ATOMIC ENERGY IN CANADA: PERSONAL RECOLLECTIONS OF THE WARTIME YEARS

by Philip R. Wallace

In December 1942 I was teaching in the mathematics department of the Massachusetts Institute of Technology (M.I.T.) when a letter came from J.L. Synge, my former department head at the University of Toronto, informing me that there was an important wartime project in Montreal for which I was needed. He could give me no clue as to the nature of the project, but it had been understood that when such an occasion arose I would be ready to accept.

When I informed my department chairman, he granted me formal leave, on the understanding that I could return to the M.I.T. when the war ended. But I never did. My career was to take a very different, unanticipated direction.

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When my wife and I arrived in Montreal, a city which I had only briefly visited, it was brutally cold; the thermometer hovered around -20°C for a week or so. We spent a few days in a luxurious bed and breakfast in an old mansion on Sherbrooke Street, then moved to a suite a few minutes' walk from the old house on Simpson Street which was the first home of the project.

The project leaders, recruited from Great Britain, France and the European refugee community, were already at work. I was briefed by Georges Placzek, who was to be the leader of the theoretical division. He introduced me to nuclear fission and spoke of the possibility of building nuclear reactors which would produce both energy and new transuranic elements. These elements would be the raw material for weapons thousands of times more powerful than anything previously known. The role of the Canadian project was to explore the feasibility of a graphite-moderated nuclear reactor, which would be the first step into the new territory.

I knew almost nothing about nuclear physics. However, as a graduate student I had given a seminar on a paper by Peierls and Kapur on nuclear reactions. Now I learned that Peierls was the leader of a British team which was to work closely with us and with a newly formed American project, and that he had been the major figure in establishing the feasibility of the whole enterprise. Our rapid education in nuclear physics was facilitated by a very fine article by Hans Bethe in *Reviews of Modern Physics*, which served as a sort of bible on the subject.

I was very impressed by Georges Placzek, a refugee Czech theoretical physicist of international stature who had worked with Bohr, Heisenberg and Peierls, among others. Placzek was to prove himself an inspiring leader. His task was formidable, for the theoretical team being assembled consisted mostly of young physicists and mathematicians with little experience or knowledge of nuclear physics. This was particularly true of the Canadian contingent. In those days, the world of theoretical physics had its base in continental Europe. The leading lights were Einstein, Bohr, Born, Fermi, Szilard, Weisskopf, Bethe, Peierls, Schrodinger, and Wigner. The United States owed its power to European refugees, of which Canada had few. The icon of Canadian physics was Rutherford,

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who made no secret of his scorn for theorists (aside from Niels Bohr, its most incomprehensible exponent).

Theoretical physics was almost nonexistent in Canada. There had been a few isolated individuals: King and Watson at McGill, Barnes at Toronto, Archibald at Dalhousie. But only Infeld at Toronto, another emigré from Europe, had generated a "school" around himself, of which I was luckily the first member. It was primarily here that theoretical physics put down roots in Canadian soil.

Although the project was under Canadian jurisdiction, its members constituted a veritable League of Nations. A few Canadians worked with a much larger group of Britons, Americans and European refugees, including Free French and Jewish and anti-Nazi scientists from Germany.

My first colleague at Simpson Street was George Volkoff, whose credentials were more impressive than mine. Although he came from the University of British Columbia, George had studied under Oppenheimer, perhaps the most distinguished of the first generation of American protégés of the European theoretical establishment. George's doctoral work was on the theory of neutron stars, decades before they became a major issue in astrophysics.

