THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

WILLIAM H. GAUVIN

Transcript of an Interview
Conducted by

James J. Bohning

in

Montreal, Quebec

on

11 July 1991

With Subsequent Corrections and Additions

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY
Oral History Program

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William H. Gauvin

(Date) November 8, 1991

(Revised 20 February 1989)

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

WILLIAM H. GAUVIN

1913 Born in Paris, France on 30 March

Education

1941 B. Eng., chemical engineering, McGill University
1942 M. Eng., chemical engineering, McGill University
1945 Ph.D., physical chemistry, McGill University

Professional Experience

McGill University, Department of Chemical Engineering
1942-1945 Lecturer
1947-1961 Associate Professor
1961-1971 Research Associate
1971-to date Senior Research Associate

Pulp and Paper Research Institute of Canada, Montreal
1951-1957 Consultant
1957-1961 Head, Chemical Engineering Division

1945-1947 Plant Superintendent, F. W. Horner Ltd., Montreal
1961-1970 Research Manager, Noranda Research Center, Montreal
1970-1982 Director, Research and Development, Noranda Mines Ltd.
1970-1971 Délégué-Général, National Research Council of Canada-Policy and Planning
1982-1983 Director, Advanced Technology, Noranda Research Center, Montreal
1983-1990 Scientific Advisor to Director, Hydro-Quebec Research Institute
1983-to date President, William H. Gauvin Technologies, Inc.

Honors

1958 L. H. Weldon Medal, Canadian Pulp and Paper Association
1960-1961 Chemical Institute of Canada Awards (for best papers published in the Canadian Journal for Chemical Engineering)
1963 R. S. Jane Award, Canadian Society for Chemical Engineering
1964 Senior Moulton Medal, Institution of Chemical Engineers of Great Britain
1966 Palladium Medal, Chemical Institute of Canada
1966 Médaille Archambault, ACFAS
1967 D. Eng., Honoris Causa, Waterloo University

1968 Membre d'Honneur de la Société de Chimie Industrielle de France
1968 Best Paper Award, Canadian Society for Chemical Engineering
1969 Fellow, Royal Society of Canada, Academy of Science
1970 Alcan Award, Canadian Institute of Mining and Metallurgy
1972 Distinguished Lecturer Award, Canadian Institute of Mining and Metallurgy
1973 Fellow, American Institute of Chemical Engineers
1975 Companion of Order of Canada
1979 Gold Medal, Société d'Encouragement pour la Recherche et l'Invention, France
1981 Honorary Fellow, Institution of Chemical Engineers, United Kingdom
1982 Honorary Fellow, Chemical Institute of Canada
1982 Chemical Institute of Canada Award (for best paper published in the Canadian Journal for Chemical Engineering)
1983 Montreal Medal, Chemical Institute of Canada
1983 D. Sc., Honoris Causa, McGill University
1984 Jules Stackiewicz Award in Heat Transfer, Canadian Society for Chemical Engineering
1984 D. Sc., Honoris Causa, Queen's University
1984 Prix Marie-Victorin (Prix des Sciences du Québec)
1985 Medal of the Canadian Research Management Association
1986 Thomas W. Eadie Medal, Royal Society of Canada
1986 D. Sc., Honoris Causa, McMaster University
1986 Julian C. Smith Medal, Engineering Institute of Canada
1986 Founding Member, Canadian Academy of Engineering
1987 Foreign Member, National Academy of Engineering of the United States
1988 The Izaak Walton Killam Memorial Prize in Engineering
1988 Award for Innovation in Drying, Versailles, France (Sixth International Drying Symposium)
1989 Inaugural Lecturer, First Eugenie Lamothe Symposium, McGill University

ABSTRACT

William Gauvin begins with background information about his childhood experiences in Europe, his formative education, and his emigration during the Depression to join his family in Canada. He describes his education at McGill University, which culminated in both wartime work on RDX as well as several early electrochemistry papers. He next recounts his employment with Frank W. Horner Ltd., and the initiation and development of his lifelong spray drying work. Gauvin relates his recruitment to the Pulp and Paper Research Institute, his move to Noranda, and his associations with Hydro-Quebec and other industrial research centers. While recounting the circumstances behind each of these professional "turning points," he discusses the evolution of the chemical engineering department at McGill and the involvement of his graduate students at these research centers. Throughout the interview, he emphasizes the often difficult balance between research and management views on R&D, and between technical feasibility and economic feasibility of new technologies. Gauvin reviews his contributions to science policy, industry-academe cooperation, and government support for R&D. He concludes the interview with a consideration of chemical engineering in Canada today, and of the highlights of his own career in the field.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Beckman Center, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1987, and has been associated with the development and management of the Center's oral history program since 1985.

TABLE OF CONTENTS

INTERVIEWEE: William H. Gauvin

INTERVIEWER: James J. Bohning

LOCATION: Montreal, Quebec

DATE: 11 July 1991

BOHNING: Will you be attending the Vancouver meeting of the Canadian Society of Chemical Engineering?

GAUVIN: First of all, it is the Canadian Society for Chemical Engineering, not the Canadian Society of Chemical Engineering. Don't ask me what the legal requirement is [laughter], but it's an odd one. Often in Europe they'll say to me, "Bill, do you mean the Canadian Society of....," and I say, "No, it's the Canadian Society for...." [laughter]

No, I won't be attending the Vancouver meeting. The reason is that I'm not presenting a paper. If I had presented a paper, then the little consulting company I operate could have paid for it. But if I don't present a paper, then I want to be honest and not travel. I wish more professors in Canada would follow my example. [laughter]

BOHNING: Well, why don't we get started. Dr. Gauvin, I know that you were born on 30 March 1913 in Paris.

GAUVIN: That's right.

BOHNING: Could you tell me something about your parents and your

family background?

GAUVIN: Surely. That's an odd question but I have nothing to hide. I was born in 1913 on the eve of the war. There is a reason for my first name William, because my real first name was Guillaume (the name of the German Kaiser), and like the first names of Benito Mussolini or Adolf Hitler, my dad (during the Second World War) felt it had to be changed. He could not legally make a radical change, but the equivalent of Guillaume in English is William. That was accepted; legally, my first name became William. The French, of course, not knowing any English (or German for that matter) didn't make the connection between Guillaume and Wilhelm and it has remained Bill, or William.

My dad was at the time a wealthy Canadian. He tried to escape, but we didn't suffer too much during the war. On one occasion we were in a suburb of Paris about twenty kilometers from the city. That was in 1917. I remember an incident. I was almost five, playing in our big garden. I saw those tall fellows dressed in very striking green uniforms, with a strange helmet on them, and they were on horses. They were Uhlans as I learned later. That doesn't mean anything to you but that was the mounted German cavalry. It was a small advance party and they had reached about twenty kilometers from Paris through the forest. An officer at that time picked me up in his arms and said in very good French, "Don't worry, little fellow. We're going back now but we shall return." That was my one contact

with the enemy and it made a very strong impression, which I remember well.

Shortly after that we moved to London, where my dad had interests. We were in a little place near London called Croydon. At the time it was a small airport--still is, actually. I spent three or four years there. Things quieted down, and we went back to Paris. And that's it. I went to elementary school in Croydon, where apparently I learned a smattering of English (at that age you pick up a language very quickly) which I promptly forgot when I went back to Paris.

After that my parents decided to come to Canada, and at age fourteen they left me with my grandparents in Brussels. My mother was Belgian, and her maiden name was Van Halle. My grandfather was a man who made a great impression on me. He was a wonderful chess player, and also he was a terrific admirer of Napoleon. He had copies (I don't know how he got them) of some of the campaign orders that Napoleon always prepared before a campaign, in which he ordered so many cannons and so many this and that--book after book after book for each campaign. He was quite a planner, let me tell you. Then I returned to Paris, because I wanted at that time to be a chemical engineer. Incidentally, all my grandfather's Napoleonic memorabilia were seized by the Germans when Brussels was occupied in 1940.

BOHNING: Why did you want to be a chemical engineer? Where did that interest develop?

