so you've got this set of regulations that you have to deal with. Well, there's another set of regulations that you have to deal with about the safety of people. So what do you do if geese start nesting near the sidewalk and you can't disturb them, and anybody coming up the sidewalk is in danger of being attacked by the geese? Now, that's a simple example.

If you get into the laboratory, then you get OSHA [Occupational Safety and Health Administration] and RCRA and goodness knows what-all regulations that seem to be at cross-purposes. I'll give you an example. If professionals—these are people that have spent many years being trained in chemistry and handling chemicals—want to handle sodium hypochlorite, they've got to meet all of these constraints and controls that are put on them. On the other hand, the identical chemical, sold in the grocery store as Clorox, can be bought by anybody that has no knowledge of what it is, no training, and it just doesn't make sense. It's like they're putting the most restrictions on the people that know the most about it. Any one of these you can live with. Two you can live with. But then it gets to the place that there's just so many constraints that it's extremely difficult to get anything done. We've got something that's called "lock-out/tag-out"—"LOTO"—which, if somebody is working on a plant, makes incredibly good sense to me; if an electrician is going to go out and be working on a power line, he wants to lock out the main circuit breaker, literally, put a lock on it so that somebody can't turn it on while he's got his hands in it. Likewise for the big equipment. Well, some regulations come along and they say, "Well, you really should be treating the lab equipment just like plant equipment." You say, "Oh? Then we've got to have lock-out/tag-out procedures." The definition was: anything that was connected to an energy source. So that's electricity, or it can be a cylinder of hydrogen or helium for a gas chromatograph. So then the question comes up: if I want to change the septa on my gas chromatograph, do I have to lock-out/tag-out the chromatograph while I'm changing the septa? Do I have to pull the plug out of the wall, put a plastic box around it, lock the plastic box so nobody can plug this in while I'm doing this? Well, I think they finally decided, no, you didn't have to do that. But for other lab activities, it's really hurting productivity and I don't think that there's any real gain that's coming out of it.

TRAYNHAM: To go back to your example of sodium hypochlorite, do you think that the quantity of materials involved and thereby the potential for greater hazards to personnel is driving such regulations?

BRYANT: I haven't researched this, but I would say the likely explanation is the regulations are being made by people who probably have never had a chemistry course in their life and who, when they read the newspaper, always see the word "toxic" in front of "chemicals" and simply don't understand. They'll ask questions such as, "Is this chemical safe?"—you know, in an absolute sense. You could say, "Is electricity safe?" Now, how would you answer that? If it isn't safe, then shouldn't we get it out of our homes? But this same kind of reasoning isn't applying in chemicals because people don't have the familiarity with them that they have with plugging in electrical appliances and saying, "Yes, it can be used," and only occasionally there's a problem. But if people had followed the guidelines they'd be okay.

TRAYNHAM: "Sodium hypochlorite" sounds worse than "Clorox."

BRYANT: Yes! You know what I think the most dangerous chemical is that we have in the lab? Could you guess? The one that kills the most people each year. Water.

TRAYNHAM: I was going to say "water," but then when you said "kills people," I wasn't sure it could be indicted on that score.

BRYANT: Well, they drown. But people are familiar with water, so it's okay. But you get to a product like glass. If we had only had plastic windows and somebody said, "Oh, I've got this new product—glass. It's harder, it's more scratch-resistant. Oh, and by the way, if you break it, you get shards that'll slice you open," do you think there's any chance that glass could be introduced into the market? I don't think so. It's this kind of problem. We can and we are living with the regulations, but it's consuming ever more of our time. I don't think that it's hit academics with the level that it's hit industry, and it may not have hit other companies with the same intensity that it's hit Carbide. We will obey all of the rules, period.

TRAYNHAM: That's the legacy of Bhopal.

