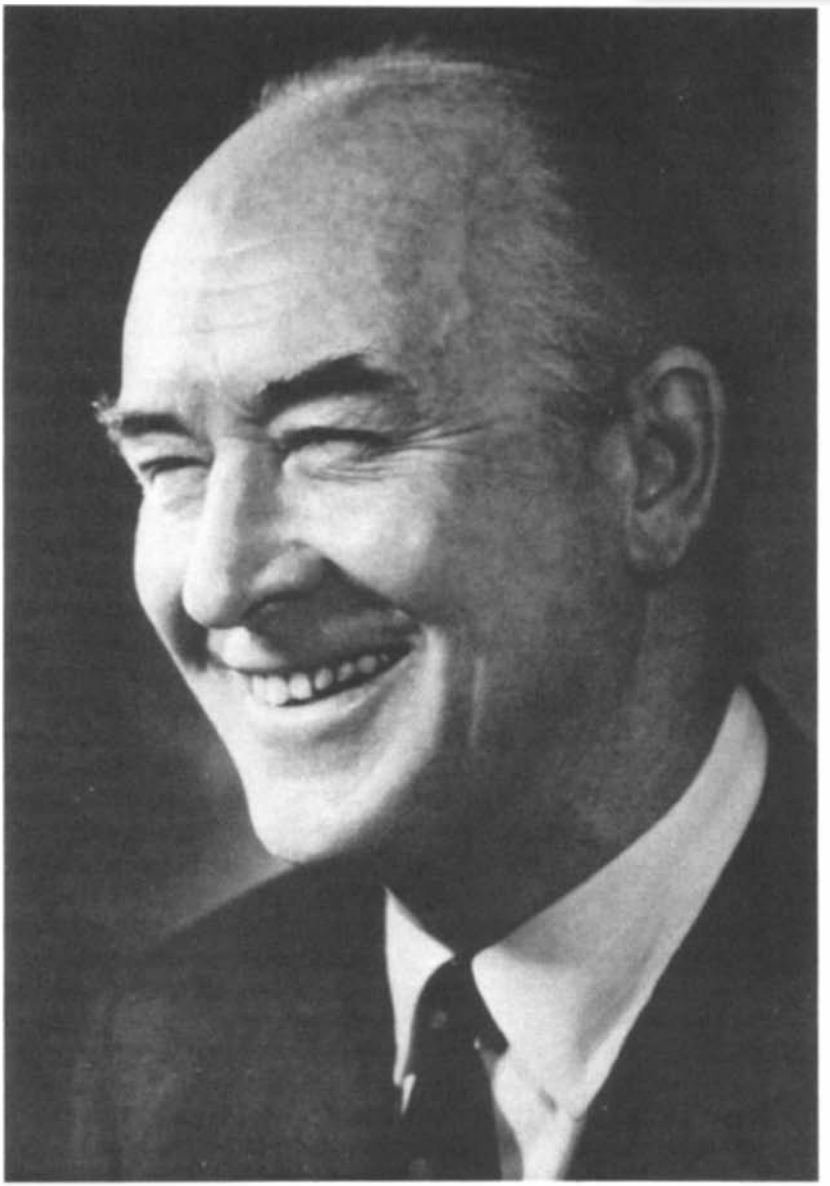

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EARLY DAYS IN UNIVERSITY GEOPHYSICS

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The editors of this series have decided that in the introductory chapter to each volume some earth scientist should have the opportunity to describe factors which influenced his or her career. This is not easy. Few of us plan our lives. We are carried along, seizing some opportunities, missing others, making great efforts on some occasions and at other times wasting chances in abysmally stupid ways. Afterwards it is hard enough to reconstruct what happened, even more difficult to judge why.

Probably ones genes provide the dominant control. I believe I was born energetic, restless, independent and contrary-minded, sociable, yet inclined to get bored and to dash off as a loner in search of novelty. Certainly my early life reinforced these natural inclinations.

I was the eldest of three children of parents who were older than most newly married couples. They had settled in Ottawa where my father had found a job, but where they knew no one. The job soon vanished and for a time they were badly off, but they founded a happy family and soon made many friends; indeed we seemed to meet many of the most interesting people to come to Ottawa, characters as varied as Queen Victoria's youngest son, the Duke of Connaught (whom I vividly remember talking to at a children's fancy-dress party at Government House given when he was Governor General during World War I), Mrs. Emmeline Pankhurst, the suffragette leader whom my mother admired, and Vilhjalmur Stefansson, whose expeditions my father helped organize. Ottawa was then a very small city, but with splendid environs which provided an invigorating place in which to grow up.