GAUVIN: At school in Brussels. It's interesting that you ask this question because, in retrospect, my life has been dictated by what I call "turning points." One of them was when I attended a gymnasium, the equivalent of which they call in Brussels Athénée [Athenaeum]--L'Athénée Royale d'Ixelles. It's a secondary school after the German pattern. A very tough course. I was in "Mathématiques Spéciales," and at the age of fourteen we already had covered most of the elementary calculus, differential equations and the theory of conics (a perfectly useless branch of analytical geometry). We had a completely unbalanced curriculum--fifteen hours of math every week, one hour of physics, one hour of chemistry, no lab. Typical of the German training at that time. I was filled with math. When I arrived in Canada I could even teach mathematics at McGill. [laughter]

When I went back to Paris I was really doing two things. The first was to study piano seriously at the Conservatory. The second was to prepare for the entrance examination for the Grandes Écoles (universities). I don't know if you're familiar with the French system.

BOHNING: No, I'm not.

GAUVIN: It's very tough to enter École Centrale or the École des Arts et Métiers or the École des Mines or Polytechnique (all famous engineering schools in France). The entrance competition was, and still is, horrendous. But I was a good student and I

was confident. Then disaster struck. That was September 1929, the big crash! I received a telegram from Canada to come back in a hurry, because the fortunes of the family were in trouble. Fortunately, I was not quite eighteen and could emigrate to Canada legally. I arrived and it was a real disaster. My father was a poet; he almost never worked and I guess he "clipped coupons." He had a number of companies which he all lost. But I had a good head for business. I went over his portfolio and there was one little company, an essential oil company, in which he owed only $50,000; the rest of the companies were hopelessly bankrupt. It was really poverty, I'm not kidding. So, I got some nerve together and went to see our bank manager. I had a lot of gall. (I was only eighteen.) I asked him for a loan of $50,000. He just laughed at me and said, "Look young man," (he didn't say "squirt," but that's what he meant) "you come back when you have a reasonable proposal." I did, several times, and finally, six months later, I came with a proposal which made sense to him and I walked out with $100,000 in credit. So we were cleared of our debt and three years later we all had our cars and were doing reasonably well in spite of the Depression. I wanted to go to the university, but I had to wait until I had enough money, which was in 1938. Then I entered McGill but still kept an eye on the little company, which was a moneymaker.

Now to answer your question about why I chose chemical engineering, or rather, plain engineering, because chemical engineering in those days, as you know, did not exist; it was

called industrial chemistry. When I was at L'Athénée Royale d'Ixelles we had a course which strangely enough had an English title, "Self-help." It got its name from a translation of an English book, by that title, which I've been trying desperately to obtain since then, which was simply a recital of the lives of famous people. The theme was always the same--terrible hardship, terrible poverty, illness and so on, and finally triumph through hard work. And I said, "By God, if those guys with all of these problems could do it, I can do it too!" I remember the life of Goethe affected me an awful lot, and the life of Blaise Pascal who was half blind.

BOHNING: Do you remember the title of the book?

GAUVIN: Yes. As I said, Self-help (1). I've been trying to get a copy ever since. I went several times to Athénée Royale d'Ixelles much later (I like to go on these memory trips), but I could never find a copy of it. This course was a real inspiration and, in a way, was a bit of a turning point. Certainly, at that time I wanted to be an engineer. That there was no doubt about. But it was many years before I could go to McGill.

BOHNING: Not a mathematician? You had all this mathematics.

GAUVIN: Yes. So, I went to see the dean (Dean Brown), and chemical engineering was a five-year course in those years. When

I explained all the courses I had taken in math, that's when he said, "Well, gee, you could teach these courses." [laughter] So I entered directly in the third year.

BOHNING: That would have been 1936?

GAUVIN: No, it was a little later. That was in 1938. Three years later I got out with a bachelor's of chemical engineering. By that time this branch of engineering had evolved and the head of the department, Jack [John B.] Phillips, had been a student of Tom [Thomas K.] Sherwood.

BOHNING: At MIT.

GAUVIN: Yes, at MIT. Jack Phillips was really up to date, and I was very grateful for the training I got under him, so that I wanted to push on. The war was fully on (in 1941), and I tried to enlist, but I was prevented by the Canadian government. They wanted me to stay at McGill and do some advanced work on the synthesis and properties of RDX for my Ph.D. thesis.

I don't know if you've heard about that damn explosive that the Italians had developed, but it was disastrous to the Allies particularly as an underwater explosive. It was very easy to make, but we didn't know a lot about the properties of it and its stability. I was under the supervision of a wonderful chemist named Dr. Carl [A.] Winkler. He had fifteen boys, all working on RDX. After a while I got so fed up with trying to measure

viscosities, thermal properties, etc., under all kinds of conditions, that he allowed me to pursue a formal thesis project. So I had two advanced projects. One resulted in a report on RDX (I think I have a copy), and the formal one was in electrochemistry (2). He had the nerve to send my Ph.D. thesis to Samuel Glasstone of Princeton, a giant in the field, who gave me an excellent grade in his review as external examiner. That explains why my first publications (about ten of them) are all in electrochemistry. That was good training. I have forgotten to mention that prior to my electrochemistry work, I had picked up a M.Eng. degree based on a complex analytical heat transfer project. No laboratory work--just mountains of calculations! The work was never published.

BOHNING: I want to back up a little bit. The company that you rescued for your father--what did that company do?

GAUVIN: They were traders. We were buying flavor chemicals, like vanillin, and coumarin, and also an awful lot of dyestuffs and essential oils, which during the war we got mainly from Europe. Usually they arrived through Portugal or Sweden, but I suspect that some of them were German products. The Germans needed the money; we needed the chemicals. In a way, a war stimulates business. I shudder now because some of the dyes that we got, like the red amaranth dye, are now banned. The yellows were banned since then. The blues were really toxic. We didn't

know what the hell we were doing, to be honest, but they were used in such small concentrations that they were harmless.

By that time my dad was well launched and the company was comfortable--not wealthy but comfortable. So I decided to live on my own. As I told you, I joined McGill and graduated with my Ph.D. in 1945. A classmate of mine was the son of Frank Horner. They're still in operation under the name of Frank W. Horner Limited. When we were at school, Frank died and his son Howden, my classmate, suddenly became the president of a fair-sized company, not as large as Merck or anything like that, but fair-sized. He said, "Bill, I like you. I want you to come to work with me. I want to develop new medicinal products. I don't want to mix vitamins like my dad did, a pound of this and a pound of that, add coloring material, sugar and stir, you see. I want to make new products. You are a lousy chemist but I have a good research group. Your job will be to take their results, develop production methods and design the full-scale plant equipment."

It was the most colossal training I ever had. He gave me a free hand. Some of the large-scale equipment we required for a number of unit operations had to be designed and built from scratch: solvent extraction, evaporation, crystallization, and filtration, and everything was controlled by heat transfer under a bewildering variety of operating conditions to maintain a high degree of product purity. In the course of this work one of the most baffling problems I had to face was how to extract amino acids from extremely dilute solutions, in the form of very fine, dry powders, without destroying their biological activity. This

is how I came to select spray drying as the possible solution. I did not invent it, but in 1946 it was still somewhat of a laboratory curiosity, with little published information. The problem was pressing, and Howden Horner told me to go ahead and build a production unit. Now spray dryers require very large volumes of drying air and are consequently very large. The one I designed was relatively small (a conical chamber, twenty-five feet high and fifteen feet in its larger diameter). It looked like a monster to us--but it worked, right from the start! A beautiful example of serendipity! I did not know it at the time, but it was another turning point in my life. Spray drying--and a modified version of it--turned out to be my life-long interest.

Then I got ill. We were working with pancreas glands from animals to decompose casein into peptides and finally amino acids, and they are wicked. To make the story very brief, in that particular department I allowed my technicians to work for only three hours a week, and even then they could develop terrible cases of dermatitis. I seemed to be impervious. However, I didn't work in that particular lab directly; I was in and out most of the time. Finally it got me! It affected my liver, put me in the hospital, and darn near killed me. I was saved by sulfa drugs which were then coming into use.

