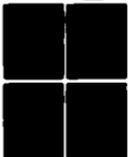
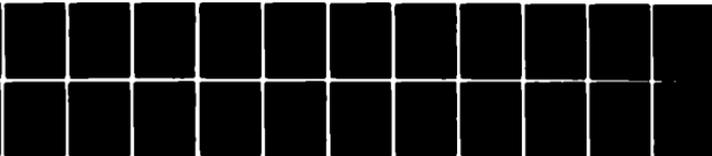


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RESEARCH AND DEVELOPMENT BRANCH

DEPARTMENT OF NATIONAL DEFENCE
CANADA

6 **EARLY DEFENCE ATOMIC RESEARCH IN CANADA**
With an Introduction on the Genesis of Nuclear Energy,

by

10 A.K. Longair

14 CRAD ~~SECRET~~ 4/79

Approved



Edward J. Bobyn

CHIEF RESEARCH AND DEVELOPMENT

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ABSTRACT

This report is a history of defence atomic research in Canada until 1967. It is a highly personal account as seen from the perspective of the author, A.K. Longair, who was the Director for Atomic Research in the Defence Research Board until 1967. An attempt is made to place events that took place in Canada in their context with respect to developments in other countries. An index of Canadian personnel who were active in atomic defence in Canada is included.

RÉSUMÉ

Le présent rapport trace l'historique de la recherche nucléaire pour la défense au Canada jusqu'en 1967. Les faits sont présentés dans la perspective très personnelle de l'auteur, M. A.K. Longair, directeur de la recherche nucléaire au Conseil de recherches pour la défense jusqu'en 1967, qui a tenté de situer les faits qui se sont déroulés au Canada par rapport aux progrès enregistrés dans d'autres pays. Le document renferme une liste des Canadiens qui ont pris une part active dans la recherche nucléaire pour la défense.

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PREFACE

This report is an extension and expansion of a seminar I gave at the Defence Research Establishment Ottawa on 1 May 1978. It is a history of Defence Atomic Research in Canada as seen from the perspective of my role as the Director for Atomic Research in the Defence Research Board until 1967. As such it is a highly personal account in which I have endeavoured to place the events that took place in Canada in their context with respect to developments in other countries. Throughout this report scientific terminology has been kept to a minimum so that the account is historical and in some places anecdotal.

The description of Canada's involvement in Defence Atomic Research has been introduced by a section on the genesis of nuclear fission. This coincided with my early career as a physicist and I have drawn on my recollections of that period both to impart some of the excitement that gripped all who became involved with this new science and to provide the background for Canadian participation in this area.

For the history of nuclear fission I have leaned heavily on Margaret Gowing's "Britain and Atomic Energy 1939-1945" (1) especially the introductory chapter "A Glance at Prehistory". This chapter was written by the late Kenneth Jay, a fine scientist and an amiable colleague, to whom it is perhaps not too late to pay tribute.

All of this history since I came to Canada and some of the earlier part is written from memory, a fallible thing. So it may contain errors and is certainly not comprehensive. Accordingly, in the old accounting style, I must mark it E. & O.E.-errors and omissions excepted!

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AVANT-PROPOS

Le présent rapport se veut le prolongement d'une conférence que je prononçais au Centre de recherches pour la défense (Ottawa) 1er mai 1978. C'est un historique des activités de recherche nucléaire pour la défense au Canada, envisagées dans la perspective du poste que j'ai occupé jusqu'en 1967, soit celui de directeur de la recherche nucléaire au Conseil de recherches pour la défense. Il s'agit donc d'un exposé très personnel des faits qui se sont déroulés au Canada dans ce domaine et que j'ai essayé de situer par rapport aux progrès enregistrés dans d'autres pays. Les termes scientifiques ont été réduits au minimum; le lecteur trouvera donc là un exposé historique et, dans certains cas, anecdotique.

La participation du Canada dans la recherche nucléaire pour la défense est présentée dans le cadre d'une section sur la genèse de la fission nucléaire. Cela remonte au début de ma carrière de physicien et j'ai rassemblé mes souvenirs de cette époque en vue de communiquer un peu de cette espèce de fièvre qui avait envahi alors tous ceux qui étaient engagés dans cette nouvelle science, et constituer un dossier sur la participation du Canada dans ce domaine.

Pour tracer l'historique de la fission nucléaire, je me suis inspiré en grande partie de l'ouvrage de Margaret Gowing: "Britain and Atomic Energy 1939-1945" (1), en particulier le chapitre d'introduction intitulé: "A Glance at Prehistory". Ces lignes sont l'oeuvre d'un sympathique collègue maintenant décédé, M. Kenneth Jay, un homme de science distingué à qui il n'est peut-être pas trop tard pour rendre hommage.

Tous les faits que je décris avant et depuis mon arrivée au Canada sont tirés de ma mémoire. Or, comme chacun sait que la mémoire est faillible, le document peut contenir des erreurs; et il va sans dire qu'il n'est pas complet. Je le présente donc avec la vieille formule de mise en garde: "sauf erreur et omission".

EARLY DEFENCE ATOMIC RESEARCH IN CANADA

Introduction - The Genesis of Nuclear Energy

The final year of the Honours B.Sc. course in Natural Philosophy (Physics to most North Americans) at the University of St. Andrews in Scotland, where I was educated, was in my day spread over four terms. So, to graduate in June 1933 I took my place in that class on 6th April 1932.