Before describing life there I should mention my relations. I had few in Canada, but many in Britain and several times during my youth my grandmother enabled mother to bring her family or to send me alone to see them.

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By a coincidence, the forebears of three of my four grandparents had come from the land to make money in Dundee, Edinburgh, and London during the Industrial Revolution and the expansion of trade and wealth which followed. My many elderly relatives had been brought up as members of the merchant and professional class and they led conventional and rather dull lives in comfort. Accustomed to affluence, they ineffectually frittered away their inheritance and their lives, or looked back to a happier past when they still had money and prospects.

It was fascinating to visit them, whether we stayed in large houses or shared genteel poverty, but I envied none of them. I preferred our moderate means but greater independence from convention, which weighed heavily upon the English. The lesson that I learned was that although the accumulation of wealth and power is a stimulating activity, the extent of wealth, once acquired, bears little relationship to happiness. At any rate, in my own life I sought excitement rather than fortune.

As a boy of four I well remember visiting my grandmother in Surrey during the summer of 1913. We travelled by train and steamer, saw whales and sailing ships on the Atlantic, and rode to and from the stations in horse-drawn carriages. My grandmother had a large house, garden, and woods outside of London, looked after by plenty of servants who were poorly paid by present standards, but who were nevertheless devoted and well trained.

Except when we went on a shopping expedition or to the zoo, Sundays provided the principal excitement of each week. After a long walk to church and back, a contingent of my many great-aunts and uncles arrived by the London train. The ladies wore silk dresses that swept the ground and the men, cutaway morning coats and silk top hats. Like characters from John Galsworthy's novels, they whiled away the pleasant afternoon chatting or strolling in the garden until it was time for tea and the 5:15 train back to town. A few had exciting tales to tell. Several of my grandmother's brothers had served in India and Africa as soldiers, merchants, or administrators. My Scottish grandmother could recall sailing to the Baltic Sea where my grandfather's ships had traded. Because of that, he was an Honourary Consul of the Czar in Dundee and the couple had visited the court at St. Petersburg on state occasions, such as coronations.

All was not quite as rosy as it then appeared to my young childish eyes. Both my grandfathers were long since dead and my Scottish grandmother had lost her money when her husband died. It occurred to no one that within five years the old Victorian Raj would be swept away forever and that most of the sons of those I met would be killed. Nevertheless, prewar London was a splendid sight for a boy to see with its panoply and grandeur, its confusion of horse-drawn traffic, its red-coated soldiers with bands playing, and its steam engineshissing and tooting every few minutes over the

great city viaducts or through the rolling countryside.

My parents, profiting by what they had experienced and seen, grew up to take a stern and realistic view of life, which I suppose they imposed on me. I enjoyed holidaying in Europe, but preferred to live in Canada. This involvement with North America was greatly heightened by the small amount I ever learned about my other grandfather's North American lineage. It epitomized the romance and excitement that a new world made possible for those who dared. His family were descendents of Angevin Huguenots who had migrated to Virginia in the seventeenth century. I believe that their name was originally Touselle or Touzelle, later corrupted to Tuzo. They prospered, but after the American Revolution left for Bermuda taking some of their slaves with them and thence moved to Quebec City where my grandfather grew up. More adventurous than most in 1853, he joined the Hudson Bay Company as a doctor upon graduating in medicine from McGill. That summer he crossed the continent with Sir George Simpson travelling by canoe to Manitoba, on horseback to the mountains and down the Columbia River by longboat. On the prairies the party avoided hostile Indians and shot buffalo from horseback. After many adventures, he settled in Fort Vancouver, then still part of the Hudson Bay Company's territory, although now in the state of Washington.

After twenty years he visited England, married, and went back to British Columbia where my mother was born. Soon afterwards, by a mysterious transformation he became a bank manager in New York and retired to Surrey where he built the house I remember, and then died young. My mother was forced to leave medical school and wait upon her selfish mother. It was not until she was 34 and attending the first camp of the Alpine Club of Canada that she met my father and escaped. That camp was held at Moraine Lake in the Valley of the Ten Peaks, a famous beauty spot near Banff. She and others made the first ascents of the peaks then numbered one to ten. Years later the authorities changed the names of the peaks from numbers to the surnames of those who had first climbed them. Thus it is that Mount Tuzo can be seen as the conspicuous peak in the engraving on the back of the Canadian twenty dollar bill.