When I was recovering after three months in the hospital, Jack Phillips came to see me. "Bill," he said, "I need you. There's nobody in the department of chemical engineering but me. I'm going nuts. You have taught chemical engineering throughout

your master's and your Ph.D., so I want you to come and work for me." And I did. He gave me a hell of a big salary, starting as an associate professor. Five thousand dollars a year in 1947 was a fair salary, indeed. I got that salary for one year, and then I went to see him. I said, "Look, forget about my salary, but will you allow me to go on developing and selling my spray dryers" (I was making a good deal of money) "for various industrial applications?" Spray drying was still in its infancy, but developing fast. Phillips allowed me to do that with no salary. I taught at McGill continuously until 1961, at which time I joined Noranda but stayed on at McGill as a research associate, in charge of a large group of Ph.D. students. I am very proud of this long-standing affiliation with McGill which continues to this day, only now I only have one Ph.D. student, albeit an outstanding one. I should add that, while handling a heavy load of lectures in the chemical engineering department until 1961, I also had a job with the Pulp and Paper Research Institute, working on new processes and new techniques. It was next door to our McGill plasma lab. I am still affiliated with them in the supervision of an intriguing project on kraft black liquor recovery. It seems that all my life I always had two jobs at once. It made for an interesting life.

Then in 1961 Noranda made me an offer that I couldn't refuse. To create a brand new laboratory (the Noranda Research Centre) literally from scratch. I reluctantly told Phillips, "That's the end of my lecturing days; this is the opportunity of

a lifetime!" but that I would still conduct research. I had ten graduate students, and I couldn't just dump them. Noranda kicked like a mule; they said something about split allegiance and that they didn't believe in one of their employees doing this. I told them, "Never mind allegiance. All the things that I'm doing may be of use to you! But it's up to you to use them." They bought it. From then on, I always had a sizeable gang of chemical engineering students, with their tacit approval. And indeed, quite a few of the graduate projects turned out to be pertinent.

Noranda! Talk about a turning point! It was a terrific experience. It was the first contact with a mining company I had ever had. Highly structured, not arrogant but damn proud of their success. Typically their first move was to appoint one guy--a big shot in the Toronto seat of power--to whom I would report. His name was Joe Stovel. A prince of a guy, who fortunately knew nothing about research, but he sure knew a lot about management. We made a good team! He gave me all the freedom I required concerning the technical aspects of the future lab (facilities required for about 150 scientists, technicians and supporting staff). He selected a good group of architects (Peter Dobush et al.) and the final site (in Pointe Claire, Quebec). We rented a large suite of offices (Would you guess it? 550 Sherbrooke St. West, close to McGill!) and I started hiring right away (Thank God, he left the hiring to me). Within six months, I had hired thirty-five people, including all my division heads and senior engineers. (Quite a few were former graduate

students. We placed them wherever we could: at McGill, at PPRIC, at some of our Montreal manufacturing operations.) To prepare them for their jobs, Stovel organized a lot of plant and mine visits. They started moving to the half-finished laboratory by the end of 1962. The building was finished by the end of 1963. These were exciting days!

BOHNING: I'd like to go back to your undergraduate days at McGill. Could you tell me a little more about what the curriculum was like, and what kind of courses you took?

GAUVIN: I was given credit for all the courses in math, and strangely enough in physics and chemistry in spite of my weakness in these two disciplines and others. I've never taken descriptive geometry, for example. I'm sorry I sort of evaded that. Jack Phillips gave a splendid course. I admired him as a professor. He put in a lot of energy in his courses, constantly wiping his forehead while he taught. It was, I must admit, in those days, the latest in unit operation theory. I got a splendid undergraduate training, to his eternal credit. Later on, when he allowed me to take over some of his lectures, I put new stuff in it, and I kept up with the literature. I also benefited an awful lot from my fellow graduate students. We were breaking ground. None of these trivial things like measuring the viscosity of water to the tenth decimal place--none of this stuff. All new processes, with industrial applications as objectives.

Slowly I got involved in higher and higher temperatures of operation. In the AST (atomized suspension technique) process, for example, I was limited by the mechanical resistance of the hot walls, which were acting as the energy source. I couldn't exceed 1000° Centigrade. The temperature was too low for high rates of heat transfer and the reactors required were too large. And then I happened to read about plasmas! That was it! That changed my life completely, because then I could put a fantastic heat source right into the medium inside the reactors, with much lower wall temperatures. I've written forty or fifty papers on the fundamental aspects of this new heat transfer method.

BOHNING: When you were an undergraduate, were other faculty besides Phillips in the chemical engineering department?

GAUVIN: No. Until 1961 Phillips taught the chemical engineering so-called unit operations. Shortly after that Ken Schelstad was hired as a lecturer to teach the elementary courses in thermodynamics, etc. Then one of my wonderful graduate students by the name of T. W. Hoffman gave us a hand. I got him to teach courses in heat transfer. He was an older guy, and he was working on a very tough problem of radiation from clouds of particles at high temperature. In those days, [Hoyt] Hottel was the expert in radiation in combustion furnaces. The systems we studied did not involve combustion, but sequential chemical reactions. I don't think he was interested in that particular area.

Then [W. J.] Murray [Douglas] came on the scene, I think in the late 1950s. I'm not quite clear about the time. He was a wonderful teacher, and we formed a profound friendship to this day. He is presently taking a sabbatical in Toulouse, France; he loves France and he married a delightful French girl. He manages to come back every three months because he has eight or nine graduate students working in what is now called the Pulp and Paper Research Centre, right next to our plasma lab. He stayed with me in my home, during one of his visits to Montreal from Toulouse. I have also maintained wonderful friendships with many of my past students. As an example (and we do this probably once every ten years), a gang of these students invited me to come to New York about a month ago, and we spent a riotous three days in New York City together. There were about twelve of them, from all over the States and Canada, reliving the experiences we went through in the course of their graduate work. These memories, and the bonds they created, are probably my most cherished rewards.

[END OF TAPE, SIDE 1]

GAUVIN: As I said before, there have been a number of turning points in my career throughout my life. The spray drying development completely changed my life around. At one time as a young man, I was pulling a good deal of money selling those spray dryers. It made me financially independent, and that's important.

BOHNING: How did that develop? How did you get into that in the first place? What was the key there?

GAUVIN: The key is simply that the work I started at McGill in 1948 on the fundamental aspects of spray drying quietly got around industry. It culminated in three early theses (Knelman, 1950; Lyons, 1951; Pinder, 1952) which became part of the public domain. I also published a formal publication in 1955 (3). Industry was also aware of my successful installation at Horner's, but not of the details of the application. It would be a breach of ethics to disclose here the industrial applications in which my spray dryer design was used. But my design was still crude. I would spend the next twenty years to refine it. But it was adequate for simple applications.

In the meantime, another turning point occurred--quite unexpectedly, as usual! One day, during the early 1950s, I was having lunch at the McGill Faculty Club with a friend of mine who had obtained a Ph.D. at the same time I did. He said, "Bill, I have a troublesome problem. I'm working for the Pulp and Paper Research Institute. I'm a research associate there, and they asked me to fluidize bark and find what kind of organic chemicals you can get from the treatment of bark at high temperatures." I said, "You're nuts! You cannot fluidize bark particles. Bark is too fibrous." "But," I added, "I think I know how to do it. I'll come back to the lab with you this afternoon. For safety

reasons, do you have in your lab a bucket full of sand?" (Since every lab in those days had buckets full of sand.) [laughter] "Okay, we'll add sand to your reactor, and we'll fluidize your goddamn bark!" And it worked. We were evolving a lot of noxious gases, but what they were was his business. Incidentally, to my knowledge, nothing useful came out of this test.

