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"Odyssey of an Engineering Researcher"

by Ken Rush
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Abstract

This paper was initially dated May 1996 and published privately by the author. However, its attraction as a memoirist's contribution to the EIC's Working Paper Series was obvious (to the Series editor at least!), and the author kindly consented to it this transformation. The language and the 'idiom' of the original have, however, been preserved and the editor has touched the text very lightly.

The paper begins with the discussion of an important distinction for the author and for the paper as a whole: between scientific and engineering research. It moves on to the author's early work in aeronautical research in the 1940s at the National Research Council in Ottawa, at Farnborough in England and, on his return to Canada, on problems of aircraft icing, again at NRC. The paper then discusses changes that were taking place in the later 1950s within NRC, to research in Canada generally, and within the universities. It follows the return of the author to Queen's - as a professor of mechanical engineering - in the early 1960s, his varied experience as a well-travelled academic and, following a sabbatical leave in 1973-74, the switch in his research to the solar energy field. It ends with a discussion of his work as a consultant to the federal government in this field during the years prior to his full retirement in 1990.

About the Author

Ken Rush graduated in mechanical engineering from Queen's in 1944, and then joined the Aerodynamics Section of the NRC's Division of Mechanical Engineering in Ottawa, where he remained after the end of World War II. In 1949 he was temporarily attached to the Royal Aeronautical Establishment at Farnborough, England. On his return to Canada he was appointed to the Low Temperature Laboratory of the Division. In 1962 he received a diploma in engineering heat transfer from McGill University and, a year later, an MEng in aeronautical engineering from Carleton University. This same year he took leave of absence from NRC to join the academic staff of the Department of Mechanical Engineering at Queen's and, two years later, was appointed to a tenured position there. During the academic year 1973-74 he was on sabbatical leave at the University of Southampton in England, after which he returned to Queen's where he remained until his formal retirement in 1986. However, until 1989 he continued to teach and do research there, as well to act as a consultant to the federal Department of Energy, Mines and Resources. He is currently a member of the EIC/LMO.

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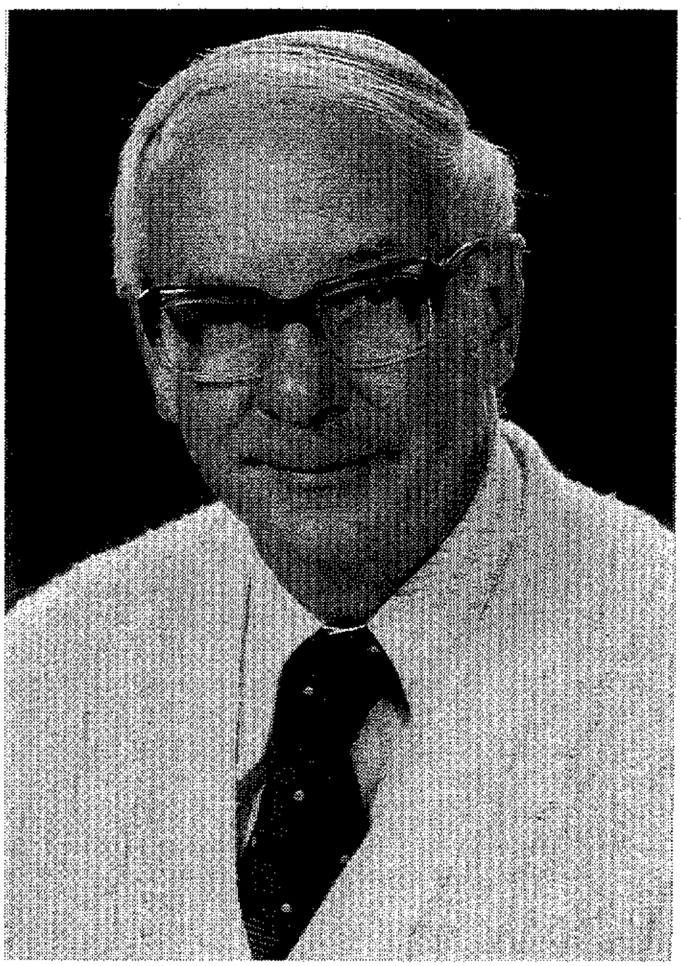

Ken Rush

Introduction

Engineering research and scientific research, what's the difference? I take the view that engineering research stems from a need to solve problems that arise when a potentially marketable product or service is being developed. The problems need to be of an engineering nature and they need to be of a research nature; facts need to be discovered, knowledge needs to be generated. Engineering research therefore has an applied or practical nature. Scientific research, on the other hand, has the objective of extending knowledge. Whether or not there is a need or a practical use of the results is not a concern of the scientific researcher, his curiosity and inquisitiveness are the driving forces.