BRYANT: The legacy of Bhopal and something we have to live with. What effect did winning the Perkin Medal have on my life career? This is an interesting one, a fascinating one. I got the letter and it was exciting, nice, but I'd say, "No change," until I looked at the book that the Chemical Heritage Foundation put out and I read about the other technologies that had won the prize. I thought, "My gosh! I studied this in so-and-so, and I learned about this process, whatever," and you suddenly realize that your technology, our technology, is now going to be on that list. That's when it became exciting. I do not view the medal as an individual award. Yes, I know it's given to an individual, but it's really given for outstanding commercialization that takes an army of people, and I just view myself as a representative, like a Senator, or a member of the House. Some individual had to be picked to go get it, but I think it's really the technology. I am absolutely delighted that we got this recognition, that we were able to go from a technology that was hardly even "me-too," to something that is the best in the world, and if the world really knew what we were doing, they'd be awed. I mean, it is the most advanced, sophisticated, incredible homogeneous catalyst that you can imagine. But it's secret.

In terms of life career, there are no more promotions for me. There are two ways to describe it. You can say as a senior corporate fellow, I've achieved the pinnacle of the scientific ladder, or you could say, I'm in a dead-end job! [laughter]

TRAYNHAM: I'm sure you prefer to view it as the first.

BRYANT: Well, I'd set a retirement date many years ago. That isn't changing.

TRAYNHAM: When is that?

BRYANT: It's May the 8th, 2000. My license plate number is DH, which means nothing, 5800. Just as a reminder. There wasn't any increase in salary, nor will there be. I mean, the salary is specified by your job level. Where a change did happen was with the family. I'd never really discussed with my family what I do. I'm not one to talk about work at home. I'll talk about other things. They knew. My wife thinks that all I ever do is go to meetings, and that I travel a lot. But beyond that, I don't think they had a clue as to what I was doing or the significance of it. I've got a brother who's a major competitor at senior tennis. He travels hither and yon and goes to all kind of award ceremonies. So I tell him there's going to be this fancy dinner in New York, and he says, "Well, how many other people will be getting awards that night?" I said, "None. Just me." [laughter] That was sort of like, "Hey, this is something different!" Then they got invited to go up for the dinner, and it was the most first-class affair I think that I've ever seen. I mean, it's really done well. Superbly. I had one sister—this was early on, when the announcement first appeared—tell her Sunday School class about it, and they didn't believe her. Now I guess she can show the publications and so forth to them.

An interesting thing has happened with friends that live in the Valley. There have been some articles in the newspaper, but I think if you ask most of them, they feel that the articles don't do justice to the award, and some were somewhere between adamant and angry at the newspaper for not doing a better job. I'm certainly the first person in the history of this technical center to get it, and I may be the first West Virginia resident to get it. I mean, even my dentist—my wife goes in there and they're just going on and on. "We're going to talk to the Governor about this." Whatever. "The next time we get together in the chair, I'm going to speak to him!" [laughter] So that there's been some real interesting reaction. You wonder, how do some of these people that aren't involved in chemistry appreciate the significance of the award, or why do they feel that way? Because I've won other awards, and there's nothing particular about them. Yes, you get it, and oh, it was a nice dinner, and so forth. Nobody has complained that the newspaper coverage was poor.

The dinner receptions were awesome. There's no other way of describing it. [laughter] Six hours—5 p.m. to 11 p.m. The talk went over exceedingly well. The chairman of the Society of Chemical Industry told me afterwards he thought it was the best Perkin presentation he had ever heard. Dr. Joyce, CEO of Carbide, I think his words were, "It's either the best or one of the best presentations of this type I've ever heard."

TRAYNHAM: How long did you take to give that talk? Not the ten minute version, I presume?

BRYANT: No, this was seventeen to twenty minutes. That was the window that was prescribed by the people giving it, the Society of Chemical Industry. The talk itself, the subject, had an interesting origin. I got the use of the Carbide president's speech writer to help. So I wrote this speech, in draft, sent it to him, and it was "too sophisticated." "Tone it way down," he says. I was talking to one of my brothers, and he said, "Before we go to the dinner, could you write out a page saying what you've done so that if somebody comes up and talks to us, we won't appear to be complete idiots?" I thought, "Now, there's a nice subject for a talk, just to explain to the family what I've done." That's really what the talk was about. It has I think only a single paragraph of chemistry in it, and the terms, they were about as simple as you could imagine. It was more talking about research philosophies and approaches and how you bring things from everyday life in and use the examples in the work we do here.