"Nature" of 27th February 1932 had contained a letter(2) from James Chadwick at Cambridge suggesting that the penetrating radiation which resulted when paraffin wax was bombarded with high-energy alpha particles from polonium consisted of uncharged particles of approximately the same mass as the proton. Chadwick named this uncharged particle the "neutron". The issue of the same journal for 30th April published another letter(3) from Cambridge, this time from J.D. Cockcroft and E.T.S. Walton describing an experiment in which they had bombarded lithium with high energy protons and produced large numbers of alpha-particles. In our shorthand
$${}^3_7\text{Li} + {}^1_1\text{p} = 2{}^4_2\text{He}$$
 Cockcroft and Walton had "split the atom" and at the same

time verified Einstein's mass-energy equation. In the same year 1932 Anderson in USA identified the positron (just ahead of Blackett in the UK) Harold Urey showed that Hydrogen had an isotope of mass 2 which was named deuterium, while a young man in California named Ernest Lawrence combined magnetic and pulsed electric fields to accelerate protons to 1,200,000 electron volts. When pressed some time later to name his machine he called it the "cyclotron".

With these and other achievements in physics, is it any wonder that A.S. Eve in his life of Lord Rutherford described 1932 as the "annus mirabilis" - the wonderful year? It was indeed and not only in Cambridge. I have said, and will say again, that to be a physicist in the nineteen-thirties and forties was to live in an enchanted world. Experimental results poured out, one theoretical prediction after another was verified and the pieces of the jig-saw began to fit. I am told that we were arrogant: if so, there was some justification for it.

Young physicists today may be interested in what E.V. Appleton told me years later - that Cockcroft and Walton hesitated to try the vital experiment because they had not achieved as high a voltage from their cascade of condensers as they had hoped. The best they were getting was 300-400,000 volts while the potential barrier to the lithium nucleus is well over 1 Mev. However, quantum mechanics said there was a probability of less energetic protons making it and Rutherford encouraged them to try—and it worked. Of course the trick in the Cockcroft-Walton experiment lay not only in the voltage applied but also in the number of protons accelerated, because the probability will increase not only with the voltage applied, but also with the number of shots at the target. It is a

matter of history that they observed the effect with voltages as low as 125,000. Please note that Harold Urey identified deuterium from the optical spectrum. The mass spectrometers of the day could not have separated the very faint D line from the H line, but the mass difference meant that these lines were much more widely separated in the optical spectrum.

The flood gates were now open. In 1934 Curie and Joliot(4) achieved artificial radioactivity by bombarding beryllium, lithium and boron with alpha-particles. Enrico Fermi in Italy realized that the uncharged neutron had the best chance of penetrating any nucleus and he and his collaborators systematically bombarded over 60 elements of which all but 18 yielded radioactive end-products. In the course of these experiments, he found that if the neutrons were slowed down they were more likely to enter the nuclei.

Among the elements he bombarded was uranium and he proved that the resulting radioactivity could not be isotopic with any element between lead and uranium, so he deduced that he had produced minute quantities of elements heavier than uranium and therefore unknown on Earth - element 93 and possibly 94 and 95. Otto Hahn and Lise Meitner in Germany repeated and extended Fermi's experiments and showed that the activity could not be attributed to any element between mercury and uranium. So they concluded, uneasily in the case of the physicist Meitner, that it was transuranic. In 1937 Irene Curie found a 3.5 hour activity in bombarded uranium and concluded, also with misgivings, that it was transuranic. Hahn and Strassman found that this activity followed the chemistry of radium, leading to the unappealing conclusion that the uranium had emitted alpha-particles - a result difficult to accept with the neutron energies involved.

The 3.5 hour cat was now among the pigeons. In their experiments, Hahn and Strassman had used barium as a carrier for the "radium". To make a stronger preparation of this "radium" they now tried to separate it from the barium carrier. They could not. They were therefore forced to the conclusion that the bombardment of uranium by neutrons had produced elements in the middle of the periodic table(5). Before publishing Hahn wrote to Lise Meitner about it; she, forced to leave Germany by Nazi persecution, was spending Christmas with her nephew Otto Frisch in Copenhagen where he was working. They discussed the results in the light of Niels Bohr's 1936 concept(6) which likened the nucleus to a drop of water and they thought that the addition of a neutron to the uranium nucleus would set up a violent instability, that it would split into two almost equal parts and that the binding energy released would be shared between these particles, which should therefore be far more energetic than alpha-particles and produce intense pulses in an ionization chamber. Frisch assembled the necessary apparatus and found the pulses, confirming also their expected energies. He suggested the name "fission" for the new phenomenon, after the biological analogy. He and Meitner wrote to "Nature" on 16 January(7),(8), it appeared in February but in the meantime Bohr had

announced the discovery at a meeting of the American Physical Society in January and the world of physics was on fire.

Why was fission missed for so long after Fermi's experiments? The 3.5 hour activity, after all, must have been present in all the experiments of bombarding uranium with neutrons. It was due to the Achilles heel of all scientists: the idea was contrary to the accepted physical thought of the day. As early as 1934 a German chemist, Ida Noddack(9) in a criticism of Fermi's experiments and conclusions suggested

"heavy nuclei under neutron bombardment might possibly.....
.....fall into several large fragments which are indeed isotopes of known elements but are not neighbours of the irradiated elements"

This idea was so far-out in 1934 that no one took it seriously and it had no influence on subsequent events. In 1938, von Droste in Germany observed very energetic pulses in an ionization chamber, but explained them as alpha-particles; similar pulses were observed in experiments at the Cavendish Laboratory in Cambridge but were attributed to equipment malfunction. Moral: if your results don't fit accepted scientific ideas, pursue them with vigour.

Hahn and Strassman had suggested that neutrons might be released when fission took place and Hans van Halban, Lew Kowarski and Frederic Joliot(10) in Paris found such neutrons in 1939 and later estimated that on the average 3.5 were emitted at each fission(11). This figure is too high but brought up the possibility of one fission leading to another - a chain reaction, if enough neutrons remained available for fission. This in turn led to the concept of a "critical mass" - a mass below which a chain reaction was not possible - put forward by Francois Perrin(12) of the Paris group.