At this moment, about four o'clock, in walks a tall, handsome man. "Hello, gentlemen. What are you doing?" And my friend Allan, who was kneeling near his reactor, got up on his feet immediately--he almost saluted. [laughter] And he said, "Dr. [Lincoln R.] Thiesmeyer [the new president of the Pulp and Paper Research Institute of Canada], I want you to meet a good friend of mine. He's giving me a hand. His name is Bill Gauvin; he's attached to the department of chemical engineering." "Oh?" he said. "Hello, Bill. My name is Thies." (That was his nickname). "Do you know of the problems of the pulp and paper industry?" I said, "No. What's the problem?" "A shortage of sulfur." And it was true. You wouldn't believe it, Dr. Bohning; you're too young to remember those days. [laughter] In Canada, no sulfur, no pulp and paper. He asked, "Is there any way of recovering sulfur from the spent liquors?" I said, "No, but I could take a crack at it." Then as now, I was the eternal optimist. We talked and he said, "Let's have a drink." We had a drink. "Let's have dinner." We had dinner. "Come back to my house." I went back to his house. He was a heavy drinker, and I don't mind having a drink. At twelve o'clock I walked out. I

had a job, and a damn good one, in the Pulp and Paper Research Institute, which is next door (it was ideal!), at a good salary. This time I accepted their salary; no B.S. [laughter]

A few years later I became head of his chemical engineering division--which was small, mind you. There were never more than twenty people, including a good number of my graduate students--as usual! We worked like hell, and we recovered the sulfur a few years later, but by that time sulfur was plentiful. So my initial raison d'être disappeared, but in the meantime I had developed a new technique of treating waste pulp liquors called AST--atomized suspension technique, which we patented. That one we patented, widely. It had many applications in other areas. That was promptly taken up by the industry. I was head of the chemical engineering division of PPRIC until 1962, at which time, as I told you before, I latched onto the Noranda people because I realized that I was developing a lot of interesting processes, few of which applied to the pulp and paper industry. And Thiesmeyer was under fire, and couldn't continue to support my work. By that time I'd already received about a million dollars in support of my work from him, which was a good sum in those days. So we kissed goodbye with a great deal of sorrow on my part. He had an uncanny ability to motivate his people which influenced me for the rest of my career. Shortly after I left, strangely enough he also resigned. But he had left his mark. For one thing, he had built this huge lab on St. John Boulevard. You could see it from here if you had your room facing the other direction. That lab opened its door in 1956, but I stayed

downtown. It was a beautiful lab, well equipped and well appointed. That's to his credit. Then he was replaced--by another friend of mine, incidentally.

When I resigned, I felt that Noranda offered a better future. It was a big company, even at that time, and we dickered a heck of a lot because they didn't want me to go on with my research work at McGill, even without lecturing. I said, "I'll never leave McGill! I'll never leave! Take it or leave it!" So they left it. But they came back a few months later, and we were good friends again. I immediately started the recruiting campaign which I already described to you.

Once we were ensconced in our new building, we continued to recruit, and the planning of our R&D program began. I was now responding to a research committee in Toronto. (Thank God! They were far away so that they were not breathing down my neck all the time.) My marching orders were simple: improve existing production processes and develop new applications for some of our more exotic by-products, such as selenium and tellurium. The nature of the work was completely new to me. I managed to motivate my staff and they respected my gray hair (I was by far the oldest guy on the team) and my research experience. Many of my senior people were former students of mine, which provided for a very close rapport. We simplified the administrative headaches as much as possible. We played ball with the research committee and in return they gave us a great deal of freedom, providing we stayed within budget. For the first time, I had to pay close

attention to pollution problems and the ever growing demands for improved productivity. What a training I got--which I needed very badly. My young tigers were forever trying new things. For example, we had the guts to develop a new technique to assess, in dollars and cents, what actual returns we were making to the company. This was carried out by an independent group of MBAs. The first results, for the period 1963-1973, were lousy. That was par for the course for a young R&D group. The next two, for five years each, were excellent. We were returning to the company three or four times the amount they were spending on R&D. The president, Alfred Powis, issued a press release on our performance.

So my Noranda experience was another turning point in my professional life. It was a good company, and growing! It is now a $10 billion empire. They kept me on a tight leash at first (by the simple expedient of controlling my budget!) but with time, we were allowed to work on really exciting projects: fiber optics, hydrogen production, continuous casting of thin strips, new smelting techniques, and even plasma technology developments. One of these (on molybdenum production) resulted in the design of an entirely new plasma reactor design which I patented in sixteen countries with George Kubanek as co-inventor. I did not know it at the time, but this invention would play an important role in my twilight years. I am a strong believer in planning for retirement, and by the time I reached the age of sixty-five, I'd formed a small consulting company, and I was all ready to move. Then I got a call from my boss in Toronto, a very good friend.

He said, "Bill, we can't find a replacement for you. Will you work for us another five years?" "What's the incentive?" "Ten thousand dollars more a year." I said, "That does it." So I stayed with them until 1983.

When I left them in 1983, my small company was in operation, and then I got a big contract with Hydro-Quebec. I had negotiated the sale of our plasma reactor patents on behalf of Noranda with their director of research (another good friend!) for a very impressive amount indeed, several times what it cost Kubanek and me to develop this invention! Believe me Dr. Bohning, R&D pays, but it takes a guy with the soul of an Armenian carpet vendor (like me!) to sell it. The director of research had foreseen the possibilities of exploiting this new technology and he wanted me to promote the development of this technology for Hydro-Quebec. And then, for the first time in my career my proverbial luck abandoned me. Disaster struck! I was hardly installed in my new office at the Hydro-Quebec Research Institute, when the director had a stroke. The new director didn't have the same vision. The promised assistants and working facilities did not materialize. I struck an arrangement with McGill, however, and partly remedied the situation by training new graduate students in the Hydro-Quebec Research Centre called IREQ.

BOHNING: That's here in Montreal?

GAUVIN: No, it's in a suburb called Varennes, not quite forty miles from my home. A big lab. Six hundred and fifty researchers. Well appointed. So there I was every morning leaving in horrendous traffic and coming back at night. I damn near killed myself. In the meantime, I had struck a similar arrangement with another laboratory called the Industrial Materials Research Institute, a division of the National Research Council of Canada, in the nearby village of Boucherville. This time I was developing advanced ceramics, TiN, Si_3N_4, based on plasma technology, and also titanium production. Between the two labs, I trained three Ph.D.s and one M.Eng. Finally in the early 1990s I got fed up with this mad travelling and I resigned from both labs. Since then I am semi-retired. (It's tough to quit!) But I am active at McGill. I'm on the Board of Governors. I'm on the Patent and Invention Committee, which I love; the Pension Funds Administration Committee, which I love less; and various lesser committees. That's it. That's the story of my life.

BOHNING: What changes have you seen in McGill from the time you first arrived? How would you characterize McGill when you arrived, and how would you characterize it today?

GAUVIN: When I first joined the Board of Governors, I had the impression that McGill was overburdened by administration, the desire of the former principal, to please everybody: the board, the senate and very vocal faculties. So as a result they were very slow in making decisions, but things began to change when

David Johnston became the new principal, five years ago. McGill is in a hell of a financial situation. Our debt is now about $70 million. McGill gets most of its funds from Quebec, which gets its funds from the federal government. Quebec insists on allocating these federal funds to the various universities. To be honest with you, they have been very tough with McGill. It is now being corrected, so I think this year for the first time, we'll close our budget, but the monumental debt of $70 million still remains. That caused David Johnston to engage in a very difficult job of belt-tightening. Even now, the closure of the faculty of dentistry is being contemplated. Priorities have been established in detail and are being implemented. To my mind, Johnston is handling the job with a great deal of panache. He is diplomatic most of the time and can be darn tough with the government. As McGill is rated as the best university in Canada, he can yield a big stick in the press.

I am darn proud of our department of chemical engineering. The latest Gourman Report rated us fourth among the leading departments in North America. Our chairman, Dr. M. R. Kamal, is a distinguished researcher in his own right in spite of a very heavy administrative load. Under him, our curriculum has significantly improved, due to the extensive use of computers.

BOHNING: Let me go back again to your early days at McGill. You ran into Winkler through the RDX program.

GAUVIN: That's right. How? When I got my B.Eng. in 1941 and found I couldn't enlist, I had to work on RDX, and chose Carl Winkler as my thesis supervisor. After a while, as I said before he allowed me to work on another project as well. What I wanted was to get a Ph.D. in chemical engineering, but I couldn't--a) it was not allowed by the department of chemistry, and b) a one-man department could not have a graduate program at the doctorate level. So Winkler turned around the difficulty and my own Ph.D. is not in chemical engineering--it's in physical chemistry. I don't know if you noticed that.

BOHNING: Yes, I did notice that, and that's what I was getting to. I wanted to ask you about that.

GAUVIN: By that time Phillips came back from London, and I started to work for him; we decided that we had to have a doctorate program. So we insisted and insisted with the faculty of graduate studies. At first all my students were master's students. Finally, we got permission. I think my first Ph.D. student graduated in 1954. Ironically, his doctoral project was in electrochemistry!