An odd chain of circumstances also brought my father to Banff that summer. When he was sixteen, my grandfather had died, forcing my father to learn engineering as an apprentice. He took a job in India until malaria caused him to look for work in a colder climate, and this he found in Alberta.

This background has had a considerable influence upon my outlook. Although our family never had much money, mother in particular behaved exactly as though we did and, indeed, we enjoyed many of the benefits of wealth without its debilitating effects. Thus I grew up expecting to travel, taking a good education for granted, and being used to meeting people and

having lots of books. We lived in a large old house on the outskirts of Ottawa surrounded by two acres of woods and a garden in which the family grew much of its food. From an early age I hoed and weeded vegetables, tended chickens, and picked fruit. We were also expected to study hard, and to walk four miles a day to get to school and back. On weekends we swam, paddled, or skied. Later, this upbringing made geological field work seem natural and not a labor.

Since both my parents had had their own education cut short, they were determined that their children should fare better and as a consequence in 1913 mother brought back a friend from England who acted as a governess to myself and my sister. I have always felt that this early start was of great benefit to me although scarcely in the manner intended. As the youngest and smallest in my class, I sailed through school at its head. This did not make me popular. Because I was out of my own age group, I was forced at the private school to play games with older and larger boys. Thus I came to dislike all team sports and this has since saved me a great deal of time. Although naturally gregarious I became accustomed to following my own path. I learned early to distrust orthodoxy and whenever possible quietly to find a different way from the accepted.

So far from feeling that I suffered, I am grateful that I was not held back. Apart from a few sad moments as a child my life has been enormously enriched by the encouragement my upbringing gave to originality. I can well imagine that if one lives a routine existence without much travel, that clubs, lodges, and the like can greatly enrich ones life; but if one has an occupation that is varied and rewarding, one has little time for such activities.

Another advantage of the hard upbringing that my parents imposed was that when I was fifteen they dispatched me to the woods for the summer. At seventeen I had the good fortune to become a field assistant to Noel Odell, a charming man and natural leader, who had just returned from his triumphs on Mount Everest. He showed me the wonders of field geology. At ninety, he is still hale and travelling about the world from the University of Cambridge.

As a result of his inspiration, I found, after I had successfully completed my first year in honors math and physics at the University of Toronto, that I preferred field geology to laboratory physics. Much as I admired the elegance of physical theories, theories which at that time geology wholly lacked, I preferred a life in the woods to one in the laboratory. This decision caused great consternation. My professor in physics was shocked that a promising student should abandon physics, then in its heyday, in favor of geology which Ernest Rutherford had likened to postage stamp collecting. My geology professor was equally unreceptive and said that I must repeat my first year because I had not taken any elementary courses in geology and biology.

Fortunately an elderly classical physicist, Professor Lachlan Gilchrist, rescued me. He had recognized early the potential of geophysical prospecting and he was able to arrange that I could take a double major in physics and geology. Thus I became, more by accident than intent, the first student in Canada to graduate with a degree in geophysics, although men trained in physics and engineering had been making geophysical measurements in Canada two centuries before.

University life really suited me and on graduating I won a scholarship to Cambridge. I had no clear idea of what I wanted to do, but someone made the logical suggestion that I should study geophysics under Harold Jeffreys. He is a remarkable and charming man, but he was still a bachelor then and about as unworldly as a great English eccentric can be. Teddy Bullard who knew him well and who was certainly one of the brightest geophysicists of our generation once remarked to me that the only time he ever felt in the presence of genius was when he went to see Harold.