We were joined by Jeanne LeCaine-Agnew, an excellent mathematician educated at Vassar, and Carson Mark, an amiable mathematician from the University of Manitoba. Neither knew much about physics, much less the new frontier of nuclear physics. From the United States came German refugee Ernst Courant, son of the famous mathematician of that name, and Bob Marshak, a brilliant nuclear physicist who had studied under Weisskopf and whose later career was very distinguished. In the later days of the project we were joined by Boris Davisson, a gentle and modest man of

en souvenir
du premier centre canadien
de recherches nucléaires
son altesse royale le duc d'edimbourg
a dévoilé cette plaque
le 17 mai 1962

du 1er mars 1943 au 30 juin 1946 une partie de cet immeuble de l'Université de montréal a abrité des laboratoires où plus de 580 personnes venues du canada, du royaume-uni, de france et d'ailleurs ont poursuivi des travaux de recherches et de mise au point sur l'énergie nucléaire obtenue par fission.

l'administration de ce laboratoire relevait du président du conseil national des recherches, c.j. mackenzie. parmi les canadiens qui ont participé aux travaux figurent:

e.w.r. steacie, directeur adjoint du laboratoire

g.c. butler	d.g. hurst	f.t. rosser
a. cambron	g.c. laurence	b.w. sargent
a. cipriani	j.c. mark	j.w.t. spinks
h.h. clayton	s.c. mason	h.g. thode
l.g. cook	n. miller	g.m. volkoff
p. demers	j.h.p. matheson	p.r. wallace
l.g. elliott	j.e.w. prévost	a.c. ward
	l. yaffe	

parmi les savants et ingénieurs d'autres pays se trouvent:

h.h. halban, premier directeur de ce laboratoire

j.d. cockroft, son successeur

w.j. arrot	b.l. goldschmidt	g. placzek
p. auger	j. guéron	b. pontecorvo
s.g. bauer	l. kowarski	m.h.l. pryce
h. carmichael	j.s. mitchell	w. seligman
j.v. dunworth	r.e. newell	h. tongue
d.w. ginns	h.r. paneth	c.n. watson-munro

Fig. 1 Plaque presented to the Université de Montréal by the Duke of Edinburg on May 17, 1962 in honour of its contribution of laboratory space to the nuclear research effort during the War.

immense talent who had been educated in the Soviet Union, and Maurice Pryce, a theorist of outstanding intellect and experience who had collaborated with Dirac. Another world-renowned theorist in our midst was Nick Kemmer from the U.K. He accepted duties as a liaison officer and so did not directly contribute to the scientific work of the group, but was a kindred spirit to us all.

Such were the human resources available to Placzek. It is a great testimonial to him that he led this motley group to impressive achievements. Placzek's leadership drew the best from every one of us. He created an atmosphere of mutual respect and esteem in which we all thrived.

The director of the laboratory was Hans Halban, who led a distinguished group of Free French scientists including the renowned Pierre Auger. One of the early recruits to the laboratory was Bruno Pontecorvo, an outstanding young physicist from Fermi's group in Rome. Pontecorvo had been working with Joliot-Curie in Paris when the German occupation began; he had escaped over the Pyrenees to Spain and thence to the United States, where he had obtained a job with an oil company in Oklahoma involving radioactive detection techniques. When Fermi fled to the United States, he urged Pontecorvo to join the project in Canada. This was a happy decision, since Fermi's Chicago Laboratory had closer links to the Canadian project than to the American projects in Hanford, Los Alamos and Oak Ridge.

The diverse origins of the members of the Montreal project was one of its most pleasant features, but it also brought problems. From the European perspective, Quebec in the 1940's was a backward society. The University of Montreal sustained very little science, and the modern building on the north side of Mount Royal, completed in 1929, had been left unoccupied. The project moved to this building when the full team had been assembled. When the European physicists read the University's prospectus, they were astonished to find that it was dominated by the Catholic church; a specific condition of appointment to its faculty was that one be a practising Catholic. The Church censored films and books; drive-in theatres were banned on moral grounds, and there was no city-wide library system. Worst of all, perhaps, were the political attitudes of French Catholic Quebec, which had overwhelmingly backed the fascist side in the Spanish Civil War and was now supporting the collaborationist régime of Marshall Pétain in France. On the other hand, in the English-speaking community, support was strong for the Spanish Republicans and the French Resistance. The European scientists, particularly the French, found they had little in common with the French-speaking community in Montreal and were drawn closer to the cosmopolitan English-speaking minority.