The two often go hand in hand. Engineering research gives rise to scientific research and sometimes vice versa. For example, in my work with two phase solar collectors there arose a need to know more about the boiling characteristics of water at sub-atmospheric pressures so that better collectors could be designed. A laboratory apparatus was needed that allowed study of the boiling action under conditions controlled to simulate those encountered in the real world. That, for me, is engineering research. If while conducting such research, my curiosity is aroused about the boiling action of freon-12 under similar conditions, then I am venturing into the realm of scientific research because a cursory inspection of the properties of freon-12 shows that it is not suited for the intended use because high pressures are possible under some operating conditions. Safety and cost considerations rule it out as a practical working fluid. If how it performs fascinates me and I embark on an investigation, that's scientific research.

This kind of distinction is often blurred. Perhaps using freon-12 in place of water will generate knowledge that gives a better understanding of how water boils under the particular circumstances encountered in an arrangement for a solar collector. Would it be scientific or engineering? The answer lies in what is driving the researcher: the creation of a useful, marketable product or service - engineering; satisfaction of intellectual curiosity - scientific.

The National Research Council

I have been cast in the engineering research mould. What we become is a product of our experiences with institutions and people. For five decades, two institutions have been central to my experiences: the National Research Council of Canada and Queen's University at Kingston, Ontario. As an undergraduate student at Queen's I was not aware of any graduate students in engineering and there was no visible research done in the Mechanical Engineering Department. In fact, very little research was done in any Canadian engineering school of that time. I graduated with my Bachelor of Applied Science degree in 1944 during the Second World War. Engineering students had been enrolled in the Canadian Officers Training Corps and encouraged to complete their studies because of Canada's need for engineers in the Armed Forces, in industry and to some extent in research institutions such as the National Research Council. During the war the Technical Service Council was created to direct engineers to the places where they were most needed. Because I had done well academically, I was encouraged to seek employment at NRC, where the Mechanical Engineering Division was rapidly expanding. With several other new graduates, I joined the ME Division that might better have been called the Aeronautical Division because 90% of its work had to do with aircraft.

My position was in the Aerodynamics Section, where there were about a dozen engineers, a couple of mathematicians, several tehnicians, and five or six young women who were known as 'computers' because they did our calculations, graph plotting, etc. Almost everyone in the ME Division was a born Canadian. The engineers were graduates with bachelor's degrees, mostly from Canadian universities - and especially from the University of Toronto - with a few from British or American universities. The only person with a doctorate was a chemist, Dr. J. Broughton, who headed the Fuels and Lubricants Laboratory. The Head and founder of the Division was Mr. J.H. Parkin, a former professor at the University of Toronto, who came to the Research Council in the 1930s to build and operate Canada's first wind tunnel of significant size. With the coming of war, the Mechanical Division under the direction of Mr. Parkin grew to a cluster of modern, substantial laboratories, including two large and impressive wind tunnels.

My first work was to assist in the wind tunnel testing of models of aircraft or parts such as wings and control surfaces. After a few months the responsibility for a project came to me. When the tests were finished, I prepared a report and sent it off to Mr. Parkin for his approval. When it arrived back on my desk I was dismayed to find a lot of writing with a red pencil. I kept it out of sight of my new friends while I painstakingly prepared a corrected version. My friends fared no better! This was a humbling and unpleasant experience at the time but, in retrospect, it was a good thing to have to write passably well. I learned the value of a good critic who takes the time to help on a one-to-one basis. Later in my career I remembered the virtues of using a red pencil!

Before long the operation and maintenance of the two thousand horsepower horizontal wind tunnel was added to my responsibilities. To broaden my experience I flew as an observer in experimental aircraft. Those were exciting days when, at last, we felt we were making a contribution to the war effort.

An important opportunity came to me about a year after I started at NRC when my Section Head, Mr. Bud Levy, assigned me the task of improving our testing abilities by adding powered propellors to our wind tunnel models. Arrangements were made for me to visit the Langley Field Laboratories of the U.S. National Advisory Council for Aeronautics (the predecessor of NASA) and the General Electric Company in Schnectady, New York. At Langley I was shown several wind tunnels, two with the kind of equipment we would need. I enjoyed an enriching few hours talking with several researchers doing my kind of work. At the GE plant I spent an hour with the Chief Engineer of the Division that could supply our equipment. Satisfaction came my way from being able to discuss details of the required equipment and being commended for my knowledge - not always the case, I was informed!