Jeffreys is courteous and affable, but aloof. A few years ago at 85, he astonished my wife on a main street in Cambridge by lifting his hat and bowing to her over the handlebars of his bicycle as he pedalled past. Jeffreys is primarily a mathematician, which I am not. He worked alone, hardly ever said anything, and had no idea of teaching. It is said that he only ever had one true graduate student, another brilliant mathematician, Keith Bullen. I took his course of lectures, but failed to understand them and spent my first year rowing, learning to fly, and travelling around Europe. My tutor was James Wordie, a canny Scottish geographer famous for having spent a winter in the Antarctic, living on nearly raw seal meat under an overturned lifeboat during one of the less successful of Shackleton's expeditions. I am indebted to him for giving me an interest in the polar regions, the idea of collecting travel books, and for introducing me to many interesting people, among them Louis S. B. Leakey, then an unknown young anthropologist working in St. John's College. If this appears to be a vague way to do graduate work, it was, but graduate study was not well organized then and the concept of having a Department of Geophysics at Cambridge was not agreed upon until after I had been at the university for a year. Even after I had left, there was still no building for geophysics or any regular courses apart from Jeffreys' and some surveying classes. Later Teddy Bullard returned from East Africa where he had been doing his Ph.D field work to establish what was to become a great department. During my stay at Cambridge, I took miscellaneous courses in geology and physics and another B.A. degree.

I returned more sophisticated but no clearer upon what to do in life and spent the next year in Ottawa working on Sudbury with Dr. W. H. Collins, then Director of the Geological Survey. From that experience I gained an affection

for Collins, but I reached the conclusion that none of the theories that he or anyone else had advanced to explain the origin of the Sudbury Basin could possibly be correct. However, I couldn't think of a better one. This was part of a more general philosophical problem. My courses in physics and chemistry showed me that science could and indeed should have precise theories, but at that time geology lacked them and all right-minded geologists scoffed at the search for any. They said that this was armchair geology and that more maps were both the aim and the method of geology. So sterile a concept baffled me, but I was too stupid to accept, until I was fifty, the explanation which Frank Taylor and Alfred Wegener had advanced in the year I was born.

In my youth scarcely anyone mentioned Wegener's ideas of a mobile earth and moving continents. Only in middle age was I converted and understood why it was impossible to theorize successfully about the earth as a static body if in reality it was a mobile one. The great impediment was that geologists only studied that one quarter of the earth's surface not covered by ice or water; at that time no one had any means for exploring the great interior or the ocean floors.

Collins sensed that geophysics might be useful to geology, if only in prospecting, but he could get no money to employ a geophysicist. He recommended me to get a Ph.D. degree in geology and to return when the depression was over.

I accordingly wrote to Harvard, M.I.T., and Princeton and was accepted by all. I chose Princeton both because it offered the most money and because Professor R. M. Field said that he hoped to start teaching geophysics there. In the same year he also recruited George Woollard, an engineer from Georgia Tech, and Kelly Skeels, a mathematician from Montana who had won a Rhodes Scholarship and studied geology at Oxford, but he failed to bring anyone to Princeton to teach us geophysics.

Field had conceived the idea, at the time considered eccentric, that it might be feasible to study the ocean floor. To do this he had pursuaded the US Coast and Geodetic Survey to give a research grant of $2000 to Maurice Ewing, then a physics instructor at Lehigh University, to start this vast investigation by applying seismic methods of prospecting first to the New Jersey coastal plain and thence out to sea. He had picked the key idea and the right man, but at the time few appreciated his perception.

As a result George and I on a few weekends drove over to Bethlehem to help Ewing and learn what we could. He later said that he regarded us as his first honorary graduate students. Harry Hess arrived at Princeton as a lecturer at the same time that we did, but his interests then lay in mineralogy and petrology. What little geophysics we learned we taught ourselves with primitive magnetometers and a Geiger counter connected to a backpack loaded with 100 pounds of electric batteries.

After the first winter Professor Taylor Thom gave Skeels and me $180 each and told us to buy ourselves cars and spend the summer mapping structural geology in Montana. By buying cars for about $50 each and camping out, this proved possible. In theory Taylor supervised our work, but since he was occupied with establishing a geology summer camp at Red Lodge, 150 miles away by gravel road, Kelly and I were essentially on our own, the more so because my thesis area ranged across 50 miles of mountains varying in elevation from 4500 to 12,100 feet and his area was much the same.