Early in the Simpson Street phase of the project, we were startled to receive a volume of poems by Pushkin sent by mail from the Soviet Embassy. It was an unexpected and perhaps ominous gesture of welcome. The Soviet Union was, of course, an ally, but many recognized it as such rather grudgingly.

THE SHADOW OF HEISENBERG

It is reasonable to ask why scientists did not question their motives in working for a project with such frightening possibilities. However, the initiative which had led to the project had originated with highly respected scientists - Leo Szilard, Eugene Wigner and Edward Teller - who had convinced Einstein, a lifelong pacifist, to write a letter to President Roosevelt strongly urging him to initiate such a project. Their concern was that Germany might be first to develop a nuclear weapon. This fear was based on the conviction that Werner Heisenberg, Germany's leading physicist and heretofore a close friend, and who shared their knowledge of the possibility of a nuclear weapon, had the ability to lead such a project to fruition.

Early in the project, Placzek related to me events which had marked the immediate pre-war period, when the discovery of nuclear fission was already known to the inner circle of the world's nuclear scientists. In the late summer of 1939, on the eve of war, there was an international conference of nuclear physicists at Ann Arbor, Michigan, where the threat of nuclear weapons was the subject of intense informal discussion. Scientists such as Bohr, Bethe, Weisskopf, Fermi, Szilard and Placzek himself strongly urged Heisenberg not to return to Germany, where, they were certain, he would be drawn into a nuclear weapon project. Many of the other scientists were refugees from Nazism; a good number of them were Jewish. But no amount of argument could shake Heisenberg, who held firmly to the line that, as a patriotic German, he was honour-bound to go back to help his country win the war, and only after that would Germans turn to the problem of ridding themselves of Hitler. This response aroused fear and despair among his colleagues, to whom he had been an esteemed personal friend. It revealed Heisenberg as at least naive and unrealistic and, for some a traitor to the scientific community. In the years that followed, the Allied project was covered by Heisenberg's shadow, and we kept looking for clues to the state of the German atomic weaponry efforts.

At one point in 1944 our concern was heightened by the publication of an article by Heisenberg on fundamental physics in a German physics journal. Physicists in the Allied countries were so intensely involved in their project that they had no time even to think about basic physics research; they reasoned that if Heisenberg had the time, the Germans must already have succeeded in producing a bomb.

History would show that they had both overestimated Heisenberg and failed to reckon with the rigidity and paranoia of the Hitler regime, which lacked the insight and could not generate the motivation to match the Allied effort. After the war, Heisenberg found a convenient excuse for himself and his German colleagues, hinting that they had failed because they had stronger moral reservations about developing such terrible weapons. The historical evidence supports a far less generous estimation both of the German project and of Heisenberg's personal integrity. In any event, the fears which spurred the Allied projects were later shown to have been unfounded.

LIFE IN THE THEORY GROUP

The pattern of project work was established early on. The senior members were responsible for the general direction of the project, and thus worked closely together. At the junior level, our tasks were more specialized and there was little scientific or social interaction among members of different work groups. We were subject to the "secrecy principle", which meant that we were not told more than we had to know, although this was not rigidly enforced. Hierarchy prevailed, and the atmosphere was in some ways more military than academic. In the theoretical division, however, Placzek treated us with understanding and respect and kept things as open as possible.

Jeanne Le Caine and I were assigned an ambitious task which, Placzek informed us, was of prime importance: the study of neutron diffusion in "piles", graphite-moderated reactors driven by fissile materials. The building of such a reactor was one of the main goals of the group. Our work was to investigate the diffusion process in a wide variety of geometries and for a wide range of the key parameters. This was an exercise in classical mathematical physics, requiring little in the way of original ideas. Because of my background in mathematical physics, it was fairly routine work for me. As for Jeanne Le Caine, who was well trained in "pure" mathematics, she adapted to the task very rapidly. Along the way we both managed to learn a few tricks of the trade. To appease us for being assigned what could be considered "donkey work", Placzek assured us that our final report would be the most widely read document to come out of the project. After the war, it was published in condensed form in two articles in the journal Nucleonics.