With the end of hostilities, a period of uncertainty about the future of the Research Council ended when the federal government, under the urging of the Hon. C.D. Howe, the Minister of Reconstruction and Supply, decided that it should be Canada's premier research organization and that the wartime staff be offered peacetime positions. In the Aerodynamics Section we were soon testing models of new aircraft, such as the Husky bush plane, the CF-100 fighter and the C-102 Jetliner. Working closely with designers from Fairchild, de Havilland and Avro gave a satisfying taste of engineering research because we discovered the problems with our wind tunnel testing and helped with the solutions. A special sense of accomplishment came to me when I devised a wing-fuselage fairing that corrected a precipitous drop in lift when the wind tunnel model of the Jetliner reached its stalling angle. When test speeds higher than those available in our NRC wind tunnel were required for the Jetliner, arrangements were made by Avro for use of the high speed tunnel of the Cornell Aeronautical Labs at Buffalo, New York. I went as an observer and spent a week at an institution dependent mainly on contracts for its income so that control of costs was always a major concern.

Farnborough Sojourn

Many Research Council employees decided for various reasons to go elsewhere after the war. In the Mechanical Division replacements began to arrive, particularly from Britain. They were experienced researchers from the National Gas Turbine Establishment, the Royal Aircraft Establishment and the National Physical Laboratory. Many had advanced degrees and soon took leading roles in the various laboratories. Dr. D.C. MacPhail, a Canadian trained in Britain, came from the Royal Aircraft Establishment to become the Assistant Director and the successor to Mr. Parkin. He soon imposed his views. Labs were reorganized, some research endeavours stopped and new ones started. His perception of some of us working in aerodynamics was that we needed more experience, and to get it he arranged for sojourns of several months at the RAE, in Farnborough, England. So in the summer of 1949 I said goodbye to my ex-computer wife and two-year-old daughter before boarding a train at Ottawa's Union Station, bound for Quebec and a voyage aboard the Franconia to Liverpool, and thence to London for a few days before making my way to Farnborough.

At RAE I was first assigned to a low speed wind tunnel where I helped with testing and data reduction and learned about Miss Bradfield and 'keeping the book' - the meticulous recording of the day's activities - in a particular format according to her dictum. My new friends didn't know how long she had been there and they all shared an unwillingness to fall into her bad graces. I got to do almost everything but keeping the book - a chore that was reserved for the most experienced. Miss Bradfield warmed to me after an occasion when she invited several researchers to tea at her home. When it came time to make the tea she brought out several different tins and announced that she would like us to try a mixture of which she was fond. She took a bit from one and then a bit from another, and so on until she had what she wanted. When the tea was brewed and poured, I was the only person that didn't dump two or three spoonfuls of sugar and a slug of milk into the Spode china cups!

Working with experienced researchers I learned about new aircraft developments and gained an appreciation of our newer and better Ottawa tunnel. Morning and afternoon tea-times were opportunities to meet people from other wind tunnels and from theoretical aerodynamics, all gathered to chat and talk about their work. One of these, Dietrich Kuchemann, I met briefly. Many years later I got to know him better.

Half way through my stay at Farnborough I transferred to the high speed wind tunnel. Those were exciting times! That state-of-the-art tunnel capable of wind speeds close to the speed of sound was being used to test models of an array of exciting new designs. The people running it were a different lot; much younger, recent graduates, and even a young woman aerodynamicist! Before long I found myself working with a young man of my age doing the model testing and reporting for a new fighter plane. With his theoretical and my practical knowledge, we were a good team.

Towards the end of my time at Farnborough I received a letter from Dr. MacPhail informing me that, upon my return to Ottawa, I would be going to the Low Temperature Lab to put into operation a new icing research wind tunnel that was under construction. He also advised me to arrange a visit with Mr. J.K. Hardy, the leading authority on aircraft icing. I spent an afternoon with Mr. Hardy in his not very large office. His desk was the repository for piles of papers and reports. After I was seated and my chair adjusted to have a line of sight through the piles, he started by asking me whether I knew anything about icing. When I replied that I knew nothing, he sighed and handed me a report on the thermodynamics of aircraft icing. Then he began to instruct me about super-cooled droplets, droplet diameters, collection efficiency, and on and on. I was soon saturated with new knowledge and realized that I had a lot to learn. At the end of the afternoon, with several reports to take away, I made ready to leave. "Why do you want to get mixed up with this icing business?" Mr. Hardy asked. I stood dumbfounded and couldn't think of a reply. "Personally, I'm getting out. I'm going to grow apples to make cider." At the time this seemed a strange desire.

The Low Temperature Laboratory

Back in Ottawa I settled into the Low Temperature Laboratory under the headship of John L. Orr. This Lab had a large refrigeration plant that cooled several cold test chambers and the icing wind tunnel that was nearing completion. The Lab also conducted flight tests of ice protection equipment with the 'Rockcliffe Ice Wagon' - a specially modified and equipped North Star aircraft belonging to the Royal Canadian Air Force.