The highest point of my area, Mount Hague, in the Beartooth Mountains, was rather inaccessible, but I reached it by walking, climbing, and running for the best part of three days and sleeping at night by a fire. I was rewarded by finding that the top of Mount Hague was a flat area. This I found surprising, and the discovery later influenced my ideas that North America had overridden and been uplifted by the East Pacific Rise. I had been taught that mountains were usually pointed, and that flat areas were only produced by prolonged erosion down to sea level, and hence I thought that Mount Hague must have fairly recently been uplifted 12,000 feet. The top was also completely undisturbed which supported the view of the local dude ranchers that the peak had never been climbed before. One of them was so pleased when I arrived back at his ranch dripping wet from swimming across the Stillwater River that he arranged for me to visit a remote but excellent fishing camp of a friend. To do this I had to learn to ride. This was accomplished in the simplest possible way by mounting me one morning at 7 o'clock on a docile mare with instructions to follow a trail for 25 miles and to remember to get off and pull the horse over a 9000 foot pass. In spite of a snow flurry, I managed to pick up the trail on the other side and I believe I enjoyed the trip more than the horse and was in better shape at the end of it.

After one more winter, I completed my formal education having been one of the first students in geophysics at three great universities. By today's standards my studies were extremely casual, but I had learned to work hard, taking courses which it turned out were often irrelevant, old-fashioned, and frequently wrong. Nevertheless, the very casualness encouraged independence in thought and action. I am grateful to all the fine people who helped me.

Dr. Collins kept his promise and I joined the Geological Survey of Canada for four summers of strenuous reconnaissance mapping in Nova Scotia, Quebec, and the Northwest Territories. Since I had already spent ten summers in the "bush" the work was familiar, but challenging and exciting. I was in turn frightened by rapids, exhausted by portages, and thrilled to walk out of the woods onto the Arctic "barren ground" or tundra. On some of the portages we found ancient Indian birch-bark canoes rotting and several times we paddled up to moose and caribou swimming in large lakes. I jumped out of the

canoe on to one moose's back and killed another by hitting it on the head with an axe when we were short of food. Caribou are more dangerous than moose because they have sharper horns, are more lively, and when cornered they are inclined to try to climb into a canoe.

Intellectually it was also stimulating. We had some of the first air photographs and used them to pick the best routes and to extend our ground observations. The older geologists regarded this as a form of cheating, but large features such as great faults, the bedding of stratified rocks, and glacial deposits could be expeditiously mapped from aerial photos. The experience enabled some of us later to produce, largely with the aid of photographs, the first maps of the tectonic features and glacial deposits for all of Canada.

The winters were delightful too, and I had the good fortune to become engaged and happily married. For a time World War II disrupted our early married life. In 1939, I joined the Canadian Army and spent four years overseas, chiefly in technical work. I can recall two benefits. Enforced practice with reports taught me to write quickly and the end of the war provided a great opportunity.

Secretly during the war, the Canadian forces had accumulated much new equipment designed for Arctic operations. To make full use of it, in the winter of 1945–46 after the war was over, some of us organized the first and still most extensive motorized expedition ever to cross the Canadian Arctic. Forty men in ten fast, tracked-vehicles travelled 3400 miles north from railhead at Churchill, Manitoba, on Hudson Bay, across still unmapped wastes to reach the north magnetic pole on the nearest Arctic Islands, thence west over the sea ice to Coppermine, inland to Great Bear Lake, and back to railhead again near Edmonton. I travelled part of the way and spent one night in an igloo on the sea ice with some of the last Eskimos who still led traditional lives. For supper, we enjoyed seal meat stewed over stone lamps. In the evening we watched them drum-dance and slept at night in caribou-fur robes.

Many flights over the Arctic followed, searching for the last islands to be discovered outside of Antarctica. I flew as an observer on the first United States Air Force flight to the North Pole. These adventures stirred my interest in exporing remote places and nourished the broad outlook that I already possessed.

In 1946 I had to choose whether to remain in the army engineers, where I had reached the rank of colonel, return to the Survey where I was promised that I could soon be Director, enter industry, or succeed Gilchrist as Professor of Geophysics at Toronto. I sought the advice of C. J. Mackenzie, then the wise President of the National Research Council of Canada. He is the third man (with Odell and Jeffreys) who greatly influenced my career and also is still alive at the age of ninety. He advised me to go back to the university and to take no administrative job for twenty years for he predicted that I could be

successful in research. I accepted his advice and he rewarded me with ample opportunities to travel and to organize projects.