TRAYNHAM: Since it was praised so by those who had heard many Perkin medalists speak, perhaps it should be a model for those who come after you in the Perkin Award.

BRYANT: Well, the corporate speechwriter said, “You ought to get this published,” and I thought it was an off-hand comment, but he said, “You know, you really ought to get the talk published.” As I said, I may, when I retire, write a book on research strategies and philosophies, since I don’t think that such a thing exists, one that really goes through the array of approaches that are available, the tools in your toolbox, whatever language you want to use—just be there at your fingertips so that when you’re presented with something, you say, “Oh, now it’s time to do this.”

I was up talking with somebody, this was after the talk, and I said, “The approach you need to take now is to turn your brain off and go get a sauna.” We tried to figure it out and with some success. We followed the surveyors—closed the circle, and found pieces, and we’ve got other strategies: one, adding model compounds; two, the bedrock—isolate the material and get a crystal structure. We worked on that. But another approach is just try things. The advantage of just trying things, I’d say, without a great deal of thought is that you can get lucky, and when you do get lucky and you can’t give a good explanation for it, then it means that your competitor doesn’t have any basis that they can go in the literature and figure out how the things work. In the vinyl acetate example, everything you needed to know was in the literature, and I think there was a patent interference in Canada during that episode that twenty companies were involved in. So here’s at least twenty companies that had read the same journal article and they appreciate the significance, and everybody goes in and does the same thing. But if you can get real lucky, you can get in an area that, you don’t know why you’re there; nobody else knows how you got there, and you’ve got free run, free rein until you go out and the patents come out, and you give some talks, which was the case with the bisphosphites. It wasn’t until I actually gave a talk at Cambridge that I think that the world really paid any notice to what we’d done.

[END OF TAPE, SIDE 6]

TRAYNHAM: The Priestley Medalist spoke at the American Chemical Society about the end of the “Golden Age” in science, in which he lamented the fact that it’s far more difficult to get support for what he referred to as basic or fundamental research. You’ve spoken a bit to that point, about your career at Carbide. I was wondering how you viewed the future from that point of view. Do you see that industrial workers are having the same kind of increased difficulty that academic workers are having in getting basic research within the company done, or is Carbide continuing to see the benefits of the kind of work that you did?

BRYANT: Okay. Going back to the “Golden Age,” I’ve also read that somebody thinks that everything significant that could be done has already been done.

TRAYNHAM: There was a book written to that effect called *The End of Science* (2) in 1996.

BRYANT: Yes. You think back to around the turn of the century where the physicists, when they said, “All we need to do is get the speed of light and a few more significant figures, and we’ll be done.” I could believe the same thing could happen in chemistry, that somebody will be working by accident in some area and get some combination that nobody ever thought of, and then it’ll open up again.

In terms of the Golden Age, in terms of getting funding, who was it? Alcott, I assume?

TRAYNHAM: Yes.

BRYANT: He’s far more authoritative on that subject than I am.

TRAYNHAM: Well, I was really thinking in terms of the industrial situation.

BRYANT: Oh, in terms of the industrial situation, within the constraint that I mentioned—that you can’t work on just anything, you’ve got to work on something for which there’s a business plan—the funding can be enormous. It’s just unbelievable. It’s the strategy of Dr. Joyce, CEO, and I endorse it completely. Figure out fewer things and give them a critical mass of money and people and make them successful—in part, because an individual just can’t do it anymore. It takes a team of people and it takes a critical mass, and you’ve got this time schedule, too. It has to be accomplished in some window of opportunity. As I say, we’ve put out our next-generation technology. It’s been offered for license. Nobody else has started up a plant yet. But one of my assignments is to figure out what’s going to replace this technology that’s just starting up. Our intention is to continue to be the leaders and that takes money. You have to look to figure out where the real deficiencies in the technology are, where the opportunities are, what could make it better. I mean, when you’re up at the high nineties in term of percent efficiency to butyraldehyde, there is not a lot of room for improvement there. So you’ve got to figure out where the costs are—in the processing equipment, or in energy, or some other way of doing things.