The icing wind tunnel was similar to other return circuit wind tunnels, but with a bank of cooled pipes that enabled freezing temperatures to be obtained in its 4.5 square-foot testing section. After commissioning and calibrating the tunnel, my next task was to devise a spray rig to produce a cloud of super-cooled water droplets - droplets that remain liquid at sub-freezing temperatures - and a means of determining their size and concentration. With knowledge obtained from Nick Golitzine of the neighbouring Engine Lab where spray rigs were used to conduct icing tests on gas turbine engines during the winter season, I designed and built a satisfactory rig. To obtain droplets of desired size requires a large flow of pressurized air. During its passage through the spray nozzle, the air speeds up to sonic velocity with a consequent drop in temperature - a drop sufficient to cause the production of ice crystals rather than super-cooled droplets unless the water is heated to a sufficient temperature. Testing ice protection equipment in clouds of ice crystals rather than super-cooled droplets gives faulty results! This was vividly illustrated when a parliamentary committee visited the Lab not long after the wind tunnel was put into use.

Any time a committee that influences financial support visits a lab, whether government or university, intense preparation takes place. A few days before the scheduled visit of our parliamentary committee, we installed a new design of wing de-icer in our carefully groomed tunnel. This design had a continuously electrically heated, one inch wide strip along the leading edge of a section of wing. Super-cooled droplets landing on it were sufficiently heated so that they remained liquid and flowed back to the cold surface immediately behind the heated strip, where they froze. Periodically this surface was heated to break the bond between the ice and the surface. The collected ice blew away and the next cycle was initiated. This de-icer design used much less energy than a continuously heated 'anti-iced' surface.

Well before the scheduled 10.30 am arrival of the committee we had the tunnel cooled down and were all set to 'do our thing'. That hour arrived and we waited. We waited until noon when the committee, led by Mr. M.J. Coldwell, Leader of the CCF Party, finally arrived. After introductions I explained about our wonderful invention, about the heated leading edge strip keeping an ice-free parting strip, and about turning on the heat under the collected ice and having it shed from the surface. Then we started the 1000 horsepower wind tunnel fan to circulate the cold air. That's when I noticed a cloud of something passing through the testing section before we had started the sprays. Nevertheless I gave the signal to turn on the wing de-icing system. Very quickly the leading edge parting strip acquired a thick layer of icy mush. Taken aback by this development I asked for more heating to be supplied. Water began to flow back and freeze on the shedding zones. The parting strip never did clear of mush - all this without ever turning on the spray nozzles. Eventually enough ice formed on the shedding surface for heat to be applied. We were able to de-ice! My mind raced to think of an explanation.

When we shut down the tunnel and the noise abated I stood at the wing, not knowing what to say. I need not have worried. Mr. Coldwell and other committee members came forward and congratulated me on what we were doing, thanked us for a wonderful demonstration, and implored us to keep up the good work. Not one remark was made about the parting strip performing just the opposite to my description of what would happen. Then they said goodbye and went on to the next lab.

We soon realized that the cooling coils had accumulated a coating of frost during the long wait for the arrival of the committee. Starting up the tunnel caused the frost to blow off the coils and to fill the moving air with ice crystals. These crystals were caught on the heated parting strip to produce the observed mush because the heat required to melt ice crystals is much greater than to prevent an equivalent amount of super-cooled water from freezing. This experience started me thinking about the consequences of exposing heated surfaces to impinging ice crystals. I did some experiments and produced a couple of reports identifying a problem that showed up on a British jet engine where the air entering the engine underwent a reversal in direction before entering the compressor. This engine suffered flame-outs when flying through clouds of ice crystals because mush formed in the elbow where the flow reversed in direction. The partial blockage starved the combustors of air.

Research activities with our unique icing wind tunnel led to a thorough understanding of cloud physics and the technology of aircraft icing prevention. My taste for engineering research was abetted by the completion of several successful projects and consequent patentable inventions. Opportunities for engineering research also arose because of the advent of the NRC Building Research Division and our Fairhaven adventure. Fairhaven is a co-operative community near the Montreal Road Laboratories to which the Rush family moved in 1950, not long after I started work at the

Low Temperature Lab. I was an originator and President of Fairhaven Co-Operative Community Inc. until we left in 1963 for Queen's. For a few years my spare time and energy were devoted to a young family, the development of Fairhaven and the building of our own house. Decades later it would be identified as a passive solar house. It incorporated innovative features such as the radiant heating system supplied with hot water from an automatically stoked anthracite 'furnace'.

John L. Orr was the best boss I ever worked for. He created an environment conducive to the formation of a research team that became renowned for its aircraft icing prevention knowledge, both theoretical and practical. He arranged visits to interesting places, encouraged participation in technical meetings and societies, gave praise for successful efforts, and was unstinting in his efforts on our behalf.

Changing Times...