The next twenty years were the most exciting in the history of the earth sciences. During that time new methods and adequate support led to great discoveries about the earth's interior and ocean floors, culminating in the scientific revolution in the earth sciences marked by the acceptance of plate tectonics. The same period saw the rapid introduction of air travel which in those days had for the geologist the great advantages that it was cheap and by propellor-driven aircraft. Although slow, rough, and noisy, those planes flew close to the ground and stopped often. Because of this, in 1950 I took four and a half days to reach Johannesburg by flying boat from London, stopping at seven places on the Mediterranean, the Nile, and the Great African lakes en route. On other trips I visited islands not often seen today, including Iceland, the Azores in the Atlantic, Wake and Canton Islands in the Pacific, and Seychelles and Cocos Islands in the Indian Ocean. In those years and subsequently, nine trips around the world and scores of other journeys have given me a glimpse, if only a superficial one, of all the continents and indeed most major regions of the earth. More valuable still have been the insights given me by men and women explaining their own discoveries. At Little America, Lloyd Berkner told me about Byrd's first expedition of which he was a member. In Moscow, Shatsky, and on the steppes north of the Caspian Sea, a party chief, explained Shatsky's great discovery of aulacogens. On the Great Barrier Reef, local biologists told me its history. In South Africa, Mrs. Plumstead, Nel, and King, all students of du Toit, showed me the geology of that country. In South America, I spent three weeks on reconnaissance flights over the Peruvian Andes which gave me a great appreciation of those mountains. During 1971 in China, I tried to explain plate tectonics to eighty sceptical Chinese geologists through the help of C.-Y. Fu, who had studied at McGill at the same time that I was a student at Toronto. Many such experiences have certainly enriched my life.

Many of these trips were made with the object of attending some international meeting or other, meetings which were necessary to bring together the results of new discoveries and which the advent of air travel greatly facilitated. The peak of activity was reached during the International Geophysical Year and this led to my most unusual journey.

It happened that the largest meeting of geophysicists during the IGY was held in Toronto. At that meeting, both of the two parts of China expressed an interest in participating officially. This was impossible. Neither would cooperate with the other, and worse still, if the matter came to a vote the United States and the USSR representatives were bound to support different governments. Inasmuch as everyone considered that their cooperation was essential, especially in the exchange of data from satellites which both the USSR and the

US were about to launch, a solution had to be found. Berkner, the leading American, and Beloussov, the senior Russian, discussed the matter amiably and, realizing that scientists could not resolve a difference which was of a political nature, agreed to temporize. They would send a committee to both parts of China to study who in fact had control of geophysics and thus to delay matters at least until a report had been submitted.

Since I was host and had just been elected president of the largest international union involved, and above all because I had no connections with nor much knowledge of China, I was appointed to be the one-man committee. I crossed Asia by the Trans-Siberian Railway, toured much of China, passed through Hong Kong, and visited Taiwan. Both sides knew of this, but treated me royally. I duly reported that the Academia Sinica in Peking controlled geophysics on the mainland and that a separate Academia Sinica in Taipei controlled activities in Taiwan and that neither had any influence or control over the other, no matter what the politicians might say. I also wrote two books in which I described the two parts of my mission, one in each book, without making any comparisons. This was a fortunate introduction to a fascinating country at a time, twenty-three years ago, when no Americans and few Westerners could visit most of it. This prestigious introduction and my own documentation of my views made me known to some Chinese. It has stood me in good stead since, and I have returned to Peking on three other occasions.

During that time and indeed throughout the twenty-eight years I spent at the University of Toronto, my research interests ranged widely.

In 1946 I again took up the study of the great faults I had seen in the field before the war. Using some of them as boundaries and combining radiometric ages of rocks then available, I outlined the major divisions of the Canadian Shield. Quite independently, Professor J. E. Gill at McGill University did the same thing simultaneously and we both arrived at similar divisions. Soon afterwards when graduate students had begun to produce more ages and when I had travelled more widely, R. D. Russell, R. M. Farquhar, and I first showed that every shield has similar major divisions and we indicated the special features which distinguish the older Archean rocks from the young Proterozoic rocks. With the help of Arthur Holmes, who provided samples, and Harry Shillibeer, we first established the existence of Archean rocks in Scotland.

Mapping large faults from aerial photographs disclosed how many ore deposits lay near intersections. I wrote a paper about the application of this to the Canadian Archean but missed concluding that important deposits should be found near Timmins, Ontario, and Thompson, Manitoba, as they have been.