At the end of June 1943 the project moved to a wing of the "new" University of Montreal building, still unoccupied a dozen years after its construction. Built in the architectural style of the 1920's, it is superbly situated on the north side of Mount Royal. My wife and I found an apartment in a new building in the Snowden area, about a kilometer from the university. Two of our neighbours were associated with the project: the Sargents, who lived just above us, and Dennis and Renée Ginns, who were below on the ground floor. Even after 55 years I have maintained contact with Dennis Ginns, an engineer from ICI in Manchester who, although long retired, is still active. Our eldest son, Michael, was born in November 1943, and the Ginns had two young daughters, so we naturally became friends. As for Bern Sargent, although he was a quiet man, I got to know him well because we often walked together up the hill to the university.

Social life in the theory group developed largely due to the initiative of Carson and Kay Mark. Carson became my officemate, and I learned of his problems as a young mathematics professor at the University of Manitoba. By the time he came to Montreal, he already had four children. Salaries at our rank were around $2,000 to $3,000 a year; and, in the late Depression days, professors were sometimes not paid during the summer vacation months, so Carson took camping holidays with his family. In Montreal, the Marks led a life devoid of pretension but strong on hospitality. If you arrived at their home for dinner at the prescribed hour, you were likely to be put to work helping to wash the noon dishes. There was always a certain chaos as Carson and Kay fed the children, put them to bed, and so forth, while encouraging lively conversation among the guests, who became honorary members of the family. It was anything but dull.

Since there were no good restaurants near the university, most of the younger generation brought sandwich lunches, which they ate together in a common room where conversation thrived. In this international atmosphere, we talked mostly about world affairs rather than local matters. Somewhat later we organized lunch-hour discussion sessions on various issues. Our discussions were exceptionally interesting because of the great diversity of our backgrounds. They rarely touched on problems surrounding our own work. Mostly, I think, they served to reduce our feeling of isolation from the outside world.

It was in our informal luncheon discussions that Bruno Pontecorvo made his presence felt. His contributions reflected his broad interest in physics, science and philosophy. He tended to seek out interaction with members of the theory group because he felt he could engage us in discussions of broader aspects of physics, beyond the technical problems with which the project was preoccupied. Many of the questions he raised in our discussions anticipated the revolutionary developments in physics of the decades following the war.

Bruno came to the project with a reputation of being an outstanding athlete; it was said that he had been a top-ranking tennis player in Italy. I was a regular squash player and considered myself reasonably good at it, so I invited Bruno to join me on the squash courts of the McGill gymnasium. He had never played the game before, but he took to it enthusiastically. His athletic skills soon became apparent; after the first couple of games, I almost never beat him. Still, it was a diversion we both enjoyed.

Bruno and his family lived in an apartment off Côte des Neiges Road, backing on St. Joseph's Oratory. He liked to entertain his guests by showing them to his balcony, from which one could watch worshippers mounting on their knees the many steps leading to the shrine.

Although it would later be marked by tragedy, his family history was very interesting. His wife was Swedish, and they had lived for some years in Paris and later in the American West, with the consequence that his two young sons had command of four languages -- Swedish, Italian, French and English. They had a remarkable facility for addressing each guest in her or his own language. But Bruno spoke little Swedish and his wife little Italian, so the boys were able to use this fact to advantage in family conversations.

A TURNING POINT

Early in 1945, as the bomb projects were reaching their critical stage, Placzek was assigned to duties at Los Alamos. He came to the office shared by Carson Mark and me to announce this move and to tell us that he was authorized to take one of us with him. After a brief but inconclusive discussion, Carson proposed that we decide by flipping a coin. The decision was that Carson would go. I do not remember precisely my state of mind at that moment, but it was not long before I came to a feeling of relief. Though I had made my commitment to the project, I had never been comfortable with the bureaucracy and secrecy which surrounded it. I realized their necessity, but I looked forward to the day when I could resume a normal life in academia, which had always been my goal. As time wore on, I became constantly more satisfied with the way things had turned out. Carson committed his life to the manufacture of nuclear weapons, becoming head of the theoretical bomb production group in 1947. For my part, I realized that my fulfillment lay in academic research and teaching and that I would not have been able to adapt to the sort of life Carson led. The coin toss had been lucky for me.