BOHNING: Could you tell me some more about Winkler? What kind of a person was he to work for?

GAUVIN: Carl Winkler was a prince of a fellow. We all loved him. He was an excellent lecturer. He could see a relationship in some experimental graphs on heat transfer coefficients I

showed him one day, where all I could see was the scatter. At the beginning of my RDX work, I must admit I was rather lax in my lab attendance. He didn't bawl me out; he just gave me the silent treatment. That did it! From then on, I really worked hard.

Then one day, I asked his permission to do something else, in addition to my RDX property measurements, which I had mechanized to the point where I was carrying out thirty tests at once. "Well," he said, "I'm very interested in the electrochemical deposition of copper" (strangely enough, because I didn't know that particular type of work would play a role in my Noranda career, eventually). "I'd like you on the q.t. to start a project on the effects of addition agents on the properties of the metallic copper deposit that you get." My first ten papers were on that.

BOHNING: There was an industrial relationship there too, wasn't there? I mean, was he consulting for companies?

GAUVIN: Yes, he was consulting for Inco [International Nickel Company], and I got a scholarship from Inco in 1943 and 1944. Inco was not very responsive; I could never get them interested in my work. I was there in his office when Winkler got mad one day. He called the vice president and said, "I'm coming to see you next week with my student, Bill Gauvin. We want to show you our results, which are very interesting." Reluctantly the guy

accepted us, but then he kept us there in Sudbury for three days because he got interested. From then on, we got his attention. They were also depositing copper, but not at the same level of production as Canadian Copper Refineries, which belonged to Noranda in Montreal East. So I went to see Canadian Copper Refineries as well, not knowing that one day, in thirty years, I would have research projects in the works there, too!

BOHNING: So you did this electrochemical work on the side while you were doing the RDX work, is that true?

GAUVIN: Yes, that's exactly it.

BOHNING: Well, you said you had the energy. It must have required a lot of time.

GAUVIN: Yes, twelve hours a day and weekends, etc. We all worked hard. To this day, Ph.D. students still work hard. They were wonderful days.

BOHNING: You said the first ten papers were mostly electrochemical with Winkler. But by 1955 you had a paper on spray drying (3).

GAUVIN: Yes.

BOHNING: So you were moving in that area already.

GAUVIN: That's right, yes. My work actually started in the late 1940s. To be honest with you, all the spray dryers in existence at the time which I had designed were either identically the same size as the one that I had developed for Frank W. Horner or a multiple thereof, without ever altering the relationship between the important parameters. In other words, I didn't know what the hell I was doing, but I was doing something right. Then I said, "This has got to stop." That is when I started the whole series of studies on all factors involved in spray drying, starting with particle dynamics, heat transfer, mass transfer, fluid mechanics, effects of turbulence, etc. This would take about thirty years, and finally we knew what we were doing!!

BOHNING: I recall one paper that you had where you did some radioactive tracer work to try and follow the particles (4).

GAUVIN: With Len Torobin. That's right, yes. That was a clever piece of work. Torobin, who did that particular work with me, was at that New York party (I mentioned earlier) a few months ago. I shook hands with him and I found that his right hand was still deeply scarred, thirty years after this particular project. As you said, the experimental technique consisted to fire individual particles (previously irradiated in the N.R.X. reactor at Chalk River) vertically upward in a large ballistic tunnel, equipped with ultra-sensitive Geiger-type sensors. The particles of various sizes were received from Chalk River in a lead

insulated "castle." One day, in his haste, Len took a particle out of the castle with his hand. It was a foolish thing to do. I said, "Len! What are you doing, for Christ's sake! Those particles are hot." To this day he bears the scars, but he is a big fellow and has regular check-ups, so there is no danger at all. His work was excellent. It was a very, very effective way of measuring the instantaneous velocity of particles under all kinds of conditions, all kinds of shapes, all kinds of acceleration, deceleration, and so on. The work went on with other fellows, to investigate the effects of oscillation and other factors. We certainly learned a great deal about particle dynamics. Stuart Churchill was very interested in this work.

[END OF TAPE, SIDE 2]

GAUVIN: There is one thing to my small credit. Basically, I'm an industrial engineer and all my projects, or most of them, had an eventual ulterior industrial application. But I've always felt the necessity, to this day, of knowing what the hell you're doing from a fundamental point of view. That has paid off handsomely.

BOHNING: Well, certainly your mathematics background paid off there.

GAUVIN: Oh, sure, you're right.

BOHNING: I was trying to figure out when you started using computers because there was one paper where I noticed you were using an IBM 650 (5).

GAUVIN: Yes! [laughter] You are referring to Torobin's work on turbulence. That's all we had at McGill around the middle 1950s. But from then on, practically every graduate student used a computer. But sometimes our requirements exceeded the capacity of our central facility. This was the case, for example, of [N. N.] Sayegh's work on very high-temperature heat transfer to spheres (6). We had worked out all the required correlations (very complex) but our computer was too slow to make the equations converge. So in desperation, I called [A. E.] Hamielec, a good friend of mine, in Hamilton at McMaster University. They had, for these days, a high-power computer, and even then we kept that damn thing running for hours at a time. But we solved all our equations.

Since that time every one of our students has his own computer and they use them profusely. I'm lousy on computers. The reason is that at Noranda I had an army of guys working for me. If I knew what I wanted, I'd say, "Okay, you figure out the programs, and you work out all the computational steps," and they did the work. So I considered computers a little bit like plumbing. We had plumbers, and we had draftsmen, and we had computer guys! I deplore this now, because now that I have time, and my own computer, I find I have a lot of catching up to do.

BOHNING: There are some other things that I would like to discuss. [shuffling papers]

GAUVIN: Where the devil did you accumulate all this?

BOHNING: Mostly I'm just following through the papers that you wrote and piecing that together.

GAUVIN: Oh, I see. I have a few write-ups here which are different. There is one in Canadian Chemical News from March of this year which presents a summary of my career (7). It starts out here, "William Gauvin"--of course, you have to read it with a grain of salt. But the substance is there. "Researcher, teacher, and manager." Well, that's true.

I got a prize in 1984. It's described in this book. It's in French, but it's a very good summary of my career (8). [showing photographs] There I am at Noranda when I was visiting the mines. I became a mining engineer in a limited sense. That's me and some of my former students, because naturally you will not be surprised to learn that in those days, most of the staff of Noranda was filled with Gauvin's boys. [laughter]

[showing photograph] And this is at the official opening of the Noranda Research Center. There I am, there is Madame Kirkland-Casgrain, who was a minister of some sort, and this was the premier of the province, René Lévesque. You probably have heard about that character.

BOHNING: Yes.

GAUVIN: It's a book; it's well done. It's La Passion de la Science--A Passion of Science. But it's in French. It has portraits of various people who got the prize [Prix Marie-Victorin, Prix des Sciences du Québec]. It's the big prize of Quebec. But the prize that I most cherish is the Killam [Memorial] Prize in Engineering. It carries more money. [laughter]

BOHNING: Would it be possible to borrow this to photocopy? I might be able to do that downstairs in the hotel before you leave. That way I won't have to take it with me.

GAUVIN: Yes, that's right. But it is in French. I also brought the program, which they may not have sent you, of the [Eugenie] Lamothe Lecture (a bit of a misnomer, since it was actually a series of lectures, spread out over a day and a half). You will recognize that L. B. Torobin was our luncheon speaker. He described to us his invention of a process to make hollow glass micro-spheres, based, he said, on his training in particle dynamics at McGill. As a director of one of his companies, I can vouch for the immense financial success of his inventions. Stuart Churchill was the dinner speaker. Do you know Stuart?

BOHNING: Yes.

GAUVIN: I respect him immensely, you know. There are a few other personalities who were there. Julian Szekely of MIT was one of our speakers. There was Ed Crosby, University of Wisconsin. (Bob Marshall was dead, although he was also a close friend.) Crosby is a very good spray drying man. Also Pierre Fauchais, who is president of the University of Limoges. And Emil Pfender, you may or may not know him, from the University of Minnesota, one of the outstanding authorities in plasma theory in the world.

BOHNING: No, I don't know him.