The nature of Canadian engineering schools began to change in the 1950s. Graduate programs began to appear, especially at McGill and the University of Toronto with its Institute of Aerophysics. Some of my colleagues, particularly those that came after the war, decided to go back to school to obtain their doctorates. New doctoral graduates arrived to take their places. In Ottawa a number of evening graduate courses were sponsored by McGill University and given mostly by staff members of NRC. During the next few years I took eight courses from several gifted teachers, who gave me new analytical skills to complement my practical research experience. Then Carleton University started up its engineering school. When John Ruptash became Dean, the Faculty blossomed and soon a program of evening graduate studies was initiated to take over the McGill program. I signed up for more courses with the objective of obtaining a master's degree in aeronautical engineering. In the meantime, Dr. Bruce, Head of Mechanical Engineering at McGill approached me with a proposal that I come to McGill for several months to satisfy the requirements for a master's degree and perhaps stay on for a doctorate. With four small children and a tight budget, it was not possible to undertake such a program. Besides, with fifteen years of research activity in the best laboratories in Canada, I didn't think much of the need to do a university research project.

Towards the end of the 1950s icing protection for aircraft became less important because the increase in flight speeds provided aerodynamic heating to susceptible surfaces, except for some inlet components of engines and the blades of helicopters. Research for these continued, as well as the development of better ice detection instruments. The lessening interest in aircraft icing was marked by a decrease in the number of researchers. John Orr moved from NRC to another government department. George Samolewicz of the Engine

Laboratory was his replacement. Our Division Head, Dr. MacPhail, suggested new projects. Two of these came to me. The first was to design and supervise the construction of a small icing research tunnel capable of wind speeds approaching sonic. The second was to design, construct, and prove a compressor impeller test rig operating at very low temperatures - approaching that of liquid nitrogen. Both of these involved innovative components that required experimental development. The main challenge for the cryogenic test rig was the development of a liquid nitrogen-cooled heat exchanger that cooled the circulating air in the closed circuit test loop. My design for this component was a tube and shell exchanger with the circuit air passing through the vertical tubes that were cooled in the shell by liquid nitrogen adjustable in depth so as to control the circuit air temperature.

The purpose of the cryogenic test loop was to enable the testing of experimental centrifugal compressor impellers at reduced rotational speeds while using the reduced speed of sound at low temperatures and the ability to adjust the pressure of the circulating air to maintain the desired compressor blade tip Mach number and Reynolds number. This was a wonderful project for me because I could apply new knowledge gained in the courses that I was taking to develop the analytical background needed to prove the theory of the idea, while using my design and project management experience to bring it to fruition. With the consent of Dr. MacPhail and John Ruptash, I was able to use this particular project for my master's thesis.

With increasing numbers of graduate students in Canadian universities, the National Research Council established a program of hiring for the summer periods. For several years students were assigned to work under my tutelage. Through them I learned about the happenings at various universities and about the satisfaction gained by a teacher imparting new knowledge or increasing the understanding of knowledge already implanted. My taking evening graduate courses culminated in 1962 when I attended the Spring Convocation at McGill University to receive a diploma in engineering heat transfer and in 1963 the Spring Convocation at Carleton University to receive a master's degree in aeronautical engineering, the first graduate engineering degree ever awarded by Carleton.

For some time I had the feeling that NRC was stagnating. Other federal agencies and departments had become research conscious and had begun programs that lessened the Council's importance. In particular, the Defence Research Board became a major player. The growing research activities at the Canadian universities also detracted from NRC, and the political attractiveness of regionalism caused some dispersal of NRC's laboratories. These developments and the positive feelings generated by association with summer graduate students, along with encouragement from John Ruptash, caused me to think about switching to university teaching. One day I noticed an advertisement in the Journal of the Engineering Institute of Canada seeking candidates for a position in the Department of Mechanical Engineering at Queen's.

Back to Queen's

With a multitude of arrangements accomplished, we moved from Ottawa to Kingston in August 1963, and I began a trial session at Queen's after obtaining a leave of absence from NRC. Hugh Conn had just become Dean of Applied Science and Bill Gilbert took his place as Head of the Department of Mechanical Engineering. They made the recommendation to Principal Alex Corry that I be appointed to an associate professsorship beginning on the first of September. Professor Gilbert allocated some discretionary funds so that I could spend most of August preparing the courses that I was to teach and getting oriented to my new surroundings. My courses, in the area of thermodynamics were to be taught in separate classes of mechanical, civil and chemical engineering students. That first session was devoted to those courses and the associated labs. No time was left for research. Even established members of the Department didn't have time for research during the teaching periods. When undergraduates were away on their summer jobs and graduate students were finished with their courses was the time for research. With final exams marked, departmental and faculty meetings finished, I returned to NRC for the summer to continue work on projects at the Low Temperature Lab.