Speculation began immediately on whether the energy from a chain reaction in uranium could be used, either explosively or under control. Niels Bohr had suggested on theoretical grounds that fission was more likely to occur in uranium 235 than in 238, while Fermi's experiments had shown that neutrons were more likely to enter the nucleus if they were slowed down. So the French group started experiments on the feasibility of a controlled chain reaction using heavy water to moderate the neutron velocity*. Meantime the idea of a "superbomb" had been postulated and received a great deal of publicity. However, as experiments proceeded in 1939 scientific opinion changed to the feeling that an explosion was unlikely and, like others not close to the work, I accepted this. James Chadwick, in his usual judicial manner, felt that more experimental data were badly needed.

* The author is one of those, probably very few now, who believe that but for World War II the first man-induced, self-sustaining nuclear reaction would have been achieved in Paris, not Chicago.

When the Second World War broke out, Otto Frisch was visiting England and he decided to stay and work at Birmingham University, where he was invited to contribute an article on nuclear fission to the Annual Reports of the Chemical Society for 1939(13). In accordance with the prevailing scientific climate he wrote

"Fortunately, our progressing knowledge of the fission process had tended to dissipate these fears and there are now a number of strong arguments to the effect that the construction of such a superbomb would be, if not impossible, then at least prohibitively expensive and that furthermore the bomb would not be so effective as was thought at first."

But, paradoxically, the very writing of this article left Frisch uneasy about the theoretical questions involved. He and Rudolf Peierls, Professor of Theoretical Physics at Birmingham, studied the possibilities together and asked themselves the following questions:

- a. What is the total cross section as a function of neutron energy for 235?
- b. What is the ratio of fission cross section to total cross section for 235?
- c. What would be the critical mass (m_c) for 235?
- d. What would be the consequences of a chain reaction in pure 235?
- e. Would the consequences justify the cost?

They answered these questions on purely theoretical grounds and concluded that the critical mass might be as low as 1 Kg and that a 5 Kg bomb would be equivalent to several thousand tons of dynamite. They gave a cost estimate, a warning about the dangers of the radiation effects and even suggested a means of assembling and detonating the bomb. This three page memorandum dated March 1940(14) must be considered climacteric because it made the British Government take the weapon seriously enough to set up a high level Committee (the MAUD Committee) while at this time the main interest in USA was in an atomic engine as a source of power. The MAUD Committee report of July 1941 triggered the US weapon project - by then US scientists were becoming increasingly concerned that the weapon possibility was not being taken seriously.

Questions such as those that Frisch and Peierls posed themselves in March 1940 were not asked in the United States until many months later and in Germany, fortunately, they were never asked at all.

The MAUD Committee of high-level scientists was formalized in June 1940. In that month, too, Halban and Kowarski escaped to Britain from

France, bringing with them what was essentially the world stock of heavy water and arrangements were made for them to continue their work at the Cavendish Laboratory. In Canada, meantime, George Laurence, with the encouragement of the Director of his Division in NRC, but with little else, began an experiment to test the possibility of a chain reaction with thermal neutrons. In the light of later knowledge this experiment could not have given a useful answer, but it was important from another aspect.

In the autumn of 1940, Dr. Henry Tizard headed a British mission to USA which disclosed a great deal of the British wartime research, including the atomic work, to US scientists. John Cockcroft was a member of that mission and concluded that, generally speaking, the American work was months behind that in Britain. On the way home he visited Canada and discussed atomic work with George Laurence including arranging some modest British support for the work. This was the earliest British-Canadian contact on atomic work and it became a continuing one, so that when the Anglo-Canadian joint project was arranged, there was a scientific focus. So far as atomic energy in Canada is concerned, it all started with George Laurence.

During 1940 the British work crystallized into two projects - a "slow neutron" project or plant to produce power (and plutonium) and a "fast neutron" or weapon project with nearly all priority going to the weapon work, which included work on gaseous diffusion separation of 235, nuclear constants to get a better idea of the critical mass for 235, the metallurgy of uranium and the chemistry of uranium hexafluoride. During the year the slow neutron team under Halban showed that a $U_3O_8-D_2O$ arrangement could be divergent (but not critical).

In July 1941, the Maud Committee tabled a report of two parts(15), the more important of which established the feasibility of a weapon, with 10 Kg of 235 estimated to be equivalent to 1800 tons of TNT. The other said that atomic power was not relevant to wartime. The MAUD reports, plus the report of the US scientists Harold Urey and G.B. Pegram on their return from an October visit to Britain where they had been shown everything, changed the picture in the USA completely. To Pegram and Urey, Chadwick had said "I wish I could tell you it won't work, but I am 90% certain it will". In August Vannevar Bush and James Conant of the USA suggested a joint US-British project, but because the US was not at war the British Chiefs of Staff said No.

"It was a case of the biter bit" - a year later the British were desperately trying for a joint program, but by that time the US had put the shutters up.

For in December 1941 USA was at war, the American atomic work went into top gear and overdrive and an impressive performance it was. The Manhattan District of the Corps of Engineers was set up in June 1942; BGEN Groves took charge in September and all cooperation between US and Britain

ceased for which at the time, we blamed the soldier; post-war revelations seem to show that the blame lay mostly with a scientist, James Conant. (In passing, the US estimate at this time was 300 tons of TNT equivalent from 10 Kg of 235; the British received better advice on the partition of energy from G.I. Taylor).