GAUVIN: What you may not know is that this was just the first in a series of annual lectures made possible by the bequest to McGill and to our department of funds from the will of the late Eugenie Ulmer Lamothe. The theme of the day-and-a-half conference was "particle systems and plasma processing," and covered most of my activities at McGill. That's why so many of my former graduate students had been invited.

I've been doing a lot of talking, but what is the purpose of all this?

BOHNING: The purpose of all of this is, as we've done with Stuart Churchill (9), to create a document for future use, to outline your career in such a way that other people might use

some of this information in the future, for scholarly purposes. It's an archival project.

GAUVIN: But I'm not a Stuart Churchill. Let's be very honest about this. In Canada I'm fairly well known.

BOHNING: Exactly.

GAUVIN: But Stuart Churchill in his way is a giant, and I don't compare with that kind of guy.

BOHNING: I have talked to Hoyt Hottel, by the way, too, a number of years ago (10).

GAUVIN: Yes. And what did he say? Shot me down, eh?

BOHNING: No, we didn't talk about you at all.

GAUVIN: That's even worse. [laughter] Hottel was damn good in his days.

BOHNING: Let's see. Who else have we talked to? Donald Katz (11).

GAUVIN: Oh, yes.

BOHNING: I just talked to Neal Amundson last year (12). He was

at Wisconsin.

GAUVIN: Oh, yes. Another giant, for gosh sake.

BOHNING: And Manson Benedict, who was in nuclear engineering at MIT (13). He's actually a physical chemist.

GAUVIN: I don't know him.

BOHNING: Well, let me ask you a few more questions, if I may.

GAUVIN: Sure.

BOHNING: Can we go back to Horner again? I was quite intrigued by that time period and the work you were doing there.

GAUVIN: Yes, that's right. It was at the end of the war; industry was trying to spread its wings again and face again the competition that would surely come.

BOHNING: You really developed spray drying through that association.

GAUVIN: Absolutely. I have photos of the spray dryer that I designed for them. Those were very formative years.

BOHNING: Who built these spray dryers? As you said, they were

large.

GAUVIN: Yes--quite large (twenty-five feet in height, and fifteen feet in diameter). It's simplicity itself. It's simply tinsmith kind of work, of the kind used in large-scale industrial ventilating systems. Mind you, though, in such a large size, the walls have got to be sturdy. The first time we tried our prototype, the volume of hot drying air was so large and the walls were so thin, that you could actually see the damn thing bulging out! [laughter] Which frightened us no end. I had miscalculated the resistance of materials, of the thin plate I had prescribed. We cut down the air temperature, which cut down the air volume and the bulge disappeared. Some plates had to be replaced by thicker material. That was all.

I worked for Horner for only two years, and all my time was used in developing production methods for the new materials coming out from the R&D group, which was darn good. I understand that, after I left, they used the spray dryer more and more to prepare dry products, which could then be used to form solid tablets or pills.

BOHNING: Well, you have over the years been involved with and talked about science policy and the government here in Canada.

GAUVIN: Yes.

BOHNING: Maybe we should talk about that a little bit, because I

was intrigued by that. You've commented that here you had money and a free hand, but later on in your associations you didn't have that free hand.

GAUVIN: No. It would be more correct to say that I had sufficient money to carry out a well-thought-out program, but not new intriguing ideas only remotely relevant to our business or market plans.

BOHNING: Do you think that that stifles creativity?

GAUVIN: This question requires careful consideration. If I were a pure type of academic, I would say (in agreement with all my friendly professors), "Yes. Absolutely." However, as a half-baked industrial I hedge my answer. You need a certain amount of freedom, but my dear fellow, it must be oriented freedom. I mean by that that before we embarked on anything, we did an awful lot of preparation, not least of which were clear statements of the objectives. (I didn't know you were interested in that aspect, because it's quite a story.) To make a long story short, I've become really obsessed with the necessity of adequate government support for R&D. I published a paper on this topic, which I think is entitled "Contributions of R&D to Economic Growth." Unfortunately it was published in Chemistry in Canada (14), a journal with very limited distribution. I took one of our companies (we didn't name which one) at Noranda, as a test case, and calculated what contributions accrued to the government from

a successful research grant to the company. Of course the company which served as a model provided us with all the actual figures which were required for this analysis. But the results were irrefutable. The government was, by far, the greatest winner from its industrial R&D support program. I was astounded to see what the government, out of the amount invested in research, got back from all sources. The article is quite long and difficult to read, but at that time (it was written in 1978-1981) it summarized most of the important findings on the effects of R&D on the many components of a nation's economy. In effect, it was a mini-course in this particular area of economics.

BOHNING: What kind of a response did you get to that paper?

GAUVIN: Very good, from a limited number of people. To this day, I still receive responses, generally quite positive. It was published in 1981. I started working on this material in 1975, and began to give talks on it while I was president of the Chemical Institute of Canada in 1977-1978. When it was finally ready I felt duty-bound to submit it for publication in Chemistry in Canada, the official publication of the CIC. Unfortunately, this was the wrong journal, with a small readership, largely consisting of academics. As expected, it was heavily panned by certain government officials who bombarded me with unbelievably imbecillic objections.

BOHNING: I'd like to have a copy of that.

GAUVIN: As a result of all this soul-searching, I promoted this concept of what we called "actions concertées" ("concerted actions"), in which we got industry, university, and government together in concert to work on a number of promising projects of common interest. The one I participated in involved McGill, Noranda, Sherbrooke University, Hydro-Quebec, and the Industrial Materials Research Institute. Those were glorious years. It never went very far (our personal interests were too divergent) but the concept is still very much alive, albeit on a more limited basis. The one I am still involved in is the Plasma Technology Research Centre.

BOHNING: In addition to the one paper we've just mentioned, you had papers such as "Chemists and Science Policy" and "National Planning for Innovation" (15, 16). Were you involved with the government directly or was this effort on your own?

GAUVIN: Actually both. I was always involved with quasi-governmental organizations, such as the Science Council of Canada (1966-1970 and 1971-1976), Le Conseil de la Politique Scientifique du Quebec (1971-1979), the Council of NRC (1964-1970) and a number of interesting groups in the U.S.A. But my closest contact with the senior levels of government came when Alf Powis, then president of Noranda, called me one day to say that Bill Schneider, then president of the National Research Council had asked him to loan me to the Council on a half-time

basis, and that he had accepted. My title would be Délégué Général (in fact vice president for policy and planning). What a bombshell! Now NRC is a big lab. I knew that Bill Schneider was under fire, because a lot of business people were complaining that the work of the Council was too academic, and my job would be to develop a plan to give their various programs a greater practical orientation. Now as a Frenchman I knew immediately what he meant when he mentioned the title of Délégué Général, but a lot of people who came to see me asked, "So what the hell is a Délégué Général?" So when I finally left the Council some twenty months later, I said, "The first thing you're going to do is do away with that stupid title." [laughter] In his telephone call Powis added, "I'll give you all the facilities and all the time, providing you could spend half of your time in Ottawa," which I did, religiously. I had a grand office with all these assistants. It was a tough job, talking to all these directors of divisions and their staffs. They were all powerful men, and very proud. And slowly, slowly, not losing my temper (which was tough), I think I contributed a little. I think I was aided in this by Noranda's big reputation as a driving force in the economy.

At first they thought, because of the powers I had, that I was going to be a hatchet man. I visited all the darn divisions and talked to all the guys, and I entertained a lot. I think I initiated some changes which are probably coming home to roost now. I started at that time to impress them with the necessity,

"Do fundamental work, like I have. I'm an example! But try to orient towards the eventual industrial application." By that time Noranda had obtained some two hundred patents. So I insisted that in all the programs (on certain projects, not all of them) they indicate the relation of this fundamental work to the industrial implications of the program. I insisted that this be done. Well, some of them were convinced and they did. I did that for a year and a half and it damned near killed me. I remember sometimes going to Ottawa twice the same day. Mind you, it's only an hour and a half each way. For example, leaving the house about seven o'clock for an eight-thirty meeting with the president, whipping back at around eleven o'clock for a luncheon appointment with some of my group at the Centre, whipping back for a three o'clock meeting at NRC, coming back home, limping, for my supper. [laughter] I didn't do that often, but I did it on a number of occasions.