With the departure of Placzek, it was necessary to find a new head for the theoretical group. In my mind there was no doubt that George Volkoff was the right man for the job. And in fact, the day after Placzek's departure I found George installed in his office. When I congratulated him on the appointment, he corrected me, saying with characteristic candour, "I thought that the first person to take over the office would get the job." George was a very open and forthright person, incapable of guile. This and many other agreeable characteristics, made him an excellent administrator. He later became chairman of the physics department and Dean of Science at the University of British Columbia.

There was not a great deal of collaboration within the theory group. Each of us was involved in a particular task, and most of the interaction took place at the higher level of group chairmen. When there were collaborations, they were invariably one-on-one. Thus I worked closely with Jeanne Le Caine on producing the neutron diffusion manual, for some time exchanging ideas in our joint office with Carson and, at Placzek's suggestion, collaborating with Ernest Courant in determining whether random fluctuation effects in reactors could create critical conditions.

So it was that when our project produced its first reactor, the low-intensity ZEEP, it fell to George Volkoff, as head of the group, to predict at what point it would become critical. There were numerous elements of uncertainty in the calculations, but George's prediction came within 3% of the experimental finding. This was somewhat of a miracle, since some of the parameters of the problem were subject to larger uncertainties. Thus was born "Volkoff's Theory of Errors", the first rule of which was never to make a single error in a calculation, because a second error might cancel out the first. One was hesitant to rely on this theory unless one had a deep belief in one's luck.

ENCOUNTERS WITH OUTSTANDING PHYSICISTS

A positive feature of working on the atomic energy project was the opportunity to make the acquaintance of some of the leading physicists of the time. We enjoyed many visits from Eugene Wigner, a gentlemanly Hungarian of a conservative disposition. Wigner was a brilliant man of great imagination whose activities covered the whole spectrum of physics. The universal physicist -- Fermi, Szilard, Peierls, Weisskopf and Bethe were other examples of the species -- has since become almost extinct. Because Wigner's theory group at Argonne was concerned with many of the same problems as ours, we got to know him well and to respect him deeply. Our relationship continued after the war; in 1957, he was one of the major speakers at a conference on theoretical physics organized by the newly-founded theoretical division of the Canadian Association of Physicists in 1957.

I remember vividly a trip to Chalk River when the laboratory there was under construction, in which Wigner accompanied George Volkoff, myself and the American "liaison officer". On the long drive to Chalk River from Montreal, there was time for a great deal of conversation. The "liaison" man, undoubtedly an agent of American military intelligence, was usually cautious in his speech, though not always in his actions; he was once caught searching secret files of the Canadian project at night. This revealed an undercurrent of distrust between the two projects, and the agent was subsequently recalled. But during our long car trip he was rather indiscreet, boasting that in every research group in the American project there was a "spy" who reported regularly to the intelligence organization. This shocked Wigner, who vigorously affirmed that no one in his group would play that role. He was told just as firmly that his group was no exception and that someone was reporting on them. Wigner reacted with shock and incredulity. I believe he felt that only communist governments played such dirty games, and that in democratic societies spying on scientific colleagues was not acceptable conduct.

There is an apocryphal story about Wigner which testifies to the respect he commanded. As the story goes, he assigned a complex problem to a graduate student. In due course, the student reported his results to Wigner. Wigner took from his pocket a little notebook, thumbed through the pages and, on finding the right page, announced to the student, "Yes, you are right".