I also looked at the Sudbury area again. Two undergraduate physicists who were helping me, Anita Evans and Mary James, discovered that the bedding of the sedimentary rocks within the Sudbury Basin could be traced on air

photographs into the so-called granite or micropegmatite part of the Sudbury intrusion. This showed that the micropegmatite was not really part of the intrusion at all, but its baked upper cover. Unfortunately, we never published this, but the results strengthened my view that much of the Canadian Shield was made of cooked sediments. Although unorthodox at the time, it was, I realized, a return to an earlier view.

These two very capable students and several others did a remarkable job of examining the air photographs of about a million square miles of Canada. When combined with the work of others this resulted in the first glacial map of Canada, which the Geological Association of Canada published for the International Geophysical Year in 1957. This provided a splendid picture of the previously unknown effects of the ice age over vast areas. Again I failed to publish the interpretation which the work deserved.

The map showed clearly for the first time that the great ice sheets flowed away from linear divides, which corresponded to watersheds, and not as had been previously believed, from point centers. It showed how most of the central ice flowed south out of Hudson Bay across Ontario to melt in the northern United States, rather than out through Hudson Strait. It demonstrated that over the whole interior of Canada, moraines are scarce. This was unexpected because in the southern regions that had been mapped they are abundant. This distribution makes sense because moraines are dumps of debris left where the ice melts, but they should not form in regions in which the ice is actively moving. The map also showed that the carrying power of ice, like that of water, appears to increase with the velocity of motion. The marginal areas where the surface of the ground dips most steeply away from the raised Canadian Shield have been denuded of soil more than other areas where the ice motion would not have been as fast. This offered an explanation for the superb exposures around the north shores of many large Canadian lakes, such as Great Slave Lake or Lake Huron, and how it was that those pavements of rock are in such contrast to the soil-covered barrens and swamps of much of the interior of the Shield. Much else could be seen, but overall the results showed that to understand the earth one had to assemble a grand view of the earth. Individual little maps might be accurate, but they were hard to interpret.

The activities of the IGY led me away from these efforts, and when I got back to research in about 1960 I recalled the remote ocean islands I had seen and started to compile their geology. This was not easy because most of them had only been examined on chance expeditions and the results of many of these expeditions were published in obscure places. Fortunately I discovered a clue to locating them. I found by good fortune that the standard bibliographical work which anyone would use for references was inconsistent in dealing with small islands, and frequently listed different references to one island under several headings. Thus it might list different references to one

island, such as Santa Maria, under these headings: Santa Maria, Azores, Portuguese Colonies, and Atlantic Ocean. If one only looked under one heading, one would miss many important references. With this knowledge, I made a better compilation of what was known about the islands. This information, combined with the work that Harry Hess and H. W. Menard were producing, provided a basis upon which to support Hess's concepts of seafloor spreading and my own ideas upon the hot-spot origin of many of these islands.

Hess's ideas, built upon a discovery by Ewing and Heezen that the rifts in the mid-ocean ridges provided sharp loci for the places where separation and parting took place, led me to think of transform faults. Once I had that idea it became obvious that the stability of the Canadian Shield (which had so long been an impediment to my acceptance of continental drift) could be understood if a few great faults had broken the surface of the earth into rigid plates. Then movements and earthquakes would be largely confined to their sharp boundaries. Menard's views also helped me to visualize the uplift of continents where they have overridden hot spots and mid-ocean ridges.

Later Kevin Burke and Bill Kidd were kind enough to include me in their more extensive investigations of hot spots. I believe that hot spots are long lived and relatively stationary and that their locations determine the lines along which continents break up. Since hot spots only move slowly relative to one another, they provide a reasonably good frame of reference to the absolute motion of continents over the interior of the earth.

By that time it was 1967, and the excitement of the recognition of the role of great faults and of the existence of plates and their relative motions (which others were working upon) had reached its peak. I considered myself fortunate to play even a small part and I could not see anything else that I could obviously do. It was an appropriate time to move on, and by chance, Claude Bissell invited me to build a new campus, Erindale, of the University of Toronto. I was doubtful about making a move, but when my wife and I went to see the 300 rolling acres with one handsome mansion beside a river she was delighted and I agreed. The twenty years of scientific research which C. J. Mackenzie had suggested and which I had enjoyed was up. I was glad to try something fresh and I realized that unless senior academics took their turn at administrative jobs, universities would suffer.