I enjoyed that first session at Queen's, and especially the interaction with students. When Professor Gilbert proposed a tenured appointment I accepted, but kept my options open by applying and receiving a second leave-of-absence from NRC. By the end of the second session at Queeen's I resigned from NRC and cast my lot with Queen's. The research activities at that time were:

Two phase flow	-	Professsor Gilbert
Free piston engine	-	Professor Campbell
Manufacturing	-	Professor Rice
Controls	-	Professor Corneil

My first thought about research was to continue development and use of the cryogenic impeller test rig. My request to Dr. MacPhail at NRC to have the rig transferred to Queen's was refused. So I undertook the development and testing of an aircraft icing detector based on the heat insulating nature of an ice accumulation on the detecting probe. My first graduate student and I utilized a small refrigeration plant in the thermodynamics lab to set up an icing test facility.

My aircraft icing knowledge brought me into contact with Rosemount Engineering of Minneapolis when the head of the firm, Frank Werner, asked me to consult for them on the icing protection of one of the instruments that they were developing. Rosemount was for me the model of a successful technology company having its origins in university research. Frank Werner developed an aircraft total head temperature measuring instrument at the University of Minnesota and then with two or three associates put it into marketable form as a 'home basement' project. With encouragement from the U.S. Air Force he, with friends, set up Rosemount Engineering. My first contact with Frank Werner came at NRC when he brought their total head temperature instrument for testing in icing conditions. The temperature sensor in this instrument was based on the temperature sensitivity of platinum. Platinum resistance thermometry became the springboard for Rosemount's growth. When I visited in 1965 there were about a thousand employees producing instrumentation for military and industrial use. When my initial consulting was finished, Frank asked me to consider spending a month with them teaching the technology of aircraft icing and assisting in the evaluation of a number of inventions submitted to Rosemount for possible purchase.

My month with Rosemount gave me the experience of working in an uplifting environment of a company at the leading edge in its field, its strength dependent on engineering research, and with a work force entirely devoted to its success. At that time Rosemount was very much Frank Werner's baby. He and his secretary seemed to me to run the company. The Rosemount history is typical of companies founded by talented, hard driving, completely devoted entrepreneurs. Here's the sequence: entrepreneur takes an idea and develops a marketable product that is successful and makes money; company's founded; he is chief executive and majority shareholder; new products are conceived and shares sold to finance development; company grows with need for accountants and lawyers; more capital is required; founder remains principal shareholder but not majority shareholder; founder wants to plow profits back into research, development and expansion; new and some of the original shareholders want greater share of profits; founder is dissillusioned and eventually withdraws from management but retains substantial share interest; founder sells some shares to secure resources for development of new ideas to satisfy his entrepreneurial cravings; company's research and development efforts stagnate because researchers grow older, less enthusiastic and new talent not acquired; company remains profitable but ceases to grow; shareholders become restless and company passes into hands of larger company; founder has another small company under way but it never becomes the success of his original effort.

In addition to teaching about aircraft icing and evaluating inventions, I worked with Frank Werner to set up a program of part time studies for Rosemount employees at the University of Minnesota. This activity put me in contact with two or three UofM professors and an exposure to the climate of that University. It remains in my memory that they were suffering with a Dean who spent much time preparing a weekly comic strip that had a scientific and engineering flavour. They thought the comic strip served a useful purpose but regretted his neglect of administrative chores.

At the end of my month at Rosemount, Frank Werner proposed that I come to work for the company. After considerable thought I decided not to take up the offer. Even though I could claim relationship with Benjamin Rush, a signer of the Declaration of Independence, my Canadian roots were a valued part of me that I did not want to lose. So I returned to Queen's with an augmented sense of my worth as an engineering researcher.

During my first few years at Queen's, the National Science Foundation in the United States made participation in its program of summer institutes available to Canadian engineering teachers. Each institute had a particular focus and typically consisted of two courses for which graduate credits were offered, plus seminars, and visits to university labs and local industries. I participated in two of these, a ten-week program at the University of Arizona in Tucson devoted to supersonic aerodynamics and orbital flight mechanics, and a ten-week program at the University of Colorado in Boulder devoted to cryogenic engineering and solid state physics. Each of these programs was attended by 25 to 30 engineering teachers, one or two from Canada and the rest from all over the United States. The coming together of a group with common interests but diverse backgrounds gave me a wonderful overview of the nature of North American engineering schools. Two or three participants at each of these institutes became special friends with whom to share time and explore the campuses and environs. During the two summers following the NSF Institutes, I attended short courses at the Massachusetts Institute of Technology and the University of California at Los Angeles. At MIT the topic was modern developments in heat transfer and, at UCLA, computational methods in heat transfer.