Since there were several bomb-related projects in the United States, I do not understand how a first-class American theorist came to be assigned to our theoretical division. Bob Marshak came from the University of Rochester, where he had obtained his doctorate under the direction of Victor Weisskopf, an outstanding Austrian physicist in the "inner circle" of the European pioneers of quantum physics. Unlike the rest of us, Bob was well versed in the fundamental physics underlying our project. His subsequent career attests to his talent; he organized annual international conferences on particle physics in the post-war years and ultimately became a university president. Bob's working-class background had given his personality a somewhat sharp edge, perhaps the natural accompaniment to a sharp mind. His political leanings were decidedly to the left, but his was an independent spirit, governed by personal experience and convictions rather than by conventional dogmas. In any case, he was a very stimulating addition to our group and brought to it a scientific maturity beyond his years. He treasured his relationship with

Weisskopf and inspired in all of us a lifelong admiration for "Vicki" .

Regrettably, even distinguished scientists were not immune to the anti-Semitism prevalent in Quebec at the time. This became evident when I invited Bob and his wife Ruth to join my wife, Jean, and me in a Sunday excursion to the Laurentians, where we intended to dine in a highly recommended country restaurant. We received a chilly reception from restaurant staff. The manager told us firmly and clearly that we were not welcome and that we should look for a Jewish restaurant in which to dine. Bob was livid, and I was speechless with embarrassment. We had no option but to leave. A few miles down the road we did find a "Jewish" restaurant (advertised as such) where the management apparently took no exception to gentile guests. Experiences of this sort were probably not exclusive to Quebec, but this did not blunt the shame and anger we felt at encountering such discrimination in our own country -- and at a time when we were engaged in what was claimed to be a noble crusade against a racist maniac in Europe.

Another renowned physicist who made occasional but important visits to the Montreal Laboratory was Rudolf Peierls. The scientific leader of the British team, Peierls had been the first to show that it was probable a bomb could be made. He operated at first from New York, which enabled him to interact easily with both the American project and ourselves. He was a close friend of Placzek, who sometimes visited him there. Peierls later became the senior British theorist at Los Alamos.

Two other theorists who had collaborated with Peierls before the project, German refugee Klaus Fuchs and Tony Skyrme, were assigned to assist him. One of Peierls' visits to Montreal with Fuchs and Skyrme had unforeseen consequences for me. The "top brass" organized a dinner for Peierls, and, out of regard for Fuchs and Skyrme, suggested that someone should do the same for them. This fell to me, so I invited them to my apartment, along with a few of my colleagues in the theoretical group, including George Volkoff and Carson Mark and their wives. It was an interesting evening. Fuchs entertained us with stories of his experiences in a Canadian internment camp, set up at the beginning of the war to do a precautionary screening of German scientists in order to weed out those who might have Nazi sympathies or connections. Apparently the internees were separated into Jewish and gentile groups, the latter including a considerable number of Nazi sympathizers. Fuchs, a vehement anti-Nazi, found his situation uncomfortable and convinced some of his Jewish friends to declare him an "Honorary Jew" so that he might join them.

Cold War paranoia would later transform this amiable evening into a possible clandestine rendezvous of spies. For, as would later be revealed, Fuchs had made another visit to Montreal, alone, in order to pass information to a Soviet agent. So it was that, some years later, I was visited by an officer of the RCMP who wanted to talk to me about my contact with Fuchs during a Montreal visit. It was of course not difficult to establish that there had been two quite different visits, under quite different conditions.

Another question inevitably arose: did Peierls know of Fuchs' communist sympathies? Indeed, Peierls' relationship with Fuchs was later discussed, with dark overtones, in an English journal. But Peierls was an ardent anti-communist; he sued the journal and was awarded an impressive sum in compensation for the damage to his reputation, an outcome which delighted all of his fellow scientists.

As a physicist, Peierls was of the old school. He was no specialist; the whole of physics was a challenge to his clear, incisive mind. Possibly it was the sheer breadth of his interests which denied him the Nobel Prize which many of his colleagues felt he merited. Later on, I had the good fortune not only of working in his department, but of having him and his wife Genia as neigbours in Old Boar's Hill, south of Oxford, through the efforts of Genia herself.