The subsequent history of the US project - the first man-induced self-sustaining fission reaction in December 1942, the great plans for gaseous and electromagnetic separation of 235 and the large scale reactors to produce plutonium were well told by Harry Smyth in the 1945 official history(16) and subsequently elaborated by professional historians. The British contribution after collaboration was resumed in September 1943 was set out in an anonymous publication by His Majesty's Stationery Office(17) in August 1945. The US was generous enough to say the British made a great contribution. The road to the bomb was not easy*. Almost nothing was known about the fission process, the equations of state for materials at the temperatures and pressures developed by the explosion entailed a good deal of guess-work and the critical mass had to be determined in practice by getting closer and closer to it, requiring dangerous experiments like the "Dragon's Tail" when a plug of fissionable material was shot through a ring of the same material. There was no fatal accident; at least not until the war was over when a US scientist working alone had the misfortune to make an assembly critical by accident. The absolutely essential experiment was to test plutonium in an implosion assembly; no-one could be sure how efficiently this would work until the test at Alamogordos in July 1945. The uranium weapon was dropped untested. There was eventually total integration between US and British scientists at US establishments; one amusing point was that General Groves got a much clearer picture of what was going on at Los Alamos by reading the letters Rudolf Peierls wrote from there to James Chadwick, now the Head of the British teams in USA, in Washington.

CANADA AND DEFENCE ATOMIC RESEARCH

Canada's Defence Atomic Research Program had its origins in the collaborative efforts of World War Two. Canada's contribution to the atomic weapons that closed that war however, consisted of uranium ore, or concentrates and, if memory serves, of graphite and such like. No scientific work in the Canadian project affected the weapon project, and the Canadian scientists and those from Britain working with them were not privy to weapons information. The McMahon Act (Public Law 585 of the 79th Congress) passed in USA in 1946 continued the veto on the release of such information, so immediately after the war, the Department of National Defence was heavily dependent on the Atomic Energy Division of the National

* Refer to "Independence and Deterrence. Britain and Atomic Energy 1945-52 Vol. 2 Policy Execution" by Margaret Gowing - Macmillan 1974, chapter entitled "The Weapon" for an impression of the difficulties of a weapons program.

Research council (as it then was) for scientific information in the atomic field other than published reports. The story of defence atomic research in Canada therefore really starts with the Canadian Atomic Energy Project.

As early as 1941 there had been talk that the slow neutron heavy water work might better be pursued in North America, possibly USA. By August 1942 with the conditions prevailing in Britain, the British Government proposed that a joint project with Canada should be set up in Canada, and this suggestion was received enthusiastically by the Acting President of the National Research Council on behalf of Canada. The Canadian Government very generously met practically all costs except the salaries and foreign allowances of the scientists on the UK staff, and the slow neutron team started arriving in Montreal late in 1942 - first of all at the Windsor Hotel, then at a place in Simpson Street and finally very good accommodation in one of the completed wings of the University of Montreal. George Laurence and many other Canadian scientists joined those from Britain, to whom the snow was a shock but the availability of fresh fruit and other food a delight.

At first all went well. Then came a blow; the United States said they intended to supplement their graphite reactor program with an intensive effort on a heavy water reactor and that Canada could only have the Trail heavy water, which was contracted to US, if the Montreal Laboratory in effect acted as a research laboratory for the Dupont Company, who were to build the reactor. There were other frictions; the scientific head of the Montreal Laboratory, Dr. Halban, had a style which irked the Acting President of NRC, while the formal NRC purchasing procedures upset the UK scientists who were used to direct, informal wartime procedures. Finally, although the Canadian Government, through Mr. C.D. Howe, had acquired control of Eldorado Mining Company, the source of uranium, contracts had been allowed which committed the entire output to USA. As said earlier Britain and USA were at odds on collaboration and Canada did not feel she could line up with Britain against USA, so friction developed between UK and Canada. Morale at the Montreal Laboratory was very low and it was clear that the Anglo-Canadian project was in trouble unless US and UK patched up their differences.

The Quebec Agreement of August 1943 between Mr. Churchill, President Roosevelt and Mr. Mackenzie King appeared to do so, but the Montreal project still faced uncertainty because of the stated American plan for a heavy water reactor. Early in 1944, for discussions with UK and Canada, Gen. Groves' staff prepared a paper for him which concluded that there was little to be gained by building a heavy water reactor and that it was undesirable to do so in Canada. When shown this paper, Chadwick expressed shock and Groves invited him to make his own proposals. Chadwick rewrote it, used many of the arguments in the original paper but concluded that a heavy water pile of moderate size should be built in Canada. Groves accepted Chadwick's recommendations.

Again we have a climacteric; without Chadwick's action there probably would not have been a Canadian atomic energy project on the scale which ensued and certainly not for many years later and not taking an independent line. An American condition for collaboration in the Canadian project was that the Director of the Laboratory be acceptable to them and early in 1944 John Cockcroft was persuaded to accept the position. The Canadian chemist (and future president of NRC) Dr. E.W.R. Steacie became Deputy Director.

By the summer of 1944 a site had been chosen for the Canadian "moderate sized reactor" (the NRX reactor) at Chalk River, Ontario. There was an inclination to call it Indian Point, but there was already a Post Office of that name somewhere else in Ontario so Chalk River it was. The pace of the work picked up and the design of the reactor, mainly the work of Mr. D.W. Ginns, on loan from I.C.I. in England, proceeded. When the accident happened in 1952, everyone was very happy that he had designed it so that it could be taken apart.

Then the war was over, academics began to leave the project and Britain established its own reactor program and its own Atomic Energy Research Establishment (but Montreal and Chalk River were the birthplace of both). Another crisis arose between Canada and Britain when John Cockcroft was recalled to head up this establishment. If that had always been the intention, it had not been made clear and Canada was furious with Britain. An adequate replacement was promised and in W. Bennet Lewis, UK certainly kept its word for the names of Lewis and CANDU, the best nuclear reactor in the world, are inseparable.