A significant difference between American and Canadian financial compensation for engineering teachers came to my attention during those summers. Canadian university engineering teachers are paid on an annual basis with a monthly salary every month of the year, whereas common practice at American universities was not to pay during the summer months when undergraduate students were off campus. For their summer income, Americans depended on research grants, contracts and summertime teaching such as occurred at the NSF Summer Institutes. As a consequence, contracts and grants from government agencies and industry encouraged participation in engineering research. My contacts with some American engineering teachers continued when I became aware of the American Society for Engineering Education and began attending annual and district meetings.

NRC was the granting agency for scientific and engineering research during my first years at Queen's. Committees had been established to promote and fund research in specific fields. Members of these committees were drawn from NRC, industries and universities. Their function was to review grant applications, make awards and, in some instances, to suggest specific projects. Grants were made for equipment purchase or rental, supplies etc., but not for the financial support of graduate students, who depended on scholarships and undergraduate laboratory supervision for their sources of income. Because of my NRC background and contacts, I became involved with research of ducted air flow. The projects undertaken stemmed from ideas gained from the Committee on Internal Aerodynamics, of which I was a member.

Then - in 1978 - the Natural Sciences and Engineering Research Council (NSERC) was established to take over the funding of university research. A strong emphasis of NSERC was that grant applications should be peer evaluated. University teachers soon dominated the evaluation committees. With the passing of a few years this, in my opinion, had a marked influence on the nature of research at Canadian engineering schools because scientific research became dominant over engineering research. The reason for this was the propensity of peer evaluators to regard the number of published papers of applicants as highly significant in judging their worth as researchers.

I think of engineering researchers in three categories: the entrepreneur, who undertakes research in order to accomplish the primary objective of producing a prototype of a marketable product or service; the facilitator, who produces engineering data to be applied to the development of a particular product or service; and the expert, who extends basic knowledge in a particular field so that developers have design information available.

The entrepreneur does not, in general, produce many published papers. He or she regards that activity as a distraction from getting things done. The facilitator may publish lots of reports giving valuable information for others to use, but probably these would not be accepted for publication in the 'right' journals. The expert produces papers acceptable for publishing. His or her activities may be similar to those of a scientific researcher, but differ because of interest in using the knowledge gained for marketable purposes. Because he or she cannot choose to ignore significant variables in the design of experiments, he or she may be an infrequent paper producer compared to the researcher who can claim to satisfy his curiosity while ignoring a troublesome variable. For these reasons, engineering researchers tend to be poor producers of published papers. Consequently, many university engineering teachers deliberately choose scientific over engineering research because they know that the published paper yield is substantially greater and the chance for NSERC grants enhanced. The engineering researcher fares better with contracts for research coming from industry and government departments wanting specific projects.

Southampton and NRC

Fluid mechanics, thermodynamics, heat transfer, and numerical methods for computing were the main subjects of my teaching, and I became interested in district heating, co-generation and alternative energy. Several months of a sabbatical leave in 1973 were spent at the Aeronautical Department of the University of Southampton in England, primarily because its reputation for research in fluid mechanics and the aerodynamics and hydrodynamics of sailing vessels. When I arrived I spent a few days visiting the labs and meeting the staff. Then I was assigned a desk in a room of a house somewhat off campus. Feeling isolated, I spent lots of time thinking and writing. Taking lunch at the Faculty Club gave me the 'flavour' of this University and attendance at a series of talks by staff members and invited lecturers gave me an overview of their research interests. One talk was of particular interest to me because it was about the collaboration of one of the departmental staff with researchers at NASA in the development of a supersonic test facility operating at cryogenic temperatures. In the talk he referred to pioneering work that had been done at an unlikely place - the National Research Council of Canada. He was quite surprised when I introduced myself after the lecture. Thereafter my status as a visitor to the Department substantially improved. Even the Department Head seemed more aware of my presence. But his autocratic style of management left him little time for casual discourse. He, the originator of most activities and research in the Department, not the facilitator, was a busy man.

The last two months of my sabbatical were spent in Ottawa at the Division of Building Research of NRC where I became familiar with their alternative energy activities, particularly solar energy, for which several areas for engineering research became apparent. The elaborate controls needed in a solar hot water heating system to avoid heat losses when clouds obscure the sun stirred my interest in finding a better solution to the control problem.