TRAYNHAM: As you think back over our interview, do you think of any aspect of your career that you want to talk about now to make the story complete, or do you think you have given rather complete coverage?

BRYANT: I’ve hit highlights over the time. I’ve been involved in so many different things, even within Oxo, and so many different activities. In a sense I’ve had so many adventures. Many of them I can’t discuss for proprietary reasons. They may come to me as we continue to talk, but right now I can't think of anything. The way I closed the speech was to say that I have thoroughly enjoyed what I've done and there's really nothing that I would have rather done. When you look back to the start and say, "If I had it to do over again, would I have rather been a this or that or whatever?" The answer is "Absolutely not!" It's been fantastic. It's a great group of people to work with—intelligent, motivated—and I'd say for the most part, nice guys and gals.

TRAYNHAM: Well, aside from your career as an industrial chemist, and a very successful one, you've only told me fleetingly about your family, made reference occasionally to it. Tell me something about your family.

BRYANT: Okay. I've only had one wife! [laughter] We've been together a long time. I'm not sure exactly—over forty years, and we plan to keep it that way. We have a daughter and son, and the grandchildren—they've been nicely spaced, so that we've been able to enjoy them over the years. Grandson—twenty; granddaughter—thirteen; grandson—five; and a granddaughter—four months. So each has had the chance to be special for a while.

TRAYNHAM: Any of them living near you?

BRYANT: Yes. We have two that are about twenty-five minutes away, a grandson and granddaughter. Yesterday, the grandson was at our house and we tried to teach him to stop a ball for T-ball. I went through a whole episode with my son now that's started again. The pressure's really off with grandchildren, but you get a chance to spoil them and when they're dirty, you can send them home!

TRAYNHAM: I remember a saying, "If I'd known grandchildren were going to be so much fun, I would have had them first!"

BRYANT: Yes, that's right. For my son, when he was growing up, whatever he wanted to do is what we did.

TRAYNHAM: That's a remarkable statement for an industrial chemist to make.

BRYANT: If he wants to bowl, we go bowling. If he wants to fish, I learn to fish. We'd go mountain biking before mountain bikes existed, on these narrow-wheel bicycles out in the woods. He likes to shoot. I don't own any guns, but he needs help sighting in the rifle, well, I do sighting in the rifle. We had some great times while he was interested in golf.

TRAYNHAM: What is he doing as an adult?

BRYANT: He's an accountant, works for the state. His interests are—one was powerlifting, like bench-pressing four hundred and fifty pounds and crazy stuff like that. Now he's gotten into running and has a black Lab that he takes out and walks. I think keeping their day-to-day operation going consumes about every minute of their time because both of them work. It's a tough schedule.

My daughter lives in Nashville. She doesn't work outside the home. She manages the household, takes care of all those things. My son-in-law is an environmental engineer, which has him traveling a lot to godforsaken places. I get to go to good places and he gets to go to those you've never heard of and stomp through some awful fields. So she carries a big part of the load of keeping the household going. My daughter told me that they were at some party and—I don't know how it came up—she starts talking about what I could do. You know, I could do this, I could do this, this, this, this, and she ended it by saying, "And he plays Barbies!" [laughter] If the granddaughter wants to play Barbie, I'll play Barbie. [laughter] I do have trouble getting the outfits right. She gets real upset.

TRAYNHAM: Getting what right?

BRYANT: The outfits. You see, it seems like my Barbie doll will end up with an evening gown and a drum major's hat on! [laughter] But they grow up so fast, you just have to enjoy them while you can.

TRAYNHAM: A fashion designer you're not!

BRYANT: A fashion designer I'm not. Just anything to aggravate her a little bit. In terms of interests, I'd say they're extremely broad. If you exclude religion, politics, and sports, I'll say name a subject and [laughter] I'll see if I can carry on my part of the conversation. I'm just familiar with, read loads of things. I have a rule. Sometimes people wait 'til they dislike doing something before they quit. This is for things that are voluntary. My rule is: when you cease to anticipate it, stop!