Unfortunately, circumstances gave us very little direct contact with Hans Bethe. However, he so dominated the world of nuclear physics that all were conscious of, and learned from, him. There seemed to be no problem beyond his capacities.

By good fortune, and through the initiative of Maurice Pryce, some of us had the opportunity to spend an evening with Niels Bohr. Bohr's elder brother Harald, a mathematician and one-time football player, was the scientific attaché of the Danish Embassy and lived in a mansion on Pine Avenue. George Volkoff and I were invited, together with Maurice. The experience confirmed several aspects of Bohr's personality and manner which had become legendary. Bohr spoke with a heavy Danish accent, his voice sometimes dropped to the level of mumbling, and he had a habit of changing his train of thought in mid-sentence. Even when I could follow the words, I had some difficulty following his thought. Although there were moments of clarity, I gained very little from the encounter and can recall almost nothing of what was said.

The Theoretical Division was considerably enhanced in 1944. John Stewart and Haank Clayton came from the Canadian army, while Boris Davisson, Maurice Pryce and E.A. Guggenheim all arrived from Britain. Maurice was the most distinguished; he had been Dirac's sole collaborator and had an international reputation. Guggenheim was known for his work on statistical mechanics. Boris Davisson, however, had the most interesting history (see article by W.J.L. Buyers, in this issue). He was the son of a British engineer who had lived for many years in the Soviet Union, where Boris had been educated. While his background was primarily in mathematics, he had a solid grasp of the fundamentals of physics. One very quickly discovered that Boris was strongly anti-communist and looked back with no pleasure on his life in the Soviet Union. In addition, his health was poor; tuberculosis had already cost him a lung.

Boris was a very amiable and popular colleague. He was modest to the point of self-deprecation and rather fatalistic in outlook. Yet there was a gentleness in him, and an underlying sense of humour and irony. In a short time, he became a friend to all of us; no one in the project was more universally liked and esteemed.

Moreover, the quality of Boris' scientific work soon became evident. Whatever his social or political problems in the Soviet Union, he had been well trained in a rigorous educational system. He showed his wry sense of humour by proclaiming that he had not learned classical Newtonian mechanics in university, but was thoroughly versed in quantum theory. His manner of solving classical problems, he avowed, was to solve the corresponding quantum problem and then put Planck's constant equal to zero.

Boris was a very self-sufficient worker; his interaction with the rest of us was social rather than scientific. His method of working was quite unique. He would simply open his notebook and start writing. His ideas flowed smoothly onto paper. There were no afterthoughts or corrections; his notes seemed to emerge directly in publishable form. Nor was any problem too difficult or complicated. It was because of the quality of his work that, when Placzek, Carson Mark and Bengt Carlson went to Los Alamos, Boris was also seconded there. Boris had just recently been married to a very gregarious Russian girl, and all seemed well. However, the thin air at the altitude of Los Alamos created pulmonary problems, and he had to return to Montreal.

Boris' testimony to the dark side of Soviet life was reinforced by George Volkoff. One day he told me his family history. They had emigrated from Manchuria to Canada when George was quite young. During the Depression of the 1930's they fell upon hard times. Relatives still in the Soviet Union wrote his father to say that he could find good employment there. His father decided to return, leaving his family in Canada until he was well established. With time, letters from him became less and less frequent, and he ultimately disappeared in the Stalin purges. Despite his feelings about the Soviet regime, George showed a strong and justified feeling of national pride when the Soviet Union was turning the tide of war against Nazi Germany.

FEYNMAN FROM A DISTANCE

My first experience with Richard Feynman occurred just after the war ended, and was due to the fact that we both worked in the atomic energy projects of our respective countries. In Montreal, I had cooperated with Ernest Courant on the problems of neutron density fluctuations in nuclear reactors. Since they were multiplicative devices, there was concern that fluctuations might also reach a critical (explosive) level. Our work showed that this was not a danger. Fluctuations of local density in a gas were well known to be proportional to the square root of the number of molecules involved, so that fluctuations could be significant only in small regions containing few particles. We found that in systems in which there was a chain reaction, the fluctuations in the high density limit were more or less proportional to the density itself. This would not lead to criticality in a subcritical system, but merely affect the level of overall neutron density at which criticality would occur.