Solar Energy

When I returned to Queen's, solar energy became my primary research interest. One day, when preparing lecture notes on steam plants, it occurred to me that a steam generation-condensation combination could be used in a solar collector for heating liquids. Solar rays shining on a blackened surface attached to a steam generator cause a liquid to boil; the resulting vapour rises to a condensing chamber through which passes a pipe carrying a fluid to be heated; as long as the fluid is colder than the rising vapour, it is warmed by the vapour condensing on the outer surface of the pipe; if the sun stops shining, boiling ceases and the vapour cools; little heat is lost from the pipe to the vapour because of the high thermal resistance of the vapour. The diode nature of this two phase heat transfer process eliminates the need for a sensitive control to prevent loss of energy during cloudy periods. Tests with a prototype collector demonstrated its practicality and several unique characteristics. Then began the process of patenting, devising a trademark, and marketing the invention. Good fortune came when Jim Ramsden of Norsun Solar decided to take up the patent license for the Q'Sol solar collector. For a few years a fruitful collaboration yielded a commercial product with sales of several thousand collectors, substantial royalties to the University, several engineering research contracts with the federal Department of Energy, Mines and Resources, a number of research projects for graduate students under my supervision, and a well-equipped laboratory.

The Division of Energy was created at NRC to coordinate the energy activities of several other divisions. At the Division of Building Research, a Solar Calorimetry Laboratory had been established by Steve Harrison. Its function was to develop means for measuring the performance of solar devices such as hot water heating solar collectors and installed solar hot water heating systems. When NRC received cuts to its budget in the early 1980s, a decision was made to close the Energy Division and some of the labs engaged in energy projects. The Solar Calorimetry Lab was one of these. As soon as this was known at Queen's, Norman Kerr, then Head of Mechanical Engineering, and I began exploring the possibility of transferring this Lab to Queen's. An arrangement was worked out to 'lend' the considerable amount of equipment to Queen's and for Steve Harrison to be employed by Queen's and engage in part-time graduate studies towards a doctorate. This was a gratifying development for me because of my approaching retirement.

To accommodate the equipment from NRC, a small shed attached to the south wall of the main solar lab and containing a roof-enclosed thermal diode solar hot water heater was dismantled to make room for an elaborate solar collector test platform. My research projects, mostly to do with two phase heat transfer, became laboratory set-ups where incoming solar energy was simulated by electric heaters. By the time of my formal retirement in 1986 the winds of change swept through the Solar Lab and my penchant for entrepreneurial engineering research was supplanted by the facilitating and knowledge extending activities associated with solar calorimetry.

Towards Retirement

Retirement for university professors doesn't usually begin with a step change from a full load of academic duties - undergraduate teaching, graduate teaching and supervision, research activities and administrative chores - to no involvement. My choice was to teach two graduate courses, supervise some graduate students, and continue research projects. I also contracted with the Canadian Solar Industries Association to provide technical and scientific services for the solar energy development program of the federal Department of Energy, Mines and Resources. This meant spending a day or two each week in Ottawa reviewing proposals for solar research, appraising progress on contracts already in effect, and participating in meetings with contractors and professional societies. I was provided with a desk in a room located in a large multi-floored building with several floors leased by EMR. The offices on the floor where I worked were occupied by technical officers, secretaries, assistants etc. devoted to looking after the Department's interests in a number of fields, such as wind energy, photovoltaics, small scale hydro and solar energy.

The opportunity to observe and to participate in the contracting process was of special interest to me because engineering research for alternative energies depended almost entirely on contracts let by EMR. The Department also provided subsidies to the solar industry. Thus in a very real way the solar industry in Canada depended on decisions made in that Department. The Minister at that time was Marcel Masse, who is now a member of the Quebec National Assembly and a prominent advocate of a sovereign Quebec. The Department was very conscious of its obligations to French Canadians. So I worked with a mix of people coming from anglophone and francophone backgrounds. Those with a French-Canadian background and a few others were bilingual. But most of the non-French-Canadians were unilingual with two or three struggling to become bilingual by engaging in the programs provided by the federal government. Each of the alternative energies was looked after by one or two technical officers who made contract recommendations. One of these was a young French-Canadian woman engineer who came from a rural Quebec family. Some brothers and she were of the generation that had left the farm to become professionals. How things change over the course of a career! When I first started in government service at NRC there were no French-Canadian or women engineers in the Mechanical Division. Most of our janitors and some of our administrative and lab assistants were bilingual French-Canadians. Later on, a summer graduate student from Ecole Polytechnique in Montreal, wanting to gain experience in an English-speaking environment spent a summer under my tutelage. It wasn't until the early seventies that women began studies in mechanical engineering at Queen's.

Over the two and a half year period of my attendance at the EMR offices I gradually became familiar with all of the current and completed contract research in solar energy. Judging by the submitted reports, I concluded that most of the research was of poor quality and that many contractors were token researchers seeking financial support for operating purposes. The awarding of contracts seemed to me to be based more often on promotional skills and political considerations rather than technical merit. My role at EMR ended when alternative energy began to be down-played.

By 1990 my activities at Queen's and my engineering research career had petered out. So I took up other interests: keeping the accounts for a small business corporation, crafting small wooden boxes with inlay, and writing creatively.

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