In 1947 NRX went critical and now Canada had an unmatched research facility and engineering prototype. Intense beams of neutron and gamma radiation were available for all sorts of experiments, including experiments of interest to DND but I cannot speak about those years because I was on the staff of the British Embassy in Washington and my visits to Canada had nothing to do with defence. In 1947 US, UK and Canada reached a "modus vivendi" within the framework of the McMahon Act including an agreement for an exchange of technical information. It was largely a pipe dream and little of substance was exchanged, but there was one item which dealt with the detection of distant nuclear explosions. When USSR exploded its first atomic device I spent the night of 9th September 1949(18) in the Pentagon in communication with Britain to get British filter carrying aircraft in the air in time to intercept the fission product cloud. I followed this with a telephone conversation of great circumlocution with Dr. Lewis from which it was clear Canada had not yet taken any steps to meet such an event. By the time I arrived in Canada in December 1952, it was otherwise.

In this year of 1952, Britain's production reactors started their output and the first British test (offshore in Australia) was planned for later that year. The Universities were still full of the back-log from the war years so that the teaching demands of these institutions left a

shortage of scientists in the Atomic Weapons Research Establishment to man the test. Onond Solandt, the first Chairman of the Defence Research Board had met Bill (W.G. later Lord) Penney many times during the discussions which went on almost continuously from 1947-52 between U.S., U.K. and Canada on tripartite co-operation. He offered to lend Canadian scientists if that would help, and Penney gratefully accepted. Four employees of the Defence Research Board, most recruited specially, made measurements at Operation Hurricane, as the test was code-named. They were

Alec Carruthers	Thermal measurements
Geoff Kerrigan	Telemetry
Alec Cruikshank	Radiochemistry (fission products)
Dick Kendall	Radiation field measurements

Alec Cruikshank had come to DRB from Chalk River.

When I reached DRB, I found Ed Massey deeply involved in the effects of atomic weapons; in addition to his DRB duties, he was also Scientific Advisor to the Federal Civil Defence Coordinator, Gen. Worthington. The US had published in 1950 a volume entitled "The Effects of Atomic Weapons". Although the information in it was based only on (a) theory and TNT experience (b) one nuclear explosion in the New Mexico desert (c) the destruction of Hiroshima and Nagasaki which, naturally, were not instrumented and (d) five post-war detonations, all over water, it was nevertheless a great help to those planning atomic defence. There was another publication, issued by the US Department of Defense and at first classified SECRET. The late Dr. Otto Maass, a towering figure in Canadian wartime defence science, was given a copy when on a visit to the States in the late forties - perfectly legal at the time under our mutual defence agreements. Ed Massey was happily using both books and anything else he could get his hands on.

Some radiation field experiments had taken place at Suffield in 1952 but achieved little except to give experience in handling radiation sources and instrumentation. The Canadian Army had formed a Radiation Detection Unit trained to map radiation fields and it had been a great help to Chalk River in the fall of 1952 when a serious accident overtook the NRX reactor. Presumably No. 1 RDU was to form the basis of a larger Army organization skilled in nuclear attack effects, but examination showed the concept to be too inflexible and it was later dropped in favour of some nuclear expertise in all units. The DRB Operational Research Group were inserting atomic weapons effects into many of the situations they were considering.

The operation which was contributing rather than consuming information was that between the DRB and the Royal Canadian Air Force to help detect Russian nuclear explosions. Lancaster and some CF 100 aircraft were fitted with air scoops and filters to collect debris in the air. RCAF

freight aircraft on regular runs in the North West were also equipped and collections of rain water and snow were being made at various points in Canada. In my first few months in Canada the only nuclear debris reaching the atmosphere was from US tests, but in August 1953 all hell broke loose; there was a Russian test which the Americans suspected was thermonuclear. Our task was to devise some means of condensing water out of the upper (10,000 ft) atmosphere to see if it contained abnormal amounts of tritium. This apparatus had to be ready by the time the cloud reached Canada, so we had about 48-72 hours. The late Mr. Fraser of the NRC Engineering Division whose help I invoked did the impossible; the apparatus was reminiscent of Rube Goldberg, but it got the water - and it was loaded with tritium. When I arrived on the scene, the active participants included John Langley, Burke Stannard, Otto Fischer, Ross MacDonald, Mary Down and an RCAF Officer whose name I cannot recall.

The Vice-Chairman of DRB told me they wanted 50% of the capability of Suffield and of the Defence Research Chemical Laboratories in Ottawa (now DREO) turned over to atomic defence, a mandate which I did not regard as very sensible, since it seemed to me you had to do the things that had to be done, and not invent work. If this goal was achieved it was due to the inspirations of the laboratories, not to me. One of the first problems was to relocate the four scientists who were returning from Operation Hurricane. Kerrigan solved 25% of it by leaving the service of DRB. Alec Cruikshank returned to Chalk River, but as a DRB employee, where he and Bob Brown, also DRB, worked in the radiochemistry section under Bill Grummitt analysing fission product samples. Dick Kendall went to Suffield. Alec Carruthers went to DRCL where he set up the Radiation Section.

In 1953 the Chiefs of Staff set up two committees - the Joint Special Weapons Policy Committee (JSWPC) consisting of senior officers from the three Services and DRB and its working committee, the Joint Special Weapons Committee (JSWC) on which I and my opposite numbers in the Services met. There were really no policy decisions for JSWPC to make and it did not meet often, but JSWC was a very useful committee for keeping the three Services and DRB in step on nuclear, biological and chemical warfare matters. With integration in 1964 the committees disappeared, of course, since a single Service can't have meetings with itself (but it does!). To the Advisory Committee structure of DRB was also added a Committee on Defence Against Atomic Warfare; there was little it could do except receive an annual report from me on what was going on and consider applications for University Grants which were relevant. It was abolished after a few years and the grant applications referred to other Advisory Committees.