We moved into the house, where Isabel entertained 10,000 people in the next seven years. By good fortune Peter Robinson was among the very few staff already recruited and he became the Dean. Like many chemists, he was an excellent adminstrator and he had looked after all the details. We both regarded the project of designing a campus and buildings, and of recruiting 3,000 students and the staff to teach them, if not as scientific, at least as a research enterprise of another sort. We certainly tackled the problems like research projects. I also believe that in directing staff and students and in

negotiating with higher authorities, the experience that I had gained from chairing large international gatherings was valuable. In both circumstances, persuasion and goodwill are the only routes to consensus.

So far from having any regrets, my wife and I found Erindale to be a delightful experience, one much more interesting to me than continuing to get deeper into the same ruts in research. One achievement which pleased me was that by recruiting the help of a psychiatrist, I was able to persuade the authorities to throw out $100,000 worth of architectural drawings and to start again with a simpler concept.

In 1974 when I had reached 65, the university regulations required that I cease to be in charge of any administration. Oddly enough the Premier of Ontario offered me the job of directing the Ontario Science Centre, which I still do.

This has been most rewarding, for the Ontario Science Centre is a novel, perhaps unique, institution. There are several small science centers in the world and many science museums, large and small. Some of the latter have some aspects of centers, but no other institution is so large and so fully a science center as this one. This requires explanation, because most people are quite confused about the differences between science museums and science centers.

A museum deals with history, and its original collections are all-important. Copies of original artifacts are of little value. In the whole field of the arts nothing can be said about the future of the arts and there are no precise mathematical principles. Thus, there are no other related institutions. In science, the situation is different. One can make a great many precise forecasts, such as times of sunrises, eclipses, or seasons, and one can state the precise principles upon which these depend and relate them to such mathematical laws as those of mechanics, gravity, and optics.

No artifacts to illustrate these principles and physical laws can be collected, but models can be built to illustrate all of them, and these, rather than collections of ancient artifacts, are the stock-in-trade of science centers. Thus, besides science museums, which hold stocks of artifacts, there are science centers which show manufactured models. Everyone knows that the best way to teach science is by doing experiments. Science centers are in effect public science laboratories and they are exceedingly popular. To point out this difference upsets many curators, but the solution lies in their own hands. Many, if not most, institutions are a mixture. It is perfectly reasonable to display what artifacts one can alongside participatory exhibits that enable the public to try scientific experiments with their own hands.

In directing such a center, a wide experience is useful. Among the staff of 250 are a wide diversity of skilled people representing more than 20 countries. Some, like teachers and craftsmen, are conservative. Others, like scientists

and designers, tend to be more radical. Some, such as bookkeepers and security guards follow the rules very carefully while others, like public relations officers and writers, seek the new and the unusual. Visitors and callers come from all over the world. A few among the million and a half visitors a year get themselves and the Centre into unexpected predicaments.

Nor is a science center a static affair. Under my predecessors and myself, the Centre has staged many temporary exhibitions on all manner of subjects. The latest one, which we have agreed to hold during the summer of 1982, will illustrate with the help of many Chinese demonstrators the achievements of Chinese science and technology through 3,000 years. My past visits to China and my friends there have proved to be invaluable in arranging it. By interchanges of visits, the Chinese designers and ours are studying how best to combine the Chinese knowledge of their history and their great artifacts with a science center's desire for active demonstration and public participation.

The Centre has many other activities and opportunities. It has dispatched travelling exhibitions to hundreds of places. It produces and gives away over 50,000 copies each month of its small newspaper. It has organized the industrial manufacture of copies of its exhibits, has started a day school on the premises, and launched a society to popularize science. Its successful operation requires imagination and a good background in science, but not the intense application of original research.

I appreciate that I am extremely fortunate to enjoy so varied and interesting a life, and in concluding I wish to thank innumerable people all over the world for their help and friendship. Most have been in Canada and the United States where my colleagues have been exceedingly generous, but nearly as many are in Britain and Australia where I have also worked extensively. I hope this account illustrates how delightful and rewarding a life in earth sciences can be. I cannot help noticing that while some scientists have achieved great success by staying with the same field of research throughout their lives, others have also succeeded in changing fields and in so doing transferring ideas obtained in one situation to other quite different circumstances. Certainly my own life has been and continues at age 73 to be varied and full of excitement and happiness.