I was a keen amateur photographer. I probably have ten cameras now of different descriptions. I was in a camera club for twenty-five years. I realized that I no longer anticipated going to the meetings, and by "anticipate" I mean that sometime before that, you looked forward favorably to the activity. It was just like, "Oh, it's something to do." I decided, "I'm stopping." The whole decision was probably less than ten minutes. This was after twenty-five years. I don't think I've been back since. I don't miss it. I've gone onto other things.

I'll subscribe to magazines for a year. That's the goal. Then once that year is up, I dump it and go to something else. So I've gone through taking flying magazines, scuba diving, tropical fish. I can't remember all the different ones. Because you find out, after about a year, you've pretty much gone through whatever the main group of articles is, and if I find even on some that I'm getting regularly, the thing arrives and I don't feel an urge to look at it immediately, that it could be a week later, then it's time to stop it.

TRAYNHAM: That's the signal.

BRYANT: Yes. I've never heard anybody describe it in terms of "lack of anticipation." My wife has been in a lot of activities and there's one now, and she keeps talking, and I said, "Do you look forward to it? If you don't look forward, stop, because it gives you time to do something else."

TRAYNHAM: Good idea. Well, you have picked a rather definite date for your retirement from Carbide. Is that predicated on the diminished anticipation of coming to work?

BRYANT: Why did I pick that date? No, the date was arbitrary. Somebody walked in the office one day—this was years ago—and said, "When are you going to retire?" Well, I didn't have a date, and 2000 was a nice round number.

TRAYNHAM: And far off at the time?

BRYANT: Yes, it was far off at the time. So I said, "2000. I'll be sixty-four in 2000." Why not pick your birthday? So that's how it ended up. It's 5/8/00. I mentioned that I started to work real early, age ten, and I've been working a long time, and I just thought, it sure would be nice to have some time for myself. Do I have anything in particular planned? No. Maybe sit out in a chair and just enjoy a nice day. Will I want to consult? I don't know. It's just like the Perkin speech. People said, "Are you nervous?" I said, "Until you actually stand up, you don't know." In the case of the Perkin speech, about thirty minutes before, it dawned on me, "I'm ready!" [laughter] "This is going to be okay."

TRAYNHAM: There's nobody out there that wants me to fail!

BRYANT: Yes. It’s the only time I’ve ever given a speech where every word was written down, although I did ad lib a little bit. I got to the last thing, the last page-and-a-half, and I looked down, and I thought, “Damn! It’s almost over!” [laughter]

TRAYNHAM: You were still having fun with it!

BRYANT: I was still having fun. I mentioned that to one of the Carbide officers and he said, “Yes, it was obvious you were having a good time.”

I’ve got another thing, just while we’re throwing random bits of wisdom in. It’s called the “Bryant Decision-Maker.” You’re going to think this one is a joke, but you’re going to try it and you’ll find out it works. You’ve got two circumstances. Do I eat in Restaurant A or B? Do I buy the blue jacket or the green jacket? Oh, I guess I can’t decide. Should I get this or that? So what you do is you say, “Heads, it’s the blue jacket. Tails, it’s the green jacket.” You flip the coin and in that fraction of a second when you get the answer, this emotional feeling comes over you: you’re happy or you’re not happy. If you’re happy, you do it; if you’re not happy, you do the opposite! [laughter] I don’t know why it works, but it does. I’ve told other people about this, and they’ll come back and tell me they’re using this technique now.

TRAYNHAM: That’s a very good note to close on, and thank you for being so generous with your time today in giving the interview for Chemical Heritage Foundation. I’m sure it’ll be a valuable resource in the archives there.

BRYANT: Certainly, and I’ve enjoyed it.

[END OF TAPE, SIDE 7]

[END OF INTERVIEW]

NOTES

1. Douglas M. Considine, ed., *Process Instruments and Controls Handbook* (New York: McGraw-Hill, 1957).

2. John Horgan, *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age* (Reading, MA: Helix Books/Addison-Wesley Publishing Company, 1996).

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