With the exception of certain individuals who had degrees in both physics and medicine such as the late André Cipriani in Canada, J.S. Mitchell in Britain and one or two people in USA, the medical profession, by and large, had to be dragged into the nuclear age. It was no different in Canada and in DRB. There was an Advisory Panel which considered the effects of atomic weapons on humans but it consisted of medical men only who at their meetings listened to presentations from various sources and

then made recommendations. In the five years I had been the link between the UK Ministry of Supply and the US Atomic Energy Commission I had been obtaining information, mostly unclassified, on all aspects of the work. Although a physicist, I knew a lot about the biological effects of atomic weapons, so I set about convincing the Advisory Panel that medical men and physicists must meet as equals on this matter and work together. I know I made enemies in the process, but I believe the eventual Advisory Committee on Radiation Protection and Treatment was an effective advisory instrument to the Board, the Forces and other Federal Government Departments.

Britain was still glad to accept replacements when the four who had been at Operation Hurricane returned to Canada, so in 1953 the following staff went to the UK Atomic Weapons Research Establishment

Innes MacKenzie from SES

Tom Sterling from CARDE

Cyril Turner from DRTE

MacKenzie and Sterling worked on quite sensitive parts of the British weapons program and so were under a double burden of security allegiances when they returned to Canada. This they did in 1955 when the UK Atomic Energy Authority, as it had become, realized that there was no possibility of Canada embarking on a weapons program and that they were therefore politically vulnerable in having Canadians on loan working in the weapons program.

Britain was at the stage of planning further tests and in August 1954 I visited the Atomic Weapons Research Establishment in England to discuss with the now Sir William Penney whether he wanted Canadian participation in the tests planned for 1956 and 1957. It turned out that AWRE still did not have the capability to make thermal measurements, so a team led by Alec Carruthers would be more than welcome. I carried the message that our Suffield scientists were keen to try to measure the early neutron flux, which was dangerous ground since the early neutron flux is diagnostic of weapon design. However, Penney said they were welcome to try but didn't think they would succeed. He was right. In turn, he asked if Canada would undertake shock and blast measurements using the smoke rocket technique, I got CDRB's approval and SES did the work. Through other channels AWRE was being offered the services of No. 1 Radiation Detection Unit and this offer, too, was accepted.

On this same trip I visited the UK gaseous diffusion plant at Capenhurst, was shown through the Plant and given certain of its parameters. I did so because at this time there was thought in both Britain and Canada that Churchill Falls in Labrador would be a good site for a gaseous diffusion plant; such plants require enormous supplies of electricity and the power of the Falls was at that time unharnessed. I expect the idea was politically unacceptable in Canada.

attached to Ed Massey's civil defence section. However, it turned out not to be a good way to use operational research people.

The fifties was a very busy time in the matter of collecting nuclear debris from the atmosphere. John Langley had been succeeded in turn by Ivor Bowen, Jack Arnell, Harold Larnder and then Guy Eon in the period I write about. From 1954 to 1960 there were some 200 nuclear tests in the northern hemisphere, many of them in the multi-megaton range, of which 50 were in USSR. Jake Koop, the RCAF and our radiochemical group at Chalk River were all very busy. DRB also funded a mass spectrometer at McMaster University which could analyse debris. But it was a losing game. The air was now so full (from the micro-radiochemical point of view) of old fission products that it was difficult to make sense of a filter. The procedure had been to analyse some filters ourselves and send others to USA. Eventually I was delegated to approach an old contact in a technical branch of the USAF; I asked him not to be diplomatic, but please to tell me if these filters were any use to them. He said "Since you ask it that way, Alec, the answer is - No". So we had either to re-equip the RCAF with aircraft which could fly up to and above the tropopause or drop out of the game. There could only be one answer and early in the nineteen sixties the operation was terminated.

During the long cold evenings in the huts on the Nullarbor Plain at Operation Buffalo, the British fell to discussing how their high explosive program to simulate nuclear explosions was constrained by working in a small and populous island, their limit being a few tons. They felt there was a need to test the phenomenology of explosions in the range between tons and the smallest nuclear test to that date, about one kiloton. They suggested that Canada with its large, relatively uninhabited open spaces could tolerate much larger explosions and hinted that here was a contribution we could make, especially since we now had the smoke rocket capability. Jim Flynn took part in these conversations and when he was posted to our London office after returning from Buffalo, he took the suggestion up again. I did the paper work at headquarters and got agreement for such a program at Suffield.

Our sights were set for an explosion of 100 tons of TNT, the great question being whether such a large amount of TNT would detonate completely and uniformly. The Chief Superintendent of the day at Suffield was Archie Pennie, who had much explosives expertise from his time at CARDE. A special method of casting TNT was developed, one ton was exploded in 1958 and then 5 tons in 1959, all satisfactorily. At the DRB Symposium in December 1958 I asked Dr. Curt Lampson of the US Ballistics Laboratory if they would be interested in the 5 ton explosion the next summer and got an enthusiastic Yes as an answer. So USA participated in the 1959 explosion thus beginning a long US - Canadian collaboration in large-scale shock and blast field work, our interest being in phenomenology, theirs largely in the exposure of materiel. Oddly enough the British, who had suggested the program, did not take up the invitation for 1959, but they were there for 20 tons in 1960 and for 100 tons in 1961; then we all raised our sights and

went for 500 tons, which amount was detonated successfully in 1964 (and again in 1968 and 1972). 500 tons of TNT is quite costly, but we got donations of TNT from UK and USA, who claimed they got "more bangs for a buck" at Suffield than anywhere else. The 20 ton experiment in 1960 featured a tripartite blast line, the first time British, US and Canadian blast gauges had ever been directly compared. This was an extremely productive program; it gave Canada the expertise it wanted, it was of help to US and Britain in evaluating the resistance of equipment to blast and it yielded fundamental information in geophysics, some of which was helpful to the astronauts who landed on the moon.