But I had wonderful assistance from all my people at Noranda. They knew what I was trying to do and they were proud of the fact that the boss was doing this. I was trying desperately to set examples. I strongly believe in motivating people; that's the key to success in research. So that's what I tried to do in a very, very small way. It's strange that it's not on my curriculum vitae. Probably I thought it was not important enough.

BOHNING: You also had a paper called "Northward Looking--Strategy and Science Policy for Northern Development" (17).

GAUVIN: Oh, yes. At that time I had been re-elected for a second term (another three years). Early in my term, the president of the Science Council said, "Bill, you're going to head a team. We're going to give you the funds, and you're going to crisscross the North, talk to the people, and see what the hell your team can do with these Inuits, Eskimos, Metis, and Indians. You have a free hand." And I did. For three weeks a small gang of us, with a variety of backgrounds, travelled from Aklauik, Inuvik, Tuktoyaktuk and the Beaufort Sea in the West to Resolute, Arctic Bay and Pond Inlet in the East. I must say, however, it was in the late summer so I can't tell you glorious stories about the long nights. [laughter] On the contrary, we were working on too much daylight, so we couldn't go to sleep. [laughter] To survive, we walked half of the so-called night, and played interminable games of chess the rest of the time.

[END OF TAPE, SIDE 3]

GAUVIN: We wrote a very thick report on this, which of course nobody paid much attention to. Our North is still in a sad state of development. The report is available from the Science Council.

BOHNING: What was the major result from that report? What did you recommend?

GAUVIN: Well, we recommended that they should promote cottage industries. There were a few in operation. I should tell you that I had been brainwashed by Finland. I presented a paper in Helsinki in 1973, and then because of this coming assignment for the Science Council, I arranged to spend a week in Lapland. There I saw how the Lapps, who are at almost the same latitude as our own North, were canning caribou meat, for example. They were doing their own canning (which is a very simple operation after heat treatment) and selling it as a delicacy in Helsinki, at something like five bucks a pound. Whereas in our North they kill the caribou and save only what they are going to consume themselves. It's different, of course, in the major centers of Inuvik and Resolute.

They had a bead industry. It was an example of how people with initiative can really function under extreme northern conditions and if they're proud and dynamic, make a living out of it and stand on their own feet. There was something about those Lapps. If you ever have a chance to visit Lapland it would be a real eye-opener--the way they stand, the way they're dressed, the way that they treat the caribou skins, the ornaments that they have on.

We tried to inject some pride and some motivation into our northern people. I don't think it worked. In the first place, we didn't know how to do it. It was a strange phenomenon, when you can't reach the people, which I certainly wouldn't want to repeat. In all the localities which we visited, we insisted on

attending council meetings. Everything is handled according to the tribal system, or at least it seems that way. To have an idea of the problems they face, you must get to the council, and get competent interpreters. Their main concern, as expressed to us at these meetings, is that they want to have their native rights respected--which we all agree with. They also complain about the poor assistance provided by the government, which is absolutely not true. When we pointed out the tremendous amount of waste and garbage over most of their communities, they said that the waste was only temporary. If you go to Arctic Bay, for example, which is one of the loveliest locations in the high North (that's north of Baffin Island), what do you see? You see two things. There are hundreds and hundreds of empty oil drums (they depend on oil, you see, shipped at a cost I'll leave it to you). There are also dozens and dozens of snowmobiles which are left abandoned, sometimes for ridiculous reasons--spark plugs (they didn't know how to replace them), or batteries, or things like that. The waste was abhorrent to us, you see. Our North is in a very sorry state. And of course, those guys are forever complaining, yet they're well supported by the government--the nurses, the doctors, the counselors, the schools. Well, I'd rather not talk about it.

BOHNING: Okay.

GAUVIN: I don't think we were effective. We wrote a factual report, which should be available, if you are interested in it.

But the result was the recommendation for the development of a cottage industry in the small communities, and manufacturing activities in the larger ones, keeping however the severe transportation problems in mind.

BOHNING: I have on one occasion been at James Bay, and once at the Iron Ore Company of Canada in Labrador. I can identify with some of that problem with what I saw at James Bay.

GAUVIN: Did you visit some native villages?

BOHNING: Yes. We were out in an area where the government had actually built houses for them, and where they had taken the doors and the windows out. We were told how they would take snowmobiles in the dead of winter and go north, surviving as their ancestors had.

GAUVIN: Yes. I know. It's not unique to the North, you know.

I did a fair amount of traveling for Noranda. In Europe, of course, but mostly in Africa and South America, some of it in Iran, and quite a bit in Turkey and Australia. However, it was mainly in Zaire and Zambia that I became particularly aware of the role of tribal relationships. While in Kinshasa, for example, I was receiving calls in the middle of the night, denigrating the influence of a particular minister whose support I was seeking for a large-scale hydrogen production project (for

the production, in turn, of fertilizers) using electricity from the immense hydraulic power of the Zaire (formerly Congo) River. Apparently, this minister belonged to the wrong tribe for this kind of activity, or so was I told by my midnight caller. In our Palabora copper mine in the north of Transvaal, I was told by the mine manager that great care was taken that only workers from the same tribe be allowed to work on the same job.

BOHNING: How did you make that connection with Noranda?

GAUVIN: They came to me. I don't know where they got my name, but they approached me. Very business-like. It was not an instantaneous marriage, because of my affiliation with McGill. Actually, years later, they saw the wisdom of letting some of my senior men direct some projects at McGill or at Université de Montreal. (Mike Avedesian, N. Bharucha, and George Kubanek worked in that capacity to some extent.) Noranda has almost doubled in revenues since I joined them in 1961. I supervised two expansions to the Research Centre after 1963, but my staff never exceeded 175 people. A new director of research is now at the helm (Dr. Frank Ledderman) and he has recently completed a third and very large expansion. His staff is now 235. I wish him luck! In R&D, your headaches go up exponentially with the size of your staff!

BOHNING: What was their principal thrust in research when you were there?

GAUVIN: To improve the efficiency of the present operations, find applications for the by-products from their current operations, and don't look at other things (unless business is exceptionally good!). Don't bother us with titanium, for example. However, I supervised a Ph.D. project at IMRI in Boucherville recently, and I must say to their credit that they hired my student, a bright Greek. We had developed a method of producing a titanium alloy as a precursor to titanium metal (18). Noranda had had a good year and the research center was being expanded, so they said, "Okay, we'll give it a try." They hired Peter (he had his Ph.D. by then) and they gave him a large sum as research funds for two years. "Go ahead and develop the technology." He made good progress. When the 1991 budget was finalized (a very painful process, incidentally), titanium received support for part of the year only, and Peter decided to resign. He has made good progress with the original project, and I have little doubt that he will succeed eventually. Whether the process will be economically attractive is another matter. All my life I have observed that technical feasibility is easier to achieve than economic feasibility. Incidentally, I am given to understand that Noranda and Peter are parting on a friendly basis.

The only exotic metal that they allowed me to develop was molybdenum, as previously mentioned. There again, it was the plasma process which was the most attractive. For excellent reasons, Noranda decided to have the first plant built in

Belgium, where we had good contacts with the European market. Then the stainless steel business collapsed (that's about seven or eight years ago) and the high-strength, low alloy steel business as well, which, between them, were very big users of molybdenum. So molybdenum was no longer in large demand, and the whole project was wrapped up.

BOHNING: The plasma technique as a chemical engineering process has a lot of applications, many of which you've developed.

GAUVIN: Yes, I have developed a few, but commercialization has been slow, at least as far as I know. It is quite possible that quite a few companies are using plasma technology based on some of my publications--and I wouldn't know about it. About ten years ago, I latched on with an engineering consulting company in England, Davy McKee, in Stockton-on-Tees near Newcastle, in northeast U.K. I admired their guts; they got a license from Noranda for my plasma patents. (Hydro-Quebec had bought the rights for Canada only.) Davy McKee succeeded in selling a few steel-melting installations in Australia (up to five megawatt capacity) and have quoted for a slightly smaller unit for Pohung Steel Company, in South Korea.

BOHNING: There's an article in Chemical Week in 1985, about the commercial promise of plasma processing. Your name is mentioned a few times (19).

GAUVIN: Yes.

BOHNING: There seems to be some disagreement as to how commercially promising this process really is.

