GENESIS OF NRC LABORATORIES

W. H. COOK

This pleasant gathering forms part of ARL's 25th anniversary celebrations. Such gatherings reflect a pride of accomplishment, and I would like to congratulate the staff for planning and executing such an event. As one who served only as a sort of midwife at the birth of the laboratory, it has been gratifying to witness growth and achievements of the offspring. Halifax must certainly provide a healthy environment for intellectual endeavor - the phenomenal growth of Dalhousie University in the last quarter century confirms this fact. On the personal side, this gathering has provided an opportinuty to meet old friends and associates and, on behalf of my wife and myself, may I thank you for your kind invitation.

But there was a catch in this. I was asked to say something about the histroy of ARL to an audience that had a full day and a full meal. While I accepted this challenge with enthusiasm, it soon became evident that the factual and detailed history of ARL had been written from several vantage points by those who made it. My personal concern with ARL was during its gestation period, viewed largely from a distance and now dimmed by time. I have, therefore, chosen to include something about the genesis of other NRC laboratories, for all had a different beginning, and are products of their parentage, environment, and time.

Science had some important ancestors going back to prehistoric times. We don't know when to celebrate the anniversary of the discovery of the wheel, but it did require creative imagination, and facilitated human progress. The experimental method must have been developed before early man could recover metals and chip his way out of the Stone Age. Then a much more abstract concept brought an even more creative advance. By devising symbols and arranging them in a consistent order, man gained a written language and mathematics. Now he could record his observations and experience for future generations.

By this time it was evident that there was an order in nature extending from the life cycles of living things to the orbiting of the planets. Agriculture and astronomy were about the earliest scientific disciplines. The reasons for this order were not evident, so man again used his imagination and developed mythologies to provide an explanation. Astronomy is one of the oldest sciences, but in Copernicus' day astrology was at least as important. A cynic, seeing the astrology columns in our daily papers, might say this has not changed in the past 500 years!

But a few believed that the order in nature could be explained, and undertook the acquisition of basic knowledge by experimentation to obtain facts. Progress was slow initially, for the experimental method requires both time and facilities. University staff had some time but few resources to pursue such a hobby. Gradually they acquired sufficient new basic knowledge to add Natural Philosophy to the curriculum. Meanwhile, government found that this basic knowledge could be developed for military purposes and civilian advancement. Industry found these developments to be profitable. As a result, both government and industry began to support research wherever it could be done and eventually to establish their own research laboratories.

The events that precede the birth of a government laboratory reflect most of the stages involved in human reproduction. Conception takes place at a mating of the scientific and political communities. It starts with individuals, but the exchanges that occur in the barrooms and bedrooms of the nation are seldom recorded. The initial advances may be made by either party; but the only indication of sex is that the scientist is always for motherhood and the politician is often fickle. Conception is followed by a highly variable gestation period and the risk of a miscarriage. Birth is accompanied by labor pains and the screams of assorted midwives from every corner. Even when healthy at birth, the infant laboratory may be stunted by malnutrition or follow a wayward course into limbo. All of these events are reflected in one or more of the family of NRC laboratories.

In my opinion the formation of NRC in 1916 was promoted by politicians. Canadian scientists and industrialists were too few and too divided in opinion to have had much impact. Britain had started DSIR, and it was good political wisdom for Canada to take comparable action to enhance its scientific capabilities. The initial action was to appoint a Privy Council Committee for Scientific and Industrial Research, that in turn appointed an Advisory Committee that became known as NRC.

This Advisory Committee had only a limited mandate, to promote scientific research, and only limited funds to support such activity in existing institutions. Two facts were evident from the outset: first, there was little to promote or support a scientific capability which would have to be created; second, a central laboratory would be required to undertake major problems. The first need was undertaken by establishing graduate scholarships in science, and by supporting Associate Committees of professors who directed approved projects in their spare time without compensation. These were highly successful ventures but initially the scientific capability was so restricted that even the limited funds could not be fully used.