After the change of Government in 1957 MGEN Pearkes was appointed Minister of National Defence and immediately stated he was determined the Canadian Forces should have the most modern equipment, and that included nuclear submarines. Well, of course, there was immediate enthusiasm in the RCN and in DRB. The Navy set up a study group on which DRB served and which was called a "feasibility" study, which seemed odd to me because there were several US nuclear submarines in service which meant they must be feasible. It was, of course, really a "can we afford it" study. The RCN put together quite a powerful engineering team headed by Capt. (N) S. Davis (later RADM) to which DRB scientists were assigned and both RCN and DRB personnel were seconded to England, to the Atomic Energy Research Establishment and to the Royal Navy, for Britain was designing its first nuclear powered submarine. Innes MacKenzie on his return from AWRE in 1955 had been assigned to help me where he stoically accepted the desk work he hated and produced a number of memos of some later value. He was now posted to the Atomic Energy Research Establishment in England, along with K.N. Barnard and George Christie both of the Naval Research Establishment (now DREA).

The study got information from USA up to a point, but the point at which it stopped was the information being given to the British about the reactor. It was quite simple - Britain had shown its willingness by committing money to a nuclear submarine program; Canada had not. It was a very good study pointing to a unit cost per submarine of about \$65,000,000. Some aircraft cost that nowadays, but in 1959 for a naval vessel, it was much too rich fare for Canada. That was the end of Canada's nuclear powered submarine - we bought conventional ones instead - and in that year MacKenzie, Barnard and Christie came home, MacKenzie to leave the Board for academic life.

In 1957 the Prime Minister of Great Britain, Mr. Harold MacMillan and the President of the United States Mr. Eisenhower, met in Bermuda to examine the technological lead USSR had shown by launching Sputnik 1. They invited the Prime Minister of Canada, Mr John Diefenbaker, to join them later. One result was the Tripartite Technical Cooperation Program in defence science (the Tripartite was dropped when first Australia and then New Zealand joined). The original list of topics for exchange did not include atomic matters; the release of US "Restricted Data" was controlled by law which had been amended in 1954 and 1958 so that it was now possible to release such information to nations with a weapons program roughly

comparable to that of the USA, but of course Canada did not qualify. I felt that there was an area of information, classified military information but not "Restricted Data", in which a useful exchange might take place in the atomic field. (Remember we were still in the "missile gap" era and civil defence was high in everyone's mind). The British had a fruitful bipartite agreement on weapons with USA and probably felt - correctly - that they could get all they needed through that, and their reaction to my suggestion was cool. But USA listened. In 1961 I led a DRB team to the Pentagon to display Canadian defence research capabilities and as a result a subgroup on the effects of nuclear weapons was set up with myself as Executive Member (as it then was). Until then all Executive Members had been officers of the Pentagon. The British still demurred so for the first year it was a Canadian-US subgroup with the papers going to the British for information. After a year it became tripartite. Except for one year, I remained Executive Member till 1968. The subgroup was dropped at the reorganization of TTCF in 1972, but it had been productive; its panel on shock and blast was the vehicle for the tripartite collaboration at Suffield, although it is true this could have been arranged in other ways.

Let me clear up some odds and ends now. In 1958 a "Conference of Experts on the Detection of Distant Nuclear Explosions" was held in Geneva. The Canadian delegation was headed by Omond Solandt and Harold Larnder went with him. I was sent to Geneva for a week, at short notice, to support them. Then in 1962 the Eighteen Nations Disarmament Conference convened, General E.L.M. Burns heading the Canadian delegation. Jack Arnell, then Scientific Advisor to the Chief of the Air Staff went to Geneva to advise General Burns; he was well versed in nuclear matters and knew more about delivery systems than I did. However, things moved slowly (16 years later it is still meeting under a different name) and after three weeks I took Jack's place, also for a few weeks. It resulted in my being Scientific Advisor to General Burns till 1968, sitting in Ottawa except for a second visit of a week or two to Geneva in August 1962 and a very brief visit in 1963.

There were two pieces of fall-out from this. First, the British by the summer of 1962 had results from a seismic array in Scotland which they felt might be the basis for a system to distinguish between seismic activity from an earthquake and from a nuclear explosion. They needed a larger array on seismically quiet rock such as the Canadian Shield, and approached DRB to see if they could help. The Department then responsible for seismology was Mines and Technical Surveys but with the rigidity of public service budgeting, they could provide neither money nor staff until fiscal year 1963-64, which did not fit with the urgency of nuclear test detection in 1962. Fortunately, Defence Research Board had much more flexibility and the Chairman allocated me \$50,000 followed later by another \$50,000. The RCAF came through in a big way in transporting equipment and providing technicians, the guiding spirit being Jack Arnell again. Mines and Technical Surveys oversaw the whole operation which started with the choice of a site outside Yellowknife by a Canadian-British survey party (each leg of the array is about 15 miles long). Very hard work by everyone

resulted in a short array of 4 seismometer assemblies in one leg and three in the other being in operation by December 1962 which was better than we had hoped for, but it was subject to much repair in the spring since it turned out that rabbits like the insulation then used on cable(20). Mines and Technical Surveys eventually took over the whole operation in which External Affairs was keenly interested as a Canadian contribution to a difficult international problem.