At this time much of the wartime secrecy had been relaxed, but more so in Canada than in the United

States. Ernest and I decided to publish our results, but before submitting them for publication we learned that Feynman had worked on the same problem at Los Alamos. We thought it unfair that we could publish and he could not, so we proposed preparing a joint paper which could await clearance in the United States. Feynman rejected our proposal, saying that we should proceed to publish immediately and that he was not concerned with credit for the finding. This was typical of Feynman's attitude toward physics: to him, the important thing was the discovery, not who made it. But, in retrospect, I regretted losing the opportunity to co-author a paper with Feynman.

THE CLIMAX

When word came of the successful explosion of a test bomb, we were all very excited. Everyone's attention was suddenly directed to the problems of global and domestic politics. We put aside our technical problems for a while and started to evaluate the consequences of what had been done. That the end of the war was in sight was, of course, a source of elation, but the question of whether the bomb would be immediately used on Japan was on everyone's mind. Only Bruno Pontecorvo was certain of the answer: for political reasons, he said, the Americans would have to use it on Japan before the Japanese could surrender and before the Russians could play a role in their surrender.

Shortly after, when the bombs were dropped on Hiroshima and Nagasaki, elation gave way to sober second thought about how we might all be affected by the irreversible consequences of our efforts. In a sense, however, we experienced a liberation; our isolation behind a cloud of secrecy was over, and we were now at centre stage of a great historical event. My wife, who had had no idea what I had been doing during the three years of the project, had a brief but understanding comment: "Shame on you."

LOOKING AHEAD

Although there were several layers of bureaucracy between most of us and the people who made the important decisions, we were always conscious of their power over us and over our future. Our prime minister was a remote and mysterious character, more a symbol of power than than a real embodiment of it. Our ultimate boss was the dominant minister in his cabinet, the powerful and tough-minded C.D. Howe. He cast a large shadow over our landscape.

His deputy minister, C.J. MacKenzie, was quite another story. Although he was seldom seen, his decisions directly affected our lives. He was an engineer by training, highly regarded and trusted by his superiors, but somehow also enjoying the trust and respect of those under him. If Howe had the brawn, it seemed that C.J. MacKenzie had the brains and initiative to make things happen. It was, in fact, MacKenzie who set the direction of the whole project.

Before the war, the National Research Council of Canada had been an organization at the edges of academic science; it functioned primarily as a sort of bureau of standards. When it was put in charge of the atomic energy project, it took on a new stature. This was really a big league job, and C.J. MacKenzie was at its head. After the bombing of Hiroshima and Nagasaki, the question of the future of the enterprise came to the fore. But MacKenzie, it seemed, already had his vision of the future -- and it was an ambitious one.

I am not entirely sure why, but MacKenzie invited George Volkoff and me to meet him; he wanted to "have a talk with us". Why us? Perhaps because a number of senior members of our group had gone off to Los Alamos and others who had come from abroad were expecting to return home, while George and I were committed to staying in Canada. MacKenzie revealed to us his intention to turn the National Research Council into an agency of basic science which would provide the resources, human and financial, to nurture science in the universities and laboratories of the country, and thus raise Canada to the position of a world power in science. We would build on what had been accomplished in our wartime project to carry Canadian science to a higher level than it had known in the past -- more global, less isolated and provincial.

MacKenzie's attitude was benignly paternal. He expressed the hope that we would be a part of his vision. Perhaps it was just a case of spreading word of his plans as widely as he could. But the vision was clear and inspiring, and I could not help being grateful for the opportunity to share it.

It was the end of an experience which none of us would have hoped for, full of the agonies of war. There were dark clouds on the horizon. But, in MacKenzie's vision, this period of trial and stress would give way to a new and hopeful beginning.