While it was evident that major projects could not be tackled without a central laboratory and full-time staff, such a plan would require a new Act to provide NRC with a broader mandate. It took five years for Council to develop an acceptable plan and create a receptive political climate. When it seemed certain that these requirements would be met, the first chairman of NRC, Dr A. B. Macallum, resigned. The required Bill did pass the Commons but was defeated in the Senate. The recorded reason was that expenditures on new things should not be made following a major war. The suggested reason is that the Senate was influenced by the mandarins of the bureaucracy who wanted NRC to be disbanded as a wartime agency, and thus remove competition for funds for Departmental science.

This miscarriage threatened the life of NRC. The Council did not resign - that would have been an admission of defeat - but continued to discharge its limited mandate. But building up a scientific capability that could not be used was a dead end, and Council had three chairmen in the next two years.

The NRC was saved from extinction by Dr H. M. Tory, a Nova Scotian from Guysborough County. During his career he established and presided over two Canadian universities in addition to NRC. He had a firm conviction about the value of science, had the sympathy of scientists and educators, and the ability to convince the public and confront politicians. When he heard of the NRC debacle, he began a crusade, with speeches at universities, Canadian Clubs, and other formal and informal gatherings. In 1923 he was appointed a member of NRC and within a few months became its fifth chairman. In less than five years he obtained a new Act that gave NRC considerable freedom of action, a central laboratory and staff, with himself as its first fulltime President.

Then he was confronted by two new problems. The economic depression struck about the time the new building was started and slowed construction. Only the few staff that could be accommodated in temporary quarters had been employed before all appointments were cancelled. Then he had a personal problem. In an earlier day he had won a confrontation with a budding politician from Calgary who wanted the University of Alberta in his city. In 1930 the Honorable R. B. Bennett became Prime Minister and he thwarted Tory's plans at every opportunity. For example, the Prime Minister assigned the excess space in the new building to other agencies and used the funds, obtained by NRC for equipment, for the purchase of office furnishings for these agencies. It was to combat this influence that I was hastily transferred from temporary quarters at the University of Alberta early in 1932. My new responsibilities were to place orders for laboratory benches and services, including such major installations as a greenhouse and a battery of refrigerated rooms for the Division of Biology and Agriculture. It was the first time I had acted as midwife at the birth of a new laboratory, and it was a long and difficult period of labor. The combination of limited funds, red tape, and my inexperience extended these labor pains over the next three years.

In 1935 General A. G. L. McNaughton became President, the personal animosities disappeared, a modest growth in staff and facilities took place, but in 1939 the total NRC funds for scholarships, grants and the laboratories were still short of a million dollars annually.

The outbreak of war brought Dr C. J. Mackenzie, a graduate of Dalhousie University, to the Presidency of NRC. The staff and resources expanded about tenfold but this was expected to be for the duration only. However, Mackenzie inspired confidence at every level and under his leadership the Council's record of accomplishment justified the retention of its war-born growth. The NRC laboratories were now large enough to have a major impact on Canadian science. Decentralization was another major Mackenzie contribution; both regional laboratories were established during his regime.

The concept of a Prairie Regional Laboratory originated with the scientific community in response to public demand. The periodic unwieldy surpluses of wheat in North America had depressed prices. The United States had established four laboratories to find industrial uses for these surpluses, and the interested public were demanding similar action in Canada. In 1943 the NRC convened a conference of scientists from the prairie universities, government and industry, to determine the needs, the new projects to be undertaken, and to coordinate these with the responsibilities of existing laboratories. The outcome was a unanimous request for NRC to start a regional laboratory to undertake the new projects. This recommendation was approved in principle by NRC with construction to start in the immediate postwar period.

When Dr Mackenzie asked me to prepare a staff establishment and building for the new laboratory, there were adequate guidelines. The conference had proposed definite projects and war time shortages had required related studies that provided an experienced staff. These detailed plans were accepted by Council and approved by Treasury in February 1945, when the end of the war was in sight. The Council appointed its own architect, construction started in late 1945, the building was occupied in 1947, and officially opened in 1948. Drs A. C. Neish and F. J. Simpson were among the small band that transferred from Ottawa to give the new laboratory a flying start. The concept of PRL originated with the scientific community and gained both public and political support.

The initiative for establishing ARL originated on a more political level. Following the decision to establish PRL, there were speculations and rumors about NRC's starting laboratories in other regions but these gradually died down. Then following the Council meeting in May 1947, I was surprised to read in the Ottawa papers that NRC had announced it would establish a Maritime regional laboratory on a site provided by Dalhousie University. I had not heard of any new Maritime problem requiring attention, and there had not been a conference to formulate plans for the proposed laboratory or coordinate these with other federal laboratories in the region.

The conception of this laboratory took place in the politcal backrooms and remains unrecorded, but I gained a bit of the background from later conversations. When the clamor for more NRC regional laboratories was at its peak, the Honorable C. D. Howe had apparently conceived the notion of having all existing federal laboratories in a region consolidated in a building administered by NRC. Details of this plan are not available, but it would have led to NRC's becoming an administrative rather than a scientific institution.

About this time President A. E. Kerr was conducting a drive for funds for Dalhousie University, and he evidently felt it would be beneficial to have NRC announce that it would build a laboratory on the campus. He therefore offered NRC a site for a building - an over-generous offer considering the size of the Dalhousie campus at that time.

Naturally the scientific community was prepared to co-operate, and I suspect it was Dr C. J. Mackenzie who harmonized these diverse thoughts and desires into the concept of ARL. Shortly after the official announcement, he referred to my experience with PRL, and asked me to prepare a building and staff establishment for the new laboratory. I had doubts about this assignment; with one exception, I had no initmate knowledge of Maritime problems, and there had not been a conference to provide a directive on the scale of activities or indicate specific projects, as there had been for PRL. The only guide-line was that the laboratory was to serve biology, chemistry, and physics. Much later I learned that the original approval in principle contemplated a laboratory about half the size I eventually recommended and had approved.

Dr Mackenzie's persuasive power overcame my reluctance. He said the Public Works Department would be responsible for the construction of the building -all I had to do was to decide on the size and functional layout. As soon as a staff establishment had been approved, a director would be appointed and he could decide on the projects. It seemed easy but one has to have some idea of the size of building and services required. A war-time emergency had acquainted me with one problem - seaweed utilization - and it was soon evident that this justified extension beyond our strictly laboratory effort. The requirements for such a project could be estimated and multiplied by three to take care of chemistry and physics when a director was appointed.

There were other problems. The initial plans contemplated the purchase of such services as steam and hot water from the University, as at PRL, but President Kerr insisted that the connections between the laboratory and Dalhousie University had to be kept on an intellectual level. By the end of 1947 the Public Works Department had completed the preliminary drawings and appointed an architect. To put it mildly, this architect evidently had no experience in the construction of laboratory buildings. He was in no hurry, seldom kept appointments, and never asked questions. The location of the building was decided while I was in the South Pacific; the new location required more expensive excavation and stone facing; costs now exceeded estimates and the size of the structure had to be reduced. On my return it was too late to remedy some of the more serious shortcomings arising from this change, such as some areas being below sewer level.

Construction started in late 1949, two years after the PWD had been provided with the initial plans. Dr E. Gordon Young was appointed Director in 1950, and the building completed shortly before the official opening in 1952. In 1951 I accompanied Dr Mackenzie to tidy up some details that would permit the staff to evacuate Dalhousie University premises.

Turning now to seaweed research. A project initiated in the Ottawa laboratories to meet a war emergency had provided my introduction to a Maritime problem. After Pearl Harbor, the usual sources of rubber and agar were cut off, and the government froze all available supplies for high priority purposes. The main user of agar was the food industry, but bacteriological and medical uses had priority. We were then asked to find a substitute for use in the food industry. The carrageenan extracted from Irish moss (<u>Chondrus crispus</u>) seemed an obvious choice for several reasons: (1) it set to a jelly in the presence of potassium salts; (2) it was used by the food industry for suspending chocolate in milk; (3) the seaweed was available along the Maritime coast; (4) this source had been developed by U.S. industry since European sources had been cut off.

The problem was not as simple as it seemed. From 1 to 2% of carrageenan was required to produce a satisfactory jelly for canned meat, and at this concentration most of the commercial extracts of that time, and our initial laboratory preparations, imparted an undesirable color or flavor. The suspension of chocolate in milk required only one-tenth this concentration, and the presence of chocolate masked both color and flavor. We were finally able to develop a suitable method of preparation and a formulation with potassium salts that was acceptable, although different extracts varied by a factor of two or more in their gelling and suspending power. This variability was only partly dependent on polymer size, as indicated by viscosity measurements.

In 1945 we resumed our work on carrageenan but with a different objective. There was a risk that this war-born export trade might be lost if cheaper suspending agents became available or if European sources of Irish moss were cheaper. It was soon found that carrageenan reacted with the milk components to produce a much higher viscosity increment than less costly suspending agents. Yet Irish moss from European sources might be cheaper but, if the extracts from Canadian moss were of higher quality, requiring only half as much to suspend chocolate, they should be worth twice as much. A better understanding of the variable suspending power might provide a clue to quality for commercial purposes.

At that time the chemical structure of carrageenan was poorly understood and there was one suggestion in the literature that it might contain two components. If these extracts contained varying proportions of two or more components differing in suspending power, the variable performance could be explained and possibly reduced. Examination by a variety of chemical and physical methods showed that the extracts, in addition to the usual polydispersity, had properties indicative of two or more components. However, we were unable to distinguish these components analytically or separate them preparatively. Later, when we completed the installation of an analytical ultracentrifuge late one afternoon, a residual carrageenan solution was the most convenient test material. The quite unexpected separation of two components provided an analytical tool to monitor separation techniques.

Eventually Dr D. B. Smith separated the two components by differential precipitation from dilute solutions with potassium salts. During this study the two components had been designated α- and β-carrageenan, symbols that have a special meaning in polysaccharide chemistry. To avoid confusion, the symbols had to be changed for publication. When Smith asked me what we should call them, I suggested kappa for the potassium-sensitive fraction, and the next Greek letter lambda, for the other.

At this stage the project was transferrred to the new ARL, where subsequently the staff have inscribed the terms κ- and λ-carrageenan in the scientific literature by characterizing the components and relating them to the haploid and diploid phases of Irish moss.

But my visits to the Maritimes in 1948 showed that this seaweed problem went beyond the laboratory. There was concern about the extent of the beds, the rate of regeneration, drying methods, and the sanitary qualities of the product. The Nova Scotia Research Foundation, Dalhousie University, the two provincial governments, and industry, were all engaged on these problems. It appeared that the work of these diverse groups could be coordinated and enhanced by forming an NRC Associate Committee on Seaweed Research. Such action would have to be preceded by a conference to obtain the opinion of those concerned. With the assistance of Dr E. G. Young, such a conference was organized in Halifax in the autumn of 1948. Most of those attending came from Maritime institutions, but the representatives of U.S. industry, and Dr Neville Woodward, Director of the Scottish Seaweed Research Institute, did give it an international flavor. The meeting not only gave rise to an NRC committee, but it was also considered the first International Symposium on Seaweeds, that has continued to meet periodically ever since, the most recent being in August of this year.

When Dr E. W. R. Steacie became President of NRC in 1952, his first official act was to attend the formal opening of the ARL. At that time about a third of the staff had been employed, and they were all working on biologically-orientated problems, the majority on the seaweeds project. On his return, he consulted me as to how the establishment had been arrived at, and I told him it had been obtained by formula. He expressed the view that the physical sciences should be represented in a regional laboratory, and said a project of this sort should be initiated to attract personnel. He imposed other requirements. The new project should be of regional interest and scientific importance, and I had no suggestions to offer. Later, Dr Steacie told me he had conceived of a suitable problem, high temperature metallurgy. DOSCO'S problems gave it regional interest and Sir Charles Goodeve had assured him of its scientific importance. With this decision he invited Dr C. R. Masson, who was already on staff, to head a team on high temperature metallurgy, and he was sent to BISRA for most of a year to learn the tricks of the trade. This action ensured that the physical sciences were adequately represented in the ARL.

I shall say little about the recent history of ARL. Dr Young brought the laboratory from infancy to maturity. Many new projects had been initiated and the building was completely filled when he retired in 1962. He remained active as a guest worker and completed his book, History of Canadian Biochemistry, before his death a year ago.

Dr A. C. Neish succeeded him and it was time for new growth. Neish had an addition built on the laboratory, introduced new projects including one in which he had a strong personal interest - the culture of marine plants. Neish spent his entire scientific career with NRC, first in Ottawa, then PRL, and finally in ARL. His achievements won him many honors, and the Council appointed him to the prestigious position of Distinguished Scientist, of which there are only two. The other is Dr Gerhard Herzberg. His untimely death was a sad loss to his family, his many friends, and to Canadian science. The mantle of leadership then fell to Dr F. J. Simpson, a long-time associate of Dr Neish at Ottawa and PRL, whom he had appointed as his Assistant Director at ARL.

Science has now reached an age of uncertainty. It started about 10 years ago when science policy became an international game and the Canadian Senate fielded a team. Their reports were critical rather than constructive. Perhaps they recognized that their overall policy pronouncements were a bit too vague for establishing scientific priorities, so they recommended new bureaucrasies to control and direct science. Rumor has it that they may even name a team to win the next Nobel Prize.

Then came the energy crisis that fanned the flames of inflation, and effectively reduced the budget for science. It also produced a pragmatic science policy, as laboratories everywhere increased their studies on wind power, solar energy, and the fusion reactor.

Meanwhile, society began to affect science policy. Initially they had accepted such scientific advances as antibiotics, DDT, and nuclear energy, with a sort of "Gee Whiz" acclaim. Alerted by some segments of the scientific community and by what they saw, the public began to question the long-term effects of some of these developments. First, DDT was banned because of its long-term effects on the environment. Then, the safety of nuclear power plants was questioned, and their construction delayed. In fact, extensive ecological studies were demanded before any large-scale developments were undertaken. Finally, the citizens of Cambridge, Mass. obtained a temporary ban on recombitant

DNA experiments in their district even before they had been undertaken. Scientific opinion may differ on the wisdom of some of these decisions, but there can be little doubt that the public have entered the science policy forum. They are likely to demand more rather than less research in applied fields that are related to health and the environment.

It is easier to forecast the changes that are likely to affect science policy than to predict scientific advances. Major scientific discoveries that change the whole course of science are unpredictable. In the last 25 years, science found the key to the genetic code, Sputnik opened the Space Age, and the old applied field of plant breeding produced the Green revolution. None of these major scientific advances could have been predicted when ARL was opened. The breakthroughs of the next 25 years cannot be predicted, but it is safe to say that enormous sums will be spent in studies on recombitant DNA, plant improvement, and the fusion reactor. The Green Revolution is close to one of ARL's interests. Nova Scotian bays are highly productive in plant material, and Neish's vision of a marine aquaculture may yet become a reality. If the animal species that reside in these bays could be encouraged to grow twice as fast, we might have another breakthrough that would produce a Protein Revolution.

In the international league of scientific research, a single laboratory cannot play in all the games. The games ARL has played during its first 25 years have brought the individual players a distinguished reputation and started the laboratory along the pathway toward excellence. May the standards you have established be continued through future anniversaries.