Secondly, my involvement with disarmament led External to nominate me to attend two conferences on Asian security organized by the London-based Institute for Strategic Studies in 1967. The first was held in New Delhi in conjunction with the Indian Institute of Defence Analysis, while the second was held at Nikko, Japan (in a Buddhist temple!) sponsored by the Japanese newspaper Yomiuri Shimbun. I was the only Canadian; I am sure they could have nominated a better one, but the experience was certainly a revelation in how things were looked at in those two countries. The Japanese meeting was unique at the time in that intellectuals, defence officers and newspaper men were all members of the Japanese delegation. I was grateful to the Chairman of DRB for being broad-minded enough to approve my travel!

INDEX OF NAMES

Arnell, J.C. Chemist: Superintendent, DRCL: Senior SSO(SW): DRB:
SA/CAS: SA/CNS: Assistant Deputy Minister (Finance).

Barnard, K.N. Physicist: NRL seconded to AERE, England.

Bowen, Ivor Director, in DRB.

Brown, R. Chemist, DRB staff at AECL, Chalk River.

Burns, E.L.M. GEN Head, Canadian Delegation to Eighteen Nations
Disarmament Conference.

Carruthers, J.A. Physicist: Canadian at Hurricane: Head, Radiation
Section DRCL: Physics Department McGill University

Christie, G.L. Chemist: Corrosion Section, NRL: Battery Section
DRCL: on loan to India: DSIS.

Cipriani, André Physicist and Physician: Director, Division of Biology
and Health Physics, Atomic Energy of Canada Ltd, Chalk
River.

Cruikshank, A.J. Chemist: Canadian at Hurricane: DRB staff at Chalk
River: DAR(A): SSO/DRB.

Davis, S. RADM. Naval Engineer: Head, Nuclear Submarine Study

Down, M. Miss Technical Officer/DRB.

Eon, L.G. SSO in Division A: Deputy Defence Research Member,
Washington: DRB.

Fischer, O. Chemist: SSO/DRB.

Flynn, J.T. Physicist: SES: Liaison Officer, DRB London Office:
Director, Atomic Division, DREO: Chief, DREO: Chief,
DREP.

Gilbert, G.H. Physicist: Meteorological Officer on loan to DRB as
Advisor.

Grunmitt, W.E. Chemist: Head, Radiochemical Analysis Section, AECL
Chalk River.

Harvey R.B. Physicist: Physics Section, SES: Head, Shock and
Blast Section, SES: DSIS.

Kendall, R.A. Physicist: Canadian at Hurricane: SES: Director, Acoustics Division, DREA: Chief, DREP.

Kerrigan, G. Canadian at Hurricane.

Koop, J. Information Officer, DSIS: SSO/DRB: DSTSP/ADM(Pol): Science Counsellor, Canadian High Commission, London.

Larnder, H. Operational Research: DRB.

Langley, John DRB.

Laurence, G.C. Physicist: Director, Reactor Physics Division, AECL Chalk River: latterly Chairman, Atomic Energy Control Board of Canada.

Longair, A.K. Physicist: SSO/SW(A): DAR: Planning Officer: Chief Staff Officer (Scientific): Counsellor for Defence Research and Development, Canadian Embassy, Washington.

Lewis, W.B. Physicist: Director, Chalk River Laboratories: Vice-President for Research and Development, Atomic Energy of Canada Ltd.; Distinguished Scientist Queen's University.

Maass, Otto Chemist: Advisor to Defence Research Board on Special Weapons Research.

MacDonald, Ross SSO/DRB.

MacKenzie, I.K. Physicist: SES: loaned to AWRE, England: DAR(A): seconded to AERE, England: Professor of Physics, Dalhousie University: Professor and Dean of Physics, Guelph University.

Massey, E.E. Chemist: SSO/SW(CD) and SA to Federal Civil Defence Co-ordinator: DAR(CD): Operational Research.

Pearkes, G. MGEN Minister of National Defence 1957, later Lieut. Governor of British Columbia.

Pennie, A.M. Chemist: Secretary, DRB: Superintendent, DRNL: Chief Superintendent, SES: Deputy Chairman, DRB.

Solandt, O.M. First Chairman, DRB: Head R & D Canadian National Railways: Hawker Siddeley: Chancellor, University of Toronto: Director of Companies.

Stannard, R.B. SSO/DRB: Liaison Officer, DRB London Office:
Operational Research and SA to Emergency Measures
Organization.

Steacie, E.W.R. Chemist: President of the National Research Council.

Sterling, T.S. Chemist: Explosives Division, CARDE: loaned to AWRE,
England: DIIRD/CRAD.

Tate, Parr Physicist: Radiation Section, DRCL: Director, NBC
Defence Division, DREO: DTG/CRAD: Director,
Protective Sciences Division, DREO.

Turner, C.J.M. Physicist: DRTE: Loaned to AWRE, England: DIIRD6/
CRAD.

GLOSSARY

DRB Defence Research Board

CRAD Chief, Research and Development, DND
NRL Naval Research Laboratory, now DREA

CARDE Canadian Armament Research and Development Laboratory,
now DREV

DRCL Defence Research Chemical laboratories, now DREO

SES Suffield Experimental Station, now DRES

PNL Pacific Naval Laboratory, now DREP

DRNL Defence Research Northern Laboratory, Churchill

AECL Atomic Energy of Canada Limited

SW Special Weapons

A Atomic

CD Civil Defence

SSO Scientific Staff Officer

DAR Directorate of Atomic Research, also Director (included
biological and chemical research)

SA Scientific Advisor

CAS Chief of the Air Staff

CNS Chief of the Naval Staff

In the DRB of the early fifties, Division A was responsible for naval and electronic matters, Division B for aeronautics and armaments, Division C for Special Weapons, Arctic and Intelligence and Division D for Human Resources and Operational Research.

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