GAUVIN: Yes. I understand the situation. It's a new technology, operating at very high temperatures. It takes a lot of marketing efforts to effect a breakthrough. University professors working in the field, like my colleague at McGill, Professor R. J. Munz, or Professor Maher Boulos at the University of Sherbrooke, simply haven't the time to bang on doors to promote applications in industry. Marketing is a demanding activity. I am not very good at it. Although I have done a fair amount of research, my track record in development is poor. In other words, I cannot guarantee quick returns. All the applications I have been working on appear to be technically feasible. For some of them economic feasibility also appears attractive. But the fact remains that development work at a pilot-plant scale, or at least at a reasonable scale, for continuous operation for a reasonable period of time, is still required. And that may mean a large expenditure of money. But yes! Some applications appear to be very attractive, indeed.

BOHNING: Did you develop the peat processing? You had a number of papers on it.

GAUVIN: Yes.

BOHNING: Was that ever developed commercially into anything?

GAUVIN: It is being developed in Finland. They came to see the process, and they're using it now. Peat is a very good starting material for the synthesis of many chemicals--where the situation is right.

BOHNING: But not here.

GAUVIN: No. The harvesting of peat from peat bogs is an expensive operation, and Canada has no experience in it.

BOHNING: It is just not economically feasible?

GAUVIN: No, it's economical, as shown by detailed economic assessments carried out by Noranda, but it would be a very big undertaking requiring considerable investment. It's tough to make a buck with <u>really</u> advanced technology--in Canada. On the other hand, Canada has always done very well with well-established technology, in combination with their natural resources.

I must admit, however, that a strong marketing campaign by a technically advanced company is not always conducive to success. A good example is SKF, in Sweden. They spent millions trying to market their plasma technology, which was quite advanced, without

success. After three years, they gave up. There was a fatal flaw in their marketing strategy, which consisted of trying to sell a complete plant which costs many millions of dollars. Our great advantage is that our plasma torches are ridiculously inexpensive and highly efficient. In this connection, my major interest is to prove a new concept: developing a torch operating on water vapor. Our preliminary research yields an efficiency close to one hundred percent.

BOHNING: Has the application of plasma technology to waste disposal caught on?

GAUVIN: Partly, yes. Mainly in France. I've developed several techniques, again based on my knowledge of AST, the atomized suspension technique, and spraying the toxic waste in presence of a plasma flame. I'm approached very strongly by companies right now, but I refuse to get involved in that kind of activity. It is needed perhaps, but not exciting work.

BOHNING: How do you assess the state of education in chemical engineering in Canada today? Are you attracting students, getting native Canadians to go into the field?

GAUVIN: Chemical engineering at McGill is rated first in Canada and fourth in North America, as I mentioned before. U. of Toronto is a close second, followed by U.B.C., McMaster and Polytechnique. By and large, I would say that our Canadian

universities are doing a good job. We are well organized, and our Canadian Society for Chemical Engineering runs a good journal, well-organized conferences, and is a competent organization.

Your question is very general. If by "native Canadians" you mean Inuits, Indians, etc., the answer is zero (which I deplore). Most of our undergraduate students come from Quebec (about seventy-five percent), followed by Ontario (about twenty percent). The others are USA and foreign (non-USA). The percentage of women is rising (probably roughly twenty-five percent).

One result I remember well from last year's list of graduates (B.Eng. in chemical engineering), because it was discussed at one of our Board of Governors meetings, is our so-called productivity (graduates per year per full-time staff) is the highest in the country (around 3.5) versus a Canadian average of about 2.5 and a US average of 2. Whether that's good or bad, I don't know, but it may be due to the number of adjunct professors we have.

BOHNING: Why would U of T have more students? Your program is ranked higher than U of T's.

GAUVIN: I believe it is a combination of factors: Ontario is more populous. It is also wealthier. It has no language problem. Because of its excellent reputation, it probably draws

students from other provinces. After all, Toronto is an attractive city! It may also attract disgruntled English-speaking students from Quebec. These are, of course, all conjectures.

BOHNING: If most of your students are from Quebec, then it's considerably localized.

GAUVIN: Yes, very. I should add that we attract a surprising number of French Canadians, anxious to learn English. Perhaps it is fifteen to twenty percent of the student population, as a rough guess.

BOHNING: What about at the graduate level?

GAUVIN: At the graduate level, the student population is far more heterogeneous, and includes a fair number of foreign students, mainly Asians, Lebanese, some Turks, Greeks, and Brazilians. As a rule, they are very good students, highly motivated. We also have brilliant professors, with attractive projects, such as Murray Douglas, Musa Kamal and Arun Mujumdar, to mention a few.

I'm trying to promote, though, university-industry projects, and I have one going on in which I'm going to act as technical director. It's one of my ideas to make fused silica, and that's going to be paid by a small company. They want to upgrade their production of silica. It will be done at McGill. This is the

sort of thing that I'm promoting. And I definitely want to slow down.

[END OF TAPE, SIDE 4]

BOHNING: You said earlier that you had many turning points in your career, turning points that weren't planned on your part but events that sort of happened that were crucial events. Looking back, what would you say was the high point of your career? What gave you the greatest satisfaction?

GAUVIN: In retrospect, the high point in my career was my long association with Noranda (1961-1983). This is where I learned to manage a large group of researchers, while at the same time carrying out a very active program of personal research, mainly at McGill, but not entirely. It is to Noranda's credit that they allowed me to do this. It is during that time that I developed a unique approach to plasma technology, culminating in my patents with George Kubanek that would allow me to pursue a final career with Hydro-Quebec from 1983 almost to the present. I got a great kick in being able to repay Noranda for their confidence by selling our patents to Hydro-Quebec with an impressive financial return.

To answer your second question, the greatest satisfaction I got was the Killam Prize. In the States you have so many higher prizes, but in Canada the Killam Prize in Engineering is quite

an honor. I was the first McGill guy who got it. And David Johnston, our principal, made a big thing of it. He gave us a fabulous reception, and I was a hero for one day. And I enjoyed the cheque!

BOHNING: The Killam Prize is not just chemical engineering?

GAUVIN: No, there are three. There is a Killam Prize in health sciences, a Killam Prize in natural science, and a Killam Prize in engineering. It's a very high honor in Canada, as far as pure engineering is concerned.

BOHNING: What organization sponsors it?

GAUVIN: The funding of these awards is provided by a bequest of Mrs. Dorothy J. Killam, and administered by the Canada Council. More correctly, these three awards are called "Izaak Walton Killam Memorial Prizes."

Well, my dear fellow, I've kept you very busy. It's twelve o'clock.

BOHNING: I've enjoyed it very much and I appreciate your taking the time this morning to come over and talk to me.

GAUVIN: Yes, I talked too much.

BOHNING: No, no.

GAUVIN: Please excuse me; I didn't know what to expect. It's not the sort of thing that you can prepare and make a crisp presentation.

BOHNING: I would prefer that it be spontaneous, actually.

GAUVIN: Well, it was spontaneous, I assure you.

BOHNING: Yes. Because it's meant to be a conversation. It's been very, very interesting to me. Your career is quite different in many respects. You have an interesting blend of your long association with McGill and the academic...

GAUVIN: Yes, that's right.

BOHNING: ...and a very, very strong industrial tie, probably more than a lot of other chemical engineers in the sense that they may consult but your association was more than just consulting. You were actually being employed...

GAUVIN: Yes, that's true.

BOHNING: ...while you maintained doctoral thesis direction at McGill. That's a very interesting combination and not a very common one.

GAUVIN: It's not, no?

BOHNING: I don't think so.

GAUVIN: Well, not in Canada, I agree, but in the States surely you must have examples of that.

BOHNING: Well, there the academic tie is the major one and the industrial tie is the consulting aspect.

GAUVIN: Oh, yes.

BOHNING: But consulting is not the same as what you were doing. You were the research director, so your input was a lot different.

GAUVIN: But I must say, to explain this arrangement which seems different to you, that chemical engineering is an engineering profession that lends itself beautifully for this, because chemical engineering the way I always thought about it is processes. Processes have wider applications. That explains it, I think.

BOHNING: Well, thank you again.

GAUVIN: You're most welcome.

[END OF TAPE, SIDE 5]